Critique of a paper submitted to the Environmental Protection Authority (EPA) of Western Australia entitled '*Fire Regimes and Biodiversity Conservation: A Brief Review of Scientific Literature with Particular Emphasis on Southwest Australian Studies*' by Grant Wells, Stephen D Hopper and Kingsley W Dixon

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One of four components of CALM's submission to the Environmental Protection Authority, in response to the EPA review of CALM's fire management policy and practices

Critique of a paper submitted to the Environmental Protection Authority (EPA) of Western Australia entitled *'Fire Regimes and Biodiversity Conservation: A Brief Review of Scientific Literature with Particular Emphasis on Southwest Australian Studies'* by Grant Wells, Stephen D Hopper and Kingsley W Dixon

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As noted by Bond and Van Wilgen (1996), 'there is a very large applied peer-reviewed global literature on fire science and fire ecology, but discussion of fire ecology is lacking in general ecological textbooks. As these texts often introduce the conceptual foundation of ecological science, this means that few foundation concepts of fire ecology have been well developed'.

This summation applies equally to south-west Western Australian ecosystems. Bond and Van Wilgen also note that because fire ecology concepts are buried in the applied literature, they have escaped the attention of most ecologists who write general ecological texts. They also note that fire ecology suffers from a philosophical bias. Most textbooks are written, and ecologists trained, in parts of the globe that are not fire-prone, or which burn infrequently, and it was in these regions of the globe that the seductive concept of the 'balance of nature' originated in the 1900s. Fire, as a natural environmental driver of dynamic ecosystem change, does not fit into the 'balance of nature' or equilibrium concept that was readily embraced by most ecologists in the 1960s or from these non-flammable parts of the globe.

In this critique, we identify several important shortcomings of the review paper:

- It is selective and biased in its interpretation of the literature.
- It denies the reality that prescribed burning and fuel management are fundamental to reducing the severity (scale, intensity and impact) of wildfires.
- It fails to recognize the importance of fire as a natural environmental factor which, at appropriate intervals, seasons and scales, maintains biodiversity and ecosystem processes.
- It reflects a lack of understanding of the logical linkages between fire behaviour (intensity and scale) and the physical environment (fuel characteristics, topography, climate and weather).

It is a standard practice in scientific publication for authors of reviews to be expert in the topic being reviewed. Expert standing is attested by sustained research in the topic (in this case fire management and fire science) and is demonstrated by numerous publications in peer-reviewed science journals. Expertise in related matters (in this case botany or plant ecology) is not relevant.

It is claimed that the review also draws upon the 'collective reading of the two junior authors of the scientific literature over the past two decades (p.1)', but there is no indication of the extent to which the authors have read the substantial fire literature. Given this, comment such as 'much more research is required to approach an adequate understanding of fire regimes and biodiversity conservation (p.1)' is without context, unhelpful (how much more? What is 'adequate'?) and overlooks the fact that management must proceed with the best available knowledge. It fails to recognize that management is a process of continuous improvement, adapting to new knowledge, experience, threats and community expectations.

1. Inadequate understanding of fire management and fire science

Lack of fire logic

The paper is permeated by an inadequate understanding of the logical linkages between climate, weather, fuel quantity, fuel dryness, rate of accumulation of fuel, topography, previous extent of fire, period since the last fire, moisture (flammability) differentials present in the landscape, and the number of ignitions (see diagram on p. 442 and pp. 87-106 in Abbott and Burrows 2003).

Perhaps this logic is so basic that it is assumed to be simplistic; however, it is certain that extensive, low fuels will not permit high intensity fires to spread far. Moist fuels will not support high intensity fires. Frequent fires result in low fuel loads and low fire intensities, and therefore create a finer mosaic of burnt and unburnt patches than infrequent, intense fires. This is basic fire physics and does not require complex experimentation except to quantify the grain size of the resultant mosaic. We fail to understand how such logical conclusions can be dismissed by the authors (p. 6) as 'opinion'. The purpose of applied fire research is to specify desired landscapes and to use such information to assist fire management.

