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REPLY

Reply to Comment on 'Self-thinning forest understoreys reduce wildfire risk, even in a warming climate'

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Abstract

Our previous analysis of mapped records of forest fires in National Parks in Southwestern Australia showed that fires initiated a pulse in flammability (the likelihood of a point being burned by wildfire), but that flammability declined as forests matured (Zylstra *et al* 2022 *Environ. Res. Lett.* **17** 044022). This reduction in flammability was contrary to that expected from modelling used by the West Australian Government to guide management, but consistent with expectations from peer-reviewed fire behaviour science and published ecological drivers of fire behaviour. Miller *et al* (2024 *Environ. Res. Lett.*) argued that our reported decline in flammability of long-unburnt forest is an artefact of poor data quality including flawed records kept by the West Australian Government, along with fewer and smaller sample sizes in long-unburnt forest. These problems, they claim, biased these age-classes toward values of zero flammability due to a rounding error. Critically, Miller *et al* (2024 *Environ. Res. Lett.*) did not test their hypothesis by repeating the analysis with these data removed. Here, we show that Miller *et al*'s (2024 *Environ. Res. Lett.*) concerns are dependent upon the mathematical fallacy that rounding errors only occur in one direction (rounding flammability down to zero), when they have an equal likelihood of rounding upward and elevating flammability. The effect of this is to introduce noise rather than bias. We tested their hypothesis by repeating the analysis of Zylstra *et al* (2022 *Environ. Res. Lett.* **17** 044022) with a better suited statistical method on an improved and expanded dataset after removing the small patches that Miller *et al* (2024 *Environ. Res. Lett.*) proposed would bias the findings. Contrary to the objections of Miller *et al* (2024 *Environ. Res. Lett.*), removing lower quality data revealed that the mature forests were even less flammable than expected, so that only annual prescribed burning could reduce bushfire likelihood below that in forests unburnt for 56 years or more. Our findings highlight the role of prescribed burning in creating a more flammable landscape.

1. Introduction

The prescribed burning program in southwestern Australian forests is argued by some to be exemplary on an international scale (Sneeuwjagt *et al* 2013). However, our recent independent analysis of West Australian Government fire records concluded that the program had instead increased fire risk across the area (Zylstra *et al* 2022). This conclusion differed from an earlier work (e.g. Boer *et al* 2009) because Zylstra *et al* (2022) examined patterns in flammability (frequency of fire at a point) over a much longer time-span. Although the findings of Zylstra *et al* (2022)concurred with earlier studies by finding a brief period of low flammability immediately after fire (Boer *et al* 2009, McCaw *et al* 2012, McCaw 2013), it contrasted with such studies by showing that flammability later declined. Because the long-unburnt (untreated) state of the forest had comparatively low flammability, Zylstra *et al* (2022)concluded that the decades-long pulse of flammability that followed a burning treatment was caused by the treatment. In support of this, Zylstra *et al* (2022) referenced fire behaviour studies demonstrating the role of the understorey as the prime determinant of difficult-to-control fire (Cruz *et al* 2022), and ecological studies showing that fire promoted a dense understorey that later self-thinned (Burrows 1994, McCaw *et al* 2002). Fire, Zylstra *et al* (2022) concluded, stimulated dense understorey regrowth that caused elevated flammability until that regrowth later self-thinned.

Ensuing work utilised extensive field surveys and biophysical, mechanistic modelling to clearly differentiate the mechanisms underpinning the measured flammability dynamics (Lamont 2023, Zylstra *et al* 2023). The resulting 'ecological control theory' postulates that processes of growth and succession such as self-thinning and self-pruning can act as 'ecological controls' on fire by creating a less-flammable vegetation structure and plant species composition in long-unburnt forest. Disturbance can disrupt these processes and elevate the flammability of the forest until key elements of the forest recover. This process of disturbance-stimulated flammability has been widely documented in other woody ecosystems, triggered by disturbances such as fire, logging, landclearing and windthrow events (Lindenmayer and Zylstra 2023).

1.1. Criticisms by Miller *et al*

Miller *et al* (2024) argued that the findings of Zylstra *et al* (2022) are flawed. Their central claim is that Zylstra *et al* (2022) used an approach that 'guarantees the finding of decreasing wildfire likelihood with TSF (time since fire)'. The arguments of Miller *et al* (2024) centre on the inclusion of long-unburnt forest in the analysis, as remnant patches of long-unburnt forest are small, uncommon, and isolated due to the extensive burning program. Miller *et al* (2024) argue that:

(A) There are less patches of long-unburnt forest than there are of disturbed forest.
(B) Rounding errors in small patches produce values of zero flammability, biasing long-unburnt forest toward zero.
(C) Small patches of long-unburnt forest are vulnerable to small errors in mapping accuracy that could produce significantly greater flammability; and
(D) Errors in the West Australian Government fire records rendered them unsuitable for analysis.

Here, we examine the arguments of Miller *et al* (2024) in detail, along with theoretical arguments raised by Miller *et al* (2024) against the science cited by Zylstra *et al* (2022).

