Let's go beyond “the effect of”: reappraising the impact of ordinary activities on cognition

Rafael Román-Caballero, Daniel Sanabria, and Luis F. Ciria

Mind, Brain & Behavior Research Center (CIMCYC), and Department of Experimental Psychology, University of Granada, Spain

Correspondence address: rarroca@ugr.es (RRC)

Rafael Román-Caballero <https://orcid.org/0000-0003-0943-6217>

Daniel Sanabria <https://orcid.org/0000-0002-4164-7607>

Luis F. Ciria <https://orcid.org/0000-0001-7067-5060>

Abstract

The demands of today's society for interventions that optimize cognitive abilities and prevent their decline have motivated the translation of scientific findings into applied programs. Ordinary activities such as physical exercise, chess, meditation, playing video games or a musical instrument, as well as specific cognitive programs, have witnessed the growth of evidence emphasizing their cognitive benefits. Here, we outline several issues that need consideration before speculating on the implications of this literature: (a) the magnitude and costs of the effect, (b) the robustness of the effect, (c) testing causality, (d) the identification of moderator variables, and (e) the underlying mechanisms. We consider that this would contribute to a critical appraisal of the extant findings by the interested researchers, to reduced overstatements in the media reports about the applicability and public relevance of the effects reported in scientific articles, and to potentially help designing new interventions.

Keywords: cognitive training, dissemination, media, intervention, effect size

Western societies are, to a greater or lesser degree, individualistic, competitive, and progressively aging. Parents want their children to excel in school, adults are expected to succeed in their careers, and the prevention of cognitive decline has become a health priority. In this context, and based on the premise that cognition is malleable through learning and experience, the last two decades have witnessed the surge of myriad interventions seeking to enhance or preserve cognitive capacities. Ordinary activities such as physical exercise, playing a musical instrument, video games, mindfulness or yoga have been the object of intensive research as potential interventions to improve brain function and cognition. Although they are activities that a considerable proportion of the population practices for multiple reasons (e.g., well-being, fitness, socialization), a growing set of evidence emphasizes their potential cognitive applications. In the same vein, many training programs have been developed in with the purpose of increasing specific cognitive skills (e.g., working memory or attention), with the additional potential of transferring their effect to less related functions (Simons et al., 2016). Here, we draw attention to several key issues that must be considered before drawing conclusions from this literature that could potentially lead to the implementation of this type of interventions at the population level and also in order to develop further investigation.

Whether for mere enjoyment or to properly excel, when practiced over extended periods of time, physical exercise, playing video games, or musical training result in individuals improving performance (compared to their starting point), and even becoming experts in the trained domain. According to many authors (e.g., Bialystok & DePape, 2009; Diamond, 2013; Erickson et al., 2011), these popular activities are seemingly appropriate tools for cognitive enhancement. Indeed, one can easily find today, both in the scientific literature and mass media, claims (strong in many cases) about the cognitive benefits of those activities (e.g., Mehr, 2015). Beyond the scientific realm, this literature is used to support public policies and recommendations (e.g., World Health Organization, 2020), fueling popular beliefs.

Despite claims about the evidence that supports the easy and fast malleability of cognitive capacities, a closer inspection of this literature leads to more nuanced conclusions. We address here what we believe are key issues that need consideration to appraise the literature critically, before the wide communication and dissemination of the real benefits of a particular activity, and before implementing intervention programs at the population level: (1) quantification of the magnitude of the effect, (2) assessment of the robustness of the effect, (3) testing causality, (4) identification of moderator variables, and (5) identification of underlying mechanisms. We argue that consideration of these issues should determine (6) the messages in media reports and (7) the

implications that these interventions might generate beyond the purported effect (**Figure 1**). On the base of these issues, we make some recommendations for researchers interested in making a critical appraisal of the state of the art in the different cognitive activities. The present recommendations aim to help researchers who plan to conduct new empirical studies or evaluate the available evidence in the cognitive training literature with a critical attitude. Importantly, we are not presenting an exhaustive list of actions to solve the aforementioned issues, but rather we intend to illustrate that there are other ways to proceed and reach reliable conclusions.

Figure 1. Flowchart depicting four issues (gray boxes) about the cognitive effects of lifestyle activities that we propose should be considered before drawing general conclusions and wide dissemination.

1. Quantification of the magnitude of the effect

Too often, findings of a single study showing that an intervention produces statistically significant changes in cognitive or health indicators lead to overstatements about its practical relevance (Moreau et al., 2019). Clear-cut headlines expressing that “*Y enhances capacity X*” or “*a population Y has superior ability X than the average*” flood the press when an individual study finds a statistically significant effect (summarized here with usual headline “the effect of Y”). However, information about the magnitude the effect is rarely provided, even if the implications of any intervention in daily life and the possibilities of translating it into real-world applications depends on the magnitude of the changes it produces.

