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School-based programmes that seem to work

Useful research on substance use prevention or suspicious stories of success?

Introduction

Unlike the situation some years ago, preventive measures are now often required to be evidence based. Thus, to an increasing extent, the principle of “no evidence, no funding” has been put into practice in many countries. In our field, many interventions have been directed at school pupils, but the research literature indicates that school-based substance use prevention programmes rarely have the desired effect on behaviour (Babor et al. 2003; Foxcroft et al. 2002; 2003). However, there are exceptions, which seem to be encouraging. An important question is whether we can believe these “success stories”, or whether they should be regarded with caution. The present paper addresses this issue, but first I will describe how my interest in the theme was aroused.

■ Youth & Alcohol – a sad story about a recommended school programme

A few years ago, a research group appointed by the Norwegian health and education authorities concluded that only one school-based substance use prevention programme in Norway – *Youth & Alcohol* – could be recommended because its effectiveness had been demonstrated (Nordahl et al. 2006). This pro-

ABSTRACT

H. Pape: School-based programmes that seem to work: Useful research on substance use prevention or suspicious stories of success?

School-based prevention programmes targeted at adolescent substance use rarely seem to have the desired effects on behaviour. It is true that some outcome studies do conclude that such programmes have been successful, but they are relatively few and far between. Even so, one may ask whether the body of published research in this field may originate from unrealistic optimism due to publication bias and underreporting of no or counterproductive effects. Thus, anecdotal evidence suggests that the literature is biased in favour of studies with positive findings. Moreover, nearly all these studies have been carried out by programme developers, and it is well known that researchers with vested interests are more likely to bring “good news” than independent researchers. Indeed, rather than approaching the field with critical reflection, some evaluators have intended to prove that school-based prevention works and have conducted their research accordingly. Examples of questionable analytical approaches and selective reporting of positive findings are consequently not hard to find. The external validity of evaluation studies with favourable outcomes is also often questionable because they almost exclusively have assessed the effects of programmes delivered under

optimal rather than real-life conditions. In conclusion, the empirical “evidence” in favour of school-based substance use prevention programmes is generally weak and does not allow recommendation of widespread dissemination of any specific programme.

■ KEYWORDS

school-based substance use prevention, evaluation research, publication bias, vested interests, research ethics, efficacy and effectiveness studies, external validity, “evidence”

programme had been evaluated by the programme developer and its short-term effects were published in *Addiction* (Wilhelmsen et al. 1994). The reported results were in fact ambiguous: Compared to the control group, adolescents participating in a standardized version of the programme reported significantly less drinking after a couple of months, whereas the opposite was true for adolescents participating in a more flexible version. The study thus indicated that *Youth & Alcohol* might be counterproductive, unless implemented according to detailed instructions and role-specifications. However, fidelity of implementation is often flawed in regular school settings (Cahill 2007; Gottfredson 2001; Dusenbury et al. 2003). Hence, if the results of Wilhelmsen et al.’s (1994) evaluation were taken at face value, one should warn against the programme rather than recommend it.

Only the positive findings from the short-term assessment have been reported in the programme developers’ later publications about *Youth & Alcohol* (Wilhelmsen 1996; Wilhelmsen & Laberg 1996). In their report to the Norwegian health and education authorities, Nordahl et al. (2006) also described the results as if they were unambiguously encouraging. Furthermore, they failed to mention that Wilhelmsen & Laberg (1996) found no effects of the intervention after one year. However, this is a “hidden” result, which the programme developer and her co-author have devoted only one sentence to – in a publication about the programme’s favourable effects in the short run.

In collaboration with two colleagues, I wrote a critical commentary about the evaluation of *Youth & Alcohol* (Pape et al. 2007) in response to the report of Nordahl et al. (2006). Our most important objection was that the central outcome measure was too crude to detect which short-term effects – if any – the programme had had on the pupils’ alcohol consumption¹. The fact that this study was accepted for publication in a high quality scientific journal gives reason for concern. Moreover, quite a few researchers have referred to Wilhelmsen et al.’s (1994) work in their own scientific publications², and they have apparently also failed to detect the serious limitations of the study.

