Journal of the Royal Society of Western Australia, 95: 95–102, 2012

Grasstree stem analysis reveals insufficient data for inference of fire history

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Grinding back dead leaf bases on the stems of arborescent grasstrees (*Xanthorrhoea* spp.) reveals a pattern of horizontal bands that has been interpreted as a record of the fire history experienced by the plant. The validity of this fire history record has previously been assessed through comparison of 100 grasstree stems sampled from shrubland near Eneabba in Western Australia against a 30 year fire history determined from satellite imagery. This analysis showed that the two records matched more than would be expected by chance, but concluded that the interpretation of the grasstree record as a fire history was not warranted as most of the grasstree fire records did not match satellite fire records. A second analysis of the same two sets of records, published in this journal, also showed that the records matched more than by chance, but concluded that the interpretation of grasstree banding as fire history was valid, though it failed to quantify the strength of this agreement. Here we examine the approaches and interpretations of the two previously published studies, and provide new analyses to refine estimates of the amount of fire-related data present in the grasstree record. We show that only ~20% of grasstree 'fire' records may be attributable to fire. With eight out of ten of records not attributable to fire, we confirm that the grasstree record in its current form cannot be interpreted as fire history, and therefore claims of the grasstree technique to support management actions are untenable.

KEYWORDS: fire history, fire management, grasstrees, *Xanthorrhoea* spp.

INTRODUCTION

Quantified recent and historic fire histories are useful in the interpretation of modern ecological processes and patterns, as well as for understanding ecological history and inferring past cultural practices. In turn, this knowledge may be used to construct management plans aimed at conservation or restoration of ecosystems and biodiversity values. An approach to determining landscape fire histories has been described that analyses the pattern of lighter and darker bands revealed on the stems of grasstrees (*Xanthorrhoea* spp.) after their leaf bases have been ground back (Ward *et al*. 2001). While the results and interpretation of this technique have proven controversial on several grounds (Lamont *et al*. 2003; Wardell-Johnson *et al*. 2004; Enright *et al.* 2005a,b, 2006; Gill 2006; Ward 2006, 2009; Miller *et al*. 2007), they continue to be promoted as support for a high frequency of managed fire in many southwest Australian ecosystems (Ward 2009, 2011). As no tests of the validity of the technique had been made (apart from showing that the colour changes have an anatomical basis: Colangelo *et al.* 2002), we conducted a study comparing the grasstree technique against a fire history derived from satellite imagery for the Eneabba region (Miller *et al.* 2007). In that study we compared the fire history record from an annual sequence of satellite images for the period 1973–2002 with one derived from a sample of 100 grasstrees in the same area. We found that while some of the grasstree fire records could be attributed to fire (as indicated by the satellite image record), most could not, and further, that this rate of error increased into the past. That is, the grasstree record tended to overestimate the frequency of fire in the distant past more so than in the recent past. We concluded that while the grasstree record did contain some information about fire, its interpretation as a fire history was not warranted because of the dominance of non-fire signals. Subsequent re-analysis of the same data also found that the records do match more than by chance (Ward 2009). However, failing to quantify the strength of this agreement, this second analysis made the unsupported conclusion that the interpretation of grasstree banding as fire history is valid.

Here we compare the findings and approach of the two papers (Miller *et al*. 2007; Ward 2009) and examine the latter's major criticisms of the former. Primarily, Ward asserted that in Miller *et al*. (2007) we: (i) misaligned the grasstree and satellite records and failed to take into account the inherently lower precision and potential systematic bias in grasstree record dating; (ii) inappropriately excluded data; (iii) unduly relied upon an inappropriate statistic; and (iv) failed to contemplate or analyse variation in fire intervals. The remaining criticisms of Ward (2009) were either not made explicit (e.g. on extrapolating findings from shrublands to woodlands or forests), or were issues that have been addressed previously that Ward returned to, but added no new arguments. An example is the criticism that we failed to account for fine-scale variability and patchiness in fires: Ward (2009) repeated the argument that grasstrees are lit or extinguished individually by various agents. We clearly addressed this issue in Miller *et al.* (2007), stating that, firstly there is no evidence for this variability in the 1973–2002 study period, and secondly that (if so) it fundamentally invalidates any extrapolation of the grasstree results to the landscape scale—arguments that Ward overlooks.