The use of standardized estimates of the effect size is widespread in psychology (e.g., Cohen’s d), as they allow a common understanding regardless of the original scale of the dependent variable and facilitate the comparison across studies (Lakens, 2013). In the case of Cohen’s d , it represents the difference (e.g., between two groups) in terms of standard deviations. Yet, the interpretation of Cohen’s d is not so intuitive, firstly due

to its scale differs from the more familiar original units. Cohen (1988) proposed benchmarks about what values represent a small ($d \approx 0.2$), medium ($d \approx 0.5$), or large ($d \geq 0.8$) effect, conceiving a medium effect as ‘large enough to be visible to the naked eye’ (p. 26) and as the usual value observed in a research field. But these values are arbitrary and decontextualized (Funder & Ozer, 2019; Lakens, 2013). Indeed, the typical d value varies across literatures. While observing d values of 0.8 or higher is common when studying basic phenomena in experimental psychology¹, lower d values are more common in cognitive training studies. For instance, recent meta-analyses examining experimental evidence in musical training (Román-Caballero et al.; 2021a), mindfulness (Cásedas et al., 2020; Whitfield et al., 2021), physical exercise (Ludyga et al., 2020), chess (Sala & Gobet, 2016), and working memory training (Sala & Gobet, 2020) showed effects around $d = 0.2$ – 0.4 . Thus, the interpretation of d value requires a comprehensive analysis of the research field in question, and the use of Cohen’s benchmarks is recommended only in exceptional cases or with extremely new findings (Lakens, 2013). Ultimately, even when the size of an effect is below that usually observed in a field, it might have large consequences in the long run—even if not very consequential for a single individual (Funder & Ozer, 2019).

Hand in hand with the problems of interpreting standardized estimates, other ways of representing the impact of a particular intervention have appeared and might assist in understanding its practical implications. When the observations of both groups (i.e., treatment and control groups) are normally distributed and with equal variability, some percentage-based estimates can be derived from the d value, all of them being more informative to the general public than the standardized estimate (Hanel & Mehler, 2019). For example, in Román-Caballero et al. (2021a), the final meta-analytic effect of learning to play a musical instrument in experimental studies with children or adolescents was $d = 0.26$, which equals that (a) the distribution of the treatment group had a 10.3% nonoverlap ($U_1 = 10.3\%$; Cohen, 1988), (b) the 60.3% of the treatment group would be above the mean of the control one ($U_3 = 60.3\%$; Cohen, 1988; Grice & Barrett, 2014), (c) there was a 57.3% chance that a person selected at random from the treatment group would have a higher score than one of the controls selected at random (*probability of superiority* = 57.3%; Ruscio, 2008; also called *common language effect size*), and (d) group membership explained the 1.7 % of the variance ($r^2 = 1.7\%$; Cohen, 1988; Ruscio, 2008). While studies reporting standardized effect sizes are increasingly common, the

¹ For example, the flanker conflict in Attention Network Test (ANT; Fan et al., 2002), in which the direction of several distracting arrows interferes with the task-relevant direction of a central arrow when they are incongruent. This effect usually adopts d values above 1 (e.g., $d = 1.78$ in Fan et al., 2005).

inclusion of some of these alternative percentage-based estimates is scarce. However, those alternative indices involve some statistical knowledge (for example, for a right interpretation of the *probability of superiority/common language effect size*, the readers should bear in mind that the chance level is 50% and then they will acknowledge that a probability of 57.3% is not comparatively large). Thus, there might be still room for new forms that facilitate the understanding of the lay audience. One possibility might be the translation of standardized effect sizes to familiar metrics such as the increase in the number of sleep hours, years of life, or intelligence quotient (IQ). In the previous example, a $d = 0.26$ would equal a gain of 3.9 IQ points.

In addition to quantifying the benefits, an appropriate evidence-based decision-making workflow requires analyzing the costs of the intervention. These can be expenses in new material or instruments, payment of services of personnel and facilities, and transport and additional travel, among others. Regarding the person receiving the treatment, the effort and the amount of hours of practice outside of training sessions should be assessed. Cognitive stimulating activities, in general, do not usually carry adverse effects of any kind², but they involve a significant investment of time that cannot be used in other activities (i.e., opportunity cost). Further, the degree of enjoyment and the motivation of the person must be taken into account. An activity with an effect that could conventionally be considered small ($d = 0.1$) might have a reduced practical relevance if the costs are high and it is perceived as little motivating, especially when the cognitive improvements are the main goal of the intervention. Otherwise, it could be consequential, especially when the activity brings multiple positive consequences apart from a certain level of cognitive improvements, such as enjoyment, social relationships, etc. Physical exercise, chess, playing a musical instrument and so on, are time-consuming activities that require dedication and, in some cases, a considerable economic cost. Their modest cognitive effects suggest that they should be dismissed as first-choice interventions if the sole purpose of practicing them is cognitive enhancement. On the other hand, when people are engaged because for them these activities are pleasant and contribute to their well-being, any other cognitive benefits, although not intended, might add value and reinforce the motivation to keep them integrated into one's lifestyle.

2. Assessment of the robustness of the effect

² However, there is evidence suggesting that meditation could be a precipitant of psychotic disorders in some cases (Sharma et al., 2019) and trigger other transient unwanted states, such as anxiety or depersonalization (Cebolla et al., 2017).

