An Examination of the Longer-Term Impact of a Combined Classroom and Parental Intervention on Alcohol-Related Harms and Heavy Episodic Drinking

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Abstract

Although fewer adolescents are consuming alcohol than was the case in previous decades, those who are consuming alcohol are still exposed to alcohol-related harms. While the evidence for the effectiveness of universal, school-based interventions is limited, a recent cluster randomised controlled trial (The STAMPP Trial) reported a significant effect at 10 months post-intervention of a combined classroom/parental intervention on heavy episodic drinking (HED) in the previous 30 days, but no significant effect on the number of self-reported alcohol-related harms (ARH) experienced in the previous 6 months. This follow-up study sought to examine intervention effects 24 months after delivery of the intervention (+ 57 months from baseline, or + 34 months post- intervention). Participants were 5029 high school students in STAMPP (38% of 12,738 pupils originally randomised into the trial), from 87 schools (82.3% of schools recruited in the original STAMPP trial). Outcomes were assessed using two-level random intercepts models (logistic regression for HED and negative binomial for number of ARH). Results of the present study show that the intervention effect for HED deteriorated over the following 2 years (OR declined from 0.60 to 0.97), and there was still no difference in ARH. This was due to an increase in the prevalence of intervention students' HED rather than a reduction in prevalence in control students. Results are discussed in the context of prevention initiatives.

Keywords: Heavy episodic drinking · Alcohol-related harms · Adolescents · The STAMPP trial · Substance prevention · Alcohol

Introduction

The consumption of alcohol by adolescents remains a public health concern. Globally, alcohol use is the leading risk factor for disability-adjusted life years (DALYs) in 15–19 year olds (Mokdad et al. 2016). Although the overall proportion of ad- olescents drinking alcohol in the United Kingdom (UK) has declined in recent years, alcohol-related health harms remain high (Healey et al. 2014). Given that those who report early initiation of alcohol intoxication are more likely to report ad- verse alcohol-related outcomes in young adulthood (Kuntsche et al. 2013; Maimaris and McCambridge 2014; Morean et al. 2014), it is all the more important that interventions which target adolescent alcohol use are carefully evaluated, and that any possible intervention effects are understood.

Few school-based universal alcohol prevention programmes have been found to be effective and reviewers have identified a lack of high-quality trials as being problematic (Faggiano et al. 2008; Foxcroft and Tsertsvadze 2011). However, interventions which develop social skills appear to be superior to those that seek to enhance only knowledge (Faggiano et al. 2008). While literature evidencing the short-term effectiveness of universal prevention programmes is sparse, there are even fewer studies examining longer-term (+ 3 years or more) impact. Where positive long-term intervention outcomes have been identified, effect sizes are small, and transportability of programme effects is uncertain (Foxcroft et al. 2003; Foxcroft and Tsertsvadze 2012; Gilligan et al. 2019; Newton et al. 2017). In this context, the current study adds a significant contribution to the limited number of studies that have examined long-term outcomes from a school-based universal alcohol prevention programme.The STAMPP trial (Sumnall et al. 2017) was a cluster randomised controlled trial (cRCT) comparing the effects of a combined, culturally adapted intervention based on the School Health and Alcohol Harm Reduction Project (SHAHRP; McBride et al. 2004; McKay et al. 2012), and the Swedish Örebro Prevention Program (Koutakis et al. 2008) interventions. The SHAHRP intervention used in the STAMPP trial study was originally developed in Australia, but had been adapted and pilot tested previously in the Northern Irish context (McKay et al. 2012). It is a universal developmental programme that includes three main strategies:

(1) teaching students to develop skills to recognise high-risk situations;
(2) increasing awareness of external influences on behaviour; and
(3) combining self-control with refusal skills training (McBride et al. 2004).

Classroom knowledge- development activities are used to shape alcohol attitudes and to support situational decision-making that is relevant to the specific environments and drinking cultures of the target group.