■ The focus of this paper

The story of *Youth & Alcohol* aroused my curiosity with respect to the following questions: Have many other outcome studies also “detected” positive effects of school-based substance use prevention programmes and arrived at an encouraging conclusion on a dubious basis? Does the research literature give a reasonably correct picture of the state of knowledge in this area, or do negative findings have a tendency to be suppressed or omitted from publication? And finally: Can any specific school-based substance use prevention programmes be recommended, or is the scientific evidence for making such recommendations inadequate? In the following, I will address all these questions.

Publication bias

Lack of behavioural effects seems to characterize most of the outcome evaluations of school-based substance use prevention (Babor et al. 2003; Foxcroft et al. 2002; 2003). It is true that some of these studies indicate that favourable short-term changes in behaviour are possible to achieve, but increased substance use and other undesirable effects have also been reported (Brown & Caston 1995; Foxcroft et al. 2002; Werch & Owen 2002). Even so, the research literature in this field is probably biased in favour of “good news” (Brown 2001; Caulkins 2008; Moskowitz 1993; McCambridge 2007; Room 2008; Roona 2008), implying that it can be the source of greater optimism than is justified.

Publication bias implies that the likelihood for research to be published depends on the strength and the direction of the results (Dickersin 1990). The consequence is that “the research literature is deprived

of unpublished findings which may alter the conclusions reasonably drawn” (McCambridge 2007, 467). Variations of this phenomenon probably occur in most research fields, but research with potential implications for policy or practice – such as outcome studies of substance use prevention programmes – may be particularly vulnerable (McCambridge 2007; Roona 2007). Publishing of the results from the evaluation of *Youth & Alcohol* is a case in point (Rossow & Pape 2008). As stated earlier, Wilhelmsen & Laberg (1996) found no indications of programme effects after one year – a result they barely mention in a Norwegian book targeted at the general public. In other words, this negative result has neither been reported in a scientific publication, nor made available for the international research community in the field.

■ The unknown re-analysis of the dataset of Tobler et al.

Recently, several examples of publication bias connected with outcome studies of substance use prevention programmes have been reported (Brown 2001; Cohn 2001; Roona 2008). McCambridge (2007) described an example that is particularly disturbing because it relates to some of the standard references in the field, namely the seminal meta-analyses of Tobler and colleagues (Tobler 1986; Tobler & Stratton 1997; Tobler et al. 2000)³. These analyses, which included findings from more than 200 outcome studies of school-based programmes, were published in easily available journals of high quality.

The results indicated that the programmes generally had little effect, but some were more effective than others.

More precisely, programmes that attempted to foster development of interpersonal skills (life skills training) and those based on active participation from the pupils (interactive measures) were found to show definite superiority.

Tobler and co-workers' research has not merely attracted attention in scientific circles, but also exerted a profound influence on substance use prevention policy and practice in many countries (McCambridge 2007; Saunders 2007). However, a later re-analysis of the meta-analytical data, which was based on a more appropriate statistical approach, did not support the previously published results. The new analysis was conducted by the same research group and the findings were presented at a conference in 2000. The conclusion was that school programmes with different content and educational approaches seemed to be about equally ineffective. This discouraging result was not published until seven years later – in a web site that is unknown to most researchers in the field (Roona et al. 2007).

One of the researchers who was involved in the re-analysis admitted to McCambridge (2007) that the content of the findings played a role in the authors' choice of publication strategy. Thus, lack of support for established "knowledge" about the effect of substance use prevention programmes was indeed an important reason why the paper was not submitted to a scientific journal. This example of publication bias was followed up by the journal that published McCambridge's article (*Drug and Alcohol Review*) – in the form of an editorial (Room 2008) and some invited commentaries (Caulkins 2008; Foxcroft & Smith 2008; Roona 2008; Rossow & Pape

2008; Skager 2008). Apart from this, the case has hardly received attention. Thus, while the work of Tobler and co-workers (Tobler 1986; Tobler & Stratton 1997; Tobler et al. 2000) continues to be widely referred to, the re-analysis of Roona et al. (2007) goes unheeded.

Favouring positive findings

It is not unlikely that editors and referees tend to respond more positively to studies with good news about the effect of substance use prevention programmes than to studies with discouraging conclusions (Caulkins 2008; McCambridge 2007; Skager 2007). However, investigations from other areas indicate that it is primarily the researchers' own choices that create publication bias in favour of specific findings (Weber et al. 1998). There may be various reasons why some researchers withhold their studies from publication, or in other ways ensure that some results are highlighted while others are toned down or omitted. Vested interests are likely to be one.