Estimates of effect size are meaningless if the measurement on which they are estimated is a result of spurious variance or methodological biases. Thus, the precision and stability of an effect are crucial when evaluating the implications of an intervention. Both precision and stability are highly dependent on the size of the study sample (Borenstein et al., 2009; Lakens & Evers, 2014). Small sample sizes yield estimates with low precision (i.e., with wider confidence intervals) and relative instability, being more variable and departing more from the true effect (Lakens & Evers, 2014). Another consequence of small sample sizes is that they lead to designs with low statistical power (i.e., below the conventional threshold of 80%), especially when the true effect is modest. Therefore, the sample size will determine how confident we can be that the observed effect size in an individual study is close to the underlying effect (stability) and the likelihood of observing a significant effect if the effect truly exists (power). Moreover, low power also reduces the likelihood that a statistically significant result reflects a true effect (Button et al., 2013). Contrary to the belief of some researchers, an effect that is statistically significant despite the low power of the study does not mean that the effect is genuinely large or robust. When a small study observed a significant and large effect, it is more probable that the result did not represent a true effect and that its estimated magnitude was inflated.

Recent comprehensive meta-analyses on cognitive training effects (Cásedas et al., 2020; Ludyga et al., 2020; Román-Caballero et al., 2021a; Sala & Gobet, 2016, 2020) showed that most primary studies in the literature had sample sizes below the required to reach adequate power and stability: for effects around $d = 0.2$ – 0.4 , at least 200–394 participants per group would be required to reach a power of 80% or higher, and 238–252 for the estimated effect to reach a point of stability (Schönbrodt & Perugini, 2013; Lakens & Evers, 2014). Meta-analyses can help to overcome the lack of power of individual studies as they are the result of pooling the effects from several individual studies. However, the results of meta-analyses largely depend on the characteristics of the included studies and, therefore, on the comprehensiveness of the review and the availability of all the conducted studies. In psychology, for instance, it is more the rule than the exception that intervention studies on the same topic do not share the research design. Studies often differ in sample characteristics (e.g., age, health condition, personality traits), assessment tools and dependent measures (e.g., self-report measures, reaction times, or percentage of times that participants accurately perform a response), and training program. This leads most psychology meta-analyses to show substantial variability between the reviewed findings so that sampling error cannot be explained. That heterogeneity entails the existence of a multiplicity of true effects (e.g., the intervention might have higher benefits in older than young adults; larger changes in

physiological than self-report measures), where the meta-analytic outcome represents just a weighted average of all of them. A positive average effect does not necessarily mean that all forms of the intervention are effective; on the contrary, some settings might produce no benefit or even, be detrimental. Estimates such as prediction intervals (Higgins et al., 2009) can help to characterize the distribution of possible effects and nuance the inferences we extract from the overall outcome of the meta-analysis. Moreover, with sufficient data, it is desirable to explore the causes of heterogeneity (e.g., by subgroup analyses; Borenstein et al., 2009; also see **4. Identification of moderator variables**).

Regarding available evidence, the scientific literature is littered with selective reporting, in which the studies and the outcomes with significant results are more likely to be published (i.e., *publication bias*; Thornton & Lee, 2000). While publication bias involves non-publication of entire set of studies conducted, other biases can appear in the reporting process, such as the non-publication of individual negative outcomes within an article (i.e., *outcome reporting bias*) or concluding that the intervention is effective despite non-significant results on the primary outcome (i.e., *spin*; de Vries et al., 2018). Spinning could be based on post hoc analyses or wrong interpretations of statistics (e.g., interpreting $p = .10$ as significant), as well as it could be done mildly (e.g., suggesting positive effects from numerical trends although the authors stated that change was not statistically significant). In addition, studies with positive results are more likely to be cited than null or negative findings (i.e., *citation bias*), which might make it more difficult to discover negative results (Hardwicke et al., 2021). The same happens with contradictory replications and critical commentaries. For example, a recent meta-analysis by Chen et al. (2020) on physical exercise and executive functions that found small benefits in older adults has received 55 citations (in Web of Science up to December 2022), but a comment on this article pointing to methodological flaws that raised serious concerns was published five months later and has been only cited once (Zimmer et al., 2020).

Thus, meta-analytic results should be taken with caution and not be treated as the ultimate resolution of a scientific question without first scrutinizing the outcomes that have been analyzed³. Meta-analytic outcomes based on biased reporting or low-quality designs and measures (*'garbage in, garbage out'*) could lead to conclusions that do not correspond with the reality of the effect. For these reasons, the results of meta-analyses are often inconsistent with each other (whereas positive findings have been observed in

³ In essence, the summary effect of any meta-analysis is the weighted mean of the individual effects (Borenstein et al., 2009), so the meta-analytic result of only one portion of the studies conducted, like those with positive and significant outcomes, should be taken as the mean effect of a selective slice of the empirical evidence.

Román-Caballero et al., 2021a; and in Ludyga et al., 2020; for null results, see Sala & Gobet, 2017, 2020; Young et al., 2015). Therefore, meta-analyses could be valuable evidence to approach the size of the effect of cognitively stimulating programs and activities and, in general, they could provide a more accurate estimation. However, we pose researchers to treat meta-analyses critically, based on how comprehensive they were, and whether they assessed the influence of publication bias and other source of bias over the estimated effect. More often than desired, meta-analyses omit these crucial aspects in their analysis. For example, a recent umbrella review of the meta-analyses of randomized control trials (RCTs) in exercise–cognition literature (Ciria et al., 2022) found that only 60% of the meta-analyses used publication-bias methods and that less than 25% adjusted the final effect for this bias. It suggests that most of the meta-analyses overestimated the effect of physical exercise on cognition.