The intervention consisted of ten classroom-based lessons delivered by trained schoolteachers over a 2-year period, six lessons in year 1 and four lessons in year 2. Lessons delivered in year 1 focused on a broad range of alcohol-related issues including, but not limited to, myths about alcohol, alcohol and the media, alcohol and the body, units of alcohol, and the relationship between increasing levels of consumption and likely behavioural outcomes, as well as a look at some scenario-based situations. The lessons in year 2 were much more focused on actual drinking contexts, and possible harms that might emerge from such contexts. Students were asked to focus on a particular 'night out' with a view to identifying and ameliorating possible harms. Additionally, they were encouraged to debate deliberately provocative statements, for example, 'drinking vodka is worse than drinking beer'. At the day-long training events, teachers were encouraged to facilitate discussion where possible, rather than focusing on completion of the workbooks which accompanied the lessons. Materials were also provided in digital format in order to facilitate interactive delivery of the programme. In addition, like the Dutch Preventing Heavy Alcohol Use in Adolescents intervention (Koning et al. 2009a b), the parents of children in the intervention group were invited to attend a one-off brief intervention, facilitated by a third party organisation, in the school setting. This event focused on parental rule setting, and culminated in an agreed set of alcohol-specific rules to be applied across the homes of all those present. This component is hypothesised to work by reinforcing the lessons received in the classroom through shaping healthier attitudes towards alcohol, reducing opportunities for alcohol use in the family home, and by providing positive behavioural models around drinking.

The STAMPP trial included 70 post-primary schools in Northern Ireland and a further 35 post-primary schools in Scotland (Sumnall et al. 2017). In the STAMPP trial, questionnaires were administered to participants at baseline (June 2012, T0) and at three follow-ups: + 12 months, + 24 months, and + 33 months. Primary outcome analyses were performed on data gathered at + 33 months from baseline (T1–T3), at least 10 months after the intervention had been implemented. Analysis of primary outcomes at + 33 months showed that, when the newly developed STAMPP intervention was compared to Education as Normal (EAN), pupils in control schools reported

significantly higher rates of heavy episodic drinking (HED) in the past month (primary outcome #1) than pupils in the intervention schools. However, the study arms did not differ significantly in terms of the number of alcohol-related harms (ARH) reported in the previous 6 months (primary outcome #2).

Considering the lack of long-term follow-up of school- based universal alcohol prevention programmes, and mixed findings with regard to transportability of programme effects, the present study examined if intervention effects were sustained a further 2 years post-intervention cessation (+ 57 months from baseline).

Methods

Participants

Participants were high school pupils in Scotland and Northern Ireland. In Northern Ireland, the first year of high school (when then the pupils are aged 11–12) is described as year 8 (with pupils continuing through year 9 etc.), whereas in Scotland this is de- scribed as S1 (and pupils continue through S2 etc.). For clarity, T0 data collection occurred when pupils were in first year (S1/ year 8), with T1 occurring in S2/year 9 etc. T5 data collection therefore represents the pupils' sixth year in high school.

In addition to those pupils who completed a questionnaire at T0 (N = 11,316), pupils who were absent at T0 but present at T1data collection (i.e. missing on the day of the T0 data collection) and pupils who joined participating schools before the delivery of phase 1 of the intervention in the autumn term of 2012 (between T0 and T1) (N = 1422) were included in the study population giving a total sample size N = 12,738. No significant differences were detected between the control and intervention arms of the trial at baseline.