Fire managers do not claim that prescribed burning will eliminate wildfire from non-human ignition sources (p. 6). This reflects a lack of understanding of fire behaviour and of the principles of prescribed burning. However, a frequently burnt landscape will not permit large, intense wildfires to form, thereby reducing the frequency and severity of such fires. Conversely, eliminating prescribed fire from the landscape will result in a regime of severe wildfires.

Prescribed burning in Kings Park bushland every 3 years is presented as evidence that prescribed burning does not reduce the frequency or scale of wildfires (p.7). Kings Park is not representative of south-west bioregions where prescribed burning is carried out. Where the predominant fuel is annual weedy grasses, as is the case with Kings Park, then it is not surprising that prescribed burning every 3 years is ineffective - it would exacerbate the problem by encouraging proliferation of weeds because of the remnant nature of the Park and the matrix within which it sits. Extrapolating this experience to the broader, more contiguous tracts of south-west forests, heathlands and woodlands is inappropriate. Similarly, using an example from blackbutt (*E. pilularis*) forests in eastern Australia is equally inappropriate. The blackbutt forest example tells nothing of the extent (area and quantity) of fuel reduction by burning every 2-4 yrs. If prescribed burning is carried out under conditions that remove only a small proportion of the fuel over a small proportion of the landscape, then of course it will be less effective – fire logic.

The authors suggest that to be effective, prescribed burning must be done at such short intervals as to be threatening to biodiversity. This demonstrates a lack of understanding of the logic and practice of prescribed burning, which entails a system of rotational, strategically placed buffers, variation in fire interval, season and patchiness to accommodate the biota based on current knowledge. To prevent (rather than mitigate) wildfires would require, as suggested by the authors, burning everything every 2-4 yrs. As already stated, prescribed burning is not implemented to prevent wildfires, but to reduce their severity and to conserve fire maintained ecosystems.

Another example of inadequate fire logic is the assertion (pp. 2-3) that balgas were individually set alight by Aborigines. The authors need to explain what stopped such ignitions from spreading to the litter in the middle of summer in the Mediterranean climate of south-west WA.

The claims (pp. 2, 4) that charcoal deposits imply infrequent fires are more likely to be an artefact – it is only high intensity (and hence infrequent) fires that are likely to leave detectable quantities of charcoal. Frequent, smaller scale fires will leave proportionately less charcoal.

It should be self-evident that the most fire-sensitive taxa will be restricted to those parts of the landscape that are less flammable. We are therefore puzzled by the authors' statement (p. 8) beginning 'Some authors consider' as though this is opinion and not logically based.

To suggest that research conducted to date on fire is insufficient to allow prescriptions to be made for fire regimes to maximize biodiversity conservation (pp.10-11) is unhelpful. It is highly unlikely that detailed understanding of the fire response of all organisms and communities that comprise the south-west bioregions (or any bioregion) will ever be known. Such an understanding is unachievable and unnecessary for effective risk management. Fire management aims to maintain and protect biodiversity at various scales. Management decisions and actions are based on the best available knowledge and cannot be deferred until some undefined threshold level of knowledge is reached. It would have been helpful if the reviewers considered what is known, as well as what we need to know (critical gaps), and then drew some conclusions about the appropriateness or otherwise of current fire management, including prescribed burning, based on current knowledge.

Missing the point

The paper (p. 3) misses the self-evident and obvious point that a landscape consisting of a mixture of post-fire stages will provide a more diverse set of habitats for the biota. A landscape that consists of a single post-fire stage will not suffice to conserve the biota that depend on the post-fire stages not represented.