■ Hope, prestige and profit

Ideally, researchers should not be biased. However, some may have great expectations about identifying positive results when they conduct outcome studies – particularly if they have been involved in the development or the implementation of the intervention that they evaluate. Such expectations can probably influence the research process as well as the way in which the results are interpreted and imparted. In line with this, studies from different fields indicate that researchers tend to be less critical when they evaluate their own than when they evaluate somebody else's interventions (Borman et al. 2003; Eisner 2009;

Lipsey 1995; Petrosino & Soydan 2005). Research about the effects of school-based alcohol and drug prevention is unlikely to be an exception in this respect.

Studies that have concluded that school programmes have the desired effect on adolescent substance use have typically been carried out by programme developers rather than by independent researchers (Gandhi et al. 2007; Gorman 2002). The occurrence of this phenomenon was recently assessed in an article on school-based interventions targeted at violence and substance use, which – based on the positive results from outcome evaluations – had been assigned status as “model” programmes by the National Registry of Effective and Promising Programmes in the USA (Gorman & Conde 2007). Altogether, there were 246 peer-reviewed publications from these studies, and Gorman and Conde revealed that almost all of them had been written by researchers with a potential conflict of interest. More precisely, programme developers were responsible for 78 per cent of these publications, while a further 11 per cent had been written by evaluators who had collaborated closely with the programme developers. Thus, there is obviously a great need for replication studies conducted by independent researchers in the field, but, so far, very few such studies have been carried out (Skager 2007; Weiss et al. 2008). In relation to this, it should also be noted that replication studies of the effects of measures targeted at various forms of problem behaviour, relatively rarely seem to confirm the programme developers’ encouraging conclusions (Eisner 2009).

To develop prevention programmes that turn out to be effective may obviously be

advantageous for a researcher’s career and professional prestige. Researchers may also have economic interests to provide evidence that their programme is effective. Combining the roles of programme developer/programme owner, evaluation researcher and businessman/woman is apparently quite common, at least in the USA (Gorman & Conde 2007). For example, such a mixing of roles has occurred with one of the most quoted programmes in the recent literature about school-based substance use prevention, namely Botvin’s *Life Skills Training* (see www.lifeskills-training.com).

For obvious reasons, research that is funded by the alcohol or tobacco industries has little credibility. There also seems to be general agreement that such economic ties are dubious from a research ethics perspective. However, credibility and ethics of research have rarely been discussed in relation to outcome studies that have been conducted by researchers with commercial interests associated with the programme that they have evaluated. For instance, as far as I know, this principally important issue has hardly been raised in relation to Botvin’s outcome studies of *Life Skills Training*. Yet, as I will soon describe, his evaluation research clearly seems to have been affected by a strong vested interest to document favourable programme effects. Correspondingly, studies from other research areas have revealed that researchers with a financial conflict of interest are far less likely to report negative findings than independent researchers (Eisner 2009).

■ Botvin's dubious effect studies of Life Skills Training

According to Botvin (see Botvin & Griffin 2004 for a review), there is convincing evidence that *Life Skills Training* (LST) works as intended. With references to his own outcome studies, LST is thus promoted as a “research-validated” and “remarkably effective” programme that is proven to significantly reduce adolescents’ use of alcohol, drugs and tobacco⁴. The business has obviously been successful and LST is now one of the most widely used prevention programmes in American schools. The programme has also been identified as an exemplary, evidence-based programme by federal and academic agencies in the USA, such as the Department of Education, the National Institute on Drug Abuse and the Center for Substance Abuse Prevention (see Gandhi et al. 2007). Moreover, in the report from the Norwegian research group mentioned earlier, LST was described as “the best foreign school programme for substance use prevention” (Nordahl et al. 2006, 112, my translation).

