ORIGINAL ARTICLE

Developmental Crime Prevention in the Twenty-first Century: Generating Better Evidence Embedded in Large-scale Delivery Systems

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Abstract

In recent decades, a strong foundation for developmental crime prevention has been constructed by a small band of criminologists, including the authors in this special issue. However, the evidence base is incomplete and needs strengthening. Moreover, the "flagship programs" that largely constitute the evidence base simply establish "proof of concept" rather than providing evidence readily translatable into policies for sustainable, population-wide crime prevention. I propose the development of a research agenda for both type 1 translation (applying basic scientific research to the development and testing of new prevention initiatives) and type 2 translation (investigating the complex processes and systems through which evidence-based practices are adopted, implemented, and sustained on a large scale). The agenda could include (a) experiments that integrate universal, selective, and indicated interventions; (b) study of the crime prevention effects of "services as usual" because routine services can be the pathway to scale; (c) research in partnership with practitioners and communities to construct risk and protective factor profiles to guide prevention planning, implementation, and evaluation; (d) type 1 research in areas where better evidence is needed; and (e) better integrating the timing of interventions with knowledge about the development of criminal propensity. I argue that type 2 translation could be viewed as the new frontier for developmental crime prevention, requiring a shift in focus from programs to systems and new research methodologies such as those developed in the CREATE Project, which has built electronic and human infrastructure to help bridge the gap between developmental prevention research and routine services.

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The articles in this special issue of the *Journal of Developmental and Life-Course Criminology* provide valuable insights into the state of research on developmental crime prevention. Five papers cannot of course tell us everything that there is to be known about the field, but there is more than enough depth and diversity in the many research studies discussed in these papers to constitute a sound foundation for reflection about what we know and what we still need to find out about what works to reduce crime.

In their paper, Welsh and Tremblay (2020, p. 2) cite Tonry and Farrington's claim 25 years ago that developmental crime prevention could be characterized as "the new frontier of crime prevention efforts," and I fully endorse their positive assessment of the field's achievements in the quarter century since Tonry and Farrington wrote those words. The papers in this special issue strengthen the case for developmental crime prevention that has been building for at least 40 years but also spell out many of the theoretical, methodological, and implementation standards that must be met to improve the quality of the evidence base and extend its reach. We continue to need more and better evidence, including replications and extensions of historically important preventive innovations. There are many crime prevention challenges for which the evidence base is fragmentary or incomplete, even for phenomena that we know from developmental and life-course criminology are of fundamental importance. An excellent example is provided by Abigail Fagan (2020) with respect to how to target change in peer risk and protective factors without triggering iatrogenic effects, where she shows that there is a need for much more research.

Even more vexing is the challenge of knowledge translation for policy influence that is posed, but not examined in detail, by Brandon Welsh and Richard Trembley (2020). My judgment is that even with improvements in quality and scope, most of the work in the developmental crime prevention field, and in the field of early prevention more broadly, essentially establishes the case for developmental prevention as a feasible strategy rather than providing the kind of evidence that can be readily translated into practical and politically defensible policies. In the language of prevention science, most research in the developmental crime prevention field and in cognate areas of early prevention could be classified as "type 1 translation." Type 1 translation applies the results of basic scientific research to the development and testing of new prevention initiatives (usually through randomized controlled trials), while type 2 translation "… investigates the complex processes and systems through which evidence-based interventions are adopted, implemented, and sustained on a large scale …" (Spoth et al., 2013, p. 319).

The key goal of type 1 translation is to establish the efficacy of an innovation as "proof of concept" that the new approach can work, at least under carefully controlled conditions (Shonkoff & Fisher, 2013, p. 1637). Type 2 translation, by contrast, is focused on the evidence-based transformation of health, education, and welfare service systems to achieve sustained impacts on child and youth well-being, at the level of the whole population (Homel et al., 2017). Type 2 translation, with its focus on achieving scale and sustainment in preventive policies and practices has become, in my view, the new frontier for developmental prevention research.

