To the San Diego Fire Recovery Network, 1/26/04

This letter is in response to the San Diego County Wildland Fire Task Force Findings and Recommendations August 13, 2003 report entitled “Mitigation Strategies for Reducing Wildland Fire Risks” as prepared for the San Diego County Board of Supervisors. We were disturbed by the way our research findings were completely mischaracterized in this report on page 8. Not only are the specific statements about our findings completely false, but also, more generally, our research does not support the claims and recommendations of this section of the report.

We turn first to the three sentences on page 8 of the report specifically pertaining to our research, These three sentences contain a host of mistakes. The report states:

> At UCLA, two mathematicians (Peng and Schoenburg) analyzed the Los Angeles Malibu fire regime from a statistical and physics perspective. They were aware of the debate over fuel-driven fires versus wind-driven fires and they concluded that, statistically, fuel was the limiting factor. Their illustration below provides a dramatic illustration of the difference between a landscape shaped with almost no fire suppression activity in Baja California compared to San Diego County's landscape, where highly efficient fire suppression forces are employed.

The first sentence contains some strange minor errors. We are not mathematicians, but statisticians (statistics and mathematics have been separate departments at UCLA since 1998). Schoenberg's name is mis-spelled. We analyzed fires in Los Angeles County, not specifically Malibu. We did not use a physics perspective.

The second sentence contains more serious errors. We never concluded that fuel was a more significant or "limiting" factor compared to wind. We assume the report is referring to either our International Journal of Wildland Fire (IJWF) paper or our Environmetrics paper which is still in review but available as a preprint online (it is difficult to tell which paper since we are not listed in the bibliography). In both of these reports we stated our belief that wind is a very significant factor in wildfire risk, and at no time did we ever claim that fuel age was a more significant factor. In fact there is to our knowledge no basis whatsoever for such a claim. In our IJWF paper we stated that fuel age and wind both seem to be risk factors for monthly area burned, but we never statistically analyzed wind at all, because, as we said in our Discussion, "Wind is known to have a particularly pronounced impact on fire incidence and spread (e.g. Viegas 1998), but wind data is not amenable to the type of analysis performed here.... Hence the analysis of the effect of wind on burn area requires a fundamentally different type of analysis than that employed here." In the Environmetrics paper, we again do not analyze wind at all, and we say in the Conclusions, "The focus on fuel age by no means is meant to underemphasize the importance of other factors in influencing fire risk. These other factors include land use policies, population density, and fire prevention policies, as well as meteorological and topographic variables.... [W]ind is a major factor affecting the size of wildfires. Large catastrophic fires are often driven by high winds and are generally immune to fire suppression."

Regarding the third sentence, the illustration in Fig. 5 is not ours but that of Minnich (1983), and in our Environmetrics paper we state "Despite the fact that Minnich's paper was highly

influential and was used as a support for modern prescribed burning policies, many other works contradict his findings." We then refer to Van Wagner (1978), Johnson and Larsen (1991), and especially Keeley, Fotheringham and Morais (1999), all of whose findings contradict the conclusions of Minnich (1983).

In our research, our main conclusion regarding fuel age and its role in wildfire hazard is simply that wildfire risk does not appear to increase linearly with fuel age, but instead seems to level off after a certain age. In other words, extremely old fuel does not correspond to extremely high burn risk. As we conclude in our IJWF paper, "A possible interpretation is that large wildfires occur primarily when conditions for their ignition are ripe, but that there is little distinction in terms of wildfire risk between conditions that are sufficient for wildfires and those that are extreme. Ours is somewhat similar to the conclusion arrived at by Keeley et al. (1999), who found that large catastrophic wildfires in Southern California 'are not dependent on ancient stands of brush.'"

Thank you for allowing us to clarify our position.

Frederic Paik Schoenberg
Roger Dean Peng

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Jon E. Keeley, Station Leader

TO: San Diego Fire Recovery Network
FROM: Jon E. Keeley, Research Scientist
RE: Report review
DATE: January 17, 2004

Thank you for sending me the report entitled "Mitigation Strategies for Reducing Wildland Fire Risks" prepared for the Board of Supervisors by the San Diego County Wildland Fire Task Force, August 13, 2003. Before using this report I believe there are some issues that you should consider.