As in Miller *et al.* (2007), and previous papers (Enright *et al*. 2005b, 2006), we re-affirm our view that grasstree banding does appear to record valuable ecological information (especially in relation to growth rates and plant age: Lamont *et al.* 2004), and that some fraction is likely to represent fires. In Miller *et al.* (2007) we provided an estimate of rates of true positive fire records in the grasstree data. However, we also noted that some positive grasstree records would be expected to match satellite fire records by chance alone, even if the grasstree record was random. Here, we address Ward's criticisms of our earlier paper, and extend our analyses to quantify more clearly the fraction of grasstree bands which may be interpreted as associated with fire.

WARD'S METHODOLOGICAL CRITICISMS

Misalignment of the records and failure to consider grasstree record precision

Ward (2009) provided a number of justifications for re-analysing the data. One of which was that our analysis was of non-matching census years. In fact, his representation of the annual period that we consider (and clearly define) was mistaken (Figure 1). In our analysis, we labelled the year of the satellite image so that, for instance, a year bounded by images from September 1981 and August 1982 is labelled "1982". However, our statement that 'fire year here is assigned as the 12-month period from September to August' (Miller *et al*. 2007 p. 910) was overlooked by Ward who believed we used a calendar year for the census period. He also assumed that the grasstree year is equivalent to the calendar year although, representing seasonal changes in phenology (Lamont *et al.* 2004), it is more likely to correspond to our fire year (Figure 1).

