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Who counts?

In this issue, we feature two sets of discussions which, on the surface, may appear not to be closely related. First, a number of authors have responded to our call for submissions on what we term 'hidden contributors to research'. We are pleased to host a small number of contributions dealing with this issue in various ways. The fact that we received rather fewer submissions on this topic than for other Discussion Series, may in itself be testament to how much, in the pattern of public recognition of who contributes to research, some people and their work are not thought about and acknowledged enough. We are very grateful to all who submitted. In our call for submissions on this topic, we also said: "We also welcome pieces which engage with the politics and ethics of recognition of research contributions, challenges regarding authorship attribution, and anything which contributes to scholarly debate on questions of recognition and attribution in science as broadly understood." We did not receive submissions dealing explicitly with this wider issue, but the question of recognition is key to another debate we host in this issue.

The second debate in this issue consists of a contribution by Marthinus Booysen on the South African National Research Foundation's (NRF) rating system, a response to this by Gugu Moche and a rejoinder by Booysen. It is our practice at the *South African Journal of Science* to support debates in and about science and research practices, which is why we asked the NRF for a response, and we are grateful for this response and for Booysen's rejoinder. The question of the use and value of the NRF system has long been debated in our Journal, and it is worth noting that, almost 20 years ago, Cherry and Gibbons[1] described the NRF rating system in the pages of this Journal as

> *an idea whose time is **long past**, and … currently undermining academic collegiality in South Africa, primarily on account of the way it is misused by the universities themselves. It should be abandoned before further damage is done.* (p. 181; emphasis added)

Citing the report to which Cherry and Gibbons were responding, Booysen, in 2026, asks essentially the same questions, and also speaks of a bureaucracy, the time of which, as he puts it, "has passed". In her response to Booysen, Moche notes: "Universities continue to attach reputational value to ratings… ." This is a valid claim, as university promotional materials and websites will attest (see, for example, the webpages of the Universities of Stellenbosch, Johannesburg, the Free State and the Western Cape). The extent to which NRF ratings are, as Cherry and Gibbon suggested, "misused" by universities remains not fully explored, and this is one reason why reactivation of this debate in our pages is perhaps overdue. More fundamentally, though, the question of the use of NRF ratings and the expensive bureaucracy surrounding them, links to underlying concerns, as Booysen notes, looping back to our 'hidden voices' series. There are serious questions at stake about what we count, and what counts, as worth and contribution to the research enterprise.

In an entertaining book-form distillation of more conventionally academic papers, the philosopher C. Thi Nguyen[2] documents how the wish for convenient, transparent and easily communicable metrics can lead to what he terms 'value capture', which he describes as follows:

> *Value capture happens when your environment presents you with simplified versions of your values, and those simple versions come to dominate your practical reasoning. Value capture offers you a quick shortcut—an opportunity to take on **prefabricated** values. You do not have to go through the painful process of value deliberation if you can get your values off the shelf.*[3(p.470)] (Nguyen's emphasis)

When universities, for example, use simple counts of the number of publications in 'accredited' journals, or an NRF rating category, or an h-index, as foundational to their assessments of whether somebody should be employed or promoted, they are effectively outsourcing their most difficult question, but arguably the most important one. This question is: how do we assess academic merit? What Nguyen terms the use of 'prefabricated' values in this context adds a layer of what appears to be objectivity to assessment of worth, as well as procedural transparency, but does not tell the whole truth. Critics of the h-index, for example, have noted how Einstein's h-index would appear to be 'mediocre' compared with those of contemporary, 'publish or perish' physicists who have played far less of a role in changing how scientists think of the world and the universe. I have sat on panels assessing entry to a learned society where the h-index has been used as a key measure of value, despite its instability in meaning across disciplines and contexts, and the fact that high citations can come from a publication being widely and publicly condemned. Similarly, on a university panel assessing the eligibility of a candidate for a senior academic position, a high-ranking academic insisted that counting the number of publications was more important and more objective than having a discussion of the academic worth of these publications, when some panel members expressed concern about the quality of the work. Because numbers or ranking letters seem more stable and objective than complex discussions, even though contestation and debate are at the heart of the research enterprise, these can easily dominate, and create perverse incentives. At the *South African Journal of Science*, for example, we have received submissions with multiple authors, and on occasion when we have asked for evidence of authorship on the basis of commonly accepted academic criteria and our own policies, some contributors have substantially shortened the author list. Although we might have wished that we had not received the long list of authors in the first place, we are also aware of the fact that being acknowledged for helping authors with an article is unlikely to 'count' as much as being listed as an author, and that institutions, when making administrative decisions, are unlikely to have the time, apart from anything else, to check whether people indeed met criteria for authorship on papers they claim to have written. No journal that we know of has the time or infrastructure to check every submission either, and gift authorship (and, to a lesser extent, ghost authorship) are likely not uncommon.[4] A number of academics (including me) have been offered not insubstantial amounts of money essentially to ghost author papers to improve the academic standing of less experienced academics, so that the 'scores' of these academics improve.

What is at stake here is not simply a question of ethics. It is also a question about the dependability of the academic enterprise. Who gets rewarded and how is not a trivial matter. Five years into South Africa's democracy, Marguerite Schneider and colleagues[5] produced a landmark report on disability statistics in South Africa, work which contributed to later international studies on disability statistics[6]. This early document was cannily titled, *We also count*, deliberately drawing attention to three issues: disabled people are often seen not to have worth; disabled people are often left out when scientists count people's needs and views on a range of issues; and disabled people, contrary to many prejudices, can themselves be active in the process of counting and tabulation, in the traditions of participatory statistics.[7]

If we are serious about the dependability and rigour of the scientific enterprise as broadly understood, we need debate about what is hidden and what is revealed through counting and assessment processes. Questions of who and what count are difficult and not reducible to 'objective' numbers. This is especially important for those of us wishing to build the best science we can for our southern context. Behind all counting systems, are relationships of power[8]; these are amplified in a context of global inequality.

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