The authors (p. 2) miss the point in the discussion of descriptive and experimental studies and erect a 'straw man'. In our experience, no objective scientist deliberately chooses a descriptive approach where an experimental one is feasible. Furthermore, the authors seem unaware that the rise of the discipline of landscape ecology in the 1990s has led to a focus on 'experiments' at large spatial (and temporal) scales. The concepts of a true control and true replicates then become questionable. Field ecologists employ a range of research design and statistical techniques. Their training and experience, assisted by statistical analysis, enables them to interpret data at various scales. Publication of findings in peer reviewed scientific journals is the quality control mechanism for science.

The most rigorous experiments are those that can be performed in artificial environments such as the laboratory/glasshouse. Most applied fire ecology research does not lend itself to this methodology, but is field-based. In laboratory/glasshouse experiments precision, accuracy and reliability are maximized at the expense of reality.

The authors pose (p. 2) the question 'How could [some plant species] have persisted in the landscape if the balga fire scare intervals applied to whole landscapes' but have failed to make the connection that frequent introduction of fire into the landscape results in a mosaic of recently burnt and unburnt patches, as explained on pp. 134-5, 139-140 of Abbott and Burrows (2003).

To suggest that igniting balgas in summer will not lead to landscape fires is naïve (p.3). If fires were lit frequently, the landscape would not be 'incinerated' (p.3) because fires would be of low intensity and patchy. It is also likely, and has been observed in the field, that very low intensity fires are not lethal to mature plant species such as *Banksia seminuda*, which has a relatively long juvenile period (5-6 years). Therefore, these species are able to escape frequent, very low intensity patchy fires, but populations are killed by infrequent, intense homogenizing fires. The authors do not appear to understand that in the absence of adequate prescribed burning, the fire regime will be one of infrequent but severe wildfires, threatening conservation and life and property. Prescribed burning for fuel management is not a strategy for preventing wildfires. It is a strategy for reducing their severity by minimizing their intensity and size, and enhancing safe suppression opportunities. It has been well demonstrated locally and abroad, that wildfires burning in heavy (long unburnt) fuels under warm, dry and windy conditions, are unstoppable, life threatening and environmentally damaging.

The emphasis placed on smoke as a cue for germination (p. 3) puzzles us. We see this focus as a byproduct of the horticultural, rehabilitation and propagation research interests of Kings Park. However, fire in bushland results in heat and ash (nutrients) as well as smoke, and emphasis on smoke alone as the single relevant factor is unjustified. Furthermore, smoke studies are simple to perform (and hence can be executed rigorously) because they can be done in the laboratory/glasshouse. Fire ecologists understand that successful establishment and growth of plants following fire is more involved that simply cueing seed germination through the products of combustion, but also requires a suitable environment, which is provided by fire – including nutrient enrichment (the ash bed effect), favourable moisture regimes, temporary reduction in competition and herbivory, and synchronous and massive seed release.

The authors point to the occurrence of tens of thousands of species in south-west WA and imply (p. 5) that it would be desirable to monitor the effect of fire regimes on all these species. Such an approach is unnecessary and it is surprising that the authors do not invoke the use of the Precautionary Principle as a guide. We suggest that not treating all parts of the landscapes in south-west WA alike in terms of fire regimes would be a sufficiently cautious approach to adopt.

The criticism made by the authors (p. 6) of the retrospective approach is unjustified. Why should managers wait for 60 years for the results of longitudinal studies instead of utilizing available records of fires from 1940-2003 to analyse the cumulative impacts of successive fires on the biota? Retrospective (space-for-time) studies are extremely valuable, and together with experimental research, longitudinal studies and field observations, add to the body of knowledge. Ecologists are well aware of the advantages and disadvantages of various methodologies and it rests with their professionalism, assisted by modern statistical methods, to diligently interpret evidence derived from such studies.

As mentioned above, the claim (p. 6) that prescribed burning does not eliminate fire from non-human ignition sources is misleading. There is ample evidence that prescribed burning beyond a certain threshold level limits the areal extent of wildfires. There is no evidence that current prescribed burning regimes in south-west forests have caused, or are likely to cause the extinction of any species (see Abbott and Burrows 2003).