However, the evaluation studies behind this success story have been widely criticised by other researchers in the field (Foxcroft 2006; Foxcroft et al. 2002; Gorman 2002; 2005a; 2005b; Brown 2001; Brown & Kreft 1998). After a thorough examination of Botvin’s most important publications on the effects of the programme, which included a re-analysis of published data, Brown (2001, 96) summarized his criticism in the following way: “Based upon the potential combination of analytical alternation, one-tailed tests, aggregation bias, non-equivalent study groups, and unreported patterns of ineffectiveness or drug use increases – there is doubt to

cast over the validity of the LST research”.

Other researchers have stated more directly that Botvin’s research is systematically misleading, and that the programme is unlikely to cause a reduction worth mentioning in young people’s substance use (Gorman 2002; 2005b). Moreover, a researcher who was involved in one of the first LST studies has publically stated that Botvin (Botvin et al. 1990) deliberately failed to report negative findings about his programme (see Cohn 2001). It has also come to light that the tobacco industry in the USA has cooperated with Botvin on promoting LST – with the safe assurance that their own evaluation research had shown that the programme did not lead to reduced smoking among young people, but on the contrary seemed to have the opposite effect (Mandel et al. 2006).

Hypothesis-confirming “research”

As I have already implied, Botvin is not the only researcher who has drawn promising conclusions about the effects of school-based substance use prevention programmes on an inadequate empirical basis. On the contrary, selective reporting of positive findings, and use of analytical strategies and statistical tests that are dubious, but which increase the probability of “documenting” positive effects, have also been revealed in many other outcome studies in this field (Brown & Kreft 1998; Gandhi et al. 2007; Gorman 2003a; Gorman et al. 2007; Moskowitz 1993; Skager 2007). It is also not uncommon to interpret uncertain or inconsistent findings in a positive direction, and to overlook the possibility that intended changes in behaviour may not necessarily have been caused by

the intervention. A related phenomenon is to over-estimate the importance of programme effects that are so small that they hardly make a difference to the prevalence of substance use and related harm in the target population.

Some evaluators have apparently intended to confirm rather than to test the hypothesis that that substance use prevention has desired effects, and have conducted their studies and reported the results accordingly. Botvin and Kantor (2000, 253) have expressed their support for such a strategy in fairly explicit terms: “The most serious challenge to the field of ATOD [alcohol, tobacco and other drugs] abuse prevention has been in *proving* that prevention works” (my italics). Another researcher in the field, Dupont (1998, 214), has expressed similar views: “It is essential that research provide evidence that, when it comes to prevention of addiction, “something works”. In his opinion, this is important because “findings from research that prevention is possible have strong positive effects on funding decisions, since they reinforce the underlying political will to support prevention programs”. Others have pointed out that Dupont’s statement is particularly interesting because it is found in a publication from the National Institute on Drug Abuse, which also finances much of the research of the effectiveness of prevention programmes in this field in the USA (Roona 2008).

Researchers who operate on the basis of a wish or a conviction that substance use prevention programmes work, and who set out to demonstrate that this is the case, violate basic principles of what hypothesis-testing research is all about. Brown & Kreft (1998) have therefore called this ap-

proach “upside-down science”. Similarly, Gorman (2005b, 42) has made the following statement on this theme: “If the field of drug education [...] is truly a science, it should be subjecting its predictions about the effects of intervention programs to genuine and rigorous attempts at falsifications and *not* attempting to verify these hypotheses”. He thus uses the concept “pseudo science” about some of the contributions in the field (Gorman 2003b; 2008).

Positive findings, but only under optimal conditions?

Many outcome studies within the substance use prevention field have been carried out in a context which implies that the external validity of the findings is questionable. The distinction between efficacy studies and effectiveness studies is vital in this respect (Flay 1986; Flay et al. 2005; Glasgow et al. 2003). Efficacy studies are controlled trials aimed at identifying whether interventions work when they are delivered “by the book” and under optimal conditions. The implementation is often under the direction of the involved researchers, and is typically monitored and quality controlled underway. In contrast, effectiveness studies assess the potential outcomes of interventions that are implemented under ordinary, everyday conditions, i.e. under the conditions that normally apply when programmes are delivered on a large scale and without supervision from the programme developer or other external experts. Effectiveness studies may also be aimed at identifying *how* a programme is implemented in practice. A central issue in this respect is to what degree programmes are delivered so that the content, dosage and form are consistent

with the programme developer's instructions and intentions.