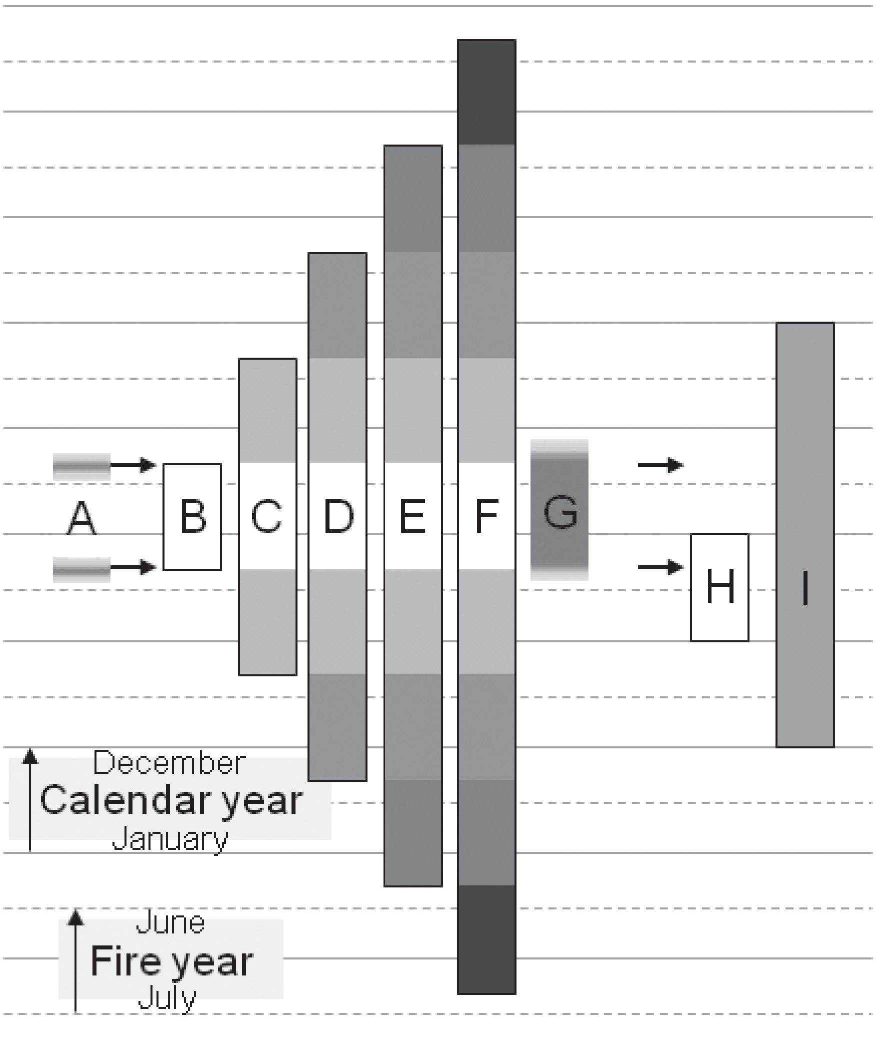

Figure 1 Comparison of census years and precision windows for grasstree and satellite records considered by Miller *et al*. (2007) (A–G), versus those of Ward (2009) (H–I). Satellite image dates (A) define the satellite census year (B) but vary with availability around August–September (arrows delimit one sample year). Miller *et al*. analysed varying degrees of temporal precision by resizing a symmetric window of agreement around each fire record to 1, 3, 5, 7 and 9 years (B–F). While the duration of grasstree annual banding is not known, in southwest Australia grasstree phenology changes from a slow to a fast growth phase in late winter/early spring (Lamont *et al*. 2004, Korczynskyj & Lamont 2005), suggesting a grasstree growing year (G) similar to the satellite year. Ward implicitly assumed grasstree year = calendar year (H) and takes this as the census year. As each of these overlap two satellite years, Ward (2009 p. 262) allowed '[grasstree fire dates] a tolerance of a calendar year either way, and fires dated from satellite images a year's extension backwards'—an offset asymmetrical agreement window of four years (I).

Ward also stated that re-analysis of the data was required because we overlooked problems with the known imprecision of grasstree records. In fact, we explicitly recognised these issues, devoting one quarter of our paper, including a section titled 'Varying precision', solely to their consideration. These results were plainly and clearly shown in that paper's figure 3 and table 4, and also were considered in detail in its abstract and discussion.

We responded to the inaccuracy of dating of fire scars on grasstree stems by varying the window across which we analysed agreement with the satellite record from 1 to 9 years (i.e. from an exact match to a match somewhere within ±4 years), and we discussed how increasing window size would increase the likelihood of finding agreement between records if they were distributed by chance. Allowing a 9 year window for matches would almost guarantee agreement if fire records had an average interval of 9 years. In the data analysed, the interval averaged 15 years, but clearly there must be some tradeoff between window size and accuracy. We analysed and discussed this somewhat intractable issue in Miller *et al.* (2007) and settled on a 5 year window (i.e. ± 2 years).

Ward failed to acknowledge this part of our study in his re-analysis, claiming that his asymmetric, 4 year analysis window is 'wider and more reasonable' (Ward 2009 p. 262). Ward also failed to provide numerical results from his 4 year window re-analysis. However, our results clearly show that when a 5 year window is allowed, 54% of the positive fire records in the grasstree record are false (they do not match fires in the satellite record), and when a 3 year window is used, 63% are false (Miller *et al*. 2007).

Omission of data

A third justification given by Ward for revisiting the analysis is a suggestion that we did not use all of the data available: that we omitted two clusters of five grasstrees from a total sample of 105 grasstrees, together with several other specific fire records. The rejection of one grasstree cluster, which Ward (2009) called no. 8 (we gave it no number), was described in our paper. We rejected this cluster as it lay too close to one fire boundary to satisfactorily determine its satellite fire record, and its omission is the reason for the subsequent differences in number schemes between the two papers. Concerning the second 'omitted' cluster, Ward correctly identified an error in our figure 2 (Miller *et al*. 2007) that showed 95 and not 100 grasstree samples as we had intended.The five missing plants (his group 18, our group 17) had no fire signal in either record and the blank space that should have represented these individuals in the chart was mistakenly lost in formatting (but is shown in Figure 2 herein). Its omission from calculations would alter rates of true and false negatives (as all of its records are true negatives) but not rates of true and false positives (as it contains no positive records). However, the omission was restricted to the offending figure and it did not influence our calculations. While Ward partly justified his re-analysis on the basis of our omission of these clusters, he repeated and extended the same omissions in his analyses. His tables 1 and 2 (Ward 2009) omit both of the clusters described above, the second of which we did not omit from analysis.

Ward additionally claimed that we omitted 12 particular records of fire – although his table 1 (Ward 2009) showed only 10 new records. It is not clear what these instances represent: we have rechecked our original data files and they match all the way through to our analyses. The omissions are described only from the four sites that burnt in 2002 and were therefore recently burnt at the time of survey in 2004. It is possible that the excluded records lie outside of our comparison period, which we constrained to the three-decade period 1973–2002, as it corresponded with the satellite fire-history record that we constructed in 2003. Although we did not have satellite imagery for the period between the date of the last image in 2002 and the time of the grasstree survey in 2004, we know that there had been no fires in the study area and so we could have extended (and thereby hybridised) the record accordingly. However, we did not do this for several reasons: (i) to facilitate comparison among decades; (ii) to ensure data consistency; (iii) to minimise analytical problems with the bookending of census data with precision windows; (iv) to enforce some level of 'blind sampling' in the grasstree survey (while evidence of recent fires is easily observable in the field and it is impossible to prevent practitioners from making their own conclusions about fire history from field observation, we thought that excluding a few years would help to remove the most obvious part of the visible fire history); and (v) as we did not feel that comparison of grasstree records from the year of observation and its preceding year with known fire histories was an adequate test when we were interested in interpretation in the deeper past. If 12 'omitted' records are incorporated as Ward described them, and we allow a 5 year precision window, the overall rate of false positives in the grasstree record is 45%, i.e. still poor.

