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**Web-based alcohol intervention for Māori university students: double-blind,  
multi-site randomised controlled trial**

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## Abstract

**Aims:** Like many indigenous peoples, New Zealand Māori bear a heavy burden of alcohol-related harm relative to their non-indigenous compatriots, and disparities are greatest among young adults. We tested the effectiveness of web-based alcohol screening and brief intervention (e-SBI) for reducing hazardous drinking among Māori university students.

**Design:** Parallel, double-blind, multi-site, randomised controlled trial.

**Setting:** Seven of New Zealand's eight universities.

**Participants:** In April 2010, we sent e-mail invitations to all 6,697 17-24 year-old Māori students to complete a brief web questionnaire including the AUDIT-C, a screening tool for hazardous and harmful drinking.

**Intervention:** Those screening positive were computer randomised to: <10 minutes of web-based alcohol assessment and personalized feedback (intervention) or screening alone (control).

**Measurements:** We conducted a fully automated five month follow-up assessment with observers and participants blinded to study hypotheses, design, and intervention delivery. Predetermined primary outcomes were: (1) frequency of drinking (2) amount consumed per typical drinking occasion, (3) overall volume of alcohol consumed, and (4) academic problems.

**Findings:** 1,789 participants were hazardous or harmful drinkers (AUDIT-C  $\geq$  4) and were randomized: 850 to control, 939 to intervention. Follow-up assessments were completed by 682 controls (80%) and 733 intervention group members (78%). Relative to controls, participants receiving intervention drank less often (RR=0.89; 95% confidence interval: 0.82 to 0.97), less per drinking occasion (RR=0.92; 0.84 to

1.00), less overall (RR=0.78; 0.69 to 0.89), and had fewer academic problems (RR=0.81; 0.69 to 0.95).

**Conclusions:** Web-based screening and brief intervention reduced hazardous and harmful drinking among non-help-seeking Māori students in a large scale pragmatic trial. The study has wider implications for behavioural intervention in the important but neglected area of indigenous health.

**Trial registration:** ACTRN12610000279022

### **Declaration of interest**

The study was funded by New Zealand's Alcohol Advisory Council. The funder had no involvement in any aspect of the study or reporting of findings. The funder was not involved in study design; in the collection, analysis, and interpretation of data; in the writing of the report; or in the decision to submit the article for publication.

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## **Introduction**

New Zealand was founded on the basis of a treaty between the indigenous (Māori) peoples and those, largely from Great Britain, who were part of the colonial expansion of European peoples.<sup>1</sup> Today in New Zealand, Māori have significantly poorer health, with a life expectancy 8 years less than non-Māori.<sup>2</sup> One contributor to this inequality is hazardous consumption of alcohol.<sup>3</sup> Māori have more than twice the prevalence of episodic heavy drinking<sup>2</sup> and an alcohol-attributable death rate more than four times that of non-Māori.<sup>2</sup> These disparities are greatest among those under 30 years of age.<sup>3</sup> New Zealand alcohol policy was dramatically liberalised in the 1980s and 1990s, resulting in increased physical availability of alcohol, a reduction in the minimum purchase age, and longer trading hours.<sup>4</sup> In the face of weak supply-side policies, interventions to reduce demand for alcohol, that can be widely disseminated, are of crucial importance to reduce alcohol-related harm.

Evidence from systematic reviews and meta-analyses suggests modest effect sizes of computerised interventions delivered to university students, however, many of the trials were methodologically flawed and interventions were delivered in conditions that could not be integrated within routine healthcare or health promotion practice.<sup>5-8</sup> There have been no large scale trials of such interventions among indigenous people.

Previous trials in New Zealand<sup>9,10</sup> have shown that web-based alcohol screening and brief intervention (e-SBI), delivered in the primary healthcare setting, can be effective in reducing hazardous drinking among university students, a particularly high risk group.<sup>11</sup> Estimating the effectiveness of e-SBI separately for Māori was not possible in those trials because participants were recruited from among those presenting to the

student health service where Māori comprised only 7% of patients. In the present study we adopted a pro-active recruitment approach based on THRIVE, an e-SBI program developed in Australia,<sup>12 13</sup> and survey methods developed in New Zealand,<sup>14</sup> in which thousands of students are invited to participate from university enrolment databases. We used this approach to ensure equal explanatory power for Māori, who have traditionally been poorly served by health research,<sup>15</sup> and to create a sustainable platform for health promotion activities at universities. The aim was to estimate the effectiveness of e-SBI in reducing hazardous and harmful drinking among Māori university students. We hypothesised that, relative to screening alone, e-SBI would reduce alcohol consumption and related problems.