Although Miller *et al* (2024) hypothesised that unequal sample sizes, small patches, and low-quality data biased the findings of Zylstra *et al* (2022), they did not test their hypothesis by replicating the study with those data removed. We performed this test using the updated dataset recommended by Miller *et al* (2024), modifying the code provided by Zylstra *et al* (2022) for such purposes. We found that the hypothesis that there is a reduction in flammability with forest age not only holds but receives greater support when data quality and statistical tests are improved, and even more so when data outliers are removed. This underscores our original finding that prescribed burning in the forests of SW WA increases flammability (Zylstra *et al* 2022).

2. Specific claims

2.1. Terminology

We have adopted the terminology and notation used by Miller *et al* (2024) for most purposes. Zylstra *et al* (2022) used the expression 'forest age' on one occasion and this was rejected by Miller *et al* (2024) as having no meaningful application to the issue because trees in the study area were rarely killed by fire. Despite this, they used the expression twice themselves, and we consider the reference to age to be appropriate in many instances. While a focus on trees alone may be appropriate for timber production, a forest is composed of many plants aside from trees, and most of these recommence growth after a fire. We therefore utilise stand age descriptions such as 'young' or 'mature' for simplicity and to maintain consistency with Zylstra *et al* (2022).

2.2. The influence of declining data quantity

It is certainly the case that patches of long-unburnt forest are less common than patches of recently burnt forest, as the limited temporal range of fire history mapping means that old patches cannot be mapped in earlier years. Miller *et al* (2024) provide no evidence that this would bias results, so the objection remains hypothetical at this point. Such issues of unequal sample size are common in analyses of natural systems, and numerous statistical analyses exist that can explicitly account for them.

We strengthened the statistical test in our analysis from a Students *t*-test to a Welchs *t*-test, comparing individual values rather than means to explicitly deal with unequal sample sizes. In addition, we minimised the difference between sample sizes in two ways. First, we reduced the number of young forest records by limiting analysis to more recent wildfire years (2000 and following), where the ages of old patches were already known. Second, we increased the number and area of old forest patches by including all mapped records. Originally we had truncated the oldest time-since-fire (TSF) records to the year 1954 to minimise error from poor mapping prior that date. However, these errors have minimal effect if we only examine wildfire interactions from the year 2000 onward, as all such areas are now very long unburnt.

2.3. The influence of poor data quality

Zylstra *et al* (2022) characterised flammability (f) for forest with a given TSF as the mean proportion of that forest burned by wildfire across the study period. If fire on average burned a larger proportion of one TSF than another, then this was considered to be more flammable, as flammability is most simply defined as the 'ability to burn' (Gill and Zylstra 2005).

The central claim of Miller *et al* (2024) is that this introduces bias when patches $<$50 ha in area are examined, because Zylstra *et al* (2022) rasterised the mapped data into 1 ha cells. If a 1 ha fire occurs in a 50 ha patch, the calculated value of f is 1/50 or 0.02, which is approximately equal to the mean value of f across all TSF. Miller *et al* (2024) argued that if fires are smaller than 1 ha or patches $<$50 ha, all values of f that are below the average of 0.02 will be rounded down to 0. This, Miller *et al* (2024) claimed, would bias long-unburned forest toward $f = 0$ as such small patches occur in all years where TSF $>$37. From this, Miller *et al* (2024) argued that forest with TSF $>$35 years should be excluded from analysis.

Before addressing this, we must first correct a minor error in the claim by Miller *et al* (2024). It is not fires smaller than 1 ha that are excluded, but fires smaller than 0.5 ha. This occurs because the process of rasterisation into 1 ha cells rounds 0.5 ha polygons up to 1 ha.

The central issue with Miller *et al* (2024)'s claim, however, is that they misrepresent the effect of rounding errors. Rounding of numbers occurs with equal frequency in both directions. Numbers are not only rounded down, 50% of them are also rounded up, exaggerating the flammability of long-unburnt forests. Miller *et al* (2024) give extensive attention to the downward rounding while not acknowledging that upward rounding occurs equally. This makes the outcome appear to be bias, when it is in fact predominantly noise. The effect is that as patch sizes decrease toward 1 ha, the precision of f estimates decreases, with values of f approaching either 1 (the cell is burned), or 0 (the cell is not burned). We say *predominantly* noise, because a small degree of downward bias does occur.

These effects can be seen in figure 1, where we assigned the patches used by Zylstra *et al* (2022) an equal mean value of $f = 0.0204$ (the mean of f for all TSF values used by Miller *et al* 2024), and varied them randomly in a normal distribution around this mean. As years since fire increase, patch sizes become smaller and noise increases (loss of precision), with values scattered both above and below the central value of 0.0204. The slight bias occurs in the clustering of values at zero, so that although the frequency of the error is the same in both directions, the magnitude of downward errors is slightly greater. Even though it has been fundamentally misrepresented, the claim of bias cannot be entirely dismissed. The question is whether it is now sufficient to cause the outcome claimed.