We do not understand the authors' focus on cryptic life stages (pp. 7-8). Species that occur in an inert form (seeds) clearly are not contributing to ecosystem processes such as photosynthesis, and are not contributing to functional plant diversity until they are stimulated to germinate, usually by fire. There is also doubt about their longevity, as many seeds, especially those of serotinous species, have a short half-life in the soil or on the soil surface. Therefore viability of cryptic life stages (canopy or soil stored seed) in the long absence of fire, which cues germination/rejuvenation of cryptic stages, is diminished. This is another example of overstatement of peripheral issues by the authors, consistent with inadequate comprehension of fire management and fire science.

The point that not all regimes favour all species has been largely overlooked. A consistent theme emerging from the global fire ecology literature, across a range of organisms and ecosystems, is that some species are favoured by certain fire regimes, some are disadvantaged and others appear little affected (Abbott and Burrows 2003). Hence the conclusion that fire regime diversity promotes biodiversity at the landscape scale. The authors focus on negative effects and overlook positive and neutral effects.

2. Inadequate standard of scholarship

Selectivity and bias

The commentary is highly biased towards criticism of work that promotes the role of fire in maintaining biodiversity and selectively quotes from the literature results that are anti-fire (e.g., change in abundance of *Acacia browniana* following regular burning, p. 251 in Abbott and Burrows 2003). Species that increase in abundance (p. 249) have been ignored. Most species remain unchanged in abundance.

The literature reviewed is highly selective and inadequate – the 50 or so papers cited by the authors are a small fraction out of the total literature relevant to this topic. The authors emphasize what we do not know instead of making effective use of what is known and thereby identifying the critical gaps in knowledge.

Much of the discussion focuses on debate about experimental method, fire pre-history and fire adaptive traits. While this is interesting, it detracts form critical discussion of the substantial literature on fire effects, as summarized in Abbott and Burrows (2003).

By quoting selectively from the literature, the authors reinforce an impression of uncertainty, which is not balanced by adequate discussion, or reflection on what is known.

The authors use the term 'hypothesis' to apply to any concept that they do not accept. For example, on p. 7, they question the concept that a mosaic of fire ages promotes biodiversity, overlooking the fact that species have different habitat and niche requirements. Some species require sparsely vegetated habitats and early post-fire stages; others need densely vegetated habitats and intermediate to late post-fire stages while others prefer a mix of stages within their home range (e.g. see pp. 298, 302 of Abbott and Burrows 2003 for factual evidence).

Personal viewpoint allowed to overshadow factual evidence and expert review

In their idiosyncratic explanation (pp. 2-3) of why balgas alone were burnt by Noongars (and not the surrounding landscape), the authors overlook the historical fact that Aborigines always carried a flambeau with them (see pp. 119-120 of Abbott and Burrows 2003).

In attempting to discredit the balga research of Lamont *et al.* (2003), which demonstrates that fires occurred frequently prior to European settlement, the authors do not consider the possibility that the interpretation of Lamont *et al.* might be correct, even if only applying to drier parts of the south-west landscape frequented by Aborigines. Instead they go to considerable lengths to present unsubstantiated, unpublished opinions as alternative interpretations of the data presented by Lamont *et al.* in an attempt to diminish the role of fire in landscapes of south-west WA. In doing so, they ignore other relevant literature, which is consistent with interpretations by Lamont *et al.*, including published historical and anthropological evidence (Hallam 1975, pp. 119-146 in Abbott and Burrows 2003).

On p. 10 the authors selectively quote the literature and mount an argument that not enough is known about fire impacts and that development of prescriptions for managing fire are not possible. They issue a call for 'further studies' instead of adopting a more practical approach (the Precautionary Principle).

We question the statement that there is 'rapid geographical turnover in less vagile organisms' (p. 11). Recent studies conducted by FORESTCHECK teams have indicated otherwise for a variety of taxa. In our opinion this is a hypothesis that requires testing.

