Montana Wildfire Cost Study: Explanation of Methods and Data Sources

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Summary:

The research conducted by Headwaters Economics, done in coordination with Dr. Mark Greenwood at Montana State University, explored what is seen by most fire experts as an increasing public policy problem—the rapid rise of fire suppression costs and its relationship to residential growth in Montana's wildland-urban interface.

The work was done with the cooperation of State Forester Bob Harrington and other staff at MT DNRC. These individuals, along with Dr. Steve Running at University of Montana also scrutinized the study, and the research since has been presented at the Western States Fire Managers 2009 Spring Conference and the National Association of State Foresters, Forest Fire Protection Committee Meeting in Helena this past June. Attendees of these meetings included directors of fire management offices in the Department of Interior, US Forest Service, and BLM, directors and chairs of various wildfire organizations, including the National Interagency Fire Center, the Western Forestry Leadership Coalition, and the International Association of Fire Chiefs, and state foresters from a dozen states. Top-level fire managers at these meetings communicated to us that the paper's findings were both informative and consistent with their experiences.

In this light, it is difficult to understand the criticisms offered by Dr. Polzin and Dr. Davis. The statistical methods we employed are neither unusual nor seldom used. While their review of the methods used in the paper point to some limitations of the statistical analysis, they do not offer opinions on how they would improve the study design or what models they would consider useful.

Instead, their critique selectively refers to, and in some cases misinterprets, portions of the statistical literature and wildland urban interface studies that confirm their position, while ignoring other relevant portions of these same papers. In addition, Dr. Polzin and Dr. Davis seem to be unaware of methods that constitute a vast area of statistics and applications of statistics.

There is not one way of arriving at a final statistical model(s) and, to quote Box and Draper (1987) "Essentially, all models are wrong, but some are useful." We attempted to build statistical models that could usefully predict daily wildfire cost data. Based on our analyses, which were conducted by experts in the field of statistics, we attempted to better explain some of the underlying aspects related to observed variation in daily costs of suppressing wildland fires.

We would welcome a more informative review of the statistical methods from an unbiased professional statistician. Dr. David Patterson at University of Montana and Dr. Jim Robison-Cox at Montana State University have expertise in the methods utilized in our study and could provide more objective and useful reviews.

Our responses to Dr. Polzin and Dr. Davis' criticisms are listed below.

"The conclusions of the Headwaters Wildfire Cost Study are based on a statistical analysis employing a complex and seldom-used methodology."
(Page 2, first and second paragraphs; Page 6, paragraphs 2, 3, and 4.)

The statistical methods employed are not unusual or seldom used. The general theory behind these methods (mixed statistical modeling) is the basic analysis used in the extension of regression models to accommodate correlated responses in repeated measures, longitudinal data, linear mixed, generalized linear mixed, and some spatial and spatial-temporal statistical models (Pinheiro and Bates, 2000).

Mixed effect statistical models are the primary set of tools for inference in longitudinal data analysis and are used in spatial, temporal, and spatial-temporal regression models. They constitute a vast area of statistics, and applications of statistics, that the reviewers must not have been exposed to.

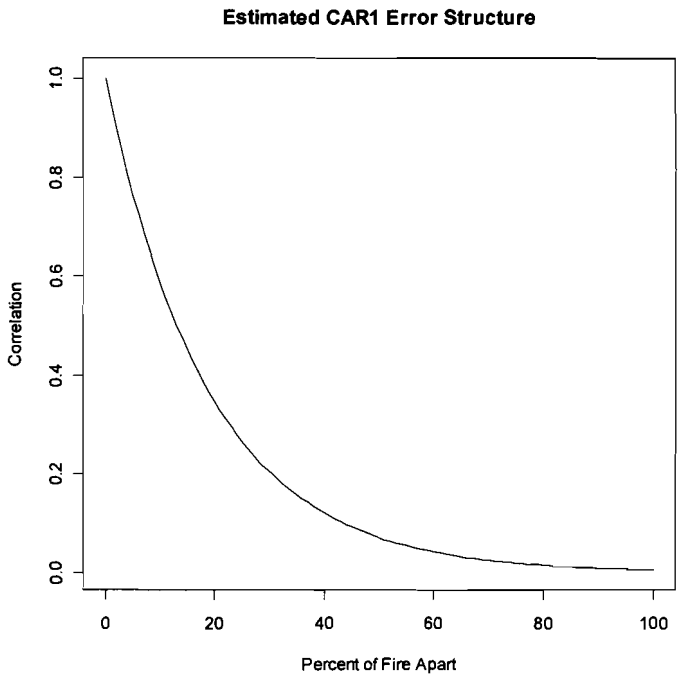
In fact, one of their references, Christ (1993), uses a special case of this structure. Our use of this correlation structure is not solely to incorporate regular patterns as Dr. Polzin and Dr. Davis suggest. Its purpose is to adjust the inferences using the regression coefficients based on correlations between neighboring observations.

