



## **Fancourt, D. and Finn, S. (2019). *What is the evidence on the role of the arts in improving health and well-being? A scoping review***

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### **Introduction**

This ambitious review, authored by leading researchers in the field of arts and health, was supported by the Division of Information, Evidence, Research and Innovation, and the Health Evidence Network team, both at the World Health Organization (WHO), Europe. It also had the benefit of peer reviews from four distinguished academics in this field. With the imprimatur of the WHO, readers should feel confident about finding a robust assessment of the current state of evidence on the contribution of creative arts in promoting health. The authors are careful to state the question they are seeking to answer: ‘What is the evidence on the role of the arts in improving health and well-being?’ Their broader aim is also important: to close what they see as an ‘awareness gap’ between the research evidence and those professionals involved in healthcare policy development and practice. A key concern is promoting ‘knowledge and technology transfer’ of evidence into service provision. Discussion of ‘policy considerations’ (p. 55) makes it clear that the intended audiences for the report include: policy makers in government, commissioners of health and care services, funders of research, managers of arts and cultural organisations, and institutions involved in the training of health and arts professionals. Arts and health policy makers, the authors suggest, should consider ‘supporting the implementation of arts interventions where a substantial evidence base exists’ – a view that will attract wide-spread agreement across the arts and health field.

The report has many positives. Section 1 is an excellent introduction to the field, and the logic model given in Figure 1 is a powerful scheme for understanding the value of the arts for health. Similarly, in Section 2, Figure 2 lays out clearly the many contributions the arts can make to prevention and health promotion, and to the management and treatment of health conditions. Certainly too, the report is very wide-ranging and brings together a considerable body of evidence, and the authors describe it as ‘the most comprehensive survey of the literature on arts and health to date’. Nevertheless, there are surprising omissions and readers should also consult ten years of research evidence in *Arts & Health: An international journal for research, policy and practice*, and *Journal of Applied Arts & Health*. Much of the consider-

able body of grey literature on arts and health is also missing, and a substantial international collection of such reports can be found here: <https://www.artshealthresources.org.uk/>. Work in the creative arts therapies and community music is notably under-represented too (Ansdell & de Nora, 2014; Boyce-Tillman, 2016; Sextou, 2016), as is research in the medical and health humanities (Crawford, Brown & Charise, 2020).

### Limitations of the scoping review

The authors clearly state the limitations of their report, but two of these are of real concern. The reader should consider Section 3 of the report and take the limitations into account before delving into the wide array of topics reviewed. Firstly, they acknowledge that the report ‘did not involve a systematic literature search’ but prioritised the conclusions drawn from over 200 ‘systematic reviews, quantitative meta-analyses and qualitative meta-syntheses’ and over 700 individual studies. Secondly, it was not possible to undertake ‘detailed discussion of the strengths and limitations of different methodological approaches or individual studies’ due to the requirements of Health Evidence Network reports. They suggest that ‘discussions within specific studies or the reviews cited here’ (p. 51) should be consulted for details, and this is sound advice the reader should take seriously as I will show below.

The principal problem with the scoping review is not the literature search, however, which is thoroughly described in Annex 1, but rather the lack of quality screening of sources following the application of exclusion criteria. Admittedly, such screening is not usual in a ‘scoping review’ but some checks on the quality of the material included is surely required for a review of any kind. Given the focus on previous systematic reviews, the authors might have indicated whether reviews were registered on PROSPERO and conducted following PRISMA guidelines. A simple check could also have been made to ensure that reviews conformed to widely accepted standards e.g. the UK National Institute for Health and Care Excellence Methodology Checklist for Systematic Reviews and Meta-analyses.

Furthermore, in Section 3, the authors highlight ‘gaps and challenges’ (p. 53) in the literature, and two points are significant. Firstly:

While this report has highlighted some areas where there are inconsistent or null findings, there is an inherent publication bias in the literature towards positive findings. Consequently, future research studies should include null findings to allow a balanced appraisal of where the arts can and where they cannot provide support to health (p. 54).

The issue of publication bias towards positive findings, has long been recognised in the social sciences, and increasingly there are moves to encourage the publication of protocols prior to research being conducted with a commitment to publish results whatever the outcome. This is important in the context of the so-called ‘replication crisis’ in the medical and social sciences, and initiatives, such as the services provided by the Centre for Open Science, are much needed to advance work in the field of arts and health.

This relates closely to the second challenge in relation to ‘effect sizes’:

...there are issues in determining the size of an effect, although an increasing number of studies have included control conditions that enable comparisons of the size of effect. In many cases, there is growing evidence that arts interventions can have a clinically meaningful impact, with some studies showing comparable or stronger effects for arts interventions than for medication, non-arts social interventions or other health interventions such as exercise (p. 54).

