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COMMENTARY

Persisting without Evidence is a Problem: Suicide Prevention and Other Well-Intentioned Interventions

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The proposition that an intervention without evidence of effectiveness for a targeted health problem should be continued to be offered until such time that an intervention with evidence is available to replace it is problematic. The persistence of such intervention offerings has been identified as one type of research-practice gap (McLennan, Wathen, MacMillan, & Lavis, 2006). Nevertheless, this appears to be the recommended approach for suicide prevention suggested in an editorial in this journal in response to a review paper finding no compelling evidence of effectiveness for two marketed school-based suicide prevention programs, Signs of Suicide (SOS) and Yellow Ribbon (YR) (Ickowicz & Schachar, 2015; Wei, Kutcher, & LeBlanc, 2015). More precisely, the editorial concluded "rejection of the suicide prevention initiatives under scrutiny would only be justified when and if something better can be offered" (p.4, Ickowicz & Schachar, 2015). While it is understandable why one wants to do something in order to address an issue as serious as suicide, it may be counterproductive to continue supporting an intervention that has failed to demonstrate effectiveness.

The editorial authors raise the potential for adverse impacts from rejecting such interventions (Ickowicz & Schachar, 2015). One concern is that hidden within the group null effect are some children that do benefit from a given intervention. However, if this is the case, the overall null effect may then reflect that the few children who might actually benefit from the intervention are counterbalanced by children harmed by the intervention, assuming the study is adequately powered. It is certainly possible that underpowered studies or otherwise flawed studies may miss true positive effects, an example of a Type II error. However, a Type II error can also go the other way such that a true adverse effect is also missed.

It could be argued that adverse effects secondary to participation in prevention interventions are more problematic than adverse effects secondary to treatment of an existing disorder given that in the latter situation one might be more willing to take on greater risk from an intervention given the potential alleviation of suffering from a disorder whereas in a prevention intervention, harm may come to someone with no pre-existing condition. Despite this there may be, paradoxically, a lower evidence standard for offering a prevention intervention, perhaps driven by the belief that "it can't hurt" or it is "better than nothing." Certainly prevention studies can be very challenging to conduct given the typical need to look at small group differences among large samples followed over significant periods of time. Perhaps these demands lead to the apparent waiver for, or at least limited scrutiny of, evidence for prevention interventions prior to their dissemination.

Even if no direct harm comes from participation in such interventions, adverse consequences are still possible through opportunity costs. Opportunity cost, a fundamental economic concept, means that "by choosing to use available resources in one way, we forgo other opportunities to use these same resources" (p. 153; Gafni & Birch, 2003). There are real resource costs from the implementation of such interventions including the purchase of the intervention, training costs, ongoing personnel costs and student

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and staff time. The impacts of a given intervention have to be considered in light of how the consumed resources might otherwise have been (or could be) used and to what effect. A net null effect is not free.

The potential that the implementation of one intervention may block the adoption of another intervention is an additional concern or extended example of an opportunity cost. For example, if a school purchases, commits to, and implements an intervention like SOS or YR, it is quite likely that they will not also pursue other, potentially more effective, interventions for the same issue. There may even be resistance to change given the sunk costs and emotional investment in the existing intervention. This may be one of the mechanisms that has impeded movement forward in the adoption of effective school-based substance abuse prevention programs. A case in point is that the Drug Abuse Resistance Education (DARE) program is likely the most widely disseminated program of its kind in the world despite its weak and problematic evidence-base (Singh et al., 2011). It is proposed that a school that has adopted DARE is quite unlikely to also adopt a second substance-abuse prevention program (or replace DARE) despite existence of other programs with more evidence of effectiveness such as the Life Skills Training program (Botvin & Griffin, 2014). In this scenario, DARE essentially blocks the adoption of an evidence-based programs, as well as removes the "press" to do something else. The school can check the box that indicates they are doing something to prevent drug and alcohol abuse.

It might be countered that this scenario for substance abuse prevention is not applicable to suicide prevention given the absence of a suicide prevention intervention with robust evidence of effectiveness that ought to be adopted instead of SOS or YR. However, there is still the problem that the adoption of one intervention reduces or removes the "press" to do something else about the given health issue, suicide prevention in this case. When asked, the school will be able to say "yes" we have a program to prevent suicide and they may believe that they have already addressed the issue and, if unaware of the weak evidence base, not recognize the need to continue looking for an effective intervention. This is additionally problematic given that there does not appear to be any particularly strong requirement for those offering prevention interventions, in schools or otherwise, to provide evidence that the offered interventions have met a threshold of effectiveness and do not cause more harm than good.

Certainly this is not a call to halt efforts to address pressing mental health problems through prevention interventions, or for that matter treatment initiatives. In fact, acknowledging the lack of evidence of effectiveness of many prevention and treatment interventions in our field should spur on more attempts, not less attempts, to more rigorously address these deficits. However, there needs to be a much clearer process whereby novel interventions, be they prevention or treatment initiatives, are appropriately rolled out and that wide spread dissemination of interventions lacking in evidence should not be acceptable. The Wei et al.'s (2015) article includes a schematic to flag the notion that an intervention should have an acceptable level of evidence prior to dissemination. Similar arguments have been made by others. An article by Schoenwald and Hoagwood (2001) is informative for child mental health as the authors consider the complex considerations in rolling out an evidence-based child mental health intervention. In particular they articulate the need for transportability studies to bridge the steps between efficacy, effectiveness, and dissemination. Such a systematic and strategic approach would require a much different way of doing business than the current, seemingly haphazard, approach in Canada. Wei et al. (2015) propose a role for an independent/regulatory arm of Health Canada and a national suicide research center to potentially address some of these issues related to suicide prevention initiatives. A recent review of reviews on suicide prevention interventions for youth recommends a national research-to-practice network for Canada linking researchers and decision-makers as an additional approach to address current gaps (Bennett et al., 2015). Perhaps these strategies will help to raise the currently low-placed bar.

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