Conflict of interests also deserved a mention. Peer-reviewed journals demand a declaration of any conflict of interest that can bias the results (and their interpretation) of the article but, most times, it focuses on direct funding of the particular study. However, the desire to achieve greater academic prestige and discover groundbreaking results can incur a conflict in itself. The publication of some types of findings, especially those related to factors promoting health and wellness, might give researchers higher media coverage and better grants for future studies. At the same time, it might increase their popularity within the scientific community and the number of offers to give keynotes, talks, and courses with financial incentives. Every step that the scientific findings are separated from the data (in their interpretation or in the report itself) to satisfy these other reasons is a compromise for the robustness and replicability of the effect.

3. Testing causality

Another relevant aspect is the method used to estimate the effect size and what part of the observed effect is actually the result of the cognitive activity. Experimental trials are widely taken as the gold standard designs to establish causal conclusions, especially when they entail the random assignment of participants without prior formal training, the inclusion of an active control group, and a blind cognitive assessment⁴. Nevertheless, RCTs would be infeasible in the long-term, due to high attrition, and even unethical, as they require keeping participants in a randomly selected and, perhaps, unmotivated activity for several years (Habibi et al., 2018). Even in the short run, the lack

⁴ A double-blind design is impracticable in cognitive training programs, as the participants are aware of the type of activity they are practicing. In any case, they may not know the purpose of the intervention and the hypotheses of the study.

of motivation for an intervention that participants have not actively choose might lead to a lower level of involvement and produce different conditions than if the intervention was intentionally sought out in real life (Corrigall et al., 2013). Thus, in contrast to the benefits this design could have in terms of internal validity, it could compromise the generalizability of its results (i.e., external validity). On the other hand, in cross-sectional studies, the cognitive assessment is carried out in naturally formed groups but only after training has taken place (mostly for years of engagement). It allows observing participants who self-selected the activity, although at the cost of increasing the probability that subgroups with a greater propensity to choose that lifestyle are overrepresented in the sample (and the differences could be inflated or completely explained by confounding variables). For instance, taking playing music as an illustrative case that can be extended to other activities, children with higher socioeconomic status, general cognitive abilities, auditory skills, higher level of openness-to-experience, and who are engaged in more stimulating activities, are more prone to take music lessons (Corrigall et al., 2013). Despite this well-known *selection bias* (which is also present in many other cognitively stimulating activities), most of the studies that examine the cognitive differences between musicians and non-musicians do not control for those third variables, usually only for the age, level of education, handedness, and sex of the sample⁵. As a result, the effects in uncontrolled studies tend to be significantly larger ($d = 0.8-1$) than in experimental studies or cross-sectional studies with extensive control of confounding variables, through matching or covariation ($d = 0.2-0.4$; Guhn et al., 2020; Nichols et al., 2020; Román-Caballero et al., 2021b; Slevc et al., 2016). When confounding variables are controlled, the observed effects in cross-sectional studies are close to the observed in meta-analyses with RCTs ($d = 0.2-0.4$).

Nevertheless, well-controlled studies do not mean that the inclusion of a multitude of third variables will necessarily improve causal inference (Rohrer, 2018). The explicit representation of the causal structure in graphical models can help to select a priori third variables that should be measured and controlled for (Pearl & Robins, 1995; for a primer in psychology, see Rohrer, 2018). Following the example of musical training, we present here a tentative causal model of the effect of musical training over general cognitive capacities (**Figure 2A**). Some of the third variables in which musicians and non-musicians usually differ before musical training can be presumed to have also an

⁵ Taking into account that a great proportion of the psychological research (including research in cognitive training) is carried out with university students, it is relatively easy to find groups of participants with the same achieved level of education and age (i.e., young adults, between 18 and 30 years old, with an achieved pre-university level). Even when there is no intention to control for them, it could be relatively common to find homogeneous groups in those characteristics and in which there is an accidental control of them.

impact on cognition, such as the engagement in other cognitively stimulating activities and the musicians' own prior cognitive skills. Controlling for these third variables (by matching, stratifying, or as covariates) will suppress their confounding effect over the musical training–cognition relationship.

We can also assume that the practice of other activities acts as the mediator of other third variables (i.e., ancestors) that influence the propensity to select musical training and post-training cognition: socioeconomic status (SES), prior cognitive skills, and the personality trait of openness-to-experience. Children and adolescents from families of middle-to-high SES might have more economic resources to afford the expenses of extracurricular activities, including activities with substantial costs such as musical training. Similarly, individuals prone to sensation-seeking will select multiple leisure activities, as well as individuals with higher self-control and prior cognitive advantages would continue more easily with interventions that require sustained learning and discipline. Finally, genetics might affect the probability of selecting musical training and late cognitive skills through conditioning the expression of openness-to-experience and early general cognitive capacities. In the present model, controlling for the level of engagement in other leisure activities (in light blue in **Figure 2A**) would be sufficient to block the influence of most of its ancestors (SES, openness-to-experience, and genetics) over the target causal relationship. Although for complete control of confounds, prior general cognition needs to be blocked as well, given it affects post-training cognitive capacities in a direct fashion. In contrast, it is not necessary to control for auditory skills even when musicians and non-musicians likely differ in their prior rhythmic and tonal capacities, as they are related only with the probability to start and keep with musical training (and not with the second component of the target causal relationship).