Lack of objectivity

Throughout the document, the authors have not taken an objective or balanced view but have taken a 'devil's advocate' approach, focusing discussion solely on what they perceive as negative impacts of fire without considering the important role of appropriate fire regimes in maintaining biodiversity (see Abbott and Burrows 2003).

They have not undertaken the review according to the Terms of Reference, but have taken the opportunity to criticize science that is not consistent with their view, and to promote their view. It is worth noting that while the authors are critical of descriptive fire ecology studies that report correlations, they readily interpret descriptive data summary (p. 9) about the number and size of wildfires in Kings Park as causal to fire management.

Poor conceptualization

Given that fire has been demonstrated to have been present in south-west WA since the Pliocene, sufficient time has existed for elements of the biota to have adapted to a range of fire regimes. The arrival of humans some 50 000 years before the present has acted to further filter the biota and cause changes in the distribution and abundance of some species, depending on which bioregion(s) they occupied. We therefore reject the artificial distinction between 'fire tolerance' and 'fire adaptation' as semantic, and recognize that existing adaptations to drought and insect herbivory have served as pre-adaptations to coping with frequent fire in certain landscapes.

The authors' claim that many fire ecologists incorrectly use terms such as 'fire adapted' in relation to characteristics and traits that enable vegetation to persist in fire-prone environments (p.2). They claim neutral terms such as 'fire tolerant' should be used because there have not been rigorous tests to prove existence of evolutionary adaptations to fire (p.2). This is a contestable notion and to some extent depends on an accepted definition of adaptation. Many fire ecologists accept that fire adaptation, or pre-adaptation, applies to species of plants that have evolved with special traits contributing to successful abilities to regenerate, survive and compete in fire-prone environments. For example reproduction and establishment traits such as serotiny, fire-stimulated seed germination (smoke, heat), fire stimulated flowering, fire stimulated seed release and dispersal and fire-stimulated growth (ash-bed effect) are considered fire adaptations. In fact, many species in fire-prone environments have evolved barriers to seed germination that are overcome only by fire-related cues (Keeley and Fotheringham 1998).

Other traits, such as thick protective bark and an ability to resprout following fire may not be fire adaptations, but whether they are or they are not, these traits enable plants to persist (vegetatively) in a fire prone environment under a range (but not all) of fire regimes. On the other hand, to suggest that plants are 'fire tolerant', rather than fire adapted, suggests a belief that fire is a process that plants must suffer, or tolerate, and denies the reality that many plants and vegetation structures (habitat) are in fact dependent upon specific fire regimes for their persistence. There is no doubt that in the absence of fire, or at the other extreme, very frequent fire, some plants would not be able to effectively reproduce or compete and could become locally extinct (see Abbott and Burrows 2003). It also overlooks the dependence of some fauna habitat types, plant communities and habitat structures on fire – for example, many thicket-forming species and even-aged cohorts are the result of synchronous regeneration following fire.

The authors imply that most fire ecology studies cannot be trusted, or are inadequate, because they are correlational rather than rigorously designed to examine cause and effect (p.2). This is dismissive and inaccurate, as well as a misrepresentation. Analyses of variance applicable to well replicated experiments in artificial settings such as in the laboratory or the glass house are not well suited to the relatively large scale, diverse and complex reality of natural ecosystems. From the local and global fire literature it is evident that the study of the ecological effects of fire has incorporated a variety of methods. For example, replicated field experiments involving different fire regime treatments have been used widely in grasslands, savannas, forests and shrublands. Other methods include BACI (before, after, control impact), BA (before and after), after fire, space-for-time experiments, and longitudinal studies. These include some of the longest running ecological experiments in the world. While some of these are poorly replicated and might fail modern criteria for experimental design, taken individually and collectively, they give valuable insights into the effects of different regimes, and to the ways in which organisms and communities respond to fire, and so should not be glibly dismissed. Studies of fire scars, pollen and charcoal deposits also give us a different temporal scale of understanding of the relationships between fire, climate and vegetation.