A discussion of the CAR(1) error is available in Pinheiro and Bates (2000) and the paper where the methods were originally proposed for longitudinal data, Jones and Ackerson (1990). Other uses of CAR(1) errors are in Mackenzie et al. (2005) and Garber and Maguire (2003). Additionally, their reference to Mac Nally (2000) in this paragraph is both confusing and seems misplaced, as that paper suggests using the BIC (a competitor for AIC) when the purpose is to find an optimal predictive model.

Additional information:

Our methods allow an extension of regular regression models to deal with correlated data of various types as well as random effects. The CAR(1) error structure is used for longitudinal data that are measured unequally in time and is also equivalent to the exponential spatial correlation structure, possibly the most popular correlation structure in spatial statistics. If the observations are equally spaced, it is also equivalent to the AR(1) error structure that is one of the more common longitudinal correlation structures used.

The implied correlation structure, as a function of percentage of the fire that is completed, is displayed below. The correlation between observations decreases as the percentage of the fire two observations are apart increases, starting at 1 and going to 0. This is similar to an AR(1) error structure, except that it can be calculated for any difference in percentages of the fire that has completed.



"The Headwaters Wildfire Cost Study does not appear to be peer-reviewed. The peer review process is an integral part of the scientific method and a prerequisite for publication in professional journals."
(Page 2, second and third paragraphs)

Although our study has not yet been published in a professional journal, this does not mean the results are without merit.

The study has been scrutinized by Bob Harrington and other MT DNRC staff that aided in the project. The current draft of the manuscript received a favorable review from Dr. Steve Running from University of Montana

The study was also presented at the Western States Fire Managers Spring Conference (Coeur d'Alene, March 2009) and the National Association of State Foresters, Forest Fire Protection Committee Meeting (Helena, June 2009). Attendees of these meetings included directors of fire management offices in the Department of Interior, US Forest Service, and BLM, directors and chairs of various wildfire organizations, including the National Interagency Fire Center, the Western Forestry Leadership Coalition, and the International Association of Fire Chiefs, and state foresters from about a dozen states. Top-level fire managers at these meetings communicated to us that our findings were both informative and consistent with their experiences. A list of attendees is provided in Appendix A.

The study does not support the claim that, "Firefighting costs are highly correlated with the number of homes threatened by a fire."...

(Page 2 and 3, bullets)

Ignoring everything else, the correlation between daily cost and homes within a mile of the fire is 0.41. Once we account for other important variables, we find evidence for the inclusion of a "homes at risk" variable in the models, which also suggests its importance. Homes threatened by fire is an important explanatory variable in our regression models.

Criticism of study design: cost data, sample size and other potentially relevant explanatory variables
(Page 8, bullets)

Starting in FY2005, the accuracy and availability of fire cost information has improved because of a cross agency reporting and data storage application called I-SUITE. This is where our daily costs data came from, which is why our sample of fires were either from 2006 or 2007.