Figure 2. (A) Causal model of the effect of musical training on cognition. Several third variables can produce a confounding effect over the relationship of the two main variables. The control of some of those variables (pre-training general cognition and the engagement in other cognitively stimulating activities; in light blue) would allow preventing third variables to alter the outcome in the target causal effect, while the control for prior auditory skills (dark blue) will not be influential. (B) Causal model of the effect of musical training on a specific cognitive skill (e.g., episodic memory; in violet), which can be direct or mediated by the changes in general cognitive capacities.

A different scenario arises when the focus of the study is to assess the effect of musical training on a specific cognitive function, for instance, episodic memory. Even when confounding variables are controlled for (e.g., prior cognitive capacities), researchers have to decide whether they are interested in the causal effect of musical training in general, including any mediating mechanism, or if they focus on a specific mechanism. In the causal model in **Figure 2B**, controlling for the post-training outcomes of general cognition (i.e., intelligence) would block this causal pathway of musical training over episodic memory. This proceeding will underestimate the general effect of musical training but allows isolating the genuine changes in memory that are not driven simply by changes in general cognitive capacities⁶. Therefore, the comparability of cross-

⁶ Jakobson et al. (2008) offer an example of the attenuation of the musical training–memory relationship when intelligence is taken into account, as the main purpose of the authors was to

sectional studies examining the musical training-cognition relationship depends on the type of control implemented.

Finally, colliders are another relevant type of third variable. Contrasting with a confounder, which is a common cause for the two main variables of the target relationship (i.e., musical training–cognition), a collider is a common consequence of both variables. In the hypothetical case that the training program were ineffective improving cognitive skills, adjusting for a collider might generate a spurious relationship that leads us to conclude the existence of an effect. Therefore, whereas it is desirable to control for a confounder to get an unbiased association, the opposite is true for a collider. For example, one can hypothesize that both musical training and general cognition promote healthy habits and physical exercise (**Figure 3A**). Musicians might be more prone to engage in physical activities that keep their musculoskeletal structure fit, as a way of preventing injuries due to the repetitive movements during musical performances. Moreover, good planning and decision-making skills can help to follow a schedule that incorporates physical activity weekly. Someone could think of fitness as a confounder and only investigate a population with low physical activity. In that case, and if musicianship and general cognition were two independent traits, people with low fitness would have either low musicianship or low cognitive skills (**Figure 3B–C**). When there would be no effect in the wider population, the previous design (limited to participants with low fitness) would wrongly conclude that musical training has a detrimental impact over cognitive skills. This situation illustrates cases in which controlling for a third variable is worse than not doing it, introducing a spurious association (i.e., *collider bias*).

collect evidence of how musicianship is associated with episodic memory ‘above and beyond any effect it [musical training] may have on general intelligence’ (p. 45).

Figure 3. (A) Causal model of a hypothetical situation (i.e., simulated data) in which musical training is assumed to have no benefits on general cognition (i.e., independent traits), but both variables are influential on fitness, becoming a collider (in green). This type of third variable is different from confounders (e.g., socioeconomic status, in blue), which affect the two variables of the target relationship. (B) In a scenario in which musicianship and cognitive skills are independent ($r = 0$), and both positively correlate with fitness ($r_s = .70$), (C) if the authors of the study decided to limit their cross-sectional design to participants with low fitness (in grey), a negative correlation between musical training and cognitive skills would emerge (i.e., they could wrongly conclude that musical training has a detrimental impact).

The scientific community is more willing to conclude causality with experimental studies. However, in cases in which such a design is not viable for long periods of time, well-controlled cross-sectional designs, guided by causal models built on theory and updated by the accumulation of empirical evidence, can characterize the effect in a complementary way. For cognitively stimulating activities, these designs offer evidence of long-life practice and under more ecological and motivational conditions. Thus, cross-sectional evidence cannot be dismissed, much less substituted by the results of randomized trials. The use of causal models to identify confounding variables, although arguably improvable, have the advantage of making explicit all the assumptions that the researchers held about the causal structure. Continuing with the example of musical training, designs different from RCTs have been used to support a causal relationship such as longitudinal studies with twins (monozygotic and dizygotic) and siblings (biological and adoptive; Gustavson et al., 2021). Twin studies model the genetic influence, whereas the longitudinal component allows measuring the change in cognition controlling for the pretest conditions in a population that is self-motivated to practice music.

4. Identification of moderator variables

If we want to go beyond 'the effect of', an important question is what factors determine the size or appearance of any effect. Apart from the obvious influence of the duration of the training (i.e., 'dose effect') and the degree of engagement (Sala & Gobet, 2016), the consequences of training might differ depending on the characteristics of the sample. In general, those participants with a less favorable background tend to improve more (Diamond, 2013). For example, lower initial cognitive performance, lower socioeconomic status (Román-Caballero et al., 2021a), higher body mass index (Xue et al., 2019), or higher baseline levels of anxiety and depression (Williams & Lord, 1997) are some of the conditions that have been linked with greater benefits in cognitive training, presumably due to a greater window of opportunity. Situations of cognitive vulnerability, such as aging (Román-Caballero et al., 2018; Whitfield et al., 2021) and brain disorders (Dauwan et al., 2021), also benefit more than people in an optimal state of functioning. And when studied in children and adolescents, greater benefits have been observed in the younger participants (Román-Caballero et al., 2021a; Sanabria et al., 2019). This is suggestive of a sensitive period during which stimulating activities are likely to have stronger and more permanent effects on general cognitive skills (White-Schwoch et al., 2013).