One key strategy for achieving impact through type 2 translation is to draw heavily on the type 1 evidence base by incorporating a large number of the most

suitable and best-supported evidence-based interventions (EBIs) into the routine practices of service systems. Fagan et al. (2019a) present some persuasive examples of how the implementation of EBIs at scale in the USA has been followed by population-level reductions in behavioral health problems, including in Pennsylvania where over 200 replications of EBIs (particularly through *Communities That Care*: Fagan et al., 2019b) were followed by "significant reductions in juvenile justice and child welfare system utilization and lower prevalence rates of delinquency and out-of-home placement" (p. 1148).

There are strategies for type 2 translation that do not focus solely on the proliferation and embedding of EBIs, such as "the scale-up of the prevention principles and practices that are most commonly used in systems" (Fagan et al., 2019a, p. 1149). However, the literature on these "system transformation" approaches is in an early stage of development, with not much known about effective strategies (Ghate, 2016). This is one important reason why my colleagues and I in Australia began the CREATE Project, described further below.

My aim in this paper is to draw on the papers in this issue to highlight ways in which the evidence base for type 1 translation might be strengthened, while "looking ahead" to some of the challenges of type 2 translation. Although the two kinds of translational research overlap and interact in important ways, in the next section I discuss themes from the papers mostly relevant to type 1 translation. These include the perennial challenge of deciding who to target (the strengths and weaknesses of universal, selective, and indicated approaches); time and timing (developing age-graded interventions at optimal times, including the use of "boosters"); the use of data on children's needs (including risk and protective factors) to guide prevention planning; and the critical importance of understanding how peer processes operate in order to better plan and evaluate preventive interventions.

Following these reflections, I expand the discussion of targeted versus universal approaches and the use of risk and protective factors for guiding interventions, emphasizing that we need to generate evidence that is scientifically sound but is also persuasive and useful for communities (the consumers of services), as well as for frontline professionals and policy makers (the deliverers and shapers of services). Finally, I reflect on "the control group problem" discussed briefly by Beelman and Lösel (2020), arguing that the recent failures of randomized experiments outside the USA to find flagship or brand name programs effective in comparison with services as usual are actually an opportunity to focus far more intensively on the conditions under which routine services operate as effective crime prevention delivery systems.

Where appropriate, I draw throughout on insights from my work and that of colleagues in the *Creating Pathways to Child Wellbeing* research program (the CREATE Project), through which we have sought to build human and electronic infrastructure to help bridge the gap between prevention science (including developmental and life-course criminology research) and the education and community service systems in the Australian states of New South Wales and Queensland (Branch et al., 2019; Homel et al., 2015, 2020). Our intent was, in essence, to make a useful contribution to the strengthening of "the prevention principles and practices" that are embodied in these systems.

I conclude with some brief reflections on how the field of developmental and life-course criminology could engage with the prevention science literature with a view to developing an agenda for type 1 and type 2 developmental crime prevention research internationally over the next 25 years.

Reflections on the Papers

Beelman and Lösel (2020) present an update of their 2003 meta-analysis of the effects of child social skills training on aggression, delinquency, and antisocial outcomes, confirming their earlier findings of positive but small effects, but mostly in the short term. Whether indeed the results should be described as "small" is a question to which I return, but their paper is important if for no other reason than their focus on behavioral outcomes confirms the potential value of child social skills training (or, better, social *competency* training) specifically for crime prevention. However, the paucity of evidence for long-term impact leads them to suggest the need for "booster sessions or even a repeated application of child social skills training matched with the different social developmental tasks at different ages or developmental periods."

The social development model (SDM) discussed in detail by Catalano et al. (2020), in the context of the longitudinal Seattle Social Development Project (SSDP) in which the Raising Healthy Children (RHC) intervention was embedded, "specifies submodels corresponding to developmental phases defined by changes in the influence of major socializing units from infancy and early childhood … [through] … later adolescence …" (p. 4). The SDM therefore provides an excellent theoretical framework for developing age-graded child social competency training along the lines suggested by Beelman and Lösel, something that Catalano and colleagues actually implemented as a component of the RHC intervention through the child social and emotional skill development program. The full intervention group comprised 156 children "who were in classrooms that offered all RHC intervention components for teachers, parents, and children for at least one semester in grades 1 through 4 and at least one semester during grades 5 and 6" (p. 5).