We analyzed all the fires in which MT DNRC contributed resources and for which incident data (both daily costs and GIS data) were available. We agree that a larger and more diverse sample is needed and are in the process of expanding the study for submission to a journal to include additional fires so that we can make inference to all the large fires in MT, not just ones that MT DNRC was involved in. However, for the study funded by the Fire Suppression Subcommittee, it was reasonable to constrain the fires to just those involving MT DNRC to ensure relevance to MT legislators.

We collected all information on potential drivers of fire suppression costs that could be gathered and quantified in the time we had to conduct the study. Excluding housing and costs variables, we collected information on 16 variables related to such factors as terrain, weather, and threatened infrastructure. That we did not have information for every factor that may influence costs in no way negates our findings. Our methods allowed us to estimate the effects of the factors that we were able to collect information for.

Review of other studies
(Page 7)

A careful review of the four empirical studies of fire suppression costs cited in the critique reveals that only one disagrees with our findings, two agree with our findings, and the fourth neither supports nor disagrees with our findings. Explanations of similarities and differences between these studies and ours are presented below.

Donovan. This study is the only one that fails to find a relationship between housing and cost. Unfortunately, the researchers used highly inaccurate data to represent fire suppression costs. Total estimated costs were calculated from the 209 forms submitted by fire crews. From our experience working with these data, they are inconsistent with the real costs. For example, the same estimated cost will often be repeated for subsequent days on 209 forms. The Gebert paper also notes that these data are highly inaccurate. In addition, Donovan acknowledges that the sample may not have contained any fires that did not threaten homes. Donovan speculates that "the region is sufficiently densely populated that wherever an ignition occurs, houses will be threatened, and fire managers will suppress wildfires accordingly." If you do not have non-WUI

fires in your sample, you cannot possibly estimate the effect of the WUI. These are two major problems with this study that likely affected their findings.

Liang. Due to differences in methods, these studies may not be directly comparable. However, some quotes from Liang 2008 demonstrate that their findings agree with ours:
"Compared with all the other variables, fire size, perimeter to area ratio, percentage of private land, and total structure value had substantially higher independent effects... Expenditures were negatively associated with perimeter to area ratio, and positively correlated with percentage of private land and total structure value..."
"After accounting for fire size and the percentage of private land, total structure value and percentage of the wildland–urban interface area (WUI) had no effect on suppression expenditures... However, total structure value and percentage of the wildland–urban interface area could be indirect factors, as they also reflected private development, and were highly correlated with percentage of private land... most fires with more than 20% of burned area as private land were close to towns."

Gebert. Again, this paper uses a different statistical approach. However quotes from this paper show that their findings were also consistent with ours:
"Variables having the largest influence on cost included fire intensity level, area burned, and total housing value within 20 mi of ignition."
"The total housing values within 5 and 20 mi of fire ignition were included in the models as a set, because statistical tests indicated that their predictive power was higher than if only one was used. Both variables suggest that as housing values increase, so do costs; however, only the housing value within 20 mi of fire ignition was statistically significant."

Canton-Thompson. The Canton-Thompson paper neither supports nor disagrees with our findings. The authors identified specific pressures outside the influence of incident team managers control that are likely contributing to increases in suppression costs. Although the authors speculate that incident management teams may not have control over all fire cost suppression factors, it is apparent that someone along the management chain is making decisions that do influence costs. The extent to which structure protection weighs into these decisions was not analyzed in this study.

It is important to note that, by analyzing daily costs, our study examines wildfire suppression costs in a way that other studies have not attempted. The three applicable studies listed above look only at cumulative costs per fire, not daily costs. Analyzing daily costs allows us to understand costs in ways other studies could not, and possibly identify variables as important that other studies could not, since they looked only at cumulative costs.

In addition, no other study looked at counts of threatened homes. Housing value averaged over census tracts or blocks were used by these other studies to estimate threats to development. This representation of "threatened homes" may be inadequate for several reasons. Both census tracts and blocks are extremely large in rural areas (sometimes tracts are the same as county boundaries, sometimes there are only 2 or 3 tracts per county). Also, home value may not factor into fire management decisions in the same way as numbers of threatened homes. For example,

fire managers may or may not spend more resources protecting expensive homes than average valued homes.