Regarding training programs, terms like physical exercise, meditation, and musical training are umbrella terms that have been used to designate a variety of activities. Physical activity includes externally-paced sports such as football, basketball, volleyball, tennis, or martial arts, as well as self-paced sports, such as track and field, running, swimming, triathlon, or cycling (Sanabria et al., 2019). Likewise, mindfulness meditation has included different practice such as focus attention and open monitoring meditations, while musical training could be used to design playing instruments and singing as well as other more diverse activities (e.g., structured listening, computerized programs of auditory training, or music education methods like Orff or Kodály; Sala & Gobet, 2017, 2020). However, the empirical evidence accumulated in recent years suggests that the cognitive benefits of different activities within a general category are not comparable. For example, externally-paced exercise has been associated with superior vigilance capacity compared to self-paced exercise and sedentary lifestyle (Sanabria et al., 2019; Ludyga et al., 2020), presumably as the former poses high cognitive demands with constantly varying exercise environments (Pesce et al., 2021). Moreover, recent evidence suggests that individuals, especially elderly people, experience higher cognitive gains from cognitively engaging physical interventions (i.e., simultaneously physical and cognitive training) than from physical activity alone (Gavelin et al., 2021). Indeed, the large-scale reallocation of cognitive resources during the skill acquisition inherent to the practice of any physical activity (i.e., skill training) has been

pointed as a key aspect for getting the potential cognitive benefits of physical activity (Tomprowski & Pesce, 2019). Therefore, general and scientific reports should consider the factors that moderate the effect in question before reaching any conclusion about the applicability of a cognitive training activity. In this line, we encourage researchers to examine the accumulated studies and meta-analyses for the existence of robust moderators, not to test many of the candidates in a singular study with the risk of incurring in false positives.

5. Identification of underlying mechanisms

In the event that a lifestyle activity produces cognitive benefits (otherwise, “there is nothing to explain”; Sala & Gobet, 2020), the search for the underlying mechanisms is crucial to explain how and under what conditions the intervention may facilitate certain cognitive processes. Constructing solid theories requires in a first step formulating plausible explanations that can be tested in subsequent empirical studies. Apart from testing hypotheses, the main principles of a theory can be translated into formal models. The formalization of a theory into a set of equations or rules, like causal models (see **3. Testing causality**), allows identifying key variables to measure and their type of relationships (e.g., linear, quadratic, logistic, etc.). In the same vein, formal models can lead to computer simulations assisting to assess the predictions (Borsboom et al., 2021). On the other hand, researchers can empirically evaluate their theories by measuring the mediating variable as well as the dependent variable (e.g., analyzing if changes in cognitive skills with physical exercise arise in parallel than changes in cerebrovascular fitness, if cardiovascular fitness is the explanatory variable), and manipulating the intervention to produce different levels of the mediating variable (e.g., implementing programs of physical exercise enriched at different levels with cognitive components, if the explanation would be the cognitive demands of the intervention).

Unfortunately, it is common to minimize the relevance of these potential mechanisms and go straight on to implement the intervention in real contexts; this is especially true in psychology. This approach to intervention development is not necessarily *wrong* as long as the intervention applied is safe. However, it has many drawbacks. First, being unaware of the nature of a phenomenon and the dynamics by which particular expected changes arise drastically reduces the chances of success of an intervention. Secondly, it slows down the development and improvement of an intervention, since adjustments to increase its effectiveness depend on a process of trial-and-error, rather than theory-based testing. Third, the lack of knowledge about the mediating variables of the expected effects increases the costs (i.e., time, money, and effort) of its implementation. Finally, ignoring the underlying mechanisms makes it

impossible to know if they are shared with other activities and, therefore, if other less costly or more motivating interventions produce the same benefits. A popular example of this in music is the so-called *Mozart effect* (Rauscher et al., 1993), in which the scores in spatial reasoning of university students were better after listening to the first movement of the Mozart sonata KV 448 than after listening to relaxation instructions or sitting in silence. The original interpretation (Rauscher et al., 1995) posited that the neural activity triggered by complex music, such as Mozart sonata, taps similar patterns to those in spatial-temporal tasks, and it ‘exercises’ non-verbal abilities. However, subsequent examinations of the effect suggest that these temporary benefits are the result of states of heightened arousal and positive mood induced by music (Thompson et al., 2001). Contrasting with the first proposed mechanism, the latter suggests that there is nothing special in Mozart’s music. The effect also appears with other types of music as well as other recreational activities with arousing and affective consequences (Nantais & Schellenberg, 1999; Pietsching et al., 2010).

It is common in the cognitive training literature to find explanations based on a metaphor of the “brain as a muscle” (Simons et al., 2016), that is, regular training on a basic cognitive skill strengthens it via neuroplasticity. Hence, if a particular activity requires memorization (e.g., the position of the pieces in a game of chess), it could increase the general faculty of memory and affords advantages to any daily task that also hinges on this cognitive skill. According to this explanation, the mechanism at the base would be common to chess, physical exercise, meditation, and many other cognitive activities: the demands on memory, executive control, attention, and so on. Despite its simplicity, this ubiquitous premise fails to explain why cognitive training programs sometimes fail to transfer their benefits to other activities (Gathercole et al., 2019; Roediger III, 2013; Simons et al., 2016) and, perversely, allows the adaptation of the hypotheses to be able to explain any result. That is, two studies in video games with contrasting results, one finding a memory improvement and the other not, could be easily reconciled by adducing, for example, that both video games had different memory demands, one video game relying more than the other on memorization.