Of these pupils, 10,405 completed a questionnaire at + 33 months (T3, 82% of the baseline cohort). Attrition at T3 was higher amongst pupils who were males (19%), in receipt of free school meals (FSM; 26%), lived in Scotland (24%), or had used alcohol at baseline (25%). There was little difference in dropout between the control and intervention arms of the trial (18% intervention, 19% control). Across individual schools, attrition varied from 2 to 32%. Further details on participant recruitment can be found in Sumnall et al. (2017), including eligibility, sampling, randomisation, and data collection procedures. Within the UK, age 16 (equivalent to T5 data collection) represents the end of mandatory education. Therefore, participants in the present long-term follow-up of the original trial cohort represent a subsample of those who previously participated in the STAMPP trial, and who remained in education until sixth year of high school. The mean age of the sample at T5 was 17.3 (SD = 0.38 as of 28 Feb 2017). The mean age was 12.6 (SD = 0.38) at T0 (30 Jun 2012). A total of 5029 pupils participated in T5 data collection across the two locations. Of these, 4857 had participated in the study at T0 or T1 and where included in the complete case analysis. In addition, 172 pupils joined the study in the intervening years between the introduction of the first phase intervention and T5 data collection. These pupils were excluded from the outcome analysis. Given that 12,738 pupils were randomised into the STAMPP trial, the retention rate at T5 was 38%. The loss to follow-up is mainly driven by the proportion of pupils who exited the education system at the end of their fifth year of high school. This is the final year of mandatory education in the UK. Therefore, the analysis will, at best, be generalisable only to the population of pupils who progress on within the school system after they complete their mandatory education at age 16, rather than the full baseline sample. These details are displayed in the CONSORT flow diagram. Details of the loss to follow-up at the individual school level are provided in the supplementary material (Table S.1).

The T5 sample contained a higher proportion of females than were observed at baseline (58% T5 versus 50% T0), and a lower proportion of pupils with low social economic status, as indicated by FSM entitlement (17% T5 versus 23% T0)

(Table 1). Attrition at T5 was relatively similar across both locations with NI pupils representing 61% of the sample at T5 and 62% of the sample at T0. As we are unable to determine whether pupils are missing at T5 because they have left education or because they are still registered within the school but were simply absent on the day of the data collection, the missing data methods used as sensitivity tests at the T3 end point (worst case, best case, and conservative case approaches) can- not be applied here.

As well as pupils leaving education at age 16, not all schools provide post-16 education opportunities for pupils. Therefore, study attrition occurred at both the school level and pupil level. All 35 schools in Scotland were retained at the T5 follow-up. A total of 52 of the original 70 (67.5%) schools in NI continued to participate (8 of the 18 schools who did not continue were intervention schools). Twelve of the non-participating schools did not have post-compulsory educational provision, and so did not have any eligible pupils; three schools declined to participate in the research; and a further three schools had closed since the completion of the T3 follow-up. However, as pupils who attended schools with- out a post-16 provision, but who wished to continue their education, are able to transfer to schools who do offer post- 16 classes, some of which may have participated in the T5 follow-up, data were collected from pupils from 95 of the original schools (50 intervention schools and 45 control schools). Pupils who transferred schools were analysed in terms of their original school at the time of randomisation.

Parents provided consent for their child's participation in STAMPP at the beginning of the study, and at each data collection point, pupils gave informed consent to participate.

Intervention

The intervention was a classroom-based alcohol education intervention, coupled with a brief alcohol intervention for parents/carers. See Sumnall et al. (2017) for a complete description.

Measures

Primary Outcomes

The study re-examined the two primary outcomes from STAMPP;

(i) The number of self-reported HED episodes in the previous 30 days (HED defined as the consumption of ≥ 6 units [males]/≥ 4.5 units [females]) which was dichotomised (none/1 or more)
(ii) The number of self-reported ARH (caused by own drinking) in the previous 6 months. Pupils were asked about the frequency of experiencing 16 drinking harms (e.g. being sick after drinking, getting into trouble with your parents as a result of your drinking) in the last 6 months. Responses for each harm were dichotomised (none/1 or more) and then summed to produce an overall count of the number of different harms experienced (a variety measure).

To assess the HED primary outcome, participants were presented with pictorial prompts of how much alcohol ≥ 6/≥ 4.5 UK units represents. Pictures presented the most popular drinks consumed in the two study areas and respondents were asked to report the frequency of consuming this amount of alcohol over the previous month. Harms associated with own use of alcohol were measured using a 16-item scale developed for the STAMPP trial (internal consistency 0.9; McBride et al. 2004). For example, participants were asked to report frequency of having a hangover after drinking, or if they had got into a physical fight when drinking.