In addition to this discussion of empirical work, there are numerous other references to the increasing costs of wildland fire suppression caused by growing residential development within the wildland-urban interface. These references are made by the agencies responsible for implementing firefighting efforts: those with direct line experience and responsibility for allocating public funds to protect resources during a fire. Collected in Appendix B is a small sample of citations that address the issue of fire suppression costs in the wildland-urban interface.

Criticism of the Homes1 and Homes6 models
(Page 4)

The Homes1 model included, among other important variables such as fire size, the number of homes within 1 mile of each wildfire on each day that the fire burned. This model was built to explain the costs per day, which is how we arrived at the estimate of $344.90 in increased costs per home per day that is within a mile of a fire. We agree that it would have been appropriate to include confidence intervals (CIs) in the report presented to the Fire Suppression Subcommittee. However, the 95% CIs are reported in a draft of the scientific manuscript that has been available to the public since April 2009 via a prominent link in the Headwaters Economic website. The 95% CI for the daily cost of having a home within a mile of a wildfire is from $50.77 to $639.03. The interval does not overlap zero, indicating that after adjusting for the other variables in the model, daily costs are positively related to the number of homes within one mile of a fire.

We attempted to provide an estimate of the total cost for each additional home within a mile of the fire for an average home that was encountered in the study. We chose a representative value of 23 days, changing the units from cost per day to cost per 23 days as that was how long a typical home was within one mile of a fire. We can incorporate the variability in the days across the fires into this result to provide an estimate for the true effect across all the fires in the population, assuming normality and independence between the slope coefficient and the average number of days each home is near the fire. This does not change the point estimate of $7,932.7 that the report discusses. The variation in the mean number of days per home is quite small since the number of unique homes encountered in the data set is very large (2,176). Our preliminary results suggest little change in the CIs when we incorporate the variability in the number of days. We are working on the methods we can use to incorporate this additional variability and will update our manuscript to include these results when available.

The Homes6 model included, among other important variables such as fire size, the number of homes within 6 mile of each wildfire on each day that the fire burned. Dr. Polzin and Dr. Davis are incorrect in their assertion that we used "the average number of days a home is within a mile of the fire perimeter (23) to derive the increase in suppression costs (53.92 X 23 = 1,240) for homes within six miles of wildfire." On average, homes proximate to our sample of wildfires were within six miles of a wildfire for 23 days. Again, the variation in mean number of days per home was quite small.

Criticism of the Acres1 model

(Page 4)

The Acres1 model included, among other important variables such as fire size, the acres of residential development within 1 mile of each wildfire on each day that the fire burned. Because this model does not include fire size as an explanatory variable, the results are not directly comparable to the previous results, as Dr. Polzin and Dr. Davis note. The discussion of this model was meant to compare the results between similarly ranked models and understand how they worked. The fact that our translation of the Acres1 slope provided similar results to the other development variables for a typical lot size was an interesting coincidence that we should have emphasized less. If we include fire size, which is somewhat collinear with the number of developed acres within one mile of fires and leads to a model that is not preferred based on AIC, the effect for the developed acres variable is $20.98 (95% CI from $6.03 to $35.93).

Numerous re-estimates criticism
(Page 5)

The number of models that were fit does not reduce the usefulness of our findings. First, we employed some of the models in order to assess the correlation structure to be used. Second, we fit numerous models to understand the range of the housing related variables that was optimally related to daily costs.

Additional information:

Essentially, each distance driven GIS-derived variable in the models contains two parameters, a window width or range for the region around the fire and a slope coefficient. This can be clarified if we write the Homes variable in the manner used in nonparametric regression models that have similar issues in selecting a transformation of the original variable and then associated coefficients (Wood, 2006). For example, we can state the term for Homes as s(Homes), implying that we need to estimate a smooth function of homes, specifically seeking a maximum likelihood estimator of s(Homes). Our top model used a one mile range to the fires (Homes1) which was entered into the regression model then as βHomes1. At different ranges, the different transformations may be more or less collinear with other variables, so we select the accompanying suite of models at the same time as we select and estimate s(Homes). While different from the models in Wood (2006) in the details, this process shares similar general traits.