The extent to which the conditions for successful implementation of an intervention have been optimized, varies from study to study. Therefore, in practice the distinction between efficacy studies and effectiveness studies may not be clear cut. However, there is reason to claim that research on the effects of school-based substance use prevention programmes is mainly limited to efficacy studies (Hallfors et al. 2006; Rohrbach et al. 2007). Such studies can undoubtedly contribute to important knowledge, but positive results from this type of evaluation research should be regarded as tentative because the generalizability from optimal to "real world" conditions remains uncertain.

Since so few effectiveness studies of school-based substance use prevention programmes have been conducted, little is known about what effects – if any – are possible to achieve when such programmes are delivered under ordinary conditions. However, it is well documented that usual practice rarely is in accordance with the programme developer's instructions (Cahill 2007; Gottfredson 2001; Dusenbury et al. 2003). Modifications and simplifications seem to be the rule rather than the exception, and often incomplete "light" versions of programmes are implemented in reality. Inadequate training of implementation staff, as well as lack of time and economic resources, are some of the reasons why such adaptations take place to such a large degree. Moreover, efforts have a tendency to diminish over time, indicating that the enthusiasm of the pioneer phase is difficult to maintain.

Obviously, positive findings from effi-

cacy studies may not be valid outside the favourable conditions that were present when the effects were assessed. Hallfors and co-workers' (2006) evaluation of Reconnecting Youth – a selective school programme targeted at substance use and psycho-social problems – is one of a few in our field that sheds light on the issue. Their effectiveness study did not support the promising results of the programme developer's efficacy study (Eggert et al. 1994). The discouraging conclusion was that "[the] positive efficacy trial findings were not replicated in this effectiveness trial. All main effects were either null or worse for the experimental than for the control group" (Hallfors et al. 2006, 2254). Hallfors and co-worker also revealed that Reconnecting Youth was difficult to implement, in part because the key personnel in the schools were reluctant and would rather direct their efforts towards ordinary teaching tasks. Moreover, a substantial proportion of the pupils who took part in the programme at the beginning, dropped out underway.

■ Complexity versus practical usefulness

Although school-based substance use prevention rarely seems to work as intended, there appears to be some relationship between effort and effect (Dusenbury et al. 2003). Therefore, the probability of detecting positive results is probably greater for efficacy studies of programmes that are comprehensive and long lasting, and that demand good planning and thorough training of the people involved. On the other hand, such programmes are likely to come out badly if one tries to implement them in ordinary rather than specially-adapted conditions.

Moreover, in practice, resource-demanding programmes have a tendency not to be chosen when the choice includes more simple alternatives (Dusenbury & Hansen 2004). The evaluation by Baklien and co-workers (2007) of a community-based substance use prevention project in Norway is an illustration of this. Qualitative interview data from this study revealed that those in charge at the local level preferred uncomplicated “plug and play” programmes, and that the evidence base for the programmes was of no significance for the choices they made. Such findings provide clear support for the view of Dusenbury & Hansen (2004), who have urged programme developers to simplify their programmes.

Positive findings from efficacy studies should obviously be considered in the light of how demanding of resources the programmes are. If such studies indicate that programmes that are simple to implement also lead to desirable changes in behaviour, there is some probability that effectiveness studies will confirm the positive results. Koutakis and co-workers (2008) have developed a parent-targeted intervention to reduce teenage drinking, which is interesting in this respect. Because it is limited to a short input in the school’s ordinary parent meetings, it is both easy to administer and almost without cost. Moreover, the programme developers’ outcome study gave promising results: “Working via parents proved to be an effective way to reduce underage drinking” (Koutakis et al. 2008, 1629). However, it remains to be seen whether replication studies by other researchers will give similar results, and whether the intervention will be equally effective if it is implement-

ed on a larger scale. Moreover, it cannot be taken for granted that the programme is as easy to deliver in practice as one is given the impression of.