Statistics of agreement

Ward (2009) criticised our use of the simple and widely employed Kappa statistic to assess the degree of similarity between the two records. The critical paper cited by Ward (Allouche *et al.* 2006) recommended use of the true skill statistic (TSS) as an appropriate alternative test: it alleviates the problem of varying record Prevalence. In our case Prevalence is not an issue and TSS (0.105) is almost identical to Kappa (0.104). These closely related indices are interpreted in the same way, and both range from 0 (indicating no agreement) to 1 (complete agreement). Neither assesses whether datasets differ significantly, but merely quantify the relative degree to which they can be said to agree. Recognising the limitations of the tests, we stated that 'In broad terms, a value for the Kappa statistic below 0.2 indicates poor agreement and a Kappa above 0.8 indicates very good agreement (Landis & Koch 1977)' (Miller *et al*. 2007 p. 911). We presented Kappa as just one of a suite of tests, including a bootstrapping test that explicitly identifies the level of agreement.

Fire interval

Ward (2009) criticised us for focussing on matching the dates of fires, when fire interval is an ecologically more important variable. It is certainly true that fire interval is ecologically important. However, our approach was to

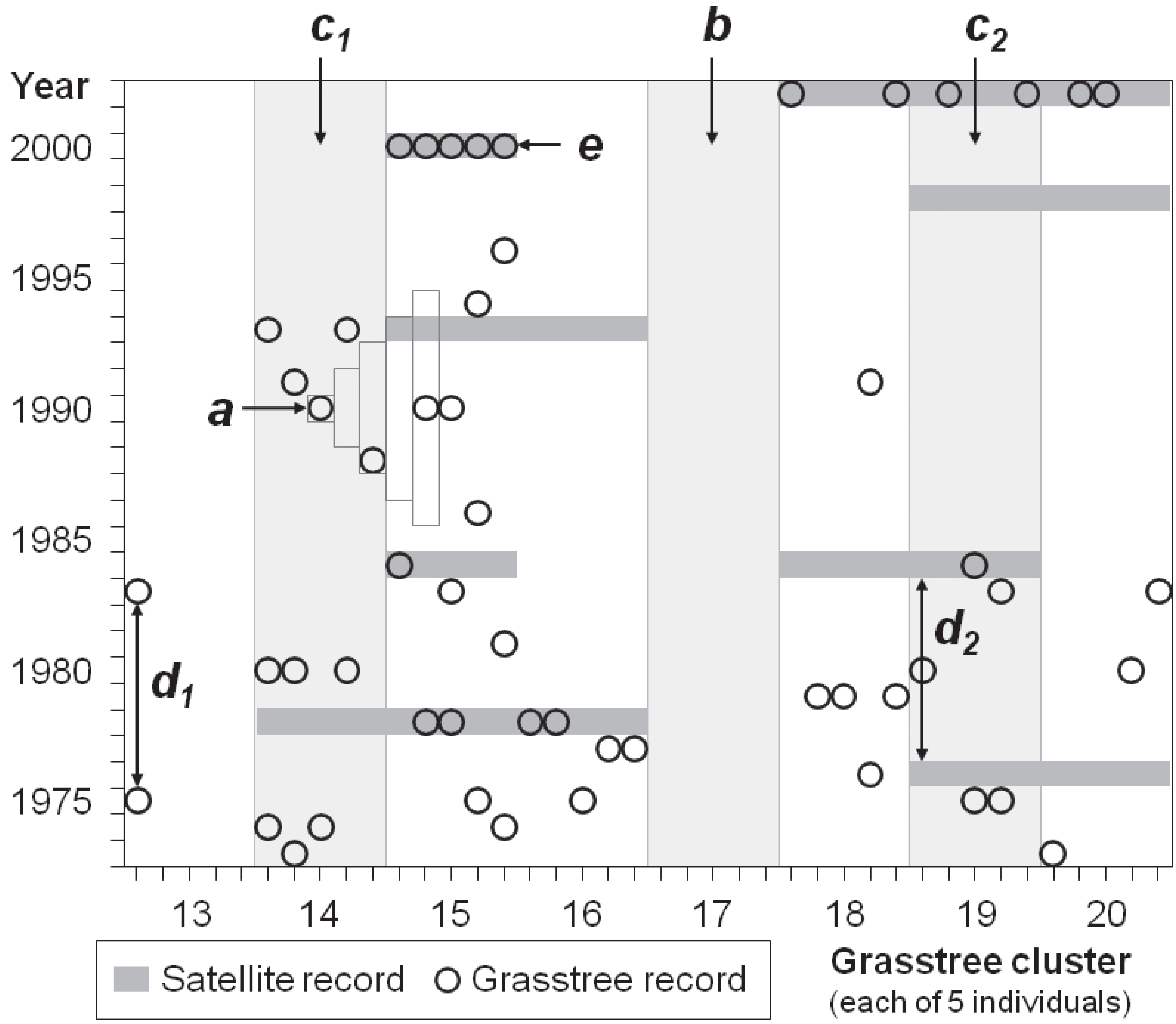

Figure 2 A comparison of grasstree and satellite records for 8 (of 20) analysed grasstree clusters, illustrating elements of the data and analysis. (a) The five precision windows employed by Miller *et al*. (2007) illustrated here for 1990 with (from left): a 1 year window (i.e. 'exact' precision) with a false positive grasstree record (a grasstree fire not matched by a satellite fire within the window); 3 year (i.e. ±1 years) and 5 year windows (±2 years), both true negatives (no grasstree fire in 1990 + no satellite fire in the window); a 7 year window (± 3years) showing a false negative (no grasstree fire but a positive satellite record within the window not otherwise matched by a closer grasstree fire); and a 9 year window (±4 years)—a true positive (a grasstree fire + an otherwise unmatched positive satellite record in the window). (b) The cluster of five grasstrees mistakenly omitted from Miller *et al.* (2007) figure 2. (c) Clusters of five grasstrees with contrasting ratios of grasstree:satellite fire incidence: c_1 with 11 v 5, and c_2 with 7 v 20. (d) A pair of matching 8 year inter-fire periods: the first of three in the grasstree record (d_1), and the first of five in the satellite record (d_2). (e) The only example of spatial aggregation (i.e. among individual grasstrees in a cluster) in positive agreement between the satellite and grasstree records.