Data were also collected on gender of the school (mixed/ boys only/girls only), country (Northern Ireland/Scotland), and level of FSM entitlement within the school (a tertile split). FSM entitlement is a proxy for socio-economic status (SES; Hobbs and Vignoles 2007) within the UK.

Statistical Analyses

As with the T3 primary outcome analysis, the T5 outcome analysis was an intention-to-treat (ITT) analysis using the complete case population, as per protocol. Logistic regression models estimated the association between study arm and the odds of HED. Negative binomial regression models estimated the association between study

arm and the number of ARH. All models included school-level random intercepts to ac- count for the lack of independence due to clustering of students within schools. All models adjusted for the school-level factors used to stratify randomisation (school location, gender, and level of FSM) and the outcome's corresponding value at baseline (T0) at the pupil level. For details of the analysis of secondary outcomes, please see the online supplementary materials. For each primary and secondary outcome, a statistically significant result was concluded if the *p* value for the treatment arm explanatory variable was < 0.025.

Results

As previously reported (Sumnall et al. 2017), a relatively small proportion of parents attended the parental brief intervention, or completed the mailed questionnaire. Respectively for Northern Ireland and Scotland, these proportions were 9% and 2.5% for attendance, and 31% and 18% for mailed return of questionnaire. Table 1 provides the sample characteristics for both intervention and control groups at T0 and T5. These are given for gender, FSM entitlement, location (NI or Scotland), and HED prevalence. Between T0 and T5, there was a reduction in the proportion of male respondents and those reporting entitlement to FSM, due to sample attrition.

Table 2 displays descriptive data (by intervention arm) for both primary outcome measures. The prevalence of HED increased across study waved as participants aged (see Table 2). The prevalence rate of HED increased from 7.7% in first year of high school (T0) pupils to around 55.1% amongst fifth year (T5; + 57 months) pupils. At baseline, there was no difference in HED between the control and intervention schools. Regarding the development of HED, by fourth year (T3; + 33 months), a gap of 9 percentage points was observed between the two trial arms with pupils in intervention schools reporting a lower level of HED. This equates to a significant odds ratio of 0·60 (95% CI = 0.49–0.73). No difference in the number of ARH was observed at T3 (incident rate ratio = 0.92, CI = 0.78– 1.05).

Figure 1 shows the unadjusted prevalence rates across both study arms. The maximum intervention effect was observed at T3, approximately 1 year after the completion of the intervention. Little difference in the alcohol prevalence rates between the control and intervention groups is observed at either T4 (45 months post baseline) or T5 (57 months post baseline).

Table 2 also provides the mean number of ARH caused by the respondents' own drinking. As with HED, the number of ARH increased with age over the course of the study, from a mean of less than one ARH reported at baseline to over three by T5. While the number of ARH was significantly lower in Scotland compared to that in Northern Ireland, little difference was detected in the number of ARH between the control and intervention arms.

Table 3 summarises the primary outcome model for HED at T5. While baseline drinking was a significant predictor of drinking in late adolescence ($p < 0.001$), no significant difference in HED was detected between control and intervention groups/arms at T5 ($p < 0.581$). Given that the T5 sample is quite different from the full sample at T3 (i.e. only comprised of student progressing beyond mandatory education), the T3 HED outcome analysis was replicated on only those pupils who participated in T5 data collection. This analysis confirmed a significant intervention effect at T3 for the T5 participants only, although the intervention effect at T3 was slightly reduced amongst T5 only respondents when com- pared to the full T3 sample (T3 $OR_{(full\ sample)} = 0.596$; T3 $OR_{(T5\ sample\ only)} = 0.670$; see also supplementary Table 4).

The model for ARH at T5 data is summarised in Table 4. Given that no significant intervention effect of ARH effect was observed at T3, it is not surprising that no intervention effect was also observed at T5.

