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**Positive youth development programmes to reduce substance use in young people:  
systematic review**

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**Short title:** Positive youth development for substance use prevention

**Background.** Substance use has detrimental short-term and long-term consequences for young people. Positive youth development (PYD) interventions, which favour promotion of positive assets over traditional risk reduction, have received attention recently as a possible intervention to prevent adolescent substance use. We aimed to synthesise the evidence on PYD interventions for reduction in substance use in young people.

**Methods.** We searched 21 databases, including MEDLINE, PsycINFO, CINAHL and CENTRAL, and hand-searched key journals and websites. We included studies with more than half of participants ages 11-18 years where interventions meeting a pre-specified definition of PYD were delivered in community settings outside of normal school hours and did not target parents or young people with pre-defined conditions. Two reviewers screened records, assessed full-text studies for inclusion, and extracted data. A modified Cochrane risk of bias tool was used for quality assessment.

**Results.** Ten studies reported in 13 reports were included in our synthesis. PYD interventions did not have an effect of statistical or public health significance on any substance use, illicit drug use or alcohol outcomes in young people.

**Conclusions.** Interventions were diverse in content and delivery. Our review suggests that existing PYD interventions subject to evaluation do not appear to have produced reductions in substance use of public health significance. However, these interventions may not be the best exemplars of a PYD approach. Therefore, our findings should not be taken as evidence for the ineffectiveness of PYD as a theory of change for reducing substance use among young people. Additional rigorous evaluation of PYD interventions is key before further investment. Evaluations were of highly variable quality. Though searches were extensive, we were unable to test for publication bias.

**Keywords:** positive youth development; drug prevention; systematic review

## **Introduction**

Youth substance use continues to pose a threat to public health. These threats are both immediate and longer-term. For example, adolescent use of cannabis is associated in the short term with increased risky sexual behaviour and injury (Volkow, Baler, Compton, & Weiss, 2014). Substance use initiation during adolescence leads to later-life chronic disease, including dependence, and is costly to healthcare systems (DWP, 2012; Viner et al., 2012). In a nationally representative sample of United States adolescents from 2011, 22% of adolescents in the last year of secondary school and 15% of adolescents in the second year of high school reported binge drinking in the last month (Patrick & Schulenberg, 2014). Another nationally representative sample showed that adolescents in the second year of high school reported last-year illicit drug use of 26% (Conway et al., 2013). Moreover, adolescent substance use initiation is associated with social disadvantage across studies (Galea, Nandi, & Vlahov, 2004), raising a key equity consideration.

A class of interventions that may have the potential to prevent initiation of substance use is positive youth development (PYD). Specifically for this review, we defined PYD from research evidence in the United States (Roth & Brooks-Gunn, 1998) and policy and practice sources in the United Kingdom (NYA, 2007) as voluntary education outside school hours aiming to promote generalised (beyond health) and positive (beyond avoiding risk) development of assets (bonding, resilience, social, emotional, cognitive, behaviour or moral competence, self-determination, spirituality, self-efficacy, clear and positive identity, belief in the future, recognition for positive behaviour, opportunities for pro-social involvement and/or pro-social norms), which addresses multiple assets or a single asset deployed in multiple domains (for example, family, school or neighbourhood). Formal usage statistics do not exist, but PYD interventions have recently been the focus of policy interest in the United Kingdom, including multi-million pound investments by the UK government in youth work, youth

centres and other related projects. PYD also features prominently in key UK government agendas, including the Department for Education (Department for Education, 2011), the Department of Health (Department of Health, 2010), the London mayor's office (Mayor's Fund for London, 2011), and the devolved governments of Scotland (Scottish Government, 2009) and Wales (Welsh Assembly Government, 2007).

The evidence base for PYD as regards substance use outcomes is unclear. Though other systematic reviews (Gavin, Catalano, David-Ferdon, Gloppen, & Markham, 2010; Harden et al., 2006) have found positive effects of PYD on sexual health outcomes, substance use specifically has not been addressed in a systematic review. Two existing reviews (Catalano, Berglund, Ryan, Lonczak, & Hawkins, 2002; Roth & Brooks-Gunn, 2003) addressing PYD and substance use are out of date, though Catalano and colleagues (2002) suggested that PYD could be effective for reducing substance use. A more recent review focused only on school extra-curricular interventions reported a significant effect in reducing problem behaviours, but a non-significant effect for drug use (Durlak, Weissberg, & Pachan, 2010). In the face of the challenges to the health and development of young people that substance use presents and the ongoing investment in these programmes, a systematic review of outcome evaluations of PYD interventions is timely and necessary to guide policymaking and set the agenda for future research.