Furthermore, we are of the view that a variety of epistemologies, including western science, anthropology, local and Indigenous knowledge, are valid sources of information for understanding fire.

The authors do not understand that at some point all habitats will be regenerated by fire in a fire-prone environment, including the habitat of noisy scrub birds. The authors appear unaware of recent evidence that noisy scrub birds will re-occupy and utilize vegetation within 4-10 years of fire and the main threat is widespread, infrequent wildfire (see Abbott and Burrows 2003). Recent studies show that a mosaic of vegetation at different post-fire stages, including long unburnt and recently burnt, provides ideal habitat for animals such as the mainland quokka (Hayward *et al.* 2003, Abbott and Burrows 2003).

The authors rely too much on presenting Kings Park as a model landscape. However, it is clearly not representative of the natural ecosystems of south-west Australia, as it is small (360 ha), isolated, suburban, degraded, and weed infested.

Insufficient acknowledgement of previous work

The authors claim that experimental (laboratory) studies that provide a link between attributes of fire and biodiversity are in the minority and cite work by one of them (Dixon *et al.* 1995), which relates to the role of smoke in stimulating the germination of some native plant species, as an example of such an experiment (p.3). Although not often acknowledged, South African scientists (de Lange and Boucher 1990 and Brown 1993) first reported the importance of smoke in stimulating seed germination. While this is an interesting and important finding, it ranks with numerous other experimental studies that have shown that other factors including heat treatment and seasonal conditions of ambient soil temperature and moisture (Bell *et al.* 1993) also stimulate seed germination of many species. These studies help explain field observations of greatly enhanced germination and survival following fire in summer and autumn. In most south-west WA ecosystems, plant regeneration is very poor or absent in the inter-fire period (Abbott and Burrows 2003). Other attributes of fire that impact on ecosystem processes do not necessarily require experimental research, but can be observed or deduced – for example, serotiny, fire-stimulated synchronized seed release, fire stimulated flowering, the ash-bed effect etc. More importantly, there are many published field studies that report on relationships between fire and biodiversity, and for south-west WA, these have been summarized in Abbott and Burrows (2003).

3. 'Straw Man' arguments

We are not aware of any proposal for a return to Noongar burning practices, as claimed by the authors (p. 4). We do, however, recognize that large areas of uncleared land (National Parks, Nature Reserves, State Forests) could be partially returned to Noongar burning practices (appropriate scales of frequent burning) as part of the diversity of fire regimes. We agree that frequent burning in small, weedy remnants in urban or agricultural landscapes is inappropriate (see p. 448 of Abbott and Burrows 2003). There is no suggestion of returning in a strict sense to an historical (Aboriginal) fire regime.

While we acknowledge that herpetofauna have been poorly studied, we question the authors' focus on this group. Even in the highly fragmented wheatbelt landscape, reptiles and frogs have experienced no extinctions of species in comparison to the mammals, birds and vascular flora. Therefore there is no compelling reason to suggest why frogs and lizards should be endangered by the careful use of prescribed fires. This is supported by the limited research on these organisms (see Abbott and Burrows 2003 p. 349).

The authors (p. 10) suggest that fire management needs to consider salinity management. However, salinity is an issue predominantly in the highly cleared wheatbelt where little prescribed burning is done. We therefore do not understand the point being made.

4. Misinterpretations

Joseph Banks did not visit WA, as implied on p. 3.

The descriptive data presented in Figures 5 and 6 (p. 9) are open to an alternative interpretation to that proposed by the authors. Prescribed fire will only be effective if a large enough proportion of the landscape consists of young fuels. The authors should have disclosed the areal extent of prescribed burns in Kings Park and graphed this against the areal extent of wildfires in Kings Park. The number of wildfires is largely irrelevant – what is important is the severity, that is areal extent and the intensity.