first verify that the grasstree record actually was indicating fire before considering analysis of the intervals its records present. If it does not indicate fire, there is no value in analysing its interval distribution.

NEW ANALYSES

Ward (2009) presented three new analyses: (i) a longer temporal context for the grasstree sequence; (ii) a comparison of fire intervals in the two sets of records; and (iii) a test of their 'agreement' in 5-15 subsets of the data. The longer temporal context shows simply that shrubland grasstree stems reveal the same pattern (i.e. shortening intervals into the past) as grasstree stems sampled from forest and woodlands elsewhere in southwestern Australia, but it does not say anything about the validity of the data.

Similarly, Ward's analysis of fire intervals also says nothing about the validity of the grasstree data. It infers from the coincidence in the mean (or median) observed inter-fire interval of the two records that these arise from the same process — fire. In Miller *et al.* (2007) we noted that the total incidence of positive fire records is matched in both grasstree (n = 202) and satellite (n = 200) records. Because both records are derived from the same sample of individuals and years (N = 100 x 30), their mean between-fire interval is also similar, and it would be surprising if their median interval differed. We previously concluded that 'the close overall agreement across all stems and the whole period may be coincidental' (Miller *et al*. 2007 p. 914). Comparing the two hundred or so positive fire incidences between the two records shows that they do not coincide in space – within the same individuals – or time in the two records. That the two records have the same fire-incidence rates merely reflects the fact that sites with more satellite fires than grasstree fires are numerically balanced by sites with the reverse ratio (Figure 2 note c). Similarly, there are many between-fire intervals of the same size in the two records (Figure 2 note d), but few of them are on the same grasstree and at the same time. Ward compared grasstree and satellite fire intervals across all grasstrees, but it makes no sense to compare aggregated mean or median fire intervals across grasstrees when the crucial correspondence is within grasstrees. Unfortunately, nothing can be concluded from Ward's aggregated analysis of fire interval in relation to the validity of the grasstree record.