## **Methods**

We conducted this systematic review as part of a larger evidence synthesis project addressing theory, process evaluations and outcome evaluations of PYD interventions. We determined our methods *a priori* and published them in a protocol (Bonell, Thomas, Campbell, Murphy, & Fletcher, 2013). We included studies in the overall review if they: 1) were published from 1985 onwards, which is when PYD interventions were first developed (Gavin et al., 2010); 2) were in English; 3) focused on youth age 11-18 years (i.e. more than

half of youth included were 11-18 years); 4) focused on PYD as defined above; 5) reported a theory of change, process evaluation or outcome evaluation that was experimental (i.e. randomised) or quasi-experimental (i.e. non-randomised, but employing a prospective comparison group); and 6) focused on prevention of smoking tobacco, alcohol consumption, drug use or violence. In the systematic review reported here, we examine and synthesise only experimental or quasi-experimental outcome evaluations that included substance use outcomes (violence outcomes are reported elsewhere). We applied the above definition of PYD and included interventions in this evidence synthesis project meeting the definition above if either at least one asset characteristic of PYD applied to multiple domains (e.g. family, school, or community), or multiple assets applied to one domain.

We searched 21 bibliographic databases on 7 November 2013, in addition to a free-text search of websites (undertaken between 7 and 16 January 2014) and hand-search of journals (see Supplementary File 1 for details of search strategies and data extraction). We initially screened studies in pairs of researchers assessing sets of the same 100 references, moving to single screening when an agreement rate of 90% was achieved. We repeated this process for assessing full-text studies where the first screening indicated potential inclusion or where the reviewers believed there was insufficient information to judge. We conducted data extraction and study quality appraisal in duplicate and independently using, respectively, an extraction form that was initially piloted on two studies and a modified version of the Cochrane risk of bias tool (Higgins & Green, 2011).

Effect sizes from included study reports concerning substance use (smoking, alcohol or drugs) as defined in the protocol (Bonell et al., 2013) were extracted into a Microsoft Excel spreadsheet and converted into standardised mean differences (Cohen's *d*) using all available information as presented for each study. As recommended by the Cochrane Handbook (Higgins & Green, 2011), when the evaluation was designed as a randomised controlled trial,

we extracted the ‘least adjusted’ effect size estimates from each evaluation (i.e. uncontrolled estimates, or estimates controlling for baseline scores). When the evaluation was a matched or otherwise non-randomised design, we extracted the most adjusted effect size estimates (i.e. estimates in which the full vector of control variables was included). We adjusted direction as necessary so that positive effect sizes indicate an effect favouring the intervention. When studies did not present enough data to calculate effect sizes, we contacted study authors several times as needed for additional information. When we needed to impute additional data to calculate an effect size, we specified a range of reasonable assumptions and sensitivity analysed our findings. We standardised direction of the effect sizes so that positive effect sizes indicated a reduction in substance use.

In preparation for meta-analysis, we grouped effect sizes into several categories that we meta-analysed separately according to whether they were measures of: ‘omnibus’ substance use outcomes (where studies reported a generic measure of illicit drug use, alcohol consumption and/or tobacco smoking); illicit drug use; alcohol consumption; or smoking tobacco. We meta-analysed these outcomes both separately and together in an ‘all substance use’ model, and we estimated a subset of outcomes in models addressing short-term outcomes captured between post-intervention and four month follow-up, inclusive. We did not meta-analyse long-term outcomes separately because of the large variation in follow-up times beyond four months post-intervention. As indicated in the protocol (Bonell et al., 2013), we intended to use multivariate meta-analysis or another method to synthesise effect sizes in this situation. However this was not possible because of the heterogeneity of reported outcomes and lack of availability of a correlation matrix for reported outcomes. Instead, we used a multilevel meta-analysis model (Cheung, 2014) with random effects at both the outcome and study level, as this model did not require us to specify a correlation matrix. The resultant pooled effect size estimate includes all information that the multiple effect size estimates

contribute while correcting for the non-independence of multiple effect size estimates from each study.

This review was managed in EPPI-Reviewer (Thomas, Brunton, & Graziosi, 2010) and analyses were undertaken using the R package metafor (Viechtbauer, 2010). This project was approved by the research ethics committee of the Institute of Education's Faculty of Children and Learning (ethics approval reference number FCL 544).