This issue is vital for the field of arts and health, and is connected both with questions of effectiveness, and cost-effectiveness too. These concerns should have been highlighted at the outset of the report, and some account taken of them in selecting studies for review. It would have been helpful, for example, if the authors had clearly highlighted the most robust demonstrations of the claims made in the quote above.

I want now to look at some examples of research covered by this report. I have chosen two areas of personal interest, but I think the issues that emerge will have relevance across the whole of this report and will show that readers should consult the original sources and make their own judgements.

### Health inequalities

To take a practical, implementation perspective, consider public health specialists who turn to the section on ‘social determinants of health’ (pp. 9-12) to discover how the arts can help reduce ‘social inequalities and inequities’ (pp. 10-12). They will find two studies involving ‘musical’ interventions.

The first is a small randomised controlled trial (RCT) of music therapy from South Korea (Kim, 2017). This evaluated a twelve-week programme of music-making for children aged 7-11 experiencing poverty and on-going ‘maltreatment.’ Only 26 children took part, and while the author suggests the study was satisfactorily powered, any critical reader would be sceptical. In any event, the author acknowledges that the ‘effect sizes’ on the outcome measures employed were very small; variability among participants was substantial, and the results could not be generalised. But these considerations are hardly the most significant concerns raised by this study. More important is the fact that music therapy is used to help support young children living in domestic circumstances highly perilous to their wellbeing. The author gives graphic examples:

... two boys were victims of chronic domestic violence. They were often restless and prone to emotional outbursts and aggression towards younger and weaker children in the group (...) Two boys missed out group music therapy sessions from time to time (...) later the therapist was informed that the boys were badly beaten at home the previous day (pp. 74-5).

So at best, the intervention was ameliorative, to a small degree, for individual children; but at worst it failed to address questions of child protection, and simply ignored tackling wider issues, of poverty and social mores – the factors Marmot et al. (2020) refer to as ‘the causes of the causes.’ All that Fancourt and Finn say of this study, however, is that ‘group music can help to prevent the development of depression, anxiety, attention problems and withdrawal’, without any reference to the dire circumstances surrounding the children involved.

The Korean study is followed by an RCT of the National System of Youth and Children’s Orchestras in Venezuela – *El Sistema* (Aleman et al., 2017). This, nation-wide initiative aims to enhance educational opportunities for disadvantaged children through music. The trial took place in 16 music centres in five regions of the country and involved almost 3,000 children aged 6-14. The parents making an application to these centres, were randomised to an offer of admission in September 2012 or a year later in September 2013. Of the participants, 16.7% were living below a household poverty level of US\$4 a day, compared with 46.5% in the regions covered. As poorer children were under-represented, far from addressing social inequalities, the work of the centres served to reinforce them – entirely contrary to the idea of an intervention designed to reduce social and health inequalities. Children taking part in the musical programme showed some improvement in ‘self-control’ and some reduction in

‘behavioural difficulties’, but statistical significance is reported as 1% and is hardly impressive. Furthermore, these outcomes are not related to the health of the children. The authors claim that the changes seen ‘are increasingly identified as critical for individual wellbeing’, so the health benefits are implied rather than directly demonstrated.

Fancourt and Finn do not identify the Venezuelan project by name but refer simply to ‘a large-scale community music programme’. In addition, and surprisingly, no link is made with Case Study 1 (p. 11), which describes *El Sistema* and related initiatives in Europe. A reference is given to the Sistema Europe website, but the Sistema Global site is not given (<https://sistemaglobal.org/>), nor is there any reference to the global review by Creech and colleagues available there, or the critical literature on the mythologies surrounding *El Sistema* (Baker, 2016).

Apart from the questionable outcomes from the Korean and Venezuelan studies, these two studies could scarcely be more different in the nature of the music involved. In the Korean example, children met weekly to improvise on instruments and to sing – but in the *El Sistema* programme, young people engage in serious musical training, over years, to equip them to play orchestral music from the Western classical canon.

I suspect that our public health specialists will find neither study of much help in developing local initiatives.

### Falls prevention

As a second example of how health professionals might use this report, consider occupational therapists involved in managing a falls prevention programme for elderly people in a local health service. They might turn to the section on ‘frailty’ to discover what dance can contribute in preventing falls (p. 25). Fancourt and Finn sum up the ‘preliminary’ evidence as follows:

...dance may help to prevent falls, particularly in populations with existing health conditions, although other studies have not found benefits (p. 25).

The sources cited in support of this statement, include two systematic reviews and two RCTs. A close reading of the reviews reveals that several caveats need to be attached to this summing up. In the earlier of the systematic reviews (Fernández-Argüelles, Rodríguez-Mansilla, Antunez, Garrido-Ardila & Muñoz, 2015), none of the seven RCTs considered incidence of falls, but focused on risk factors for falling (such as balance, gait and strength). And while some studies did show some beneficial changes on such variables, the authors highlight the heterogeneity and limitations of the studies considered and conclude that the evidence does ‘not enable us to confirm that dance has significant benefits’ in relation to falls risk factors.