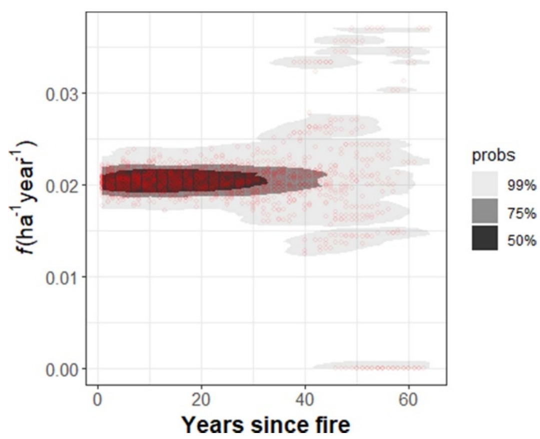

Figure 1. The influence of small patch sizes on bias and noise. Assuming a uniform value of $f = 0.0204$ and allowing for random variability around this mean, noise increased in predictions of f as patch size decreased with years since fire. Red circles show individual values of f, and shading is applied to indicate point density.

Miller *et al* (2024) argued that because patches $<$50 ha occur in all 27 of the TSF $>$37 age classes of forest, these ages (rounded down by Miller *et al* (2024) to 35 years) should be excluded from analysis. Given that 35 years is the point identified by Zylstra *et al* (2022) at which the declining trend began in the data, these data constitute all evidence for a declining trend. The finding of Miller *et al* (2024) of 'No evidence for declining forest wildfire risk with time since fire' is therefore dependent on the removal of all such evidence prior to analysis.

Figure 1(C) in the study by Miller *et al* (2024) indicates that larger patches are still present in all but three of these TSF classes, and in the data provided by Zylstra *et al* (2022), patches $<$50 ha account for only 1.6% of the area of forest with TSF $>$35 years. We argue that the removal of all evidence is not warranted, but that the analysis could simply be replicated with the problematic 1.6% removed.

Another source of error proposed by Miller *et al* (2024) was related to mapping precision, where they claimed that mapped fire records have inadequate spatial resolution for the measurement of f in smaller patches. This effect is most pronounced in older records where mapping quality is lowest, and improves with more recent mapping as better technology and resources enable more accurate and precise mapping of correct edges and patchiness of burn footprints. Miller *et al* (2024) argue that this lack of precision further confounds the bias they claim invalidates the original analysis by Zylstra *et al* (2022), yet this is the same mathematical fallacy as that underpinning the argument around patches <50 ha in area. A lack of mapping precision causes noise, not bias. An incorrectly mapped edges may mean that a patch of long-unburnt forest was mapped as unburnt when it was actually burnt. This would cause long-unburnt forest to appear less flammable than it actually is; but the reverse is equally true. A long-unburnt patch may be mapped as burnt when it was actually unburnt, thereby exaggerating the flammability of long-unburnt forest. We suggest it may in fact be more likely that this latter error would occur, as low-resolution mapping is unlikely to capture smaller changes in fire perimeter or patchiness when small patches remain unburnt.

As we have shown, all of these aspects of poor data have the same effect, producing noise rather than bias. This is not simply a misrepresentation by Miller *et al* (2024), however. Noise has a large influence on the analysis because the delineation of age classes is based on statistical differences. The likelihood of a significant difference decreases as noise increases, so that noise has the effect of obscuring trends.

The effect of this can be illustrated by examining the point with the highest value of f in the data ($f = 0.10$), which occurs at a TSF of 56 years. This high value is largely the result of the 2012 Babbington Fire burning 107 ha out of 175 ha of this age-class. Most of this area either bordered or was within 500 m of the northern control line of that fire (figure 2), presenting an issue not considered by Miller *et al* (2024). Much of the Babbington fire was contained by suppression firing ('backburning') from roads (Bennett and Rouse 2012)—a technique where fire crews ignite vegetation at the road edge in an attempt to burn it before the wildfire reaches that point. As much of the vegetation as possible is usually burned between the fire front and the control line, using ground ignitions lit with drip torches at the road edge, and aerial ignitions by incendiaries between this and the fire front (Simpson *et al* 2021). Consequently, every effort is made to burn patches such as the patch adjacent to the control line, so the fact that it was mapped as burnt is not necessarily an indication of its flammability. The f of this patch in 2012 is, however, a significant outlier from the other patches with the same TSF, so that its inclusion may obscure statistically-significant differences in age classes.

Given these facts, we expect that if flammability does actually decrease in long-unburnt forests, then correcting for poor data quality will actually strengthen the original findings of Zylstra *et al* (2022), rather than removing them as claimed by Miller *et al* (2024). A small degree of uncertainty remains, however, as we have shown that rounding errors may still produce a small amount of downward bias.

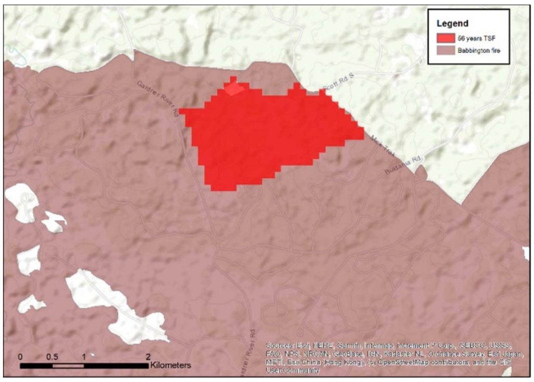

Figure 2. The influence of mapping on exaggerating bias. Northern containment line of the 2012 Babbington Fire (brown), with long unburnt (56 years TSF) vegetation mapped as forest shown in red.