## **Methods**

Ethical approval for the study was given by New Zealand's Multi-region Ethics Committee (MEC/10/01/009).

### *Trial design*

The e-SBINZ study involves parallel randomised controlled trials: one involving Māori and the other non-Māori university students.<sup>16</sup> The Māori trial reported here was a multi-site, double blind, parallel groups randomized controlled trial with a 1:1 allocation ratio (Figure).

### *Participants*

Participants were university students who indicated they were Māori in response to the ethnicity question on the university enrolment form and who were 17-24 years-old at the time they were invited (some of whom turned 25 between then and when they

participated). All data were collected via the Internet such that participants could answer screening questions, participate in the intervention, and complete follow-up assessments wherever they chose. We invited all eight New Zealand universities to participate but one did not because of internal rules that did not permit the research team to invite students directly by e-mail as per the study protocol.

### *Sample size*

We based the estimate of required sample size on the 6 month outcomes in the THRIVE trial.<sup>17</sup> Assuming a 5% level of significance, 80% power, a dispersion factor of 0.92 and attrition of 30%, 547 participants per group were required at follow-up.<sup>16</sup> Assuming that 40% would agree to be screened, a conservative estimate in light of the THRIVE trial,<sup>17</sup> we sought to invite 7,814 Māori students aged 17-24 years. The university enrolment data showed that there were only 6,697 Māori students in this agegroup enrolled across the seven universities. As shall be shown below, eligibility rates, participation, and retention in the trial turned out to be sufficiently high to more than offset this deficiency.

### *Recruitment and screening*

On 19 April 2010, all 6,697 students were invited to participate using recruitment procedures described in detail elsewhere.<sup>14</sup> In summary, up to three reminder e-mails were sent in the following weeks. Students were offered the opportunity to win a NZ\$500 supermarket voucher or an Apple iPad by participating. Respondents visited a website consisting of a branched 3-page questionnaire with items covering: (1) gender, age, and living arrangements; (2) drinking in the last 12 months (yes/no), and (3) the AUDIT-C, a validated 3-item screening tool for hazardous and harmful



drinking.<sup>18</sup> We limited the screening to three questions because there is review level evidence that asking questions about alcohol consumption can itself influence behaviour, producing reductions in self-reported drinking levels.<sup>19</sup> This evidence-base is stronger among university students than in other populations and suggests the possibility of reactivity to the research conditions and possible bias towards the null.<sup>19</sup>

### *Randomization and blinding*

Students were sent an e-mail containing a hyperlink to a web questionnaire and were informed that “the main focus of this study is student alcohol use over time and its consequences”. Respondents who scored  $\geq 4$  were assigned via simple randomisation by the web server to the control group (screening only) or intervention. This procedure was to ensure that participants were blind to the true nature of the study, which was presented as two surveys, in order to minimise the potential for performance bias.<sup>20</sup> Researchers were blind to participants’ group allocation as randomization and all other study procedures were fully automated and thus could not be subverted.

### *Intervention*

The AUDIT-C comprises the three consumption questions of the 10-item WHO Alcohol Use Disorder Identification Test (AUDIT).<sup>21</sup> Those in the intervention group were then asked AUDIT items 4-10, all of which are concerned with alcohol problems, and additional questions on the largest number of standard drinks consumed on one occasion in the last four weeks, the duration of the drinking episode in hours, and their body weight, for the purpose of estimating peak blood alcohol concentration (BAC). They then completed the 10-item Leeds Dependence Questionnaire (LDQ).<sup>22</sup>

All of these questions were presented as a seamless series of webpages immediately after screening and randomisation. The psychometric performance online of both the AUDIT and the LDQ has been confirmed in a previous study with university students.<sup>23</sup>

The intervention group received personalised feedback consisting of: their AUDIT and LDQ scores with explanation of the associated health risk and information about how to reduce that risk; an estimated BAC for their heaviest episode in the previous four weeks, with information on the behavioural and physiological sequelae of various BACs, and traffic crash relative risk; estimates of monetary expenditure per month; bar graphs comparing the reported episodic and weekly consumption levels with those of other students and the general population of the same age and gender; and hyperlinks for help with drinking problems. Further web pages were presented as options, offering facts about alcohol, tips for reducing the risk of alcohol-related harm, and where medical help and counselling support could be found. A demonstration version of the instrument can be viewed at <http://www.webcitation.org/69vNZW3BA>.