Finally, Ward (2009) addressed the 'agreement' between the datasets in a new analysis. His approach was to use contingency tables to determine whether there was more or less agreement between the two records (in the asymmetric 4 year window) than would be expected given the marginal totals of these agreement tables (Figure 3a), and then to create, and test, a large number of these tables to determine a probability of them all having more agreement than by chance (Figure 3b). As we have already shown that there is more agreement than chance between the records (Miller *et al*. 2007), this analysis adds nothing new to our collective understanding. But because Ward's presentation of this analysis is rather complex, and also to more clearly justify the preceding statement, we address this analysis in some detail.

First, the absolute difference in the number of observed and expected records that Ward tabulated to assess agreement can be extremely small, and may not be significant in any statistical or practical sense. His table 2 includes examples of 0.2 observations out of a total of 190 (0.1%) as being sufficient to count as more 'agreement' than chance, and the average difference across his 15 comparisons represents only 2.9% of observations. If the question is whether grasstree and satellite record agreement is more or less than chance, then there is only one sample with which to test this outcome—the entire dataset. However this approach gives only an absolute, binary outcome—'more' or 'less' agreement than expected by chance. It does not say how much more, nor whether it is significant (although both are calculable via bootstrapping). Instead, to assess significance, Ward created 15 separate agreement tests by splitting the data longitudinally into five sets of 19 grasstrees and then these again into three decades (Figure 3b). The justification that Ward provided for this data splitting is 'to avoid spatial autocorrelation within sites' (Ward 2009 p. 263). He assessed the cumulative probability that all 15 tests show arbitrarily more agreement than chance to be 0.5^{15} or p = 0.00003. While this value seems extremely convincing, it only shows that the records agree to some undetermined (but potentially very small) extent. It is created by exploiting the very spatial non-independence of the grasstree sample that purportedly justified its re-analysis.

Literally interpreted, Ward's concern for spatial autocorrelation implies a belief that between-record agreement (not just fire-incidence) might be influenced by the spatial arrangement of the sample. The grasstree dataset does have a unique spatial arrangement: each analysed stem occurs within one of 20 clusters of exactly five neighbouring grasstrees, and these clusters are separated by up to several kilometres (Figure 3c). We selected this design so that we could examine the correspondence of fire record within neighbouring grasstree stems (expected to be quite good, but actually not: Figure 2; Miller *et al*. 2007 figure 2). Because each cluster contains five grasstrees, the sampling is not biased by unbalanced spatial sampling intensity at that scale. However, if concern persisted that spatial non-independence of agreement between the two records among sampled grasstrees within clusters might bias results, then analysis of just one individual from each cluster would be a reasonable approach to remove the problem. However, it is clearly not sensible to suggest that five samples of 19 grasstrees, each made up of one individual sampled from 19 identical locations, with every sampled individual just metres from one individual in each of the other four samples, could be independent – especially if used in a test that was overly sensitive to small differences, and in a system with even a small amount of aggregation in agreement at the cluster scale (Figure 2). Multiplying the probability of more agreement-than-chance occurring in one sample, by the probability of the same event occurring in a sample with an identical spatial pattern and repeating this for multiple identical samples merely exploits the spatial pattern of the sample to inflate the calculated overall level of significance.

While this test might assess for more agreement-than-by-chance, it does not quantify the extent of the agreement. Its low p-value simply reports that the co-occurrence of fire incidence in the grasstree and satellite records was higher than would be obtained by a random scattering of the records in space and time. A statistically significant departure from randomness does not imply that the grasstree record is a faithful record of fire history. The necessary question is 'how much better than chance is it'? In Miller *et al*. (2007), we answered this question through a bootstrap analysis, finding that 46% of the 202 positive grasstrees fire records matched a positive satellite fire record (within a 5-year window) but that on average, 27% of matches would be expected by chance. However, we did not explicitly follow this with the observation that the difference between these values should indicate the percentage of grasstree records that were both true positives and not attributable to chance. That is to say, on average, 19% of grasstree fire records can actually be attributed to fire. The remainder either do not match a satellite fire record in the 5 year window (54%), or do, but would be expected to do so by chance alone (27%).

