tested, refined and improved. They also offer a defensible quantitative tool for prioritising conservation actions for multiple species [2]. Flather *et al*. [1] imply instead that because of (obvious) complexities, generalities are impossible. Ecology and conservation biology would not be plausible scientific disciplines if this were true.

We agree, at least in some circumstances, that 'generalizing [too broadly] among species is a dangerous undertaking', but argue that ignoring the paucity of data for most threatened species is a greater ecological and conservation biology sin. Obviously, as Flather *et al*. [1] state, if there is a robust model for a species, it is preferable to use this to estimate extinction risk directly, rather than relying on an indirect approximation of MVP. However, for the vast majority of situations where no model exists and there are insufficient data or resources available to construct such models, what must one do? Lessons from body-mass allometry [6], experimental and observed dynamics [8], and the generational scaling of population variability [9] all show that useful generalities are possible (and on evolutionary grounds, one should expect them). Conservation biology is a crisis discipline akin to cancer biology, where one must act in a timely manner on the best information available. Decision-makers cannot afford the luxury of adhering to a 'null' philosophy that says everything is unique; rules of thumb are desperately needed, including quantitative goals such as MVP.

In our 2010 review [2], we stated that biologists should aspire to conserve 'at least 5000 adult individuals (or 500 to prevent inbreeding) whilst addressing concomitant mechanisms of decline'. Despite various protestations, Flather *et al*. [1] ultimately agree: 'We also suspect. . .that multiple populations totalling thousands (not hundreds) of individuals will be needed to ensure long-term persistence'. Yet the reality is that sizes that are one to two orders of magnitude lower are still being routinely used and promoted within the conservation community [12]. The setting of higher target numbers (and more extensive habitats) is more realistic and scientifically defensible than aiming for tens or hundreds of individuals, or having no population goal at all, especially given the fact that conservation threats are growing with human populations, concomitant habitat loss and global climate change. Decision-makers need to hear and act upon this message, and avoid distraction of minor scientific squabbles on what essentially amounts to quantifying (im)precision.

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Letters Response

A general target for MVPs: unsupported and unnecessary

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In a recent article in TREE [1], we reviewed evidence for a consistent standardised estimate of minimum viable populations (MVPs) across taxa [2–4] and found that the universal MVP of 5000 adults advocated by Traill *et al.* [5] was unsupported by reanalyses of their data. We identified shortcomings in the original analyses, and found substantial uncertainty in standardised MVP estimates, both within populations of the same species and among species. We concluded that neither data nor theory supported a generally applicable MVP.

No evidence refuting the technical problems that we identified in their original analyses was presented by

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Brook *et al.* [6]. Instead, they agreed with us that a universally applicable MVP is illusory and that no such 'magic number' exists. Brook and colleagues' clear rejection of a universal MVP is important because both popular coverage [7] of their work and many statements in their own publications had suggested otherwise. For example, Frankham *et al.* [8] wrote that evidence against universality was simply '...an artefact of defining it for a fixed number of years, rather than generations'. Likewise, Traill *et al.* [5] stated that 'The bottom line is that both evolutionary and demographic constraints on populations require sizes to be at least 5000 adult individuals', judging 5000 to be a '...consensus... [and] useful benchmark' [5]. Even in their Letter [6], Brook *et al.* asserted that genetic arguments are sufficient to embrace a generalised MVP, overlooking statistical artefacts in the translation of effective size to census size and the substantive variation that characterises these data [9]. Their confidence in the merits of 5000 as an MVP conservation target is emphasised by its recent promotion as '...an empirically supported threshold MVP target' for conservation triage [10]. Given this backdrop of mixed messages, it is important to (re)emphasise the contingent nature of MVPs and the wide variability of standardised MVP estimates among populations and species [1].

Although Brook *et al.* [6] rejected a universally applicable MVP, they extolled the contradictory argument that a general rule of thumb remains scientifically defensible and pragmatically necessary. They asserted that, because conservation data are often lacking, decision-makers desperately need a general quantitative MVP target. We remain unconvinced of this 'desperate need'. Conservation practitioners and policy makers do not need unsupported rules of thumb that do not survive comparisons with data (standardised MVPs did not cluster around 5000 individuals but varied over five to eight orders of magnitude [2–4]). They are quite capable of dealing with uncertainty and context-specific conservation strategies, and are reluctant to embrace general rules of thumb for fear of being held strictly accountable to them when circumstances dictate otherwise (see [11]).