The intervention was developed iteratively over a 10 year period involving consultation with Māori and non-Māori university students, Māori student support services, and with the aid of Māori co-investigators and research staff. This consultation and research yielded an instrument that was appealing to Māori and non-Māori university students such that content was not specific to either group. Notably, on the basis of advice from Māori co-investigators, Māori-specific normative feedback was eschewed to avoid framing Māori student drinking in terms of a deficit model.<sup>24</sup>

### *Outcomes and follow-up*

Five months after randomization, in September 2010, all participants were sent a pre-notice letter and then an e-mail two days later containing a hyperlink to a web-based follow-up questionnaire. Questions concerned the frequency of drinking and amount consumed per typical drinking occasion, all with a reference period of the last 4 weeks. These frequency/quantity measures have been extensively validated<sup>25</sup> and used with this population group.<sup>14</sup> In addition, participants were presented with the 5-item Academic Role Expectations and Alcohol Scale (AREAS),<sup>26</sup> an alcohol problems measure also validated online with university students.<sup>23</sup> The AREAS asks “As a result of drinking alcohol, how often have you experienced each of the following over the past 4 weeks: you were late for a class, you missed a class, you were unable to concentrate in class, you failed to complete an assignment on time?” with response options: once, twice, three times, four or more times; and then “How much do you think your drinking negatively impacted how much you learned, or your grades?” with response options: not at all, a little, quite a lot, a great deal.

There were four planned primary outcome measures: frequency of drinking (range: 0-28 days), number of standard drinks (10 g ethanol) per typical occasion, average weekly volume ((28-day frequency\*typical quantity) / 4), and AREAS score (range 0-15). Secondary outcomes included the prevalence of drinking above New Zealand recommended limits for acute risk (for women and men respectively, >4/>6 standard drinks on one occasion in the preceding four weeks) and chronic risk (for women and men respectively, >14/>21 standard drinks per week in the preceding four weeks).<sup>27</sup>

### *Statistical Analysis*

The analysis plan was constructed *a priori*, and is described in the trial protocol.<sup>16</sup> The four primary outcomes (frequency, quantity, volume and AREAS scores) were analyzed with negative binomial regression with empirical variance using Stata 10.1. For the analyses of the two secondary outcomes (proportions of students exceeding drinking guidelines) we used logistic regression models. The results are presented as risk ratios and odds ratios respectively.

Participants were analysed in the groups to which they were randomized (intention to treat, ITT). We describe patterns of missing values as well as comparing those observed and those missing in terms of baseline characteristics. We compared the baseline AUDIT-C scores, age, and gender of participants lost-to-follow-up versus those followed up to assess whether loss-to-follow-up was differential by randomization group.

We fit two types of models for the ITT analysis. The first yields unbiased effect estimates under the assumption that values are *missing at random* (MAR).<sup>28</sup> In the second, we used the *rctmiss* command in Stata to conduct a *missing not at random* (MNAR) sensitivity analysis with the outcome variable in which the largest effect was observed (volume). We fit a sensitivity analysis with a parameter  $\delta$  that allowed for a difference between unobserved and observed in the group with the larger fraction of missing information. This model allowed there to be a difference between observed and unobserved participants in the intervention group and assumes that observed and unobserved participants in the control group are identical, i.e., conditions that would produce attrition bias. The value  $\delta$  is the multiplicative factor which controls this

MNAR mechanism: the unobserved drink  $\exp(\delta)$  times that of those observed in the intervention group (when  $\delta=0$  this is equivalent to a missing at random assumption).

To account for having four primary outcomes we set the significance level to  $\alpha=0.05/4=0.0125$ . For the purpose of comparison with several recent systematic reviews and meta-analyses, we computed a Cohen's  $d$  as a measure of the effect size for each of the primary outcomes.<sup>29</sup>

We conducted four post-hoc subgroup analyses examining whether gender, age, AUDIT-C score, and university modified the effect of the intervention on the four primary outcomes. Each of these variables was included in the regression models using the *testparm* command in Stata, which produces a  $\chi^2$  statistic for non-linear models.