Replication with data quality control is therefore needed to test the overall direction of change.

2.4. Hypothetical scenarios

Rather than testing their predictions by replicating the analysis with the data and code provided for the purpose by Zylstra *et al* (2022), Miller *et al* (2024) presented a series of hypothetical scenarios. Each scenario showed the magnitude of change that would result from adding one ha of wildfire to a small, long-unburnt patch. These scenarios were included to reinforce the mathematical fallacy already discussed; that rounding errors and poor-quality data bias results by consistently under-estimating f in long-unburnt forests. The authors did not acknowledge the equally likely possibility that the area of fire in burned sites could be reduced by one ha. They also did not acknowledge that the likelihood of a single ha increase in wildfire decreases as the patch size decreases. For example, if all other factors are equal, then the likelihood of an additional hectare of fire occurring in a 27 ha patch compared to the 130 571 ha of forest with TSF = 1 in the year 1970 is 130 571/27 = 4836 times smaller. This illustrates the very low likelihood of the hypothetical scenarios that were proposed in lieu of replication.

2.5. Erroneous data

Miller *et al* (2024) note an error in fire records which remained undetected by the Western Australian Government for 16 years, where a fire mapped in 2002 was mapped again for the same location in the following year. This has therefore likely affected all fire history analyses used to underpin management in the region (e.g. Boer *et al* 2009).

We fully accounted for all possible effects of this error by repeating the entire analysis of Zylstra *et al* (2022) using the most recently available mapped dataset.

3. Theoretical underpinnings

Miller *et al* (2024) argued that the findings of Zylstra *et al* (2022) were invalid because they were 'geographically unbalanced', and objected to the scientific underpinnings of the work.

3.1. Geographic balance

Miller *et al* (2024) discussed several geographic considerations, but did not define any measure by which they would consider these to be treated in a way that was 'geographically balanced'. As we understand it, this argument by Miller *et al* (2024) centred on the differences that may be expected in flammability dynamics across the differing biogeography of forest communities.

Although we agree that including such influences will likely improve the prediction of landscape fire risk, Miller *et al* (2024) did not show that any one of these factors invalidated the highly significant findings of Zylstra *et al* (2022). Instead, Miller *et al* (2024) used rhetorical and misleading statements to insinuate that this would be the case. For example, they state: 'Driven by these [edaphic and climatic] changes, the structure, vegetation and fuel dynamics, and fire regimes of these ecosystems vary substantially. Given the reliance of the authors on ecological mechanisms such as succession, ignoring such significant differences in vegetation and climate further call their study design and inference into question.' This statement constructs the argument that:

A. vegetation dynamics vary substantially across the study area and
B. Zylstra *et al* (2022) rely on such dynamics for their findings, and
C. Zylstra *et al* (2022) did not account for such variability; therefore
D. the findings of Zylstra *et al* (2022) are called into question.

This argument fails because Part B is false; Zylstra *et al* (2022) did not rely upon vegetation dynamics to arrive at their findings. Their finding that flammability decreases significantly in long-unburnt forest was derived entirely from their analysis of Western Australian Government fire records. Vegetation dynamics were instead used to explain these trends, based on existing published science.

3.2. Confounding effects of other fire management

A potential issue with Zylstra *et al* (2022) is the argument that if forests become long-unburnt because they naturally receive less wildfire, then, the finding that long-unburnt forests receive less wildfire is circular. As shown in Zylstra *et al* (2022), that argument fails immediately because 80% of fire in the study region was prescribed, not wildfire. Forest age in the southwest is determined by management, not by wildfire.

Miller *et al* (2024) are correct when they note that 'the likelihood that an area will experience wildfire is closely related to patterns of ignition sources and density, and effectiveness of suppression response'. The implication that this may affect the validity of the analysis by Zylstra *et al* (2022), however, ignores the reality that wildfire has only a minimal effect on forest age in the southwest. Miller *et al* (2024) also provide no analysis or mechanism to show that these factors affected fuel age distributions. Despite this, we provide here some analysis of the arguments, and propose that there may be a mechanism for an interaction which could affect the findings of Zylstra *et al* (2022).

Prescribed burn frequency is greater closer to settlements (DBCA 2021), so that areas of long-unburnt forest are more often located further from settlements. This spatial pattern creates the possibility for a confounding interaction, as suppression resources and ignition frequency can also vary with distance from settlements. Approximately 80% of wildfire ignitions in the area are of human origin (DBCA 2021), and human ignitions are generally more frequent in regions with higher population density (Collins *et al* 2015, Stanley *et al* 2021). Unfortunately, this trend has been quantified only at the scale of a bioregion (Collins *et al* 2015), so it is not yet possible to state whether ignition frequency declines with distance from settlements, or increases due to for example, agricultural or roadside ignitions. Conversely, it is well-established that fire suppression is most intensive close to assets (Plucinski 2019). This effect is likely amplified in southwestern Australia, as the State Government cost efficiency targets focus on the cost per ha of wildfire suppression rather than total cost (DBCA 2021). This will incentivise low-cost suppression measures such as suppression firing where possible, with intensive, high-cost applications of equipment and personnel more often reserved for priority areas close to settlements. As discussed earlier, suppression firing can distort trends because efforts are made to ensure that areas are deliberately burnt. In addition, suppression firing can increase the total area burnt (Simpson *et al* 2021), because the goal is to establish secure control lines, rather than to minimise burnt area.