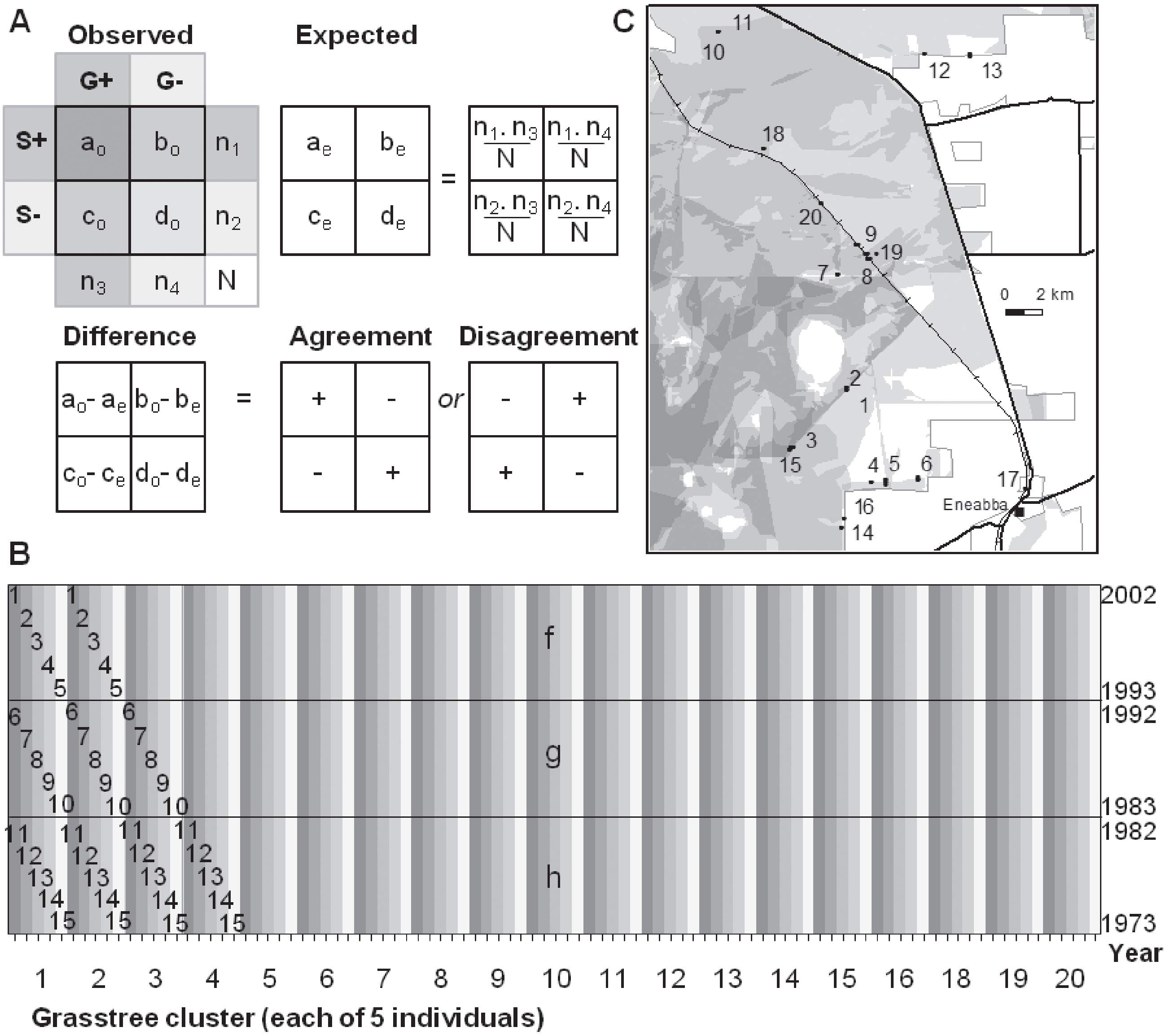

Figure 3 The Ward (2009) approach to assessing agreement among the grasstree and satellite records. (A) Counts of fire presence (+) and absence (-) observed in the grasstree G and satellite S records were tabulated in a two-way table. The difference between these observed ($_o$) and expected ($_e$) counts, calculated from marginal totals (n), indicates whether there is more agreement (a + d) or disagreement (b + c) than expected for the table. (B) To create more tables, the data were divided into three decades (f, g, h), each with 95 individual grasstrees sampled (vertical grey bars; the grasstrees were arranged in 20 clusters of 5 individuals: Ward excluded the 17th cluster as it had neither more nor less agreement). These decadal data were then further divided longitudinally, with one single individual from each cluster per group. This creates 15 tables (here numbered diagonally), where 1 = the 1st grasstree in each cluster in the 1st decade, 2 = the 2nd grasstree in each cluster in the 1st decade … to 15 = the 5th grasstree in each cluster in the 3rd decade. (C) The spatial distribution of the 20 grasstree clusters around the Eneabba sandplain, overlain on the satellite fire history (dark shades indicate more fires).