## **Results**

Searches yielded 32,394 de-duplicated abstracts, of which 689 were screened in full text (see Figure 1). We included 13 study reports of 10 distinct studies reporting substance use outcomes in our narrative synthesis of intervention evaluations and 12 study reports of 9 distinct studies in our meta-analysis. We were unable to include findings from the evaluation of Stay SMART (St Pierre & Kaltreider, 1992) because the study report presented insufficient data for calculation of effect sizes and because we obtained no further information from study authors. Below, we discuss key characteristics of included studies and interventions.

**Characteristics of included studies.** Of the ten included studies, four were randomised controlled trials (see Table 1). Five studies were prospective studies with non-random matched control groups. One evaluation included both a randomised trial component and a non-randomised matched comparison component, with both being analysed together. Comparisons in every case but one (Tebes et al., 2007) were no-treatment or minimal treatment controls. In this study, the comparison was an after-school programme similar to the intervention group but without PYD content. All included studies were conducted in the United States except for one conducted in the United Kingdom. Outcomes ranged from immediately post-intervention to six years post-intervention.

Study quality was highly variable (see Table 2 and Figure 2). Of the ten included studies, only half reported explicitly accounting for clustering in their analyses, and six did

not report strategies to account for missing data in the face of attrition. However, five of six non-randomised trials accounted for key confounders in their analyses. In appraisal items specific to randomised trials, none of the four included randomised trials reported blinding, and only one reported allocation concealment or sequence generation in sufficient detail to suggest a low risk of bias.

**Characteristics of included interventions.** Though all interventions evaluated in included studies deployed key aspects of PYD interventions, they were diverse in approach and delivery. Interventions are described in Table 3. In the main, interventions manifested a theory of change consistent with PYD by promotion of positive assets, though most contained explicit prevention-focused education as well. One programme, Stay SMART (St Pierre & Kaltreider, 1992), focused on prevention and health education over PYD, though some promotion of positive assets was evident in limited description of the intervention. Interventions were targeted in different ways. Two interventions (Maryland after-school programmes, or MAP (Gottfredson, Gerstenblith, Soulé, Womer, & Lu, 2004); All Stars (Cross, Gottfredson, Wilson, Rorie, & Connell, 2009; Gottfredson, Cross, Wilson, Connell, & Rorie, 2010; Gottfredson, Cross, Wilson, Rorie, & Connell, 2010)) were targeted at schools or communities in distress without selection for specific young people. Three interventions (Cool Girls (Kuperminc, Thomason, DiMeo, & Broomfield-Massey, 2011); Quantum Opportunity Program, or QOP (Rodriguez-Planas, 2010a; Schirm & Rodriguez-Planas, 2004; Schirm, Rodriguez-Planas, Maxfield, & Tuttle, 2003; Schirm, Stuart, & McKie, 2006); Young People's Development Programme, or YPDP (Wiggins et al., 2008)) targeted subgroups of young people in schools and communities in distress, either by programme design (e.g. Cool Girls (Kuperminc et al., 2011) was intended to be delivered to a single-sex group), by referral from school counsellors (YPDP (Wiggins et al., 2008)) or by academic criteria (QOP (Rodriguez-Planas, 2010a; Schirm & Rodriguez-Planas, 2004; Schirm et al.,

2003; Schirm et al., 2006)). Finally, four interventions (National Guard Youth Challenge Program, or NGYCP (Millenky, Bloom, & Dillon, 2010; Millenky, Bloom, Muller-Ravett, & Broadus, 2011; Schwartz, Rhodes, Spencer, & Grossman, 2013); Positive Youth Development Collaborative, or PYDC (Tebes et al., 2007); Youth Action Research for Prevention, or YARP (Berg, Coman, & Schensul, 2009); Big Brothers Big Sisters, or BBBS (Rhodes, Reddy, & Grossman, 2005; Tierney, 1995)) targeted young people on the basis of identification as 'at risk' by site-specific programme criteria (NGYCP (Millenky et al., 2010; Millenky et al., 2011; Schwartz et al., 2013), BBBS (Rhodes et al., 2005; Tierney, 1995)) or on the basis of ethnic minority status (PYDC (Tebes et al., 2007), YARP (Berg et al., 2009)). We did not have information to classify if and how Stay SMART (St Pierre & Kaltreider, 1992) targeted young people.