## **Results**

### *Screening and randomization*

Participant flow, follow-up rates, and the numbers analysed are presented in the Figure. Of 6,697 e-mail invitations, 2,355 (35%) completed screening (Table 1). Of these, 1,789 (75%) screened positive for hazardous or harmful drinking and were randomized to control (n=850) or intervention (n=939). The median completion time for the baseline questionnaire was 1.2 minutes (interquartile range 0.9 to 1.7) and the intervention took a further 4.3 minutes (IQR 3.3 to 5.5) plus reading time, which was not measurable but is expected to have been less than 5 minutes. Table 2 presents summary data illustrating the equivalence of the two study groups at baseline despite the surprisingly large difference in the numbers randomly allocated to each group

which has a 2% probability of occurring by chance in a binomial distribution. Careful checking of the computer program confirmed that randomisation was implemented correctly.

#### *Follow-up assessment*

At follow-up, data were obtained from 682 participants in the control group (80%), and 733 in the intervention group (78%). These included 18 control group participants and 28 intervention group participants who provided follow-up data by e-mail rather than via the website. The median time from sending e-mail invitations to completion of follow-up was 2 days (IQR: 1 to 8 days) in each group.

Loss to follow-up was not differential by group and measured co-variables were equivalent across the groups: among those unobserved at follow-up, women comprised 65% and 63% of the control and intervention groups respectively ( $p=0.59$ ). The mean age of those unobserved was 19.8 and 20.1 respectively ( $p=0.11$ ), and mean AUDIT-C scores were 7.3 and 7.1 respectively ( $p=0.35$ ).

Mean baseline AUDIT-C scores were slightly higher among those unobserved at the follow-up (mean difference 0.37 points, 95% CI: 0.14 to 0.60). Unobserved participants were significantly younger than those observed (mean difference -0.24 years, 95% CI: -0.42 to -0.04 years).

Table 3 presents all outcome data. It can be seen that the majority of this population exceeds thresholds for acute harm (*binge drinking*) but they drink infrequently (just

over once a week on average), thus less than one in five exceed guidelines for chronic harm.

*Primary outcomes.* Table 4 presents results for all primary and secondary outcomes. There were statistically significant effects in the main analysis for three of the four primary outcomes which were robust to the Bonferroni correction for multiple statistical tests. Twelve participants (7 control, 5 intervention) reported extreme values ( $>30$ ) for the number of drinks consumed per typical occasion. With these cases removed, the effect estimate for this outcome attenuated from -8% to -7% ( $p=0.039$ ). Cohen's  $d$  was 0.13 (95% CI: 0.03 to 0.24), 0.09 (0.01 to 0.20), 0.16 (0.06 to 0.27), and 0.13 (0.02 to 0.23) respectively for the primary outcomes in the main analysis.

*Sensitivity analysis.* For a value of delta equal to 0.3, the model yielded results with  $p$ -values similar to the adjusted alpha level ( $z = 0.158699/0.064768 = 2.45$ ,  $p=0.0143$ ). This corresponds to a model where unobserved intervention subjects were drinking 1.35 times [ $\exp(0.3)$ ] as many drinks as observed intervention subjects, while unobserved controls were the same as observed controls.

*Secondary outcomes.* Relative to controls, the intervention group had a statistically significant lower prevalence of exceeding recommended limits for chronic harm but not acute harm. They were 35% less likely to exceed recommended weekly consumption limits.

The subgroup analyses revealed no significant variation in the effects of the intervention by age, gender, or drinking level, on the primary outcomes. There was a difference in the intervention effect by university on the AREAS outcome ( $\chi^2=13.01$ ,  $df=6$ ,  $p=0.043$ ), however, this result is not statistically significant considering the multiple tests performed.

## **Discussion**

Hazardous or harmful drinkers who received e-SBI drank 22% less alcohol than controls five months after randomization, and their alcohol problem scores were 19% lower. Both of these effects were maintained under conservative analytic assumptions. The differences in overall volume consumed were principally driven by reductions in the frequency of drinking, though there was also some evidence of reduced quantity per drinking occasion and there was a large reduction in the prevalence of drinking above guidelines for chronic harm.

The rate of attrition (21%) was lower than that seen in the THRIVE trial, on which this trial was based, where 35% of undergraduates receiving e-SBI were lost to follow-up 6 months after intervention<sup>17</sup> and indeed is lower than in any entirely online behavioural intervention trial of which we are aware. Those lost to follow-up were similar with regard to gender but were slightly younger and heavier drinkers. In no analysis was there differential attrition by randomized group. The sensitivity analysis showed that the results are fairly robust to assumptions about missingness, since it would be improbable that those lost to follow-up are sufficiently different in the intervention versus control groups to account for the observed effects.