In overview, the Wildland Fire Task Force report does not adequately reflect the full range of scientific results that are pertinent to the question of wildland fire risks and management options in San Diego County. It ignores a vast body of scientific literature bearing directly on how to effectively reduce threats of catastrophic fires in shrubland ecosystems, including numerous books and papers published in prominent scientific journals like *Science*, *Nature*, *Ecology*, *Conservation Biology*, and *Environmental Management*. This report contains an undeniable bias against work that suggests vigorous and expensive fuel manipulations in the backcountry of San Diego are not an effective means of reducing the current fire hazard situation at the urban / wildland interface. For example, within the past 5 years I have published more than 10 peer-reviewed articles in national scientific journals that presented evidence directly dealing with southern California and questioning the cost-effectiveness of broad landscape-scale prescription burning. None of these papers were cited. The only reference to this work was a fictitious bibliographic entry under a title that I have never published and with a combination of co-authors with whom I have never published any article. This citation lists Dr. E.A. Johnson, a respected ecologist and elected member of the Ecological Society of America's Board of Directors. He, like myself and others, has independently found that widespread prescription burning is neither ecologically sound nor cost effective for crown fire ecosystems such as chaparral. This fictitious citation seems a contrivance to lump together scientists who disagree with the inherent bias of this task force, namely concluding there is a need to do backcountry prescription burning and other fuel manipulations. This is particularly disturbing because my research papers, and those of others ignored in the report, have many positive suggestions for the most strategic and cost effective means of dealing with catastrophic wildfires. To ignore these ideas diverts funds away from other management activities that can make a difference.

This fictitious citation is only one of several fictitious entries in the bibliography and in part reflects a failure to respect the importance of a document such as this one, which is being used as a basis for county-wide management recommendations to reduce risks to human property and safety. Also reflective of this are quite a number of blatant errors of fact in the report. An example is illustrated on page 8 where it states that "Peng and Schoenberg… concluded that statistically, fuel was the limiting factor." However, the Peng and Schoenberg study showed that, while stands less than 20 years of age had a lower probability of burning, after that there was no significant effect of fuel age. This does not translate into the message that fuels need to be managed throughout the landscape. In a recent email communication (January 10, 2004) Dr. Schoenberg has verified that the Task Force report misinterprets his findings.

Also, the report is sloppy in its treatment of facts, and we suggest that every fact be verified before accepting it for what the report claims. Here are just a few examples: (1) on page 8, the report states that Peng and Schoenberg's work provides "a dramatic illustration of the difference between a landscape shaped with almost no fire suppression activity in Baja California compared to San Diego…"; however, Peng and Schoenberg never provided any data on

Baja and the figure ascribed to them is not theirs. (2) Peng and Schoenberg are purported to have "analyzed the Los Angeles Malibu fire regime." This is not true. (3) The acreages burned shown in Figure 1 are about 5 times smaller than what is reported by the California Division of Forestry and Fire Protection Statewide Fire History Database and as reported in the journal Science (1999; Volume 284, pages 1829-1832). Also, (4) the legend in their Figure 5 claims that this figure shows fire size, but as shown in the key embedded within the figure, what is actually depicted are 5-year age-classes of vegetation. Any apparent fire in this figure is in fact an area that could have burned by multiple different fires during a 5-year period. This mistake greatly affects any conclusions about fire size drawn from these data.

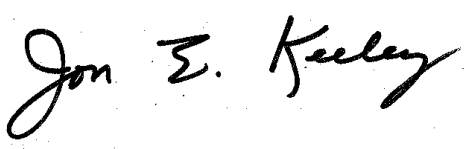
In addition to factual errors, the report makes some important conceptual mistakes that should be clarified. For example the report states, "By the early twentieth century, fire exclusion was the accepted practice." The practice was that of fire *suppression*, not exclusion, and only on certain forested landscapes has suppression actually resulted in fire exclusion. The fire history record for San Diego County shows that, despite a century of heroic efforts at putting out fires, the fire suppression policy has not even come close to fire exclusion. When fire scientists in the Western U.S. talk about fire exclusion resulting from our fire suppression policy, they are talking about landscapes where fires really have been excluded, such as many yellow pine forests, thus allowing unnatural fuel accumulation. This does apply to some high elevation coniferous forests in the county, but fire exclusion has not occurred in the foothills and coastal plain of San Diego County and this is well illustrated by their Figure 1.