As the authors describe in detail, the effects on children, teachers, and families of this pioneering theory-driven intervention have been assessed at multiple time points from age 7 through age 39, and now into the next generation, revealing an impressive array of positive outcomes for the full intervention group and their children, compared with controls. There were few significant enduring effects for a late intervention group for whom the three RHC components were implemented only in grades 5 or 6, or for a grade 5–6 parent training–only group, supporting the general finding in the literature that multimodal designs implemented as early as possible in the developmental pathway, preferably with an age-appropriate booster as in the RHC full intervention, generally produce better outcomes than single-strand interventions or those implemented later in the pathway (Developmental Crime Prevention Consortium, 1999).

Among the many positive outcomes for the full intervention group, at age 18 fewer youth reported violent delinquent acts and at age 21 they were significantly

less likely to have sold drugs in the past year and to have been charged with an offence (criminal or noncriminal) in their lifetime. However, these crime effects did not persist from age 27 onwards, suggesting that the RHC intervention may have helped reduce adolescent-limited offending (a very valuable outcome) but not life-course persistent offending. If this hypothesis has any validity, researchers seeking to build on the SSDP/RHC approach should consider including an indicated component for young children exhibiting signs of conduct disorder within a universal multimodal design, as recommended by Beelman and Lösel and implemented by Bierman, Coie, and colleagues from the Conduct Problems Research Group (2019) in the Fast Track Program. Fast Track was a 10-year multilevel preventive intervention that integrated a social-emotional learning program for all students (PATHS, Promoting Alternative Thinking Strategies) with a set of indicated interventions for children showing high rates of aggression at school entry. Project components included social-behavioral support, reading intervention, parenting classes, and home visiting to support healthy family development. At age 25, analyses revealed, among an array of positive findings for the aggressive children who screened into the program 20 years earlier, a 31% reduction in violent crime and a 35% reduction in crimes involving illegal drugs.

An important influence on the development of Fast Track was the Montréal Prevention Experiment (McCord et al., 1994) discussed by Welsh and Tremblay (2020) under the heading of the Montréal-Longitudinal Experimental Study. Like the SSDP, this prevention experiment was integrated within a longitudinal study of 1031 kindergarten boys from 53 schools in low socioeconomic areas of Montréal. The experiment, in contrast to the universal approach of the RHC, focused on 250 of the most aggressive, hyperactive, and oppositional kindergarten boys and (like RHC) simultaneously targeted peers, parents, and teachers through social skills training, parent training, and support for teachers. One of the main aims of the Welsh and Tremblay paper is to document the translation of knowledge within the developmental crime prevention field and in that context they acknowledge the significant influence on the design of the Montréal study of Joan McCord's re-analysis of data from the Cambridge-Somerville Youth Study that identified the iatrogenic effects of "deviancy training" that can easily occur when antisocial children are grouped together. To avoid this risk, in the Montréal experiment, disruptive boys were mixed with highly prosocial boys in the ratio of 1:3, an arrangement that appears to have led to lasting prosocial friendships that may be "an important causal mechanism in the effectiveness of the preventive intervention." The 2-year experiment led, by age 24, to "a higher rate of high school graduation and lower rates of criminal convictions."

Clearly, indicated interventions with aggressive or conduct disordered children can be extremely effective, a replicated finding that has important policy implications to which I will return. Such interventions are, needless to say, very challenging to implement (Bierman et al., 2019), and in this regard the paper by Koegl et al. (2019) offers some important resources for both researchers and practitioners. The paper focuses on the Early Assessment Risk Lists for Boys (EARL-20B) and Girls (EARL-21G) which measure risk for future antisocial behavior in children aged 6–11 who have already been carefully screened and diagnosed as conduct-disordered. The main aim of the paper is to assess the long-term power of the EARLs

to accurately predict criminal convictions from age 12 to 21, and results showed that items that were geared toward measuring continuity of antisocial behavior over time were consistently strong predictors. Importantly, the EARLs were constructed within the Structured Professional Judgment (SPJ) paradigm which prioritizes prevention and treatment over the sole task of prediction, making the scales very "practitioner-friendly." They are useful for children with a wide range of behavior problems, not necessarily those at the most severe end of the spectrum, and so individual clients with a specific constellation of needs can be matched to individual treatment program elements to optimize outcomes. In short, the EARLs are a powerful "real-world" tool for guiding decisions that can optimize treatment outcomes for very challenging children.