For models that have the same suite of additional variables, AIC selection of the range in these derived variables is equivalent to maximum likelihood estimation, a highly regarded parameter estimation paradigm. We report results from the top models to reflect some of the uncertainty in the different models and to allow the reader to see the common threads to some of the estimates, even in the presence of different explanatory variables.

Discussion of model selection and p-values
(Page 5)

This discussion seems to suggest that finding explanatory variables that are highly correlated with the response variable is a bad thing. Almost all methods of model selection/ building will tend to identify important variables in some fashion or another. One of their references for this

flaw in model building, Christ (1993), discusses selecting the order of a polynomial regression model. This is one of the classical success stories, called the curve-fitting problem, used to motivate model selection criteria such as the AIC. It is often able to avoid including extra variables just because they are related to the response variable, if they are not "needed".

While we agree with the discussion of adjustments for multiple testing, it is important to note that p-values were not used to build or in the discussion of our models, but were only presented as a courtesy to those more familiar with those methods of building models. In fact their reference to Mac Nally (2000) states our perspective on our model building quite well: "Selection of the 'best' model by using criteria [such as AIC or BIC] avoids the complications with type-I error rates in sequential searching protocols because no explicit statistical tests are conducted" (p. 662).

Additional information:

We agree that there is a potential issue with over-optimism if tests are performed after model selection. We were not testing in any of our work. We used a model selection method (minimizing AIC) to seek to identify a model that would minimize prediction error across all possible samples that we could have observed like ours (Akaike, 1973). When AIC selects the correct model, Hurvich and Tsai (1990) found that the coverage rates for confidence intervals from AIC selected models actually perform reasonably well, but that the performance drops off as more underfit or overfit models are selected. This suggests that we are hoping that minimizing AIC has helped us select a model that is close to "correct" to get our inference correct, just as we are hoping that AIC has selected a close to "correct" model when we discuss any aspects of the top models. There is a developing set of literature on adjusting models for over-optimism due to model selection, but it is still evolving and we did not choose to employ any of the methods as these methods are not commonplace in the literature; Claeskens and Hjort (2008) do discuss this issue.

This makes Table 1 that contains an adjustment of our p-values a mute point. However, Dr. Polzin and Dr. Davis appear to be using a Bonferroni type adjustment based on the number of potential tests that could be considered if model building was performed using testing of nested models. Again, we did not use these tests to build our models (these tests are also not useful for comparing non-nested models which describes many of our models). Bonferroni (1936) adjustments are known to be quite conservative and much research has been conducted since 1983 on alternative methods for adjusting p-values. Also, we would disagree with their characterization of Ramsey and Schafer (p. 47) as suggesting that corrected p-values of 0.4125 and 0.2812 are in the "definitely reject" range, but we suspect this was a typo. In fact Ramsey and Schafer (p. 47) are careful to use shades of grey in their "decision rule" for p-values. The correct interpretation of these levels of p-values would be to provide no evidence of a difference of the coefficients from 0, in the presence of the other variables in the model. Even this does not say that those variables are not important, just that in the presence of the other variables, the evidence was weak for including them in the model.

It is important to note that AIC-based model selection is a different paradigm of model building and does not rely on or even have any ties to hypothesis testing. To be successful, model

building using AIC requires the presence of a good model, as it will attempt to select the "best" model in the candidate set of models. We may have erred on the side of many candidate models, but many were discounted based on AIC values compared to the top models, suggesting little to no weight on those models and minimal impacts of their consideration in the modeling process.

Dr. Polzin and Dr. Davis' statement that AIC is analogous to R-bar-squared (adjusted R^2) is not correct. AIC is justified based on attempting to minimize the expected average prediction error, compared to R^2_{adj} that is similar to R^2 that measures the percentage of variability in the responses that was explained. AIC does not suffer from optimism as R^2_{adj} does when used to select and describe models with the same data set. In fact, the AIC selects the "correct model", both in terms of the correlation structure and explanatory variables quite frequently in simulation studies where the sample size and signal to noise ratio provide enough information to detect the "true" structure. While the adjusted R^2 does contain a penalty for adding coefficients, compared to R^2, it is not generally recommended beyond introductory statistics textbooks as a way of performing model selection and is never recommended as the sole method of selecting a model.