Considered together then, long-unburnt patches are more likely to be located away from settlements, and this is also where less resources are applied to minimise the area burnt by wildfire. Conversely, places closer to settlements that are more frequently burnt by prescribed fire also receive the most resources to suppress wildfires. This trend is the opposite to that suggested by Miller *et al* (2024), indicating that the findings of Zylstra *et al* (2022) may be even more pronounced if this confounding effect could be removed. The argument that wildfire ignition frequency might account for the difference in wildfire frequency across age classes is largely irrelevant due to the small role of wildfire in determining forest age and cannot be tested as sufficient data are not yet available for that purpose.

3.3. Underpinning science

Miller *et al* (2024) begin their discussion of the ecological and fire behaviour science underpinning the findings of Zylstra *et al* (2022) with the criticism that Zylstra *et al* (2022) did not present new evidence of self-thinning. This is misleading, as the study by Zylstra *et al* (2022) was an analysis of mapped fire histories and needed only to cite the existing body of evidence showing that self-thinning understoreys were well-documented across the study area (e.g. Burrows 1994, McCaw *et al* 2002). It is perhaps more relevant to note that Miller *et al* (2024) did not present any evidence to counter the established knowledge of self-thinning understoreys. The lead author of Zylstra *et al* (2022) also presented new evidence of self-thinning in a separate, contemporaneous paper (Zylstra *et al* 2023).

Understorey dynamics are central to the discussion of fire management, as they have a long history of debate in Australia. The claim is frequently made by non-ecologists that dense forest understoreys driving increased fire risk are the result of a *lack* of burning (Gammage 2011, Mariani *et al* 2022). This is a recent perspective, however. After decades of observation in the early 20th century, Charles Lane Poole, Conservator of Forests for Western Australia and later Inspector-General of forests for Australia argued that: 'The thickening up of our forests is entirely due to fire, and the exclusion of fire will render them less susceptible to fire because it will get rid of an enormous amount of inflammable material' (Stretton 1939).

The observations on understorey dynamics by Lane Poole and others were validated by the studies cited in Zylstra *et al* (2022), but by this time, fire management thinking (e.g. McArthur 1967) was dominated by modelling concepts developed in North American conifer forests which placed the emphasis on the weight of fuels (Byram 1959). Miller *et al* (2024) indirectly reference Byram's intensity model when they refer to the quantity of fuels providing 'the main source of energy released by a fire', but this is problematic. Byram's intensity model assumes that all fuels burn instantaneously in a one-dimensional moving fire front. In reality, fire may also spread in a vertical direction if the structure of the vegetation allows it. For example, a *passive crown fire* (also known as 'torching', where a slow or stationary fire ignites trees above it) may release extraordinary amounts of energy. Byram's intensity model cannot account for this upward spread, and therefore predicts such a fire to have a very low intensity because of its slow rate of horizontal spread (Zylstra 2023).

Miller *et al* (2024) use multiple statements that are difficult to address because they appear to conflate terms and misuse references. For example, they argue that 'the quantity, availability and structure of fine fuels, the main source of energy released by a fire and primary driver of fuel-driven rate of spread, does not decrease in long unburned stands, but rather maintains a pseudo steady state'. The terms quantity, availability and structure of fuels all refer to separate attributes and the dynamics of these vary considerably. The claim by Miller *et al* (2024) that the quantity of fuels drives rate of spread has been comprehensively disproved (Burrows 1999, Storey *et al* 2021). Of the four references provided by Miller *et al* (2024) to support their claim, only one examines structural traits (Burrows *et al* 2023), and rather than finding a pseudo steady state, that work showed marked decreases in measures of the height and cover of understorey plants in long-unburnt forests. No references addressed fuel availability, but rather than increasing to a pseudo steady state, the availability of fuel is known to decrease with time since fire in many forests, as the microclimate recovers (Wilson *et al* 2022).

Miller *et al* (2024) cite the 'Project Vesta' body of work to argue that other fuel components have a greater effect than the understorey, but again, their statements misrepresent those findings. For example, they cite the original report (Gould *et al* 2007) to say that 'The influence of bark fuels on fire spread exceeds that of the understorey vegetation'. Gould *et al* (2007) actually found that bark explained 36%–48% of the rate of spread, but that the understorey explained 57%—more than any other fuel descriptor. The best performing model produced by Gould *et al* (2007) was for the prediction of flame heights ($R^2 = 0.81$), and this utilised only understorey height and rate of fire spread.