Fig. 6 seems to indicate a trend of increasing size of wildfires. As would be expected, following a year when a high proportion of the Park was burnt, there is a period of little wildfire activity – this is not necessarily because of good management, but because there is less available to burn in the years following a wildfire. In fact c. 80-100 ha burns every 10 yrs of so, equivalent to c. 25% of the flammable area of natural bushland in Kings Park. This is a surprisingly high proportion given the location and nature of the park, which should facilitate early detection, easy access, and rapid suppression of wildfires.

At one level, fire impacts are not that difficult to predict (see Abbott and Burrows 2003 p. 363). At the detailed level, they are, so a holistic approach rather than reductionism is preferred when managing fire.

Statements such as 'research conducted to date is insufficient to allow prescriptions to be made for fire regimes to maximize biodiversity' (p.10) and that 'more research is required' (p. 1) are obvious but unhelpful. Fire management is evolving as knowledge becomes available. In the large, contiguous tracts of bushland in the south-west, it is not an option to cease prescribed burning until we have complete knowledge. The consequences of such a management strategy will be a regime of severe life-threatening wildfires started either by lightning or people, with devastating consequences for biodiversity conservation and the community. There is sufficient knowledge at one level to guide management, a process that will benefit and develop from ongoing research and monitoring, but which cannot be paralysed by incomplete knowledge.

To claim that research conducted to date is an inadequate basis for managing fire is a mis-informed and simplistic opinion. Management is an adaptive process – we must manage with the existing knowledge even though it is incomplete. This is completely different from the perspective of the authors, who portray that it is all too complex and difficult.

We disagree that the 12 principles stated on pp. 445-6 of Abbott and Burrows (2003) are hypotheses (p. 11). Instead they have been distilled from hundreds of empirical studies on fire behaviour and fire ecology. We agree that the three hypotheses proposed by Hopper (p. 29 of Abbott and Burrows 2003) are hypotheses and not principles.

5. Terms of reference not adequately addressed

The authors have not adequately addressed the role of fire in maintaining and protecting biodiversity. For example, no comparison is made of the beneficial effects from prescribed fires (low intensity, patchy, frequent) and the adverse impact of unsuppressed wildfires (high intensity, uniform large patches).

The authors have not provided advice on current strategy of fire management practices which promote biodiversity protection.

Discussion about monitoring of indicator species is irrelevant to the Terms of Reference.

6. Points of agreement

We agree with the following points made by the authors:

- Indicator species may not be effective surrogates for other groups of biodiversity (p. 6). CALM has adopted this approach in its FORESTCHECK monitoring program, and the Walpole Fire Mosaic project.
- Fire exclusion is appropriate for the conservation of some species. However, we differ from the authors in that we accept frequent burning (and other forms of fuel management) in landscapes surrounding such areas as the most effective methods of preventing the spread of wildfires into such areas.
- Weeds promote frequent burning, and frequent burning promotes weeds. This is probably the major issue that hampers the effective biodiversity conservation management of remnant bushland in urban and agricultural landscapes.
- Need for consideration of the impact of global warming on fire regimes and any flow on effects to biodiversity. Global warming and decreasing rainfall in south-west WA will extend the fire season, exacerbate the fire proneness but may reduce the density of vegetation and hence fuel loads. This topic needs clear conceptualization.
- Need for ongoing fire research and monitoring programs. However, we find it unhelpful and impractical to simply identify the need for much more research, and imply that current management should be altered or curtailed in some undefined way until we know a lot more. We would always support more research (although we could debate where the priorities properly lie), but we do not have the luxury of doing nothing in terms of fire management until more research is done (we all know our understanding will never be complete or perfect).
- Data sharing between researchers. Attempts by the Royal Society of Western Australia, CALM, and universities to gain funds from the Premier's Science Council to resolve this hindrance have not proved successful.

7. References

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