Discussion

The original STAMPP trial demonstrated that a relatively in- expensive and easy-to-deliver classroom-based intervention, combined with a parental brief intervention, could reduce HED, but not the number of ARH experienced, in adolescents up to 10 months after delivery of the final intervention session (Sumnall et al. 2017). One of the conclusions offered by the authors of the STAMPP trial was that, given the age of par- ticipants and the overall low amount of ARH reported, effects on ARH experienced might be 'delayed' in time and emerge with age as more participants drank alcohol, and to a greater extent. The present study sought to examine if longer-term effects of STAMPP persisted after the trial duration.

We found that in this subsample intervention effects were diminished, and effects on HED disappeared 34 months after intervention delivery (+ 57 months). The biggest impact on HED was observed at T3 (+ 33 months). This was in the period shortly after the completion of the two-phase intervention. Although we acknowledge geographic differences, school surveys from other parts of the UK (England) show that there is an acceleration in alcohol involvement across mid-adolescence, and an increase in the proportion of students reporting drunkenness in the last month between the ages of 14 and 15 years (14 to 20%), and the amount drunk (5.5 to 10 UK units), which corresponds to our T3 and T4 data collection waves (see Fig. 1) (NHS Digital 2017). We suggest that this acceleration, coupled with the cessation of the intervention, goes a long way to explaining the lack of continued intervention effect.

Therefore, we conclude that the combined interventions used in STAMPP are an effective short-term alcohol education intervention, but that its administration should not be understood by school leaders, health commissioners, or anyone else as an 'inoculation' against drinking behaviours. In other words, the evidence clearly shows that behaviour change is possible, and we recommend, in order to see prolonged effects of STAMPP, that it be extended across the lifetime of school life, either by means of further structured lessons or booster sessions. Given the extant literature, we would favour the former option. The evidence for the effectiveness of interventions employing booster sessions is mixed. Smith-Stover et al. (2019) reported on the effectiveness of an intervention incorporating booster sessions, for men in residential substance misuse treatment, which resulted in significant reductions in affect dysregulation, anger, and co- parenting problems. Similarly, Murphy et al. (2019) reported on the effectiveness of an intervention which included a boost- er session, on college drinking. However, elsewhere, the effectiveness of booster sessions in substance-related interventions has not been observed (Sussman et al. 2012). Beyond substance use, a meta-analysis of the effectiveness of booster sessions in cognitive behavioural therapy in young people for mental health problems revealed such sessions to be largely ineffective (Sun et al. 2019). Rather than adding more of the same (as it were), we would recommend extending the STAMPP intervention in an age-appropriate way across more school years than is presently the case. This would allow for on-going engagement at an age-appropriate level, in an intervention style that, at least in the short term, proved to be effective.

It is our view that to interpret Fig. 1 any other way would be injudicious. As this intervention was deemed to be is cost neutral, where the accrued savings due to the reduction in drinking behaviour exceed highly conservative estimates of the costs of its delivery (see Agus et al. 2019), there are few barriers to its successful implementation.

To put the extent of the intervention in context, ten lessons in total equate to one school day over a 2-year period. Self-evidently, this level of engagement is insufficient to counter the personal, familial, peer, and social exposure to alcohol that these participants have encountered and will continue to encounter as they grow older. However, the results previously reported (Sumnall et al. 2017) clearly suggest that this intervention can form *a part* of the response to alcohol prevention amongst school-aged children, but clearly it is not the whole answer. Based on our interaction with both intervention and control group schools, it is clear that there was a spectrum of engagement with the study in both contexts, both within individual schools and within study arms. Not all schools which were randomised to the intervention arm were fully engaged, and not all delivered the intervention with total fidelity.

