

One size does not fit all: addressing the challenges of intervention for complex developmental issues

Abstract:

The Journal of Child Psychology and Psychiatry is committed to publishing *implementation* science, or turning therapeutic research discoveries into practical benefit for human health and well-being. This editorial showcases three such papers in this issue from diverse fields in developmental science. The papers highlight the challenges of implementing intervention under real world constraints, and highlight key issues that clinicians and researchers will need to address in future work. Despite these challenges, each approach shows promise and innovation in delivering high-quality interventions to vulnerable children and families.

As I come to write this, my last editorial for JCPP, I am once again impressed with the eclectic mix of topics, methods, and populations that are contained within these pages. Child psychology and psychiatry is certainly a broad church and what delights me about JCPP, as someone who primarily works in the field of child language disorders, is the opportunity to learn from other disciplines. It is always a relief to see that researchers and clinicians in every field are grappling with many of the same issues, and to find some commonalities (as well as differences) in our approaches.

Here I would like to focus on intervention and the wide ranging impacts intervention can have on children dealing with a variety of developmental concerns. Two of the critical trends that have influenced clinical trials in recent years include the increasing focus on *implementational* research (Green, 2016), or turning therapeutic research discoveries into practical benefit for human health and well-being. In practice, this means testing interventions that can readily be implemented and 'scalable' in real world settings. This forces researchers to consider the practical and financial challenges of intervention delivery and raises important questions dosage and implementation. In addition, we are developing ever more sophisticated techniques to explicitly test *developmental cascades*. Cascade models presuppose that changes to one developmental domain, function, or system alter the development of another domain, function, or system over time. Interventions designed to target processes for change represent cascade models (Masten & Cicchetti, 2010) and have important implications for preventive interventions. This is because well-timed and targeted interventions could interrupt negative or promote positive cascades, and thereby attenuate some of the negative downstream consequences of developmental disorder. Clever trial designs that include

longitudinal follow-up and measurement of the downstream target are needed to test these cascade effects.

Three intervention trials reported in this issue of JCPP illustrate these trends and highlight the challenges involved in delivering research with immediate application. De Bruin et al. specifically test a cascade model in which improvements in adolescent sleep were hypothesized to yield positive impacts on symptoms of psychopathology. The study involved a CBT intervention for insomnia delivered over six, weekly group sessions; sessions were delivered either face-to-face or via the internet (versus a wait-list control), and included a follow-up two months later. Post-intervention symptoms of psychopathology on a self-report measure decreased significantly for both treatment groups, with medium to quite large ($d = .97$) effect sizes. Crucially, improvements in sleep quality and quantity fully mediated the effect of CBT for insomnia on symptoms of psychopathology.

Of course such findings require replication before changes to clinical practice are mandated. Outcome measures were self-report and therefore participants were not blind to treatment status. Participants were predominantly (75%) female and did not have confirmed primary psychiatric disorders; the impact of such interventions on clinically-referred adolescent populations is therefore much needed. The sample size (~40 per group) is relatively small; there is increasing awareness that small studies tend to have larger effect sizes than large studies (Button et al. 2013), which may distort findings and decrease the likelihood that they will replicate in larger, or more clinically impaired, samples. Nevertheless, this study provides encouraging first evidence that a relatively low-cost, low-intensity intervention focused on sleep could yield positive impacts on psychological well-being for adolescence with relatively minor psychopathology symptoms.

Our research does not occur in a political vacuum and the unfolding humanitarian crisis in Syria, and other crises around the world, has tested our ability to support the mental health and well-being of young people *in extremis*. Panter-Brick and colleagues are to be congratulated for so successfully conducting an intervention trial against all the odds. They provided eight weeks (two sessions per week) of group sessions to young people with profound stress experiences (versus a

waiting list control) that aimed to address symptoms of insecurity, distress, mental health difficulties, prosocial behaviour and post-traumatic stress. Here, the study was powered to detect more modest effect sizes ($d = .30$), included a longer-term follow-up of 7-14 months post-group treatment, and was delivered by local individuals working for existing organisations in the region. A major challenge for the research group was the high rate of attrition (43.5%).