It must be noted that Project Vesta work has continued to evolve over time, and the most recent publication (Cruz *et al* 2022) concluded that—regardless of fuel loads, fires will always be slow with small flames unless a sufficient understorey is present. Cruz *et al* (2022, p 91) state: 'Our findings suggest that the best fuel descriptor from the point of view of the operational prediction of fire spread is the height of the understorey fuels. This variable is defined as the average height of both the near-surface and elevated fuels weighted by their cover on a per area basis.' As claimed by early observers such as Lane-Poole, and demonstrated by the sources cited in Zylstra *et al* (2022) along with subsequent work, burning these forests produces a pulse of these fuels that later self-thins. Miller *et al* (2024) did not produce any evidence to counter this.

4. Hypothesis testing

The arguments raised by Miller *et al* (2024) provide theoretical objections to the findings of Zylstra *et al* (2022), along with hypothetical scenarios to demonstrate ways in which their objections might be validated. We have shown that the hypothetical scenarios are extremely improbable and that the theoretical objections depend upon mathematical fallacies and rejection of robust existing science. Miller *et al* (2024) did not test their predictions by performing replications of Zylstra *et al* (2022) after correcting for their own proposed changes. Here, we perform these replications, explicitly testing whether poor data quality caused the decline in flammability as claimed by Miller *et al* (2024), or partially obscured it as we have argued.

4.1. Methodology

The expectation of Miller *et al* (2024) and our response here can be tested as alternate hypotheses:

H1: if apparent declining flammability in long-unburnt forests is an artefact of poor data quality, then improving data quality will remove the apparent decline.

H2: if flammability does actually decline in long-unburnt forests but is partially obscured by poor data quality, then improving data quality will strengthen the evidence for the decline.

To test these two hypotheses, we replicated the analysis performed by Zylstra *et al* (2022) for three different sets of the data providing increasing levels of data quality (sets b and c), and a demonstration of the impact of a single outlier (set d). These were:

(a) The original dataset.
(b) The updated dataset proposed by Miller *et al* (2024), with wildfire years truncated to years 2000 and following years, the full range of mapped fuel ages included, and all patches <50 ha removed to address the central criticisms of Miller *et al* (2024).
(c) The same dataset as b, but with the removal of outliers (see Supplementary data).
(d) The same dataset as b, but with the removal of the single outlier at 56 years.

For each analysis, we tested for the presence of a statistically significant decline in f for forests with TSF >56 years. Zylstra *et al* (2022) divided flammability dynamics into three periods, where the pulse of flammability was delayed by an initial 'young' (Y) period immediately following fire, 'regrowth' (R) forests represented the pulse of increased flammability caused by regrowth from the fire, and 'mature' (M) forests were those that had recovered from the regrowth phase and returned to their original state of lower flammability. This occurred after 56 years in the grouped data of Zylstra *et al* (2022), so we maintained this TSF categorisation for our reanalysis. We expected that refinements in data quality from sets (a) to (c) would result in removal of the mature period (M) if H1 was supported and H2 rejected, but its strengthening (i.e. a reduction in f) if H2 were supported and H1 rejected. We also measured the duration and mean f for the regrowth period R, which represents the pulse in flammability that Zylstra *et al* (2022) argue results from burning the forest. We expected that if H1 was rejected, then improved data quality would enable a stronger statistical contrast that would shorten and/or intensify the estimated period of increased flammability. Finally, we determined a 'disturbance equivalent' period, which is the frequency of burning required to reduce f below its value in forests undisturbed for at least 56 years.

Table 1. Influence of data quality controls on flammability dynamics (f). Rows show the analyses detailed in 4.1. Columns show values for regrowth (R) and a mature period standardised to 56 years. Values show the maximum TSF in that period, and values in brackets give the mean f for the period. Set a is the analysis in Zylstra *et al* (2022), and sets b to d are Welch's *t*-test analyses of the new dataset with quality controls. Set c has all outliers removed, and set d has only the single outlier at 56 years removed. 'Disturbance equivalent' is the burn frequency that would be required to achieve the same or lower f as a forest older than 56 years.

Set	R	M56	Disturbance equivalent
a	56 (0.023)	(0.0031)	5
b	56 (0.025)	(0.000 73)	1
c	39 (0.023)	(0.000 73)	1
d	49 (0.025)	(0.000 73)	1

4.2. Results

Our replication study demonstrated that, contrary to the claim of Miller *et al* (2024), improving data quality strengthened rather than weakened the evidence for a decline in f in long-unburnt forests. Both levels of data refinement **increased** the value of f in regrowth, but each also had unique effects that accumulated with successive improvements (table 1, figure 3).

- Our measures to directly address the concerns raised by Miller *et al* (2024) in set b reduced the value of f in mature forest nearly fourfold, and slightly increased the flammability of the regrowth period.
- Removing an outlier from each highly variable age class (set c) reduced the maximum TSF for the disturbed period ($Y + R$) from 56 to 39 years.

Removing the single outlier at 56 years reduced the maximum TSF for the disturbed period ($Y + R$) from 56 to 49 years.