In a previous study using a similar e-SBI instrument we found an assessment effect, i.e., hazardous or harmful drinkers who received 10 minutes of web-based assessment of their drinking, in the absence of a feedback intervention, subsequently reported drinking less than a screening only control group.<sup>30</sup> On the basis of those results and systematic review findings<sup>19</sup> we sought to minimise assessment of the control group in the present trial by requesting only demographic information and asking the three questions of the AUDIT-C, which took, on average, 1.2 minutes to complete. It has been suggested that by focusing attention on their drinking, assessment may encourage participants to monitor and then modify their behaviour.<sup>30</sup> Such assessment effects have been found in relation to the 10 items of the AUDIT alone,<sup>31</sup> so it remains possible, though difficult to evaluate how likely, that the intervention effect has been under-estimated in this trial.

Given the relatively small size of the Māori student population, we had to invite all eligible individuals for screening. Accordingly, contamination may have occurred if those who received the intervention discussed their feedback with fellow students or in some other way influenced behaviour in the control group. This too would have biased effect estimates toward the null (Type II error).

Laboratory measures of alcohol-related harm were not appropriate because biomarkers are insensitive to the episodic heavy drinking characteristic of young people.<sup>32</sup> It was also judged potentially counterproductive to seek consent from participants to obtain access to health service records given that contact was so brief and entirely web-based and the population were not seeking treatment. Accordingly, we relied on self-report which is considered sufficiently reliable for alcohol treatment

trials.<sup>32</sup> In addition, there is evidence of greater candour in the reporting of stigmatised behaviours, including hazardous and harmful drinking, when elicited via computers compared with pen-and-paper methods.<sup>33</sup> It does, however, remain possible that participants receiving the intervention were inclined to underreport their drinking to a greater extent than controls which would have biased estimates away from the null. Such a possibility cannot be ruled out with this design and is challenging to evaluate rigorously.

Overall, the observed effects were similar to those found using a similar intervention with university students (7% of whom were Māori) presenting to a health service in New Zealand.<sup>10</sup> One important effect size in this study—a 22% difference in weekly drinking at follow-up—is larger than that reported in a systematic review of conventionally delivered face-to-face brief interventions in primary healthcare (13%).<sup>34</sup> The range of primary outcome effect sizes measured as Cohen's *d* (0.10 to 0.17) is very similar to the range of estimates (0.09 to 0.16) obtained in a meta-analysis of a variety of computerised interventions for drinking among university students, primarily conducted in laboratory conditions or in settings unlikely to be scaleable to widespread implementation.<sup>5-8</sup>

No previous research has examined the efficacy or effectiveness of alcohol screening and brief intervention in an indigenous population trial. Our findings have implications for the provision of health promotion services and the conduct of related research with indigenous peoples in the USA, Canada and Australia, many of whom suffer similarly elevated alcohol-related mortality.<sup>35 36</sup> This population group is relatively well educated such that findings may not generalise to other indigenous

population groups, however, the findings show it is possible to proactively reach a large number of indigenous drinkers via the Internet and engage them in reflection upon their drinking, leading to reductions of public health significance.

Screening participation rates are rarely reported in randomised controlled trials such that it is impossible to know who effect estimates generalize to. The design of this national trial, in which screening rates could be recorded, permits the quantification of generalizability to the third or so of Maori students willing to complete a survey upon a simple e-mail invitation. There is evidence from large scale web-based research projects in the New Zealand university setting that participation of more than 80% can be achieved with pre-notice letters and telephone reminders<sup>9</sup> and around 65% with pre-notice letters alone<sup>37</sup> but such resources are not affordable for annual screening programs.

An orientation in the design of the intervention towards sustainable implementation makes the likelihood of attaining public health benefit a clear strength of the study. The e-mail invitation is practically free (the cost of e-mail traffic only) and could be issued each semester by universities. e-SBINZ was delivered via open source software (<http://www.limesurvey.org/>) that can be easily modified and, as in this trial, housed on a single server for an entire country. An aspiration underlying this programme of research has been to bridge the evidence-practice gap and in this regard it is worth noting that upon receipt of these findings all of New Zealand's universities resolved to implement e-SBINZ routinely from 2012.

