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REPLY



Response to comment by Kristjansson et al. on: 'Implementation of the Icelandic Prevention Model: a critical discussion of its worldwide transferability'

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We are delighted to see that our commentary resulted in a much-needed discussion about the strengths, narratives, and challenges of the Icelandic Prevention Model (IPM). We kindly respond to the developers' points of critique below.

Point 1–3

The authors claim that IPM is not an intervention. However, the promotion of IPM worldwide states that it – the 'model' – reduces substance use. There is no evidence that implementing a 'Process-Structure designed to facilitate long-term community empowerment and systems' (Kristjansson et al., 2021, p. 2) as such affects youth substance use. Bottom-up community involvement exists in many places and is likely to increase and ecologically sustain interventions (e.g. programmes, services, or policies).

The National Institute of Health (2020) defines an intervention as '... a manipulation of the subject or subject's environment for the purpose of modifying one or more health-related biomedical or behavioural processes and/or endpoints'. A causality model is indeed proposed by claiming that IPM reduced substance use in Iceland. IPM is hence considered either an intervention or a system/package of (varying and flexible) intervention components.

Subsequently, prevention researchers expect a mapping exercise of the behavioural change interventions applied with consideration of their dosage in the light of realistically uncovering the cogs and wheels of the IPM's behavioural mechanisms. This is in line with our primary comment that the lack of adequate IPM evaluation is linked to 'a broader discussion on how to evaluate environmental interventions as well as the theoretical backdrop of intervention models, such as insight into mechanisms of change (mediation) and differential effects of interventions (moderation)' (Koning et al., 2020). Precisely because IPM is a promising intervention, it is crucial to avoid the current massive black box effect wherein the intervention is a medley of amorphous ever-changing activities, while the exact composition of components and doses remains unclear.

Moreover, the authors repeatedly point at their recent articles that describe the 'guiding principles' and implementation steps while these studies do not measure the intended outcomes directly. Moreover, the guiding process principles do not inform the scientific community on concrete behavioural change techniques, their logic model, nor on causal mechanisms.

We were wrongly assuming that the implementation of the IPM involved communities making a choice from a set of flexible evidence-based interventions to be tailored to each situation (like CTC or PROSPER). But after assessing the developers' arguments, we realise now that the IPM seems to offer much less, since there are no 'specific interventions' and none of the components we identified are 'prescribed components of the model'. The authors claim in §1–3 that the known effective behavioural change mechanisms (curfew hours, leisure time vouchers, etc.) we had considered to be core elements contributing to the presumed effectiveness of the IPM are actually not part of the IPM 'because these are not prescribed and not applied everywhere'. This weakens the case for the IPM's evidence-base since these components (of which we assumed they had probably impacted the targeted factors and outcomes) cannot be attributed to the IPM.

The authors commentary to our initial in-depth concerns (Koning et al., 2020) appear to be limited to what is *not essential* to the IPM while the *essential* components remain unknown. Additionally, stating that the curfew and supervised leisure activities are not components of IPM, contradicts what is disseminated in IPM-interviews in lay media across the world (e.g. Cave, 2021; Young, 2017). Moreover, if these are not components, they should be factored in the evaluation design. The only evidence of the IPM's success in the Icelandic studies is based on these *non-mandatory* components. So, our core question still stands: how do we know which IPM components and active ingredients account for its success?

One could argue that a replicated version of IPM in another geographical and socio-political context that does not include significant large-scale public policy developments (such as a curfew or significant investment in adolescents

structured leisure activities) cannot be considered a full and comparable IPM replication. We want to encourage national policy makers and funders to think big and to include large scale policy development alongside more local data driven interventions. We agree that this is one of the core strengths of the prevention work undertaken in Iceland that should indeed be considered for replication.