I could provide a long litany of other mistakes in this report but it makes little sense to try and correct this document. It would be better to start from scratch with a more complete and balanced report. The report has an agenda: to demonstrate that widespread fuel manipulations are the only way to protect property and lives. To do this the report attempts to downplay the importance of weather to wildfire behavior. The authors' analysis is not an accurate portrayal of the issue because they have the inherent belief that only autumn Santa Ana wind-driven fires are controlled by weather and summer fires are purely controlled by fuels. This is not true. Just take for example the Pines Fire , which they portray as unaffected by weather, yet the LA Times reported "extremely low humidity, temperatures near 90 degrees, … and [because of] gusts of wind embers skipped a mile ahead of the fire, starting new hot spots" (August 7, 2002). In general, the most catastrophic wildfires in southern California are weather-driven events, and fuel treatments when applied in the backcountry seldom stop these fires. The primary value of fuel treatments is to reduce fire intensity and increase the ability of fire fighters to approach the fire and put it out. As a result, treatments need to be strategically located where they help firefighters save homes, and the most cost-effective use of these expensive treatments is at the urban/wildland interface.

Lastly, let me suggest one of many unfortunate omissions in this report is its failure to examine the excellent US Forest Service book released in 1999 entitled "Southern California Mountains and Foothills Assessment" (General Technical Report PSW-GTR-172) by Stephenson and Calcarone. These authors thoroughly reviewed all pertinent scientific information on fire and resources in the region. While I don't necessarily agree with all of their assessments, I do heartily endorse that report as a fair and accurate study of fire issues in the region.

Attached is a brief vitae with relevant publications.

Sincerely,



Dr. Jon E. Keeley

TO: San Diego Fire Recovery Network
FROM: CJ Fotheringham, Research Scientist
RE: Mitigation Strategies for Reducing Wildland Fire Risks report
DATE: 19 January, 2004

I am a doctoral candidate at University of California, Los Angeles and have been actively involved in research of fire in southern California shrublands as well as in other mediterranean climate regions of the world for the past 10 years. I have published papers in peer-reviewed scientific journals and have detailed knowledge of fire ecology and fire regimes in southern California. A number of the papers I've published specifically address issues the Board of Supervisors is currently grappling with in order to improve public safety and decrease the loss of human life and property during large fire events.

I have reviewed the "Mitigation Strategies for Reducing Wildland Fire Risks" report and find it lacking, if not outright misleading, in a number of key areas. The report is poorly researched and on the face appears heavily biased toward influencing the San Diego Board of Supervisors and Planning Department toward a decision of managing large fires in San Diego County by control burning of remote wildland areas. I have attached a more detailed review.

It is my belief, based on my own and other's extensive research, that Santa Ana driven fires are infrequent but not unnatural events. As such, I believe that a productive way of viewing the issue in regards to public safety would be addressing this in a similar context as 100 year flood events. Like 100 year floods, the probability of a Santa Ana driven fire is low at any given time but highly probable to occur at some point. A management and planning policy that focused on strategic fuel manipulations at the urban/wildland interface coupled with planning ordinances that mandated buffer zones around new communities would have the greatest success of decreasing loss of property and life during even extreme fire events.

If I can be of further assistance, please do not hesitate to contact me.

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This critique pertains to "Mitigation Strategies for Reducing Wildland Fire Risks" report submitted to The San Diego Board of Supervisors on 13 August 2003

Page 4; The Natural Fire Regime
While the historical fire regime is of academic interest, it is a debatable whether it is relevant to the issue at hand, specifically how to minimize impacts of wildland fires to human property and lives. That said, this section brings up an issue that is apparent throughout the report-the lumping of chaparral and coniferous fire regimes as one phenomena. While these two systems occur in juxtaposition to one another in San Diego County and fires spread from one to another, fire regimes and fire behavior is very different in the two systems. To adequately address the problem of protecting human life and property, management of these systems needs based on the unique characteristics of each.