### **Author Contributions**

KK conceived of and designed the study with input from JM. The intervention was designed by KK, JM, JC, and JBS. TV assisted in the co-ordination of the study under the direction of KK. SB conducted the sample size estimation under the direction of KK. SB conducted the analyses under the direction of KK and NH. KK led the writing of the paper. JM and JBS contributed to the writing of the paper and all others reviewed and approved the final draft.

### **Competing interests**

No authors have any competing interests. None of the authors were or are in receipt of alcohol or tobacco industry funding.

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Table 1. Screening participation rates, age and drinking data by university

University	Number of eligible students*	Number (%) screened	Number (%) women	Mean age (SD)	Mean AUDIT-C score (SD)
A	994	417 (42.0)	280 (67.2)	19.9 (1.7)	6.8 (2.5)
B	1408	619 (44.0)	391 (63.2)	20.2 (1.8)	5.2 (2.7)
C	603	214 (35.5)	133 (62.2)	20.1 (1.8)	5.9 (2.6)
D	90	34 (37.8)	22 (64.7)	19.3 (1.4)	6.6 (2.8)
E	1116	316 (27.1)	234 (74.1)	20.4 (2.0)	5.7 (2.8)
F	1180	269 (22.8)	186 (69.1)	20.3 (1.7)	5.7 (2.6)
G	1256	486 (38.7)	336 (69.1)	20.2 (1.8)	6.1 (2.8)
<b>Total</b>	<b>^6697</b>	<b>2355 (35.2)</b>	<b>1582 (67.2)</b>	<b>20.2 (1.8)</b>	<b>5.9 (2.7)</b>

\* Students of Maori ethnicity aged 17-24 years at the time of invitation.

^ Women comprised 60% of the Maori university student population aged up to 24 years in 2010 ([http://www.educationcounts.govt.nz/statistics/tertiary\\_education/participation](http://www.educationcounts.govt.nz/statistics/tertiary_education/participation), accessed 15 August 2012)



Table 2. Baseline demographic and drinking characteristics of trial participants

	Control (n=850)	Intervention (n=939)
Females	66.8%	64.3%
Mean age (SD)	20.1 (1.7)	20.2 (1.8)
Mean AUDIT-C score (SD)	6.9 (2.0)	6.9 (2.0)
Drinking summary data <sup>a</sup>		
Drinks alcohol two or more times per week	28.0%	30.6%
Mean standard drinks per typical drinking occasion (SD)	8.4 (4.6)	8.4 (5.3)
Drinks 6 or more drinks per occasion weekly or more often	38.4%	39.0%

<sup>a</sup> From AUDIT-C items

Table 3. All outcome data

<i>Outcome (total N=1415)</i>	Median (25 <sup>th</sup> and 75 <sup>th</sup> percentiles)			
	Control N=682		Intervention N=733	
Frequency of drinking (No. of days drinking in the last 4 weeks)	5	(3-8)	4	(2-7)
Typical occasion quantity (No. of drinks per typical drinking occasion)	6	(3-9)	5	(3-8)
Volume consumed (No. of drinks per week)	6	(3-13)	5	(2-12)
Academic-related alcohol problems (AREAS score)	1	(0-2)	1	(0-2)
Exceeded guidelines for avoiding acute harm <sup>a</sup>	55.6%		51.5%	
Exceeded guidelines for avoiding chronic harm <sup>b</sup>	19.5%		14.8%	

Note: All measures use the preceding 4 weeks as the reference period

<sup>a</sup> Alcohol Advisory Council (New Zealand): no more than 4 drinks (40 g ethanol) in any one occasion for women, and no more than 6 drinks (60 g ethanol) in any one occasion for men.

<sup>b</sup> No more than 14 drinks (140 g ethanol) per week for women, and no more than 21 drinks (210 g ethanol) per week for men