A second systematic review (Veronese, Maggi, Schofield & Stubbs, 2017), focused on six different RCTs: four directly assessed the effects of dance on falls while three assessed ‘fear of falls’. Within the former group, only one study (da Silva Borges et al., 2014) reports that ballroom dancing, undertaken three times a week over twelve weeks, in a care home setting, resulted in a significant reduction in falls. The ‘effect size’ of 2.67 reported in favour of dancing is spectacular, but unfortunately there is a major flaw in the paper. Figure 2, which the text states gives the falls data, reproduces, in error, the results for a postural balance measure already given in Figure 1. In the absence of contacting the authors to gain access to the actual data on falls (which I have done, but with no response), this study should have been excluded from the review. Despite the Veronese et al. review adhering to

PRISMA guidelines, the authors fail to mention this major flaw in the da Silva Borges et al. paper.

The remaining three studies did not show that a dance intervention compared with the control condition reduced falls, and the review authors' conclusions are not encouraging:

In summary, we found a paucity of studies investigating the effect of dance on falls and fear of falling and the evidence base is preliminary and equivocal. Given the heterogeneity of the included samples and interventions, in addition to the short-term follow-up, no firm conclusions can be drawn (*p. 312*).

In addition to the systematic reviews, two specific RCTs on falls are cited by Fancourt and Finn.. The first is a large-scale cluster randomised trial on weekly 'social dancing' conducted in retirement villages in Australia over twelve months (Merom et al., 2016). This study (included in the Veronese et al. review) showed no differences in falls between the dance and the waiting control arms of the trial. Indeed, post-hoc comparisons suggested that for participants with a history of falls, falling was more prevalent for the dance group. This is a well-designed study, with considerable ecological validity, and is larger than all other reviewed studies on dance and falls combined. The obvious question to ask is why did 'social dancing' have no impact? The authors give this careful attention, and suggest:

Social dancing did not prevent falls or their associated risk factors among these retirement village residents. Modified dance programmes that contain "training elements" to better approximate structured exercise programs, targeted at low and high-risk participants, warrant investigation (*p. 2*).

The second RCT assessed the role of 'rhythmic auditory stimulation' (RAS) in reducing falls for people with Parkinson's conducted in Canada (Thaut, Rice, Janzen, Hurt-Thaut & McIntosh, 2018). The design and findings are impressive, but an immediate caveat is that RAS cannot be regarded as 'dancing' when compared with the interventions in most of the other studies considered. These generally involved ballroom dancing, or traditional forms of folk dancing in a group setting. In the Thaut et al., (2018) study, in contrast, individual Parkinson's patients followed a daily programme of 30 minutes of structured walking in their own homes over 24 weeks, paced by 'metronome click-embedded music'. The control condition was complex, as participants followed this programme for eight weeks, stopped for eight weeks, and then resumed for a further eight weeks. This novel design, produced striking results:

...significant improvements related to fall incidents were observed in the first phase of the protocol in both experimental groups, while discontinuation of the RAS training protocol between weeks 8 and 16 led to a significant increase in the incidences of falling for patients in the control group. Moreover, once RAS training resumed for the control group, the number of falls again decreased, suggesting that RAS training was the primary factor for reduction of falls incidents (*p. 40*).

This study undoubtedly deserves to be independently replicated, and if the findings are confirmed in further studies, there might then be a strong case for wider implementation of the RAS intervention. But it is not a dance intervention as widely understood from an arts perspective.

For health professionals looking at this literature on 'dance' and falls, the challenge is to

judge each study on its methodological merits, taking account of the considerable variations of national context, participants, interventions, timings and outcome measures, and then decide whether the evidence would justify implementation of a dance-based falls prevention programme in their local context. The decision, based on the evidence, is likely to be no.

## Conclusion

For someone who has contributed to research and practice in the field of arts and health for 20 years, I was initially pleased to see this report – and I congratulate the authors on bringing together such a wide diversity of research studies. It is hugely satisfying too, to see it published by the World Health Organization – and I hope it leads to further positive initiatives from WHO to support the development of this field. One such is the current issue of *Public Health Panorama* devoted to arts and health. However, having closely scrutinised the research cited on two topics in the report, I was struck by the flaws in the ‘scoping’ approach taken. Researchers in the field, well versed in the many forms of reviewing and the value and limits of scoping reviews, will generally welcome this report and understand the purpose of the review. However, policy makers, commissioners, and managers of health and care services, who hope to find robust and replicated evidence-based guidance ‘on the role of the arts in improving health and well-being’, may, sadly, be disappointed.

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