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Fellows, Aaron W.
Goulden, Michael L.

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Reply to comment by J. Bouldin on “Has fire suppression increased the amount of carbon stored in western U.S. forests?”

Aaron W. Fellows¹ and Michael Goulden¹

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[1] We agree with some aspects of *Bouldin* [2009] and welcome the opportunity to clarify aspects of *Fellows and Goulden* [2008, hereafter F-G]. Several of Bouldin’s claims reflect his interpretation of the core message of our paper. Bouldin feels our main message was that “fire suppression, independent of logging, has brought about a decrease in live, aboveground tree carbon via increased mortality of large trees in western U.S. forests”. Our intended message was that we found no evidence that fire suppression has led to a large increase in forest biomass, and a suggestion, along with a hypothesized mechanism, that fire suppression has led to a loss of biomass. We feel Bouldin’s interpretation is inconsistent with our paper, which repeatedly returns to the question of whether fire suppression has increased carbon stocks, and discusses the possibility of a general loss of carbon with fire suppression as a hypothesis rather than statement of fact. Our paper generated popular press, some of which may have overemphasized the possibility of carbon loss, and Bouldin may be reacting to some of these articles.

[2] *Bouldin’s* [2009] most important point, in our opinion, is the “possibility that the 1930s VTM data were a biased sample of forest conditions”. We share Bouldin’s concern. The VTM manual instructed surveyors to select plots that sampled the natural vegetation variation and to avoid biased selection (A. E. Wieslander et al., Manual of Field Instructions for Vegetation Type Map of California, unpublished report, 196 pp., Forest Service, U.S. Department of Agriculture, http://digitalassets.lib.berkeley.edu/vtm/ucb/text/cubio_vtm_fm.pdf). For this reason, the VTM has been regularly accepted as a quantitative measure of historic forest conditions [e.g., *Minnich et al.*, 1995; *Bouldin*, 1999; *Barbour and Minnich*, 2000; *Lutz et al.*, 2009]. *Keeley* [2004] found that resurveying a small number of VTM plots would result in an inaccurate assessment of vegetation change due to the inability to precisely locate the historic plots in highly heterogeneous California shrublands. *Keeley* argued that one way to minimize this issue would be to average over a large number of plots and we followed that approach. Nonetheless, the selection of VTM plots was not rigorously random, which may have skewed our results.

One aspect of our analysis hints at this possibility: we found a decrease in biomass of 28% at high elevation, where fire suppression is thought to have had a minor effect. If we consider the extreme hypothetical case, where the change in high elevation biomass (–28%) was due entirely to things other than fire suppression (e.g., bias, climate change [*van Mantgem et al.*, 2009], etc.) and the change at mid-elevation (–39%) was due to these factors plus fire suppression, we conclude the effect of fire suppression on mid-elevation biomass was a small, perhaps insignificant, decrease of roughly 11%. In retrospect, our paper might have benefited from a discussion of the possible biases in the VTM data set. However, this discussion would not have altered our conclusion that fire suppression has not caused a large increase in biomass. In particular, we found no evidence of an increase in mid elevation biomass relative to high elevation biomass, as would be expected if fire suppression increased carbon stocks.

[3] *Bouldin’s* [2009] critique identified a mistake in our original manuscript’s data description. We originally reported that we used FIA data from the 1990–1994 survey. However, our analysis used data from 1995–2000. This explains Bouldin’s confusion about plot location. We used 18 VTM plots and 47 FIA plots from Northern California, and 136 VTM plots and 93 FIA plots from the Sierra Nevada. There were no significant stem density or biomass differences between the Northern California and Sierra plots in either the VTM or FIA datasets.

[4] We disagree with several of *Bouldin’s* [2009] other points. For example, Bouldin provides no evidence for his claim that coarse woody debris created by tree mortality has not decayed, and observations on this front are largely unavailable or contradictory [*Harmon et al.*, 1987]. Bouldin accuses us of misinterpreting *Houghton et al.* [1999] by including non-ponderosa pine stands in our analysis: “(the) 2.3 Mg/ha/yr cited by F-G [from *Houghton et al.*, 1999], apply to the highly fire-prone western ponderosa pine woodlands only, as stated by *Houghton et al.* [1999, Table 2].” But *Houghton et al.’s* Table 2 refers to “western pine” and not “western ponderosa pine”, and the relevant footnote [*Houghton et al.*, 1999, footnote 22; see also *Houghton et al.*, 2000], and even Bouldin’s letter, includes work outside of ponderosa pine’s range. We regarded *Houghton et al.’s* analysis as an “order of magnitude” estimation, which used ponderosa pine’s range for expediency, and that was mostly intended to start the discussion.

[5] Finally, we disagree with *Bouldin’s* [2009] confidence that this issue has been resolved and “that fire reduction has very likely increased...live tree carbon”. Studies demonstrating a carbon sink with thickening remain scarce; forest thickening, along with woody encroachment, is a poorly constrained term in the carbon budget. We believe under-

¹Department of Earth System Science, University of California, Irvine, California, USA.

standing has progressed as follows: *Houghton et al.* [1999] used an “order of magnitude” calculation to show the carbon sink from fire suppression could be large and certainly merited study. Houghton et al.’s finding was based on limited evidence, and we set out to test it with a widely available, albeit imperfect, historical dataset. We were unable to find evidence of a large sink and hypothesized a mechanism to explain why Houghton et al.’s intuitively appealing idea might be wrong. We hoped our paper would encourage further analyses using historical data. Bouldin now reports new datasets that may support Houghton et al.’s idea. We are happy to see these data brought forward and consider this to be the natural course of science. Nonetheless, we do not consider the case closed. Bouldin’s findings have not been documented or subject to peer review; his data sets are not easily available, and there is no way to evaluate several of his assertions; many key issues remain data limited.

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A. W. Fellows and M. Goulden, Department of Earth System Science, University of California, Irvine, CA 92697-3100, USA. (afellows@uci.edu)