Most of the programmes were delivered after school or co-located with the school setting. NGYCP (Millenky et al., 2010; Millenky et al., 2011; Schwartz et al., 2013) was intended to replace schooling, and though YARP (Berg et al., 2009) included a school-year component, young people began the programme over the summer. Stay SMART (St Pierre & Kaltreider, 1992) and BBBS (Rhodes et al., 2005; Tierney, 1995) both operated apart from the school setting. Providers were generally youth workers and social services staff, though NGYCP (Millenky et al., 2010; Millenky et al., 2011; Schwartz et al., 2013) used staff members from the National Guard (i.e. military personnel) and BBBS (Rhodes et al., 2005; Tierney, 1995) was coordinated by social services staff, but run on the basis of lay volunteers. Exact Duration of the programmes was often unclear, though All Stars, MAP, PYDC, Cool Girls and QOP were mostly conducted during the school year. NGYCP, YPDP, BBBS and Stay SMART each ran on schedules that included summer activity, and YARP began with a summer session.

**Meta-analysis of studies.** After data transformation and preparation (available on request), we included 54 effect sizes addressing substance use outcomes from 12 reports of nine studies. We were also unable to include an effect estimate for alcohol use from the evaluation of YARP (Berg et al., 2009) despite multiple attempts to contact the study's surviving authors. The evaluation of PYDC (Tebes et al., 2007) measured the difference between intervention and control groups in a change model from baseline to follow up of risk for substance use. We did not have enough information to convert these outcomes into a metric completely consistent with the other studies. We thus sensitivity analysed our findings on this basis and on the basis of other data transformation decisions we took.

Our 'all substance use' model (see Table 4 and Figure 3) found that included PYD interventions did not have a statistically significant effect on reducing substance use generally, either across all time-points ( $d=0.079$ , 95% CI -0.025, 0.183) or in the short term ( $d=0.086$ , 95% CI -0.025, 0.197). In separate models for omnibus drug use, illicit drug use, alcohol and smoking, we also found no significant effects. The one exception was short-term omnibus substance use outcomes (0.169, [0.012, 0.326]). But it is worth pointing out that the difference between the all time-points and short-term time points analyses reflects the inclusion in the former but not the latter analysis of one effect size: the odds of being convicted of a drug offence measured at 18 months, reported in Millenky et al. (2010) as part of the evaluation of the NGYCP (Millenky et al., 2010; Millenky et al., 2011; Schwartz et al., 2013). Thus, this statistically significant finding should be interpreted with caution. We do not present analyses for smoking at short-term time-points, as only two effect sizes would have been included. Meta-analyses were generally characterised by a low degree of heterogeneity at the programme level, and were robust to sensitivity analyses.

## **Discussion**

PYD interventions studied to date have not have demonstrated an effect of public health or (with one exception) statistical significance on young people's substance use. However, it is not clear if these findings reflect evidence of no effect, or no evidence of effect. On the one hand, a range of interventions with high heterogeneity in targeting, design and positive youth development components, but with low statistical heterogeneity at the programme level (i.e. between different interventions), did not yield an effect that was statistically significant, despite a robust analytic strategy that preserved all information from included studies. On the other hand, the highly variable quality of outcome evaluations, the need for extensive data transformation to render effect sizes meta-analysable, and our inability to include an intervention (St Pierre & Kaltreider, 1992) in the meta-analysis and that same diversity of programmes may mean that our findings better reflect the inadequacy of the evidence rather than of the intervention model itself. Because meta-regression should be conducted with a minimum of 10 studies (Higgins & Green, 2011), we were unable to explore potential differences in effectiveness by intervention characteristics, though we originally planned to undertake this.

On balance, we believed it was an appropriate decision to meta-analyse the included studies. The challenges we faced with the data may suggest an interpretation of the pooled effect size that focuses on its general magnitude and imprecision, rather than on statistical significance per se. As all reviews are, our findings may be subject to publication bias, which we were unable to evaluate due to the few studies we included, and to biases in the search and retrieval of studies, though our extensive and pre-planned strategy was protective against this. It is also impossible to exhaustively search grey and fugitive literature, as by nature it is poorly indexed and difficult to access. All reviews involve choices of databases to search. While we did not believe it would threaten the validity of our review, we elected not to search databases of 'evidence-based programs' like Blueprints for Healthy Youth Development, nor

did we search Embase in this systematic review. While we believed this was an appropriate decision given the content area of this review, it is conceivable (as in any systematic review) that studies were missed as a result of our search strategy decisions.