The role of peers in facilitating or inhibiting child and youth antisocial or criminal behaviors is a theme that runs through all of the papers in this issue, at least as a background issue, but is the explicit focus of Fagan's (2020) review of the developmental prevention literature. For both males and females, exposure to antisocial peers is the biggest single risk factor in adolescence so it is clearly very important that we know what strategies are available that not only work to reduce crime and substance abuse but accomplish this by targeting change in peer risk and protective factors. Surprisingly, of the 27 programs that Fagan identified with evidence of effectiveness, only six tested for mediation of targeted peer factors, but all of these supported the program's theory of change in that improvements in the peer factors were associated with reductions in substance use and delinquency. Fagan emphasizes the importance of criminological theories (especially social learning theory and differential association) in both the design and evaluation of programs, a recommendation that perhaps needs to be held in tension with the finding of Beelman and Lösel that for the most recent generation of child social skills programs, theoretical orientation was less consistently associated with success than implementation quality and staff–client relationships. Overall, there is cause for optimism from Fagan's review, with successful programs encompassing individual, classroom, community, family, and multicomponent interventions that afford communities a choice in what strategies and programs to adopt.

Better Evidence for Better Policies

In this section, my aim is to build on the insights from the five papers in this issue and from the broader developmental crime prevention literature to suggest some directions for future research, including type 2 translational research.

Balancing Universal, Selective, and Indicated Interventions Beelman and Lösel (2020) in their meta-analysis of child social skills training caution that overall effects sizes on aggressive and delinquent behaviors were small at $d = 0.25$, but that indicated programs at $d = 0.49$ had a much larger impact. Universal and selective programs had similar effects sizes at around 0.10. The value of programs targeting at-risk, aggressive, or conduct-disordered children at an early age is strongly supported by the long-term outcomes of the Montréal Prevention Experiment (Welsh &

Tremblay, 2020), the Fast Track Program (Bierman et al., 2019), and the Stop Now and Plan (SNAP) program described by Koegl et al. (2019), and is possibly implied by the failure to find long-term crime prevention effects in the Seattle study (Catalano et al., 2020).

Recently, however, Greenberg and Abenavoli (2017, p. 41) have challenged the use of Cohen's *d* as an arbiter of the value of universal versus selective or indicated interventions, arguing that "… universal interventions targeting entire populations … have been undervalued to date by policymakers, in part because existing research and benchmarks of effectiveness prioritize individual-level impacts rather than population-level impacts and main effects over subgroup effects." Writing within an interdisciplinary framework and drawing on recent advances in public health and prevention science, these authors recapitulate the many advantages of universal approaches while critiquing the ways in which these advantages are often undervalued. The nub of the problem from a metrics perspective is that Cohen's *d* and other standardized metrics are quite sensitive to the base rate of a problem like crime, which typically varies greatly across population subgroups. The practical, social policy, and public health significance of effects are a product of the "population reach of the intervention, prevalence, and effect size" (p. 50) and require in most circumstances the use of risk reduction metrics. Examples are the relative or absolute reductions in the rates of new drug users as in the PROSPER community-based drug prevention experiment (Spoth et al., 2011).

Heterogeneity in outcomes was noted by Beelman and Lösel in their meta-analysis and is a feature of all the flagship or lighthouse programs discussed by Shonkoff and Fisher (2013) and the Australian epidemiologist John Lynch (2017) in his review of the impact of flagship programs in Australia. Given the natural heterogeneity in universal populations, Greenberg and Abenavoli (2017) suggest that it may be problematic to focus on main effects in evaluations since universal interventions will have differential and possibly clinically important subgroup effects. However, as both Lynch and Greenberg and Abenavoli emphasize, this means that universal experiments need to be suitably powered to detect predicted or unexpected outcomes in population subgroups.