Turning to the ARH question, the results clearly suggest that the theory about a 'delayed' effect on ARH was not supported. It remains for further work to be undertaken to examine the nature and structure of the ARH examined herein. The ARH index was developed in Australia, and it is possible that the scale wording lacked salience in a UK context. Furthermore, this scale was developed in the early 2000s and a large literature continues to evidence changing drinking patterns amongst adolescent worldwide. It may also be the case that, for the intervention to be successful, it needs to be delivered at a time when the target behaviour is emerging within the relevant population. For most adolescent drinkers, the emergence of ARH may only occur outside the effectiveness window of the intervention (up to 1 year after its delivery). The initial STAMPP trial (data collection points T0–T3) did find a subgroup effect for ARH amongst early onset drinkers (those who had started drinking before the start of the study) but this effect was not sustained to T5. Finally, it is also possible that the intervention has simply no effect on ARH, although that would be at odds with the results of a smaller study (N = 32 schools; $N \approx 3000$ participants) that was the precursor to STAMPP (see McKay et al. 2010). However, it should be pointed out that the McKay et al. (2012) study used a Latent Class analytical approach, and any significant ARH effect could be an artefact of that. Furthermore, that study began one academic year later, and it is possible that the experience of ARH was more salient in that age group.

As outlined above, the core finding of this study was that the intervention element of STAMPP was an effective intervention in reducing HED amongst adolescents, but that the positive impact was only sustained for 10 months after the intervention ended. On the basis of this, a number of robust policy and practice recommendations can be made. Firstly, the STAMPP intervention approach should be recognised as an effective and cost-effective universal prevention intervention that significantly and substantively reduces HED in the short term, in adolescents within the Northern Irish and Scottish contexts. Secondly, STAMPP is one of the few UK school- based alcohol prevention programmes to show effectiveness in reducing HED in adolescents. Therefore, the STAMPP intervention approach is one that could viably be rolled out as a key component of a 'whole school' approach to alcohol that complies with the latest NICE guidelines [PH7].

Thirdly, the implementation of the intervention element of STAMPP could justifiably be supplemented with additional lessons in the school years preceding or following the existing classroom/prenatal intervention. We base this suggestion on the divergence of the lines representing intervention and control groups in Fig. 1. This clearly depicts behavioural change that is confined precisely to the period of intervention delivery. The divergent lines clearly begin to re-converge in the school year following cessation of the intervention. Further lessons could be useful in reinforcing previously covered material and, additionally, engaging pupils in age-appropriate discussion concerning alcohol use. Given the increasing rate of alcohol consumption over the teenage years, additional lessons should have a particular focus on harm prevention and reduction. The cost implications of such sessions and the degree to which this would impact on the cost- effectiveness of STAMPP remain uncertain. These costs would depend on a number of considerations, including the duration and style of such sessions (structured, teacher- delivered sessions versus bespoke sessions delivered by external agents), the nature of such sessions, and what 'equipment' (if any) would be required, as well as any design costs. To our minds, the best approach to this would be to extend in terms of programme duration, rather than style. Therefore, age- and stage-appropriate extension of the programme in a similar style to how it currently operates, but over more school years, would be a cost-effective option, and would be intuitive, based on the limited success of the T0–T3 initial evaluation. Finally, the parental component of the STAMPP intervention should be further enhanced and strengthened to ensure greater parental involvement, particularly in relation to setting rules regarding their children's access to alcohol.

As with every long-term cRCT, there are limitations that need to be considered when interpreting the results. Whilst researchers may feel that collecting data is of paramount im- portance, other stakeholders in the research process have different priorities. We had to rely on what was achievable within the resources available to us (e.g. time and co-operation) and we are eternally grateful to everyone who participated. This meant that self-report was used instead of more robust methods that were simply not possible. We could do little to encourage parents to attend the parental brief intervention due to the means available to us for contacting them (e.g. data protection compliance by our stakeholders). The reduction of participants through natural attrition in the post-16 education sector and loss to follow-up by not being in education could not be resolved with the resources available. It is theoretically possible that the bounded rationality of our decision- making created the results we obtained (e.g. demand characteristics of the intervention on self-report), but there is no way for us to explore those ideas with the data available. A final limitation centres on generalisability of findings. The present analyses concern only those who remained in formal schooling for advanced level examinations (post age 16). In the

Northern Ireland context, this will have been predominantly grammar school attendees, and will disproportionately represent children from more affluent backgrounds.