For further discussion of southern California historic fire regimes :
Keeley, J.E., and C.J. Fotheringham. 2001a. Historic fire regime in Southern California shrublands. Conservation Biology 15:1536-1548.
Keeley, J.E., and C.J. Fotheringham. 2001b. History and Management of crown-fire ecosystems: a summary and response. Conservation Biology 15:1561-1567.
Conard, S. G., and D. R. Weise. 1998. Management of fire regime, fuels, and fire effects in southern California chaparral: lessons from the past and thoughts for the future. Tall Timbers Fire Ecology Conference Proceedings 20:342-350.
Moritz, M. A. 1997. Analyzing extreme disturbance events: fire in the Los Padres National Forest. Ecological Applications 7:1252-1262.
Moritz, M. A. 1999. Controls on disturbance regime dynamics: fire in Los Padres National Forest. Ph.D. dissertation. University of California, Santa Barbara.

Page 4; Fire Exclusion Practices
While it appears that fire suppression has been effective at eliminating fires from many western coniferous systems, there is evidence that this is not the case in chaparral. Additionally, many studies indicate that infrequent large fires were the norm long before arrival of Europeans.

In addition to the above cited literature see also:
Mensing SA, Michaelsen J, Byrne R 1999. A 560-year record of Santa Ana fires reconstructed from charcoal deposited in the Santa Barbara Basin, California. Quaternary Research 51 (3): 295-305
Keeley J. E. 2002. Native American impacts on fire regimes of the California coastal ranges Journal Of Biogeography 29:303-320

Page 5; Fire Exclusion Practices Figure 1
The graph presented contradicts earlier statements by the authors that fire suppression has been effective in San Diego County. Additionally, other reputable publications are widely available that support fire suppression has not led to fire exclusion. These studies also indicate that average fire size in San Diego County shows a significantly decreasing trend since the beginning of the century. The authors of these studies concluded that fire suppression not led to fire exclusion but also that fire suppression is absolutely necessary to minimize the impact of increased anthropogenic ignition.

Keeley, J. E., C. J. Fotheringham, M. Morais. 1999. Reexamining fire suppression impacts on brushland fire regimes. Science 284:1829-1832.

Page 5; Fire Exclusion Practices Figure 2

These data, while minimal, bring up some important points that should be pursued further; specifically structure loss per unit area burned is an order of magnitude greater under Santa Ana driven fires. This is an important observation for policy makers as it indicates that these extreme fire conditions are the ones that need to be addressed in order to affect the greatest decrease in loss to human life and property.

Fire	Condition	Structures lost/1000 ac burned
Laguna	Santa Ana	2
Harmony	Santa Ana	14
Viejas	Santa Ana	2
Gavilan	Santa Ana	7
La Jolla	Heat wave	0.2
Pines	Heat wave	0.7

Page 6; Weather, paragraph 1

This is a mis-statement of the findings. Brushland fire *regimes* in southern California are largely driven by the occurrence of wind driven fires. The difference is more than just a matter of semantics. What drives an individual fire is dependent on the conditions prevalent at the time–*ie* under moderate weather and terrain conditions, fuel is probably the most important factor but under high wind conditions such as Santa Anas then the impact of wind can override the importance of fuels and similarly in rugged terrain, topography may be the limiting factor to fire spread. However, the fire *regime* in southern California is dominated, in terms of both acreage burned and structure/life loss, by wind-driven (primarily Santa Ana) fires.

Page 6; Weather, paragraph 2

While fires under drought conditions can and do get large, they account for less area over all and cause less structure loss. Again, it is important to separate what plant community these fires are occurring in (See discussion above).

Page 6; Figure 3

Why was the Harmony fire not included in this table? A skeptic would think it was to balance the ratio of Santa Ana : Non-Santa Ana fires.

Page 7; Topography

Agreed topography is an important factor in determining structure loss. In addition, planning should include the direction from which a wind driven fire is likely to occur and additional precautions taken along this margin.

Page 7; Fuel, paragraph 1

See above discussion for clarification of fire vs. fire regime.

Page 7; Fuel, paragraph 2

The authors of the report are again confusing individual fires with fire regimes. Dr. Keeley has conducted research and commented on fire *regimes* not individual fires *per se*.

While Minnich and Bonnickson speculate about the historic fire regime, neither author has offered anything that would qualify as "proof" to support their contentions of the historic fire regimes. Indeed, a number of papers have cast serious doubt both on the methodology and the conclusions of studies conducted on southern California shrubland fire regimes by above authors. In regards to studies comparing southern California to Baja California, the methodology has been criticized as there are a number of large fires recorded in Baja California that appear to be lacking from the data set used by Minnich. Also, there were no blind tests conducted to test the accuracy of fire boundary delineation. In regards to conclusions, the study attributes all purported differences in fire regimes to one

factor (fire suppression) without addressing other differences such as climate, topography, land use, etc. that also influence fire regimes.