Some mechanisms proposed for relating physical activity to skill acquisition processes are specific to physical training, and not relevant to other sedentary forms of training (e.g., computerized training programs). For instance, physiological adaptations such as enhanced cerebrovascular fitness and increased brain-derived neurotrophic factors have been proposed to promote benefits on brain structure and functions (Cotman et al., 2007; Etnier, 2008). Motor skill learning is also conceived as a problem-solving process in which every attempt is a movement problem that can enhance cognition (if the conditions are sufficiently variable, challenging, and receive augmented

feedback; Tomporowski & Pesce, 2019). Interestingly, these mechanisms could be common to performing arts including instrumental music and dance.

Finally, other approaches propose that training on unfamiliar or highly demanding tasks can promote the development of new cognitive routines or strategies (e.g., Gathercole et al., 2019). These strategies would be specific for each lifestyle, which would make the activity unique in the way it stimulates cognition. For example, the evidence in musical training shows that the superior memory scores usually found in musicians could be largely associated with the greater use of semantic clustering during the coding stage (Jakobson et al., 2008) and the use of singular visual strategies, such as visual imagery, during the retrieval (Huang et al., 2010).

A deeper understanding of the brain and cognitive dynamics when training may provide valuable insight to customize each intervention, tailoring it to each individual to improve its effectiveness. Also, in the case that several lifestyles share the same mechanism through which cognition can be enhanced, the personal motivation and characteristics should be taken into account to offer an activity that best suits their interests and needs.

6. Communication and dissemination

According to the principle of *communality* in science (Merton, 1942), researchers have the imperative to publicly share their methods and discoveries to assure the common ownership of scientific knowledge. Often, researchers explicitly acknowledge the limitations of their studies to offer circumspect conclusions (Mehr et al., 2015; Odendaal et al., 2018). However, inferences of causation based on correlational evidence are relatively common within the literature of cognitive training. In a subsequent step, scientific findings reach a wider public through translation into advocacy documents and media reports. Mass media are constantly in need of astonishing and easy-to-understand novel findings from scientific studies to attract the attention of their audience, which often lead them to oversimplify, misrepresent, or overdramatize scientific results without the essential nuances and limitations for an appropriate interpretation. These misleading messages can have adverse effects for a naive reader who will behave accordingly to inaccurate claims, based supposedly on empirical evidence, expecting particular outcomes that will never happen. In the case of musical training and other stimulating activities, there is a trend towards seeking greater applications, which pushes reports to conclude causality without mentioning the limitations or the implications of the design of the study (Odendaal et al., 2018). Sometimes, the content of scientific articles is distorted to the point to suggest improvements in cognitive skills that were unmeasured (e.g., in intelligence when the study only assesses auditory response; Mehr, 2015). The

pressure to report novel and surprising results has led to the public narrative being dominated almost exclusively by positive results.

The role of conventional media attempting to fill the gap between science and the general audience is crucial for science, but not easy. Scientific results are mostly complex and difficult to adapt to the current format of journalism where catchy short messages are preferentially used to generate engagement. The process of selecting, articulating, and transforming scientific literature into accessible news for the general audience necessarily entails the loss of information, but also highlighting particular aspects of the main findings that will raise immediate public attention. Very often, however, the aspects highlighted by the media do not coincide with those that make research noteworthy from a scientific point of view or do not accurately represent its conclusions. It is imperative that scientific results are reported accurately, especially when they are novel findings (i.e., preliminary). Novel findings are vulnerable to unforeseen nuances and should be tested in subsequent independent studies before the implications of these findings are disseminated to the public. In addition, it will be helpful that the authors clearly distinguish data-driven conclusions from the idea-driven speculations in scientific reports (Mehr, 2015). This caution should be greater when the new findings may lead to health advice or behavioral changes for the general population. The particular cases of the cognitive benefits of physical exercise, mindfulness and musical training are clear examples of this. Exaggerated claims combined with oversimplified recommendations on these topics are easily found in media promising people great cognitive/brain benefits if they exercise regularly, practice mindfulness meditation or if they take their children to after-school music lessons. The media must be realistic about what these interventions can generate, and the magnitude and speed at which progress is made.

Importantly, scientists share a big part of the responsibility for the current situation of scientific misinformation. For instance, many exaggerated and simplistic news are already present in university press releases, which scientists approve (Sumner et al., 2016). Press releases with exaggerated health advice and causal claims from correlational findings were related to the higher appearance of these hyped messages in news. The same trend was observed for caveats in university press releases. Moreover, researchers together with editors are also responsible for this scenario because of the long-lasting developed culture of '*publish or perish*' which forces researchers to get their results published in prestigious scientific journals and sometimes leads them to overstate the relevance and scope of their findings. By adding other well-known biases (e.g., publication bias, outcome reporting bias, lack of external replicability,

etc.), we create the perfect storm of scientific misinformation, which is largely responsible for the current deterioration of the public perception of science.