Greenberg and Abenavoli (2017) suggest one practical strategy that could be adopted more widely in developmental crime prevention experiments, which is to integrate universal, selective, or indicated programs into a unified approach to prevention. The Fast Track experiment for example integrated a universal social-emotional learning intervention in four communities with a set of indicated interventions for aggressive young children. The indicated approach allowed a wide range of individualised services to be implemented with appropriate dosage or intensity levels, tailored to the needs of each child and their family. The non-stigmatizing framework of the school-wide intervention helped to normalize these intensive services for aggressive children and encourage the participation of a sufficiently large sample of families with troubled children to allow the well-powered exploration of a wide range of long-term outcomes for this minority.

Another example of an integrated strategy, one that was adopted for the CREATE Project, is to implement interventions, within selected socially disadvantaged, high-crime communities, that are universally available (if not universally taken up)

through partnerships of service providers and other community stakeholders, notably school personnel. Our framework was the Australian Government's longstanding Communities for Children (CfC) program which funds a wide variety of services for children and families in 52 disadvantaged communities throughout Australia (Homel et al., 2015). Our aim was to introduce new electronic and human infrastructure, or strengthen existing infrastructure, to better support the positive development of children aged 5–12 years and reduce the prevalence and impact of risk factors like poor social-emotional functioning (Branch et al., 2019). We wanted to extend the reach of science-based approaches through medium-scale government programs like CfC by designing practitioner-friendly measurement systems that are integrated within a comprehensive system of on-line resources in order to facilitate the adoption of evidence-based interventions that target the locally measured needs of children and their parents. A key example of a measurement system is Rumble's Quest, an innovative computer game that can be used to generate valid and reliable school-wide data on aspects of social-emotional well-being such as behavior and emotion regulation (Day et al., 2019). Critically, Rumble's Quest is cheap to administer and is scalable; both important attributes for type 2 translation.

Measuring Risk and Protective Factors to Help Decide Who and What to Target As researchers, we tend to take the measurement of risk and protective factors for granted, so ubiquitous is their presence in developmental criminology research. Measures of risk and protection are also vital for prevention research and practice. A straightforward and deceptively simple definition of developmental crime prevention is that it involves the identification of key risk and protective factors for offending and the implementation of prevention methods designed to counteract the effects of risk or enhance the effects of protective processes (Farrington, 2007, p. 606).

A major strength of the paper by Koegl et al. (2019) is that it describes a valid and reliable tool for measuring and acting on child risk and protective factors in a very challenging population. The development of the EARLs through a collaboration with practitioners, together with their proven value as both predictive and evaluation measures, their track record of use with Indigenous and non-Indigenous children with a wide range of behavior problems, but above all their capacity to provide a profile of risk factors at the individual child level, means that the EARLs can play the same role in indicated interventions for behaviorally challenging children as the CTC Youth Survey does at the community level (Fagan et al., 2019b).

If the EARLs can be augmented through the addition of more protective factors in the ways indicated by the authors, then these structured risk assessment tools could more easily be incorporated within the strength-based practice frameworks favored by practitioners. This is important because there are many pitfalls in working with high-risk children and their families, especially if they belong to First Nations communities. In Australia and other postcolonial societies such as Canada, there is a rapidly changing political landscape where First Nations communities and organizations are asserting the right to take back control not only of decisions about what is done in their communities and how services are designed and implemented but are also (rightly) demanding Indigenous data sovereignty and control over all aspects of research (Australian Governments and the Coalition of Aboriginal and

Torres Strait Islander Peak Organisations, 2020; Walter et al., 2020). In this context, the authors' humane motivations and highly rational statement that the EARLs "… increase the scope of opportunity for professionals to intervene early in the lives of at-risk children, years before they are subject to formal criminal sanctions …" would need careful explanation and translation through respectful dialogue. First Nations communities have a very long and tragic history of "professionals" intervening early and often in the lives of their children (Homel et al., 1999).