In addition the authors of the report fail to distinguish between shrubland or coniferous fire regimes. While fuels are likely the dominant factor in fires of coniferous forests (where fire suppression policy has been effective and led to fire exclusion) there is strong evidence this is not the case in southern California shrublands and there is no indication that there is an abnormal accumulation of fuels in shrublands-see figure 1.

For further discussion see:
Keeley, J.E., and C.J. Fotheringham. 2001a. Historic fire regime in Southern California shrublands. Conservation Biology 15:1536-1548.
Keeley, J.E., and C.J. Fotheringham. 2001b. History and Management of crown-fire ecosystems: a summary and response. Conservation Biology 15:1561-1567.
Conard, S. G., and D. R. Weise. 1998. Management of fire regime, fuels, and fire effects in southern California chaparral: lessons from the past and thoughts for the future. Tall Timbers Fire Ecology Conference Proceedings 20:342-350.
Keeley, J.E. and C.J. Fotheringham. 2003. Historical fire regime in southern California. Fire Management Today 631:8-9
Keeley, J.E. and C.J. Fotheringham. 2002. Impact of past, present, and future fire regimes on North American Mediterranean shrublands, pp. 214-258. In T.T. Veblen, W.L. Baker, G. Montenegro, and T.W. Swetnam (eds), Fire Regimes and Climatic Change in Temperate and Boreal Ecosystems of the Western Americans. Springer-Verlag, New York.

Page 8; Fuel (cont.), paragraph 3
Again the authors of the report fail to separate coniferous forest from shrublands. While there is evidence of coniferous forests having longer fire free intervals than historically was the case there is no reliable evidence for this being the case in shrublands. In addition, increasing age of wildland fuels is contradicted by the authors own figure 1 (see comments above).

Page 8; Fuel (cont.), paragraph 4
The recent Cedar fire burned through the 10,000 Ac Viejas fire from Jan 2002. Not only was the fire able to propagate in this 1 ½ year old stand, it was able to jump 8-lanes of the freeway at Viejas grade. Clearly, under appropriate conditions chaparral of any age can burn and young age classes cannot be a reliable prevention of fire spread.

Recent studies also indicate that age is not a reliable predictor of fuel load or dead to live ratio.

For further information see:
Conard, S. G. & J. C. Regelbrugge. 1994. On estimating fuel characteristics in California chaparral. Pages 120-129 in Proceedings of the 12th conference on fire and forest meteorology. American Meteorology Society, Boston.

Page 8; Fuel (cont.), paragraph 5
This is a complete mis-representation of studies involving these researchers and borders on outright dishonesty

1. They did not study fuel *per se* but age class (see comments above about age as a fuel predictor)
2. They found that age was a reliable predictor of probability of burning only up to ~28 years.
3. While aware of the debate of the relative role of wind vs. fuel in chaparral fire regimes they do not address the role of wind. Their studies only addressed the probability of stands of a certain age class burning.

However, the authors of the report do not list which of the studies Schoenberg and Peng have published that they are referring to (they are not listed in the bibliography) so it is hard to be more specific than the above comments.

For Schoenberg and Peng's recent work see:

Schoenberg, F.P., Peng, R., Huang, Z., and Rundel, P. (2003). Detection of nonlinearities in the dependence of burn area on fuel age and climatic variables in Los Angeles County, California. Int. J. Wildland Fire 12(1), 1--10
Schoenberg, F.P., Peng, R., and Woods, J. (2003). On the distribution of wildfire sizes. Environmetrics, in review. Available for download at http://web.stat.ucla.edu/~frederic/papers/firesize.pdf
Peng, R. D., Schoenberg, F. P., Woods, J. (2003). Multi-dimensional point process models for evaluating a wildfire hazard index. JASA, in review. Available for download at http://web.stat.ucla.edu/~frederic/papers/fireinterval.pdf

Statements in the second half of the paragraph borders on plagiarism. Peng and Schoenberg never studied Baja California and the figure presented (with modifications? Sub-title is unclear) has been published by Minnich and Chou, 1997. The figure itself has been questioned as misleading on a number of grounds and should be viewed skeptically.