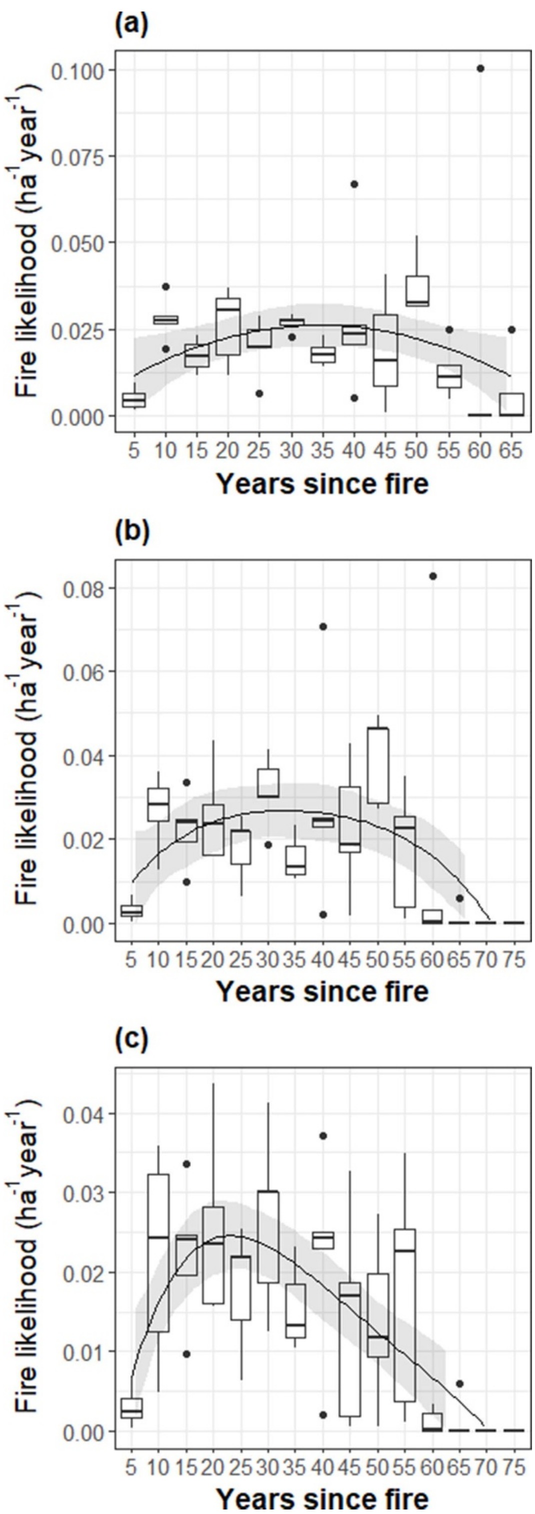

Figure 3. The influence of data quality on flammability trends. Panels correspond to the analyses in 4.1 and table 1. Analysis (a): original analysis showing the humped trend partially obscured by noise in the data. Analysis (b): reanalysis of the new data with all historical ages included but only 20th century wildfires analysed and all patches <50 ha removed. Analysis (c): removal of outliers.

Outliers occurred in years 8, 27, 37, 43, 46, 47, 48, 49, 56.

Based on these results, we rejected H1, the hypothesis that declining f in mature forests was an artefact of poor data quality. Instead, H2 was supported as better quality data revealed a far more pronounced pulse of flammability initiated by prescribed burning.

5. Discussion

We have thoroughly addressed the data quality issues raised by Miller *et al* (2024). Contrary to their claims, we have shown that prescribed burning in the forests of SW WA stimulate a more intense pulse of flammability than originally identified by Zylstra *et al* (2022). The point of maximum flammability shifted from 30–40 years down to 20–30 years, and the onset of a low-flammability 'mature' phase was identified 17 years sooner at 39 years instead of 56 years (table 1). Data used by Zylstra *et al* (2022) indicated that a 5-yearly rotation of prescribed burning reduced f to the same value as the mean for forests unburnt for at least 56 years. Implementing the data improvements highlighted by Miller *et al* (2024), however, demonstrated that the f in mature forest was so low that annual burning would in fact be required to have an equivalent effect. The pulse of elevated flammability following burning and the subsequent low flammability of long-unburnt forest were not artefacts of poor data as claimed by Miller *et al* (2024). Instead, poor data obscured the strength of the pulse.

5.1. Obscuring flammability trends

Zylstra *et al* (2022) demonstrated that the high level of efficacy attributed to prescribed burning in southwestern Australia is an artefact of analyses that exclude the changes in long-unburnt forests. Studies of prescribed burning efficacy typically focus on the initial 1–2 decades following treatment, assuming that the less-recently burnt patch represents long-unburnt forest because fuels have re-accumulated (e.g. (Fernandes and Botelho 2003, McCaw 2013, Hunter and Robles 2020). The same approach is evident in major experimental programs such as 'Project Vesta', which concluded that measures of flammability increase with time since fire, based on experiments conducted across forests aged up to 22 years TSF (McCaw *et al* 2012). This limit on age classes examined was highly relevant for the study area in which those experiments were conducted, as it was the TSF where Burrows (1994) had shown that understorey density began to decline (figure 4). The most recent analysis of those experiments concluded that the cover and height of the understorey was 'the best fuel predictor from the point of view of the operational prediction of fire spread' (Cruz *et al* 2022). It follows that excluding long-unburnt forests from analysis therefore excluded the decline in flammability, pre-determining the finding that rates of spread only increase with time since fire.