It is disturbing that, at least in Australia and probably in many developed countries, risk and protective factors are seldom measured in routine services funded by government (not to mention other critical variables such as child outcomes and the duration, intensity, and reach of services: Homel et al., 2020). Even when some of these data are collected, in our experience, policy makers and practitioners often need specialized support to appreciate the nature and critical importance of risk and protective factors at the individual, group, or community levels, as opposed to more general indicators of child or youth needs to which they generally have some access. The actual construction of a risk and protective factor profile for a local population is best done as in the CTC model through a standardized methodology like the Youth Survey (Arthur et al., 2002) that permits benchmarking with other communities and with population norms. The incorporation of this general methodology as *Phase 3, Develop a Profile* in the Communities That Care Prevention System is one important reason for the great success of this model (Fagan et al., 2019b).

The CREATE Project provides another example of how profiles of child risk and protective factors can be constructed in a form that is useful to community coalitions faced with making decisions about what EBIs are best fitted to the needs of children in their community. Our approach differs from CTC in that we are focused on capturing the voices of young primary-aged children (6–12 years) through Rumble's Quest; we are working through an existing government program, Communities for Children, to help realize our objective of strengthening established prevention delivery systems; we are using a standardized geographical classification system to define "communities" as towns or suburbs with (on average) about 10,000 residents (Australian Bureau of Statistics, 2016); and we are exploring ways of combining Rumble's Quest data with government indicators of child needs (e.g. rates of family violence) and with community-level child scores from the Australian Early Development Census (https://www.aedc.gov.au/). Consistent with implementations of CTC, preliminary analyses reveal marked variations in child risk and protective profiles between equally socially disadvantaged or ethnically diverse communities, with consequent diversity in priority child needs and the EBIs that should be implemented to target them. Our aim, as with all CREATE tools and methodologies, is to make it easier to achieve scale by working through existing government prevention delivery and administrative systems.

The Control Group Problem: Understanding Better the Effectiveness of Routine Services Beelman and Lösel (2020) suggest that one reason why effects sizes for social skills programs have not risen over the past 15 years is that routine services in child and family care in many countries have steadily improved, probably about as much as prevention programs have improved. In his discussion of this issue in Farrington

et al. (2019), Lösel takes as a case study the contradictory results of evaluations of multisystemic therapy (MST) in a number of countries, citing the conclusion of some researchers that the content of "treatment as usual" may be more important than the content of the MST treatment. Recent evaluations in the UK which have found mostly non-significant or even iatrogenic effects of MST (Fonagy et al., 2018) and other brand name programs such as Functional Family Therapy (Humayun et al., 2017) and Nurse Family Partnerships (Robling et al., 2016) reinforce the view that much closer attention should be paid to the quality and impact of routine services to better understand whether foreign flagship or brand name programs might be helpful. Fonagy and colleagues conclude that their findings that MST is not superior to management as usual "reflect the effectiveness of mental health, youth offending, and social care services in England … in reducing the risk of crime and protecting young people and society, at least when under the scrutiny of a randomised controlled trial" (p. 13).

One interpretation of these findings is that perhaps the control group "problem" is not a problem at all and that we should be celebrating evidence that routine services can produce results that are at least as good as the flagship programs which have inspired the steady growth and impact of the field of developmental crime prevention internationally. However, this evidence is not a cause for complacency! We know that there is great variability within and between jurisdictions in the quality of social, health, and educational services, and that when it comes to preventing crime and promoting child flourishing "magic bullets are in short supply" (Lynch, 2017, p. 15; Shonkoff & Fisher, 2013). In fact, we know very little about the crime prevention effects of the vast bulk of the services that are delivered every day, and in this regard the recent randomized controlled trials have performed a great service.

We need more comparative experiments, and where routine services are shown to fall short, we should either implement the brand name program more broadly or (maybe better) improve usual services in light of the prevention principles and practices that have been revealed as absent or not working well. To do this, we will need well-powered experiments of the kind recommended by Fagan (2020) and exemplified by the Raising Healthy Children project in Seattle (Catalano et al., 2020) and the Montreal Prevention Experiment (Welsh & Tremblay, 2020). New experiments should focus on the theory-driven processes through which crime reductions are hypothesized to take place, collecting comprehensive qualitative and quantitative data and conducting mediation and moderation analyses to investigate the role of key risk and protective factors, especially in subgroups. In light of the mixed results when brand name programs are compared with services as usual in new jurisdictions, a great deal of attention will need to be paid to the selection, characteristics, and operations of comparison groups.