Again, the studies of Baja California vs. southern California have been heavily criticized both on the basis of their methodology and conclusions.

Page 8; Fuel (cont.), paragraph 6
There is no data presented indicating how much of what burned in these fires was 1) older age classes, 2) chaparral or coniferous forest. The fact that Santa Ana conflagrations can and do occur begs the question as to whether maintaining an age mosaic on the landscape is even possible, *ie* when large areas burn the entire footprint of the fire is effectively set back to zero and any previous (expensively obtained) age mosaic is lost across the entire area.

Additionally, the authors do not indicate where these older classes are located. Are they in chaparral or coniferous forests? Near or far from developments? Where they occur is important to making policy and management decisions. Figure 7 would seem to indicate that these older stands are largely coniferous and/or remote from high density population centers.

Page 11; San Diego Wildland-Urban Interface Fire Issues
This section of the report again indicates wind was a dominant factor in structure loss during catastrophic fires. The authors fail to acknowledge this. As indicated earlier, it appears that the greatest losses to human life in property due to fire in southern California is during Santa Ana driven fires. Not all large fires are equal and the order magnitude greater loss under Santa Ana driven fires indicate that these are the fires on which planning should focus in order to maximize the effectiveness of management funds and efforts. Therefore, it would seem that planning for these, infrequent but highly destructive, fires would be a top priority for zoning and planning boards-much in the same way that ordinances are established to cope with the possibility of a 100 year flood.

Bibliography
I'm not sure what to say about this but 'appalling' comes to mind. There is at least one fictional citation when the authors have reputable publications. Most cited articles cannot be verified due to mis sing information such as year and/or journal/media. The number of op-ed pieces included, which are just peoples opinions and they do not generally provide support for their arguments, is inappropriate for a document that purports to be a review of the scientific literature. Particularly in light of what wasn't, and should have been, included.

Attachment I

Letter of Explanation from Dr. Scott Mensing regarding: Mensing, S. A., J. Michaelsen, and R. Byrne. 1999. A 560-year record of Santa Ana fires reconstructed from charcoal deposited in the Santa Barbara Basin, California. Quaternary Research 51: 295-305.

Whenever we use the term large fires we are referring to historic fires that burned > 20,000 ha – so it is important to point out that our focus was very large conflagrations.

"Wildland fires consume thousands of hectares annually throughout California. Periodically, large fires burn > 20,000 ha. In southern California, such fires typically occur in late summer and early fall during Santa Ana conditions, characterized by low relative humidity, high temperatures, and strong northeasterly winds (Davis and Michaelsen, 1995)." Pg 295

Our results clearly show that large fires have always been present in southern California (particularly Santa Barbara where our study was conducted) and that fire practices had no seeming effect on the occurrence of these very large fires.

"Large fires occur in every century, with the longest period between fires being 75 yr (Table 2). The record has been divided into three cultural periods representative of different attitudes toward fire. Period 1 (A.D. 1900–1985) represents the period of active fire suppression and historic fire records (Minnich, 1983; USFS Santa Barbara fire data). Period 2 (A.D. 1770–1900) encompasses the early period of Spanish and American occupation characterized by a policy of fire suppression, but with little means of enforcement (Barrett, 1935). Period 3 (A.D. 1425–1770) represents the Chumash period during which fires were purposely set along the coastal plain (Timbrook *et al.,* 1982). The average interval between large fires for each period is 23, 29, and 21 yr, respectively ..." Pg 301

Our conclusions clearly state that large fires have occurred naturally under all land use conditions.

"The charcoal record indicates that large fires are part of the natural fire regime in this region." Pg 303

"the average time between large fires has remained relatively consistent throughout the record, with no large differences between the three cultural periods. Large fires occurred in every century. Neither the Chumash practice of setting fires nor the modern practice of suppressing fires appears to control the periodic occurrence of conflagrations in the region." Pg 303

We speak of the potential for fine-grained vegetation mosaics in this paragraph in response to Minnich's argument, but the following sentence clearly points out that there is no evidence from our study to support the conclusion that such a mosaic prevented Santa Ana conflagrations. Any claim that we state that a fine-grained vegetation pattern was the only pattern or that this had any effect in preventing very large fires from burning under Santa Ana climatic conditions is a misinterpretation of our results and our published work.