Despite rigorous application of our inclusion criteria, it is clear that some of the studies included appear to be interventions that may not have consistently emphasised PYD to the degree that they emphasised ‘standard’ risk reduction programming. This is not to say that these two approaches are diametrically opposed; in fact, key proponents of PYD interventions have suggested that these two goals are synergistic (Catalano, Hawkins, Berglund, Pollard, & Arthur, 2002). But it does suggest that our studies may not be directly testing PYD alone as a theory of change for interventions. As part of our broader evidence synthesis project, we attempted to determine whether interventions embodied characteristics we identified in a synthesis of theories of PYD (under review, (Bonell et al., 2015)), but generally poor intervention description prevented this. Some interventions involved an array of sites with intervention activities varying between them, such as: MAP (Gottfredson et al., 2004); YPDP (Wiggins et al., 2008); and QOP (Rodriguez-Planas, 2010a; Schirm & Rodriguez-Planas, 2004; Schirm et al., 2003; Schirm et al., 2006). Furthermore, a number of programmes appeared to focus on explicit risk reduction as much if not more than on positive development. This was particularly the case with the All Stars intervention evaluation (Cross et al., 2009; Gottfredson, Cross, Wilson, Connell, et al., 2010; Gottfredson, Cross, Wilson, Rorie, et al., 2010) and to a lesser extent PYDC (Tebes et al., 2007), Cool Girls Inc. (Kuperminc et al., 2011) and Stay SMART (St Pierre & Kaltreider, 1992). NGYCP (Millenky et al., 2010; Millenky et al., 2011; Schwartz et al., 2013), though focused on positive development rather than explicit risk prevention, could also be regarded as atypical of PYD interventions in adopting a quasi-military ‘boot-camp’ style of delivery.

In considering whether or not PYD interventions ‘work’, it is of use to consider the two key deviant cases with positive effects that we included in our meta-analyses. Both the evaluation of the PYDC (Tebes et al., 2007) and the evaluation of BBBS (Rhodes et al., 2005; Tierney, 1995) reported significant effects on reducing substance use—in the case of the first evaluation, at four months post-intervention but not at post-intervention. The authors of the first evaluation conclude that these differences are due to effectiveness of the programme, but this conclusion is attenuated by the non-randomised evaluation design and high attrition (approximately 40% overall at the second follow-up). Moreover, this intervention may not be an exemplar of PYD because of combined promotion of positive assets with ‘traditional’ risk-based prevention education.

In contrast, the randomised evaluation of BBBS (Rhodes et al., 2005; Tierney, 1995) accompanied by extensive programme description provides some confidence about the significant intervention effects the authors claim to demonstrate. The authors describe that what makes BBBS (Rhodes et al., 2005; Tierney, 1995) different from other mentoring-based interventions (and, in fact, other mentoring-based interventions in this systematic review) is the focus on long-term relationships between a young person often from a disadvantaged social or economic background and an adult who may have ‘aspirational’ characteristics (e.g. higher education). But above all, those randomised were drawn from a pool of families who had approached BBBS (Rhodes et al., 2005; Tierney, 1995) for a match. This is in contrast, for example, to QOP (Rodriguez-Planas, 2010a; Schirm & Rodriguez-Planas, 2004; Schirm et al., 2003; Schirm et al., 2006) or All Stars (Cross et al., 2009; Gottfredson, Cross, Wilson, Connell, et al., 2010; Gottfredson, Cross, Wilson, Rorie, et al., 2010), where participation was ‘opt-out’ or otherwise brought to students and their families in the schools young people were already attending. Together, all of these factors may have accounted for the success of BBBS (Rhodes et al., 2005; Tierney, 1995).

Moving forward, any investment in PYD as a strategy to reduce these outcomes should occur only within the context of evaluation studies. Our review suggests that existing PYD interventions subject to evaluation do not appear to have produced reductions in substance use of public health significance. However, these interventions may not be the best exemplars of a PYD approach as explained above. Therefore, our findings should not be taken as evidence for the ineffectiveness of PYD as a theory of change for reducing substance use among young people. Better evaluations are required before such interventions are considered for scale-up.

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## Figures and Tables

**Table 1.** Characteristics of included studies.

**Table 2.** Risk of bias judgments for included studies.

**Table 3.** Description of included interventions and PYD characteristics.

**Table 4.** Meta-analysis findings. *k*: number of programmes contributing to the meta-analysis, *n*: number of effect sizes in the meta-analysis. Positive effects indicate a beneficial outcome of the intervention.

**Figure 1.** Flow of studies through the review.

**Figure 2.** Review authors' judgements about each risk of bias item presented as percentages across all included studies.

**Figure 3.** Substance use outcomes. *Positive effects indicate a beneficial outcome of the intervention.*