Miller *et al* (2024) advocated for an approach that would obscure the decreasing flammability identified by Zylstra *et al* (2022). The authors argued that all 435 patches covering 148 033 ha of forest with TSF

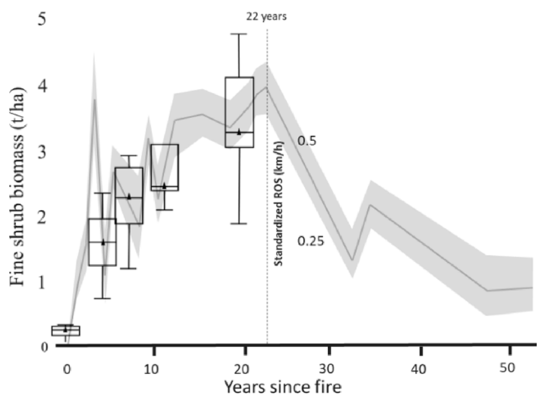

Figure 4. Comparison of Project Vesta findings with understorey dynamics in their study area. Boxplots show standardised rates of spread measured in Project Vesta experiments increasing as fine shrub biomass increases (McCaw *et al* 2012). The largest TSF examined in the experiments was 22 years (vertical dotted line), which was the point at which fine shrub biomass in the study area had been shown to begin declining (Burrows 1994). The solid line shows fine shrub biomass as measured by Burrows (1994), and shading shows the standard error of those measurements.

>35 years in the Zylstra *et al* (2022) dataset should be excluded from analysis, because 89 of those patches were smaller than 50 ha in area. Just as the 22 year point corresponded to the maximum fine shrub biomass in the Project Vesta study area, the 35 year point corresponds to the approximate maximum f in Zylstra *et al* (2022) (figure 3(a)). This argument was premised on the mathematical fallacy that rounding errors in patches <50 ha would always produce under-estimates of flammability (rounding down), when in fact rounding up is equally likely. We tested these claims, showing that instead of causing the results of Zylstra *et al* (2022), poor quality data obscured them. For example, Zylstra *et al* (2022) had reported that forests remained flammable until 56 years TSF on average, but we found this was caused by a single outlier in the data where a patch of TSF = 56 forest had been forcibly burned in a suppression firing operation.

5.2. Implications for fire management

Together with Zylstra *et al* (2022), our new analyses and findings have profound implications for fire management in the southwest of Western Australia. If forests are already older than the 56 year disturbed period identified by Zylstra *et al* (2022), our data show that only annual burning can produce a lesser likelihood of wildfire, and that any longer frequency causes an increase in f. For those forest that are recovering from past burning, we also showed that low flammability may be achieved through a significantly shorter period of fire exclusion than previously thought.

Our findings are consistent with ecological control theory (Zylstra *et al* 2022, 2023, Lindenmayer and Zylstra 2023), which posits that processes of growth and succession, including the self-thinning of forest understoreys, place controls on wildfire in the landscape. Disturbance-based management such as prescribed fire disrupts these ecological controls on fire, providing short-term benefits by temporarily clearing vegetation (the Y period), but imposing long-term costs (the following R period) until growth and succession once again create a less flammable landscape (Lindenmayer and Zylstra 2023). An alternative approach to this was proposed by Zylstra *et al* (2022, 2023), who argued that rather than disrupting ecological controls in this way, management could reinforce them by maximising the area of long-unburnt forest, and targeting suppression in places where ecological controls had been weakened by factors such as climate change. Transitioning to this 'ecologically cooperative management' involves an initial phase in which the short-term benefits of prescribed burning are surrendered to enable recovery, and managing this requires careful planning. The following principles may be applied to better enable the transition:

1. Use a staged approach to the cessation of prescribed burns. As remote areas are currently burnt less frequently than areas close to settlements, allowing them to continue their recovery will create a less-flammable landscape, reducing risk while freeing resources to protect habitation.
2. Provide an immediate focus on rapid fire detection and suppression, utilising emerging technologies (Lindenmayer *et al* 2022) and coupled with social or infrastructure programs that target the anthropogenic causes of 80% of wildfire ignitions (DBCA 2021, Stanley *et al* 2021).
3. Identify weather thresholds that permit more aggressive and effective forms of fire suppression to minimise suppression firing operations in mature forests (Zylstra *et al* 2023).

5.3. Conclusions

Miller *et al* (2024) argued that due to poor data quality, evidence for declining flammability in long-unburnt forests should be excluded from analyses. We have demonstrated that these arguments are spurious, and that when those data quality issues are accounted for, the pulse in flammability caused by prescribed burning instead becomes far clearer. That is, we found strong evidence that prescribed burning has created a ***more*** flammable landscape in southwestern forests. These findings demand an urgent response, as prescribed burning these forests has highly perverse management outcomes. As we have shown, such trends are hidden through poor analytical techniques, so it is possible that similar trends exist in many other forest ecosystems.

Data availability statement

The data that support the findings of this study are openly available at the following URL: https://github.com/pzylstra/SW_PIP.

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