The authors report small but significant treatment effects on 3 of the 5 self-report outcome measures, and sustained impact on human insecurity at follow-up. There was, not surprisingly, wide variation in outcome; at an individual level, statistical models suggested age and prior trauma exposure were important predictors of treatment success. But there were also differences between intervention cycles, with more muted effects in the second cycle of the intervention in which an RCT design was fully implemented and the baseline scores of individual participants were less severe. Variation in implementation, and access to a wider variety of humanitarian programmes were also considered to affect treatment success. To my mind, a more nuanced understanding of what individual and contextual factors contribute to treatment success or failure is more clinically relevant than a simple 'does this work?' question. Blanket approaches in such volatile settings are unlikely to fully address individual needs.

Finally, Burgoyne et al. investigate an adaptation of a tried and tested approach to developing children's oral language skills that uses parents, as opposed to clinicians or educators, as the agents of change. The treatment was much more intensive, designed to be delivered several times a week over a 30-week period, and including a follow-up assessment at 6 months that tested both core outcomes, and cascading impacts on early literacy. Parents received a modest amount of training (1.5 hours) to deliver the programme and thus, this has potential to be extremely cost-effective given the intended intensity.

The statistical approach to analysis employs latent variables, which have the advantage of exploring change in the underlying construct, unfettered by measurement error. This large study provided evidence of a moderate ($d = \sim .35$) treatment effect on language, that was maintained a

follow-up and accompanied by a similar degree of difference on early literacy measures. These differences were harder to detect on individual tests, where measurement error may obscure real change.

A challenge for this type of programme is that parents, on average, completed only 50% of the intended sessions. Issues of parent language and literacy (as the programme involved shared book reading) may limit the extent to which some families may access this intervention. Once again, participants were not clinically referred, with only 20% falling below commonly accepted cut-offs for language disorder. It is encouraging that these children make as much progress during the intervention period as more verbally able peers, but replication in clinical cohorts is needed.

Each of these studies represents an important advance in their respective fields and should influence choices about effective approaches to supporting children and young people faced with developmental challenges. Collectively, they also inform us about some common issues that require wider dialogue and consideration by those of us undertaking intervention research. First, testing developmental cascade models requires longitudinal data, and very few intervention studies include follow-up periods of sufficient duration to really test cascading effects. Second, statistical power is a critical issue that should concern us all. We know that under-powered studies can reveal positive findings that are likely to be statistical artefacts, rather than true effects (Button et al. 2013). There is a growing consensus that trials of psychological processes should be powered to detect medium sized effects, are practical in terms of the sample sizes required, and can provide meaningful results. In addition, even seemingly small changes can have large cascading impacts (Bornstein, 2016), but we need to explicitly model these using longitudinal trial designs. Third, we need to accept that many developmental concerns (such a language disorder) are incredibly stable. In these cases, change requires sustained input, especially as child behaviours and the environments children experience are often changing. What if our research question shifted from 'does this work' to 'what would it take to achieve the desired outcome?' With that in mind, intervention trials could usefully compare different implementation methods, varying dosage and agents of delivery. Technology

presents an important tool for meeting demand when resources are stretched, but there may be limited contexts in which this is applicable.

Finally, in many fields we see a move to provision of 'universal' services, designed to be delivered to all children with a view to reducing the numbers of children referred to tertiary services. While these efforts are laudable, there will always be a need for specialist interventions targeted at individuals who will not benefit from a universal approach. We therefore need to maintain flexibility in the system to deal with varying and on-going clinical need. One size does not fit all, and while this presents challenges, it is also what drives innovation and our understanding of developmental psychopathology.

Green, J. (2016). Editorial: Ingenious designs and causal inference in child psychology and psychiatry. *Journal of Child Psychology and Psychiatry*, 57, 549-551.