DISCUSSION

A number of tests of the grasstree/satellite record comparison are presented here, as well as in Ward (2009) and in Miller *et al*. (2007). As the variety and relatedness of these and the detail to which they are discussed may cause some confusion about their overall interpretation, we here list (italicised below) and summarise their outcomes for testing and quantifying the relationship between the grasstree and satellite fire records. The first group of these tests considers the number and historical distribution of positive fire records. Ward's chart of *pattern of grasstree fire records with time* (Ward 2009 p. 265) is simply illustrative; it is silent on the data's validity. We previously noted the close correspondence in *number of fire incidences* in the two records, but the incongruent distribution of these records among grasstrees and decades shows that it does not indicate a common process. Ward's *comparison of mean and median inter-fire interval* between the two records is effectively a restatement of the correspondence of overall fire incidence, it reflects the same data, and has the same interpretation.

The second group of tests relates to quantification of 'agreement' between the two records and are all based on two-way tables (also discussed as contingency tables and confusion matrices – that have a specific terminology). These are analysed with exact, and broad, precision windows and interpreted via three qualitative indices and two quantitative tests. The qualitative indices are the *Kappa* and *TSS indices*—which consider the exact match of records in the same year, here providing identical results indicating 'poor agreement'—and the *fraction of grasstree fire records that are false positives* (together with false negatives, etc). We report the fraction of false positive grasstree records in precision windows of 1–9 years: 54% of the grasstree records do not match a satellite fire record within a 5 year (±2 years) window. This index provides a suggestion of the scale of the agreement between the records but does not test its likelihood. We previously reported a *bootstrapping approach* to test this likelihood, finding that the records do agree more than by chance, but here emphasise that only 19% of the positive grasstree fire records match positive satellite records (within ±2 years) and cannot be attributed to chance. Ward's test of *manipulated agreement probabilities* is simply the absolute difference between observed and expected counts of agreement applied to different subsets of the data. We have already shown that there is more agreement between the records than would be expected by chance alone: Ward's test says no more about the data than simply confirming this result.

A theme of this discussion is the extent to which observed agreement can be considered adequate. Does it matter if a TSS of 0.105 is different to a Kappa score of 0.104, or that neither provides an actual test of significance? Both are simply indices of agreement and in this case exactly where one draws the upper boundary for 'poor agreement' is immaterial: both tests range from 0 to 1 (no agreement to complete agreement) and no-one would consider 0.1 as 'very good' or even 'good' agreement. Similarly with Ward's analysis of agreement in two-way tables: should a difference of 0.2 observations among 190, or an average difference of 2.9% be counted as an important difference between observed and expected counts? The answer depends on the number of independent trials. The outcome of Ward's analysis indicates that the agreement between the satellite and grasstree records is better than might be expected from a random coin toss, but not how much better. Our results show broadly how much better than random the grasstree record is at predicting fire history, with bootstrap analysis suggesting that around 20% of grasstree fire indications may actually be due to fire.

CONCLUSIONS

We acknowledge that the grasstree record does appear to include information about fire history; that some dark banding in grasstree stems may represent a fire response. We believe that analysis of the lighter, annual banding of grasstrees may be sufficiently accurate for determining their annual growth rates (provided the colour bands are distinct enough) and hence provide insights on important issues such as plant longevity, population structure and the effect of growing conditions on growth rates. The fire signal in grasstree bands is worthy of further study, but its presently interpreted signal is weak and increasingly unreliable the further we go back in time.

Our major concern is that because 8 out of 10 dark grasstree stem bands cannot be attributed to fire, it is wrong to interpret the whole grasstree record as a reliable fire history. This issue would represent only an academic disagreement if the interpretation was not extended to support fire management procedures that threaten biodiversity by burning at intervals shorter than most plant species require to accumulate self-replacement capacity in seed- and bud-banks (Enright *et al*. 2011, Burrows *et al*. 2008). As rates of grasstree error increase with time into the past and as this error overstates fire frequency, use of the grasstree fire record from the distant past to inform 'appropriate' contemporary fire regimes will elevate conservation risks associated with overburning. As it stands, the grasstree banding method cannot be considered useful for reconstructing fire histories, and it should have no role in determining modern fire-management practices.

ACKNOWLEDGEMENTS

We thank Journal reviewers Neil Burrows, Michael Calver, John Henstridge and Stephen Hopper for their constructive comments.

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Received 14 February 2012; accepted 6 June 2012