Table 4. Intervention effects

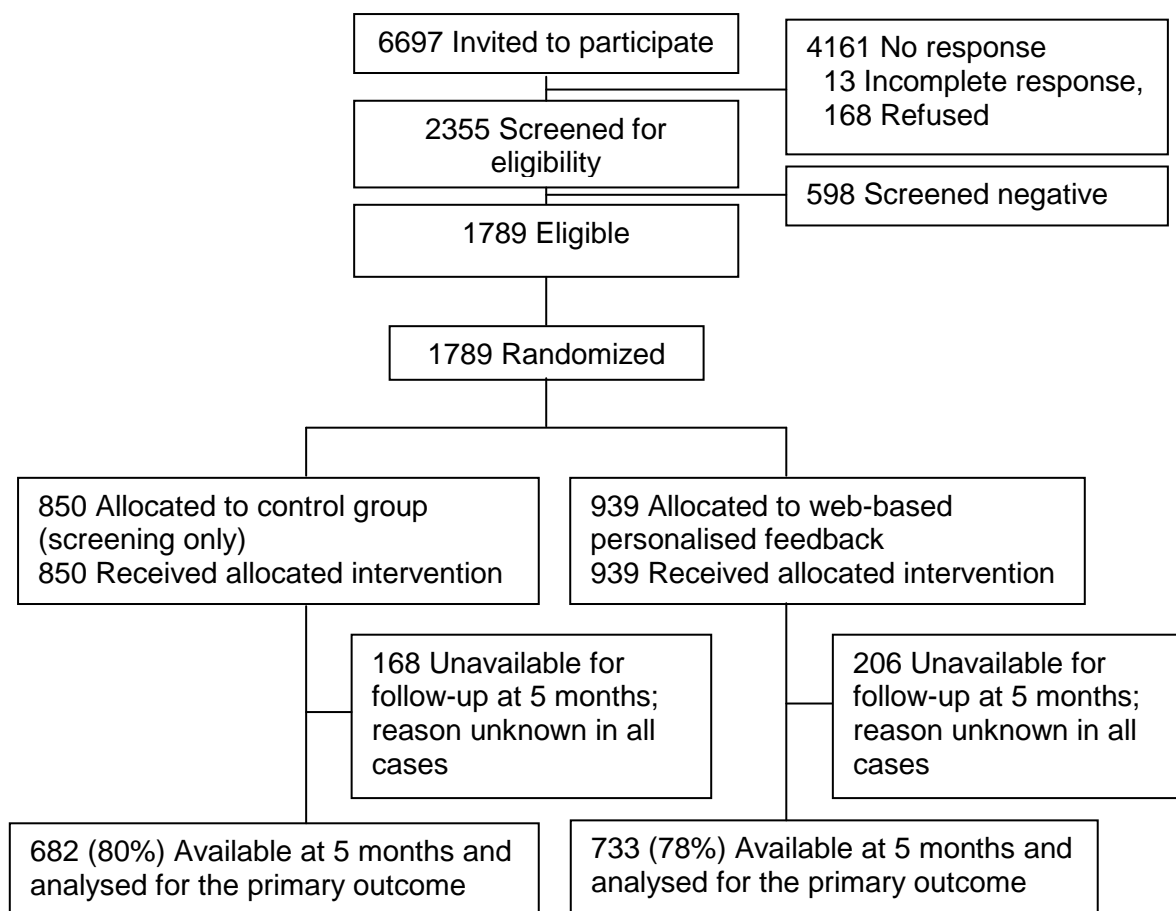
	<b>Intervention/Control</b>	
<i>Primary outcomes</i> <sup>a</sup>		
Frequency of drinking (n=1415)	RR= <b>0.89</b>	(0.82 to 0.97) p=0.01
Typical occasion quantity (n=1414)	RR=0.92	(0.84 to 1.00) p=0.04
Volume of alcohol consumed (n=1414)	RR= <b>0.78</b>	(0.69 to 0.89) p<0.001
Academic problems (n=1368)	RR= <b>0.81</b>	(0.69 to 0.95) p=0.01
<i>Secondary outcomes</i> <sup>b</sup>		
Odds of binge drinking: risk of acute harm (n=1414)	OR=0.80	(0.64 to 1.01) p=0.06
Odds of heavy drinking: risk of chronic harm (n=1414)	OR=0.65	(0.48 to 0.88) p<0.001

Abbreviations: RR, rate ratio; CI, confidence interval

<sup>a</sup> Relative risk ratios adjusted for baseline AUDIT-C score with 95% confidence intervals, from negative binomial regression models.

<sup>b</sup> Odds ratios adjusted for baseline AUDIT-C score with 95% confidence intervals, from logistic regression models

Note. Significant results of the primary analysis after the Bonferroni adjustment for the four primary outcomes (where p<0.0125) are shown in bold.



\*Analyses incorporated participants with a post randomization response. In addition, a sensitivity analysis utilising multiple imputation incorporated all randomized participants in the analysis.

Figure. Trial flowchart