Brook *et al.* emphasised three possible benefits of a generalised MVP. First, they suggested that, when data and resources are scarce, a generalised MVP '...guided by general principles that are underpinned by theory, data and models, [and treat] uncertainty and assumptions explicitly and transparently' is a necessary alternative to expert judgment. We cannot reconcile this description with the flawed analyses that led to the unsupported generality of 5000 being christened a 'magic number' [7]. Brook *et al.*'s second purported benefit of a generalised MVP is as 'a defensible tool for prioritizing conservation actions' [5,10]. Evaluating the relative merits of conservation investments among species based on their population sizes, when ignorant of their threats, trends and other traits is a highly dubious enterprise [1]. A final application of a generalised MVP target is for listing and delisting decisions [6]. A general rule could define a point when conservation efforts are deemed to have been successful. However, if conservation work has been ongoing with any success, it seems inconceivable that those responsible could be so ignorant of the biology of, and threats to, the population that they would remain reliant on a generalised rule as proposed by Brook and colleagues to make a delisting decision. Thus, the most defensible use of a generalised MVP might be in listing decisions. Raising the International Union for Conservation of Nature (IUCN) criterion D1 for Vulnerable from <1000 mature individuals by a factor of five risks translating 'threatened' (a category that encompasses 38% of evaluated species) into such a commonplace designation that it ceases to carry any weight.

Brook *et al.* characterised our treatment of a generalised MVP as a '...distraction of minor scientific squabbles'. This characterisation is disingenuous because it ignores large variances in standardised MVP estimates, a stance that poses serious practical problems for conservationists. Arguing for the validity of an unsupported general MVP risks: (i) complacency when threatened populations exceed the suggested guideline; (ii) writing off populations as lost causes that could be viable at sizes well below the guideline size (see [12]); and (iii) establishing a shaky foundation for subsequent policy decisions. In the latter case, conservation biologists would do well to heed the lessons of other scientific fields in which even minor errors of fact have proven highly damaging to much broader enterprises (e.g. [13]).

The conservation of species that are deemed to have an unacceptably high risk of extinction, by whatever criteria, is a difficult undertaking. The 'sin' is not in demanding thoughtful consideration of the circumstances leading to increased rarity and how conservation practice might reverse that trend. Rather, the 'sin' is in implying that conservation science should compare the current population size of a species against an unsupported threshold to judge its safety, whether it is worthy of conservation expenditures, or whether it should be tossed from the ark.

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Letters

Biased sampling: no 'Homer Simpson Effect' among high achievers

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The under-representation of women increases when moving up the scientific academic ladder, from 40% to 77% female at the time of receiving a PhD to circa 10% among full professors, depending on research field and country [1,2]. One of the many potential causes for the relative shortage of women among academic leaders is that men self-promote more than do women, and that self-promotion is necessary to succeed in science [3]. Successful self-promotion requires a realistic and confident self-assessment of personal scientific expertise. However, females provide lower self-evaluations than do males on interview performance [4] and both mathematical and logical reasoning [5]. Laurance and colleagues [6] therefore predict, but do not find, a gender difference in the perception of personal scientific expertise when surveying recognised scientific experts. We argue that, although the inferences that can be drawn from their findings are limited owing to methodology and scope, their study raises awareness of a serious problem. We therefore call for a more comprehensive study of the main reasons for the under-representation of women among scientific leaders.

Laurance *et al.* demonstrate that there is no gender difference in the self-perception of scientific expertise among a group of recognised scientific experts. However, if we assume that a high self-perception of scientific expertise is either crucial for, or correlated with, success in science, an investigation only among high achievers is problematic [7]. This is because the career selection process might lead to a strong bias in the data set that could completely disguise any initial variation and, in particular, gender differences. When investigating traits that are potentially linked with career advancement, studies need to also consider early career stages in which selection has not yet had a strong influence. Importantly, studies should also test the relative importance of several factors, so as to identify the major causal ones.

Laurance *et al.* asked researchers to score 'their perceived level of knowledge about their study area'. Owing to the constrained format of their publication, specific details are unclear; however, if this was the only question that participants were asked, this would cast serious doubts on their finding. It is well known that questionnaire design is non-trivial and, in particular, that single questions can be misinterpreted [8]. Finally, we highlight the importance of applying appropriate statistical tests (e.g. as dictated by the distribution of the response variable) and of reporting effect sizes so that the magnitude of the effect can be assessed [9].

We believe that Laurance and colleagues [6] are raising a timely and important issue to the wider scientific community. We hope that their letter will stimulate more studies, across all career stages, incorporating all potential causes. Many different factors beyond self-promotion and self-perception of scientific expertise are suspected to cause the gender difference in scientific academia [1,2,10,11]. Studies are therefore required to identify the underlying causes and their relative importance, so that measures can be taken to forestall the 'leaky pipeline' [12]: the loss of highly trained and talented female researchers from scientific academia.

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