To conclude, this study makes a significant contribution to the literature on the long-term efficacy of a school-based al- cohol prevention intervention (STAMPP). Very few interventions are subject to such a lengthy follow-up and these results indicate that expecting long-term impacts from time-limited interventions may be unrealistic.

Supplementary Information The online version contains supplementary material available at https://doi.org/10.1007/s11121-020-01193-5.

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Compliance with Ethical Standards

Conflict of Interest: The authors declare that they have no conflict of interest.

Ethical Approval: Ethical approval was provided by the University of Liverpool (Scottish Study), and by Queens University Belfast (Northern Ireland Study).

Informed Consent: Informed parental consent was gathered for all participants. Additionally, all participants provided consent to participate on the day of data collection.

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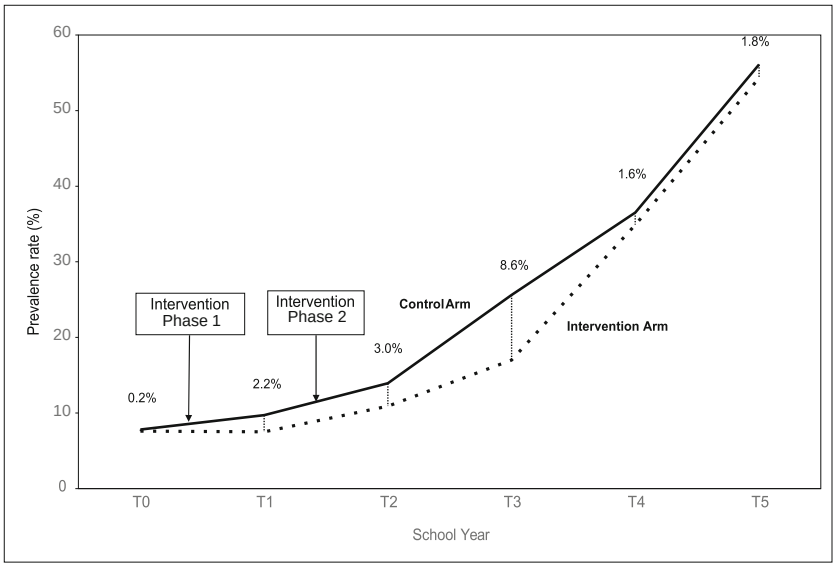

Fig. 1 Unadjusted prevalence rates across both study arms

Table 1 Sample characteristics

	Study arm (T5)		T5 total	Baseline (T0/T1)	Baseline sample absent at T5
	Control (*N* = 2240), *N* (%column)	Intervention (*N* = 2617), *N* (%column)	(*N* = 4857), *N* (%column)	(*N* = 12,738), *N* (%column)	(*N* = 7881), *N* (%column)
Gender					
Male	932 (41.9)	1110 (42.3)	2032 (42.1)	6389 (50.7)	4357 (56.1)
Female	1292 (58.1)	1503 (57.7)	2795 (57.9)	6203 (49.3)	3408 (43.9)
Free school meals					
No	1852 (82.8)	2168 (83.1)	4020 (83.0)	9739 (77.1)	5719 (73.4)
Yes	384 (17.2)	441 (16.9)	825 (17.0)	2899 (22.9)	2074 (26.6)
Location					
NI	1372 (61.3)	1605 (61.3)	2977 (61.3)	7742 (60.8)	4765 (60.5)
Scotland	868 (38.8)	1012 (38.7)	1880 (38.7)	4996 (39.2)	3116 (39.5)
HED					
No	968 (43.9)	1174 (45.7)	2142 (44.9)	10,343 (92.3)[a]	–
Yes	1237 (56.1)	1394 (54.3)	2631 (55.1)	863 (7.7)[a]	–
Mean age (SD)	17.31 (0.37)	17.31 (0.38)	17.31 (0.38)	12.64 (0.38)[b]	17.32 (0.38)