"Background levels of both large and small charcoal suggest that small fires are also common. Fires at this scale may have created a fine-grained vegetation mosaic in portions of the landscape, as suggested by Minnich (1983). However, there is no evidence that such a mosaic acted to prevent Santa Ana conflagrations. Such fires would have converted large portions of the landscape to an even-age, coarse-grained structure, possibly contributing to the importance of seedling-obligate chaparral taxa, such as some species of *Adenostoma* and *Ceanothus* (Keeley, 1977)." Pg 303

We conclude that Santa Ana fires are instrumental in helping maintain chaparral and that more frequent fires could actually convert chaparral into grassland. Our pollen evidence does not show any increases in grasses over time, leading us to conclude that the large fires maintained chaparral over time.

"Frequent low-intensity fires should have favored grasses over chaparral and resulted in an expansion of grassland (Dodge, 1975; Timbrook *et al.*, 1982; Zedler *et al.*, 1983); however, the pollen record shows a decrease in grass pollen over time. In an environment where Santa Ana fires are common events, chaparral is maintained." Pg 304

In comparing our data with tree-ring based climate reconstructions we clearly conclude that climate exerts greater control on the fire regime than do land use practices.

"Climate exerts an important control over the fire regime by increasing potential fuel loads in wet periods and providing ideal conditions for large fires in dry periods." Pg 304

"Oscillations between wet and dry phases over this period appear to contribute to large fires on a regular basis, regardless of changes in land use practices." Pg 304

Our concluding paragraph leaves no doubt that even if there was a fine-grained vegetation pattern present, this did not prevent large fires when the climatic conditions were right. Our results provide evidence that fire suppression has not significantly altered the fire regime.

"Our reconstruction suggests that between A.D. 1425 and 1900 there were at least 20 large fires in this area. The average time between fires ranges between 20 and 30 yrs and is strongly controlled by precipitation patterns, with large fires generally occurring at the end of wet phases and the beginning of droughts. If small fires created a fine-grained vegetation pattern on the landscape, this does not appear to have prevented periodic large fires. Changes in land use practices associated with the arrival of the Spanish and the introduction of fire suppression also have not significantly altered the fire regime. Fire suppression may contribute to large fires by allowing more fuel accumulation; however, fire suppression alone does not create this type of fire. The fuel and weather conditions necessary for large fires were present prior to fire suppression and are a natural part of chaparral ecology in a Mediterranean climate." Pg 304

Dr. Scott Mensing
8/19/08

Attachment II

Letter of Explanation from Witter and Taylor regarding: Witter, M. and R. Taylor. 2008. A case study in fire management and conservation from the Santa Monica Mountains. In Halsey, R.W. ed. Fire, Chaparral and survival in Southern California Revised and Updated. Sunbelt Publications, El Cajon, CA pp 109-115.

In our Fire Management Plan (FMP) we specifically rejected landscape level mosaic burning as being logistically impossible to implement, unacceptably damaging to resource values in the native plant communities to be treated, and not able to produce demonstrable reductions in wildfire hazards.

The FMP model for selecting strategic fuel modification locations was a PRELIMINARY SUGGESTION on how to approach the problem. The use of vegetation type, age of vegetation, and slope was based on our Management Officer's recommendations. This was never meant to be the final word on strategic fuel modification locations. We also had a decision tree to assess the long term utility and impacts of a specific project.

Strategic fuel modifications are problematic in general because they require a lot of assumptions about where fires will start and which way they will spread in order to conclude that they would have much value. By definition they are well away from both the assets they are supposed to protect and the assumed ignition locations of the future wildfires they are supposed to help contain. Wildfire spreads very quickly and fuel modifications are only useful if firefighters have time to go there and use them for tactical advantage to limit fire spread.

To have any relevance for fire suppression, strategic fuel modifications need to work in extreme fire weather, which is especially difficult. In fact most strategic fuel modification projects subjected to the kind of rigorous analysis we propose would fail to demonstrate much real reduction in fire hazard to specific assets at risk. It is hard to demonstrate that strategic fuel modification projects actually work as intended in the kind of extreme fire weather that burns most of our area.

By contrast, well-designed defensible space around a structure will confer demonstrable tactical advantages to firefighters in defending that structure no matter where the fire starts or comes from. No specific assumptions and imaginative storytelling about fire spread are necessary to demonstrate their value.

Dr. Marti Witter
Dr. Robert Taylor
8/18/08