Point 4

When causality is inferred by means of an association between (intervention) exposure and outcome, the evaluation design should warrant that any changes in the outcome are due to the exposure and not to other factors. Our in-depth analysis was in no sense based on cherry-picked contextual factors but quite contrarily on a non-limitative list of contextual confounders necessary to consider in environmental evaluation designs in terms of geography, demography, health and economy such as population density and rural/urban divide, alcohol affordability, its availability and socio-economic inequality. For example, since 60% of the Icelandic population live in the capital region and 37% in Reykjavik, it could be argued that the Reykjavik leisure vouchers and curfew hours cover a massive share of Iceland's youth and could therefore be an important contribution to the IPM's effect, even if they are not prescribed by IPM. Yet the authors do not seem inclined to explain or quantify how other communities have selected the different interventions, tailored them to environmental circumstances or what their influence was.

For registries of evidence-based interventions such as Xchange (EMCDDA, 2021) and others it is well accepted and standard to consider dissemination readiness (Buckley et al., 2020). Hence it is fair that we apply the same criteria to the IPM: interventions must work in different contexts in Europe to have a high rating. Interventions with robust behavioural change techniques, such as the Good Behaviour Game (GBG) have proven by means of rigorous evaluation that they work across different contexts (O'Donnell et al., 2016; Streimann et al., 2020). Interventions that have not been tested rigorously in varying contexts are considered insufficiently evidence-based to be disseminated in other contexts. We will come back to this in our response to point 7.

After years of implementing the IPM in many countries, no effectiveness studies outside Iceland have been published, while the IPM is already being marketed as an evidence-based intervention to a plethora of organisations and in various countries.

Point 7

Many publications confirm substantial declines in adolescent alcohol use, drug use and other risk behaviour across high income countries since the late 1990s (see e.g. Pape et al., 2018). Local and national prevention activities are indeed likely to have had an influence on these trends. However, lifestyle changes and in particular the reduced time adolescents spend in unsupervised social activities with friends (De

Looze et al., 2019; Halkjelsvik et al., 2020) together with shifts in parenting practices (Raitasalo et al., 2021) are likely to be key drivers of this reduction.

Given the range of social changes enacted in Iceland (adolescent night-time curfew, investment in structured leisure activities, increased parental engagement) the IPM may well have amplified these general trends in declining consumption (Vashishtha et al., 2020). However, we are not aware of any empirical research that unpacks the added value of the IPM beyond these well documented declines in adolescent alcohol and drug use experienced in other jurisdictions.

As a result, any consideration of the transportation of the Icelandic model into new geographical contexts must include a discussion of realistic future impact (potential added value), particularly in middle and low income countries where less is known about longer term trends in adolescent consumption (see Room et al., 2020) or in high income countries where consumption behaviours may be stabilising.

In conclusion

Environmental prevention as applied in the IPM is a new promising concept to be advocated for. It might lose its overall credibility, though, if too far-reaching claims concerning the transferability of the IPM are made, without robust findings from evaluations in other countries. This discussion could be an opportunity to advance the knowledge base on how and when environmental intervention components work best. Moreover, this exchange of arguments is very important because the IPM is often the subject of conferences addressed to policymakers, but has to our knowledge not been presented for scrutinising debate at the relevant scientific conferences in the prevention field (such as US-SPR, EUSPR, EUPHA, etc).

We conclude that the authors did not provide new or more convincing arguments against our two main observations. First, there is no robust evidence that, let alone how, the Icelandic Prevention Model contributed to the steep decrease in adolescent substance use in Iceland. Second, no evaluation study of convincing quality is available that confirms the effect on adolescent substance use in other contexts after years of transferring the IPM to several countries, notwithstanding the often positive experiences with the implementation process.

It is not our intention to invalidate the IPM concept, rather we aim at generating reflection on its widespread dissemination in countries with other contexts compared to Iceland. We strongly suggest that the IPM implementation initiatives outside Iceland be accompanied by robust evaluation studies – be it by means of RCTS or quasi experimental designs – thus allowing for causal inference. Then, informed dissemination of the IPM as a prevention product can follow.

Disclosure statement

No potential conflict of interest was reported by the author(s).

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