Recommending school-based substance use prevention programmes

The introductory account of the Norwegian research group that recommended the school-based prevention programme *Youth & Alcohol* on a dubious basis, is far from unique. On the contrary, there are many examples of questionable advice and recommendations being given by expertise in this field. In the USA, in order for schools to receive public funding for implementing prevention programmes, the programmes have to be evidence based. Consequently, numerous lists of programmes that allegedly meet this requirement have been produced. However, many agencies have operated with such liberal criteria for scientific evidence that even outcome studies with serious limitations have been assessed as good enough. For example, many school-based substance use prevention programmes have been given the status of evidence based, with no other documentation than apparently positive findings from more or less solid efficacy studies carried out by the programme developers (Gandhi et al. 2007; Gorman 2005c; Weiss et al. 2008). In many cases, the magnitude of the effects, whether the effects are clear, how long they last, and whether the programmes seem to be cost effective, have not been considered. Such factors were also not taken into account when Nordahl et al. (2006) concluded that *Youth & Alcohol* was the only substance use prevention school programme in Nor-

way that was evidence based.

Researchers associated with *the Society for Prevention Research* have developed standards to assess whether there is a scientific basis for implementing preventive measures on a large scale (Flay et al. 2005). At the overall level, the requirements are that there must be unambiguous, positive findings from several outcome studies of high quality, that the research is not limited to efficacy studies, and that there is documentation that the programme can be carried out in practice. It must also be documented that the programme leads to effects of a reasonable magnitude and durability. If such criteria were taken account of, it would hardly be possible to recommend a single school-based substance use prevention programme – which Flay et al. (2005) also seem to be aware of.

Even if some of the above-mentioned requirements for scientific evidence were modified, it would still be difficult – for reasons that have already been given – to give soundly-based recommendations in this area: Despite the fact that numerous outcome studies of school-based preventive measures have been carried out, the amount of high quality research is still limited (Foxcroft 2006; Foxcroft et al. 2002; 2003). Moreover, most of the published studies have only measured short-term effects, and lasting changes in behaviour seem to be much harder to achieve. There are also very few effectiveness studies of school programmes. Another weakness is that outcome evaluations with apparently positive findings have mainly been carried out by programme developers and that confirmatory results from replication studies conducted by independent researchers seem to be conspicuous by their absence.

The present body of research also gives no reason to claim that some school-based substance use prevention programmes are better than others. According to Gandhi et al. (2007) and Gorman (2005c), even the programmes that have been most warmly recommended by expert groups and governmental agencies in the USA cannot show well-documented effects on pupils' substance use. After having carefully reviewed the research behind these programmes, Gandhi et al. (2007, 67) made the following reflection: "Our findings [...] raise the question of whether *any* school-based prevention programme will substantially reduce the number of teens who experiment with drugs" (my italics). Other reviews of the literature point in the same direction: The meta-analysis of Roona et al. (2007) revealed, as previously mentioned, that no types of programmes came out better than others. Thus, they all seemed to be more or less equally ineffective. Similarly, Foxcroft and co-workers (1997) found no systematic differences in content and educational approach between the programmes that seemed to reduce teenage drinking and those that showed either no or counterproductive effects. Therefore, they concluded that "no one type of prevention programme can be recommended" (Foxcroft et al. 1997, 531).

Conclusion

Critical reviews of the research literature indicate that "success stories" of school-based substance use prevention programmes must be regarded with great caution, and that solid evidence in favour of such programmes is missing. I would still argue that there is no reason to throw the baby out with the bathwater. However, it

seems to be timely to redefine the aim of school programmes. In this regard, Room (2005) has stated that unrealistic aims to reduce pupils' substance use should be abandoned, and that factual teaching about the theme should be strengthened instead. On the background of very discouraging findings from a study of Norwegian teenagers' knowledge about alcohol and drug use, Pape et al. (2006) have supported his recommendation.

It is particularly important to disseminate information that control policy, high prices and restriction of the availability of alcohol lead to reduced drinking and less alcohol-related harm in the population (see e.g., Babor et al. 2003; Room et al. 2005; Wagenaar et al. 2009). In this respect, Craplet (2006) pointed out that only education can make a restrictive alcohol

policy acceptable, and he is probably quite right. Hence, measures to raise the level of knowledge with respect to the effects of such a policy should not only be directed towards adolescents attending school but just as much towards decision-makers and other influential target groups. Reaching the general adult population is obviously also very important – not least because politicians are unlikely to put a more restrictive alcohol policy into practice unless they have the voters' support for doing so.

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NOTES

- 1) Three separate questions – without