The percentages are calculated based on complete cases only. The T5 totals are restricted to only cases who were present at baseline (i.e. T0 or T1). Pupils who completed the survey at T5, but who joined the schools after the intervention started (i.e. T2–T5), have been excluded from figures presented here. HED heavy episodic drinking. [a] *HED baseline prevalence is based on T0 data only. Age is calculated from date of birth at baseline.* [b] *Age was calculated from date of birth at T0. 30 Jun 2012 was used as the census date for T0 and 28 Feb 2017 was used as the census date fire T5. These dates were selected as the corresponding to the end of fieldwork for each data collection point.*

Table 2 Descriptive data for primary outcomes: unadjusted prevalence of HED by study arm and mean number of reported drinking harms by study arm (T0 to T5)

	Full sample (NI and Scotland), HED		NI sample only, HED	
Data collection point	Control, % (*n*)	Intervention, % (*n*)	Control, % (*n*)	Intervention, % (*n*)
T0	7.8 (432)	7.6 (431)	6.3 (218)	6.0 (210)
T1	9.7 (530)	7.5 (410)	7.1 (244)	4.9 (168)
T2	13.9 (722)	10.9 (573)	9.0 (293)	6.9 (229)
T3	25.6 (1300)	17.0 (879)	20.8 (670)	13.6 (446)
T4	36.5 (1466)	34.9 (1488)	34.2 (993)	32.3 (947)
T5	56.1 (1237)	54.3 (1394)	55.4 (743)	53.5 (837)
	Full sample (NI and Scotland), number of harms		NI sample only, number of harms	
Data collection point	Control, *m* (SD)	Intervention, *m* (SD)	Control, *m* (SD)	Intervention, *m* (SD)
T0	0.76 (1.94)	0.80 (2.11)	0.56 (1.71)	0.57 (1.79)
T1	0.82 (2.08)	0.70 (1.8)	0.62 (1.83)	0.49 (1.57)
T2	1.18 (2.54)	1.05 (2.37)	0.79 (2.10)	0.66 (1.86)
T3	1.74 (3.00)	1.60 (2.90)	1.33 (2.61)	1.21 (2.58)
T5	3.12 (3.40)	2.83 (3.11)	3.05 (3.46)	2.68 (2.98)

NI Northern Ireland, *HED* heavy episodic drinking, *T0* high school first year, *T5* high school sixth year. Harm questions were not asked at T4 data collection. Each of the 16 harm questions was dichotomised (yes/no). The harm primary outcome is a count of the number of harms experienced (0–16)

Table 3 HED primary outcome analysis (T5)

ITT complete case analysis	Estimate	SE	OR	*p* value
Within level				
Baseline HED	1.034	0.206	2.812	< 0.001
Between level				
Intervention arm	− 0.067	0.121		0.581
Free school meals (tertile)	0.160	0.082		0.053
School type				
Boys school dummy	0.248	0.175		0.155
Girls school dummy	0.243	0.102		0.017
Location (NI)	− 0.005	0.148		0.971
Residual variance	0.200	0.052		< 0.001
Threshold (HEDT5$1)	− 0.072	0.137		0.600

HED heavy episodic drinking. The model is a 2-level logistic random intercepts model. At T5, *N* = 4773

Table 4 Drinking harms primary outcome analysis (T5)

	Estimate	SE	*p* value
ITT complete case analysis Within level			
Baseline harms	0.128	0.013	< 0.001
Between level			
Intervention arm	−0.068	0.068	0.318
Free school meals (tertile)	0.092	0.041	0.025
School type			
Boys school dummy	0.111	0.106	0.294
Girls school dummy	0.198	0.096	0.039
Location	0.049	0.077	0.526
Residual variance	0.056	0.022	0.010
Intercept (HarmsT5)	0.911	0.088	< 0.001
Dispersion (HarmsT5)	1.244	0.075	< 0.001

The model is a 2-level negative binomial random intercepts models. At T5, *N* = 4847