Toward an Agenda for Developmental Crime Prevention Research

We have much to celebrate in the field of developmental and life-course crime prevention, but there are no grounds for complacency. A comparatively small number of criminologists over the past 40 years, joined by colleagues in cognate disciplines, have established an impressive foundation for humane, evidence-based, data-driven strategies for the prevention of crime, violence, and substance abuse—strategies that now constitute a viable, cost-effective, and vastly superior alternative to the "get tough rhetoric" and "correctional quackery" that characterize the "mean season of crime control" excoriated so eloquently by Francis Cullen in Fagan et al. (2019b, p. xi). We need a new generation of criminologists to augment the ranks of these criminologist-prevention scientists because as the papers in this special issue have demonstrated there is a great deal of work to be done to extend and strengthen the evidence base. Even studies like the Seattle Social Development Project, which appears as a truly dazzling achievement from my Antipodean base where we struggle to fund even the smallest of science-based prevention projects, need to be replicated in many countries and in many cultural settings, probably with theory-driven and pragmatic adaptations so that the evidence base can better serve the purposes of both knowledge translation and policy relevance.

Welsh and Tremblay (2020) have opened a fascinating window into how in the past findings and ideas have spread from researcher to researcher and from study to study, with great benefit to the field. However, my judgment is that we are now entering an era when we need to establish a more explicit and systematic research agenda that is cross-disciplinary and cross-cultural, in order to attract major funding to address the big developmental crime prevention challenges. This research agenda will need to encompass both type 1 and type 2 translation, and I have indicated in this paper a few of the directions that could be taken, drawing on the papers in this special issue.

These directions include (a) more experiments that integrate universal, selective, and indicated interventions to give effect to the principles of "proportionate universalism" within which the criminogenic and health needs of the many at low risk and the few at high risk can be adequately addressed (Carey et al. 2015); (b) much more attention to "services as usual" delivered through the education, welfare, and health systems, not only for the selection of control groups to test brand name or other preventive innovations but because these services are the pathways to scale and it is therefore critically important to find out more about their crime prevention effects; (c) research in partnership with frontline professionals and community residents that leads to the construction of risk and protective factor profiles that can serve as a robust guide to prevention planning, implementation, and evaluation; (d) more type 1 translational research in areas where there are large gaps in the evidence base, especially how the role of peers in encouraging or discouraging crime can be addressed; and (e) more theory-driven attention to the fundamental issues of time and timing, better integrating the timing and nature of preventive interventions with what we know about the development of "criminal propensity" from

longitudinal studies tracking developmental pathways in interaction with changing life circumstances.

I have proposed that type 2 translation is the new frontier for developmental crime prevention. Developing an agenda for type 2 translation will require new research methods that focus less on programs and more on systems. The CREATE Project has for example attempted to build infrastructure for an established government delivery system through supports for data monitoring, evaluation capacity, and capacity to implement EBIs, in line with the findings of Fagan et al. (2019a) that these capabilities are important for scaling up EBIs. Ghate (2016, p. 812), in a recent paper on how to sustain real-world effectiveness in services for children and families, has stated the challenges of system reform in a complementary form: "recognizing invisible system infrastructure, co-construction involving active collaboration between stakeholders, and attention to active implementation supports for providers beyond education and training."

One way of proceeding with the development of a type 2 translation research agenda would be to engage systematically with the extensive work already done through the Society for Prevention Research, adapting the recommendations and insights of the various task groups to the specific problems of crime and violence prevention. It is noteworthy that three of the recent task groups were led or were actively supported by criminologists or by social scientists who have made major contributions to criminology or drug prevention: Spoth et al. (2013), Gottfredson et al. (2015), and Fagan et al. (2019a). Now is the time to build on this work.

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