7. Implications beyond the purported effect

There is a trend towards seeking new applications for the results of cognitive training. Educational and clinical centers, in their pursuit of being at the forefront and increasing their profits, apply cutting-edge techniques and interventions that often lack compelling evidence to ensure the effectiveness of such actions (including the four aforementioned issues). For instance, despite the small (Román-Caballero et al., 2021a) and still debatable (Sala & Gobet, 2020) cognitive benefits of playing a musical instrument, it is not difficult to find music academies advertising such benefits as an additional asset to their core services. In other cases, the purported effect on cognition responds mainly to economic interests. A clear example is the commercial transcranial direct current stimulation devices that promise improvements in physical and cognitive performance after several brief sessions of transcranial stimulation. The body of evidence supporting such claims is weak and heterogeneous (Holgado et al., 2019a, b). It is ambitious to think that this intervention deserves to assume the opportunity cost of other interventions. Even if robust, the effect of such an intervention on cognition would likely be small and imperceptible based on the existent evidence (e.g., Holgado et al., 2019b). It is likely that the resources invested in this specific case could be better allocated for medical care, resting, or additional assistance.

Even when cognitive training activities have no evident adverse consequences, the way in which many programs are advertised, emphasizing the huge malleability of cognitive abilities, could often create false expectations about the outcomes and lead to psychological costs of failure (Moreau, 2021). The rhetoric of the power of training to change cognition could be especially dangerous in combination with the rhetoric of individualism in Western societies, in which the person is responsible for achieving their own goals through effort and perseverance.

From “the effect of” to a nuanced effect

Our ability to capitalize lifestyle activities (e.g., physical exercise, chess, meditation, musical training, etc.) to improve cognitive and brain function is a pressing issue in modern societies. Here, we have provided a critical overview of several key aspects that we believe need careful consideration to critically appraise the scientific literature, before confidently disseminating ideas of their potential over the mind and the brain, and before implementing new intervention programs at the population level.

Before speculating on the implications of the positive findings of an individual study, the robustness, the magnitude, and the costs of that effect should be examined. Otherwise, inaccurate, premature messages may arise at the risk of resulting in wastes of money and opportunities, as well as at the risk of disenchantment with the science that supposedly supports such interventions.

Although it is the responsibility of the journalists to translate scientific papers into accurate media documents, the researchers (i.e., authors, editors, and reviewers) also have an important role. Reporting the effect size and its confidence interval, using alternative but more widely understandable estimates such as the common language effect size (Hanel & Mehler, 2019), contextualizing the benefits of the intervention alongside its costs, and clearly distinguishing between data-driven conclusions from idea-driven speculations are all practices that could help to elucidate the realistic implications of the activity. Although individual studies are the primary source of evidence, their outcomes are highly dependent on sample size. Smaller samples lead to unpowered and less stable estimates of the effect estimate, so meta-analyses can help to overcome these issues. But meta-analyses offer a summary of the available evidence, so their conclusions should be assessed accounting for the included studies and potential biases in the literature such as selective reporting. Before concluding that the intervention is the true cause of the changes in cognition, we recommend considering the evidence of RCTs as well as well-controlled cross-sectional studies. Regarding the later, we consider cross-sectional studies a valuable source of evidence to elucidate the cognitive impact of lifestyles. However, it is crucial they control for a wide range of confounding variables beyond the level of education and basic demographics (otherwise, there is the risk that the effect, if it existed, would be inflated by other influencing variables). Defining causal models could help to identify what third variables are relevant to control for and what confounding variables might inflate the observed effect. The impact of a cognitive program is not unique, it can be modulated by multiple moderators. Researchers should consider them when translating the conclusions from one design to another. Although some moderators could be highly specific of a type of training, some like age or the window of opportunity for improvement seem to be transversal. Finally, regardless of the design, we encourage researchers to reflect on the underlying mechanisms behind the effect. The 'brain as muscle' metaphor is an easy-to-understand but weak approach for complex programs such as meditation, chess, musical training or physical exercise that do not target one specific process. This approach, without detailed specifications, allows generating unfalsifiable hypotheses that can explain any result (see **4. Identification of underlying mechanisms**). Even more, the vague allusion to a general capacity of cognitive skills to be strengthened by

training without mentioning any constraining mechanism may create the impression on the wider public of a huge malleability of the brain (see **6. Implications beyond the purported effect**). Since here, we encourage the proposal of additional explanations, expressing them into formal models, and measuring or manipulating interventions to produce several levels of mediating variables to test the candidate theories.

In addition to all key issues mentioned above, the relevance of cognitively stimulating activities to our society, such as physical exercise, meditation, chess, video games or playing an instrument, must be disentangled from the controversial question regarding their effect on cognitive and brain function. There are powerful reasons, beyond the purported cognitive benefits, for engaging in these activities in our daily lives. As an example, being physical active has been associated with reduced chances of suffering physical disorders such as cardiovascular disease, diabetes, osteoporosis, arthritis, colon and breast cancer, etc. (Dishman et al., 2021). Regular exercise, especially when outdoors, is also associated with breathing clean air and direct exposition to sunlight (which generates higher levels of vitamin D and helps regulate circadian rhythms and sleep; Moran et al., 2013). Last but not least, we should not overlook the pleasure of doing something in itself. The value of cultivating these activities may lie simply in one's ability to enjoy themselves. In the light of the evidence of small cognitive benefits, the impact of these activities over our mental skills, if any, should be not taken as their principal goal, but as a supplementary effect that adds value to two ancient human facets with many other functions.

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