Criticism of cross validation

The criticisms of cross-validation (CV) are overly harsh. Despite initial criticism's of Stone's 1974 paper, the technique has grown in popularity. Hastie et al. (2009) describe CV saying "Probably the simplest and most widely used method for estimating prediction error is cross-validation." (p. 241) and "..., cross- validation and related methods may provide reasonable estimates of the *expected* error..." But they also mention that it can be highly variable in practice. In simulation studies, we have seen the AIC and related measures dramatically outperform CV in model selection due to its decreased variability (Greenwood, 2006).

CV is used throughout the statistics and the data-mining literature successfully to identify optimal predictive models and to evaluate their performance. It is not ideal and it is better to withhold a portion of the data set to truly validate a statistical model. However, our use of CV was to evaluate how our model would predict each fire, if we had not originally included the fire in our sample. This is a reasonable way to assess model performance especially when the data set is not large enough to support withholding some of it for model validation. It allowed us to assess our performance on each of our fires and discover that the average prediction error grew with the average daily cost of the fire. We adjusted our error structure to reflect this discovery.

Additional information:

We would prefer to use the three stage method discussed in Mac Nally (p. 668, 2000), a method recommended by many others, but the population of fires that met the criteria of this study was too small to consider reducing the data set further. Our reported models use all observations, something that Christ (1993) actually recommends along with his "scathing" attack on cross-validation in a time series forecasting situation. Regular cross-validation is actually different than the contexts of Christ's paper and it is not clear whether he is really thinking about CV. Removing observations in correlated data breaks the correlation with neighboring observations, making typical cross-validation in time series modeling unusual and potentially incorrect if applied ignoring this. So it is unlikely that Christ was thinking of standard cross-validation in his

discussion, instead thinking about with-holding the last few time points to check model performance. This issue with CV and correlations motivated our idea to remove each fire instead of removing some proportion of observations randomly as in typical CV.

Our models attempt to account for correlations over time. We use the models to understand relationships between daily characteristics of fires and daily costs, after adjusting for correlations in the daily cost measurements. Interestingly, AIC and CV-based model selection are asymptotically equivalent, although the AIC tends to have more stable performance in moderate sample sizes.

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Appendix A: List of attendees at the National Association of State Foresters Meeting in June 2009

Federal Agencies:
Kirk Rowdabaugh - Dir. of the Office of Wildland Fire Coordination, Dept. of Interior
Tom Harbour - Dir. of Fire and Aviation Management, US Forest Service
Lew Southard – Branch Chief Fire Prevention (Fire and Aviation Management), US Forest Service
Jim Douglas - Ass. Dir. of Fire and Aviation Management, BLM

Fire Organizations:
Jeff Jahnke - Dir. of Colorado State Forest Service, chair of the Nat'l Assoc. of State Foresters Forest Fire Protection Committee
Dan Smith - Fire Dir. for the Nat'l Assoc. of State Foresters, Nat'l Interagency Fire Center/BLM
Keith Smith - National Fire Plan Manager for Technical Support, National Interagency Fire Center/BLM
LouAnn Gilmer – National Association of State Foresters Finance Director
Douglas MacDonald - Chief of Los Alamos County Fire Dept., Int'l Assoc. of Fire Chiefs liason
Don Artley - Montana State Forester (Ret.), chair of the Nat'l Wildfire Coordinating Group
Caitlyn Peel – Executive Dir., Western Forestry Leadership Coalition

State Agencies:
Vicki Christiansen - Arizona State Forester
Pete Anderson – Nevada State Forester
Tom Boggus – Texas State Forester
Jim Karels – Florida State Forester
John Burwell – Oklahoma State Forester
Jim Erickson – Washington Intertribal Timber Council
Leanne Heisel – Montana Legislative Services Division
Barb Smith – Montana Legislative Fiscal Division

Appendix B: Citations that address the issue of fire suppression costs in the wildland-urban interface.

Just this week, the Government Accountability Office released its latest "Wildland Fire Management" review finding, as it had in previous studies, that "the conditions contributing to the nation's fire problem have worsened—with increasing development in the wildland-urban interface, a continued excess of accumulated fuels, and growing evidence of the effects of climate change." (Source: GAO 09-877, September 2009.)

Earlier this year, Pamela Haze, the Acting Assistant Secretary of Interior, testified before the U.S. House of Representatives that: Wildland fire and wildland firefighting are influenced by a complex myriad of environmental and social factors. Fires in recent years have become larger, consuming more acres, and fire seasons have grown longer due to climate change[1], and persistent drought and hazardous fuels accumulations.[2] The historical expansion of development within the wildland urban interface (WUI) has increased the complexity of fighting wildland fire. These trends are not expected to change....Additionally, although current economic conditions have slowed development in wildlands, regional shifts in population and demographic trends point to more seasonal recreational homes and full time residency in areas adjacent to public lands. These factors, as well as the management framework and decisions during some fire incidents, have caused costs to rise and increased annual fire suppression expenditures that have frequently exceed the 10-year average. (Source: http://appropriations.house.gov/witness_testimony/INT/Pam_Haze_04_01_09.pdf.)

In 2007, Mark Rey and Nina Hatfield, the respective heads of the Forest Service at the Department of Agriculture and for Wildland Fire at the Department of Interior, issued joint testimony to the U.S. Senate, noting: "The review illustrates the challenge of addressing wildland fire costs in land areas, such as locations in the WUI, where fire suppression is inherently more expensive." (Source: Joint Testimony, January 30, 2007.)

In 2007, the President of the International Association of Wildland Fire noted: Firefighting in the wildland-urban interface is contributing significantly to increased suppression costs. During fiscal years 2003 and 2004, about 87% of the Forest Service's firefighting expenditures on 37 large fires (with suppression costs exceeding $5 million per fire) were spent protecting private property—and some within the Forest Service have reportedly estimated that up to 97% of large wildfire costs are directly related to protecting private property and homes in the interface. This is big money in a time of generally tight, if not actually decreasing, federal budgets. (Source: http://www.iawfonline.org/blog/?p=185.)

In 2006, the Department of Agriculture's Inspector General's report, "Forest Service Large Fire Suppression Costs," noted that the Forest Service's "escalating cost to fight fires is largely due to its efforts to protect private property in the wildland urban interface bordering Forest Service lands." (Source: http://www.usda.gov/oig/webdocs/08601-44-SF.pdf.)

Also in 2006, a Forest Service report, "Factors Affecting Fire Suppression Costs as Identified by Incident Management Teams," found: "In the aggregate, the rise in expenditures can be related to

known factors such as increased residential development in former wildlands..." (Source: http://www.fs.fed.us/rm/pubs/rmrs_rn030.pdf.)

In 2004, a report by the Wildland Leadership Fire Council discusses the rapid growth of the WUI and the related increase in costs due to "increasing the values at risk and the social and political pressures to extinguish fires at all costs, regardless of the futility of the effort." (Source: Large Fire Suppression Costs, Strategies for Containment, August 26, 2004.)

In short, this problem has been identified and understood for many years. Ten years ago, the Forest Service released a white paper at an academic conference, "Strategic Holistic Integrated Planning for the Future: Fire Protection in the Urban/Rural/Wildland Interface" by Glenn Snyder, the Branch Chief of the Cooperative Fire Program. Snyder's paper concludes: "As long as Federal wildland fire fighting agencies continue to absorb protection and suppression costs for indirect structure protection in the name of wildland fire protection, we can only expect a corresponding escalation in wildland protection cost." (Source: http://www.fs.fed.us/psw/publications/documents/psw_gtr173/psw_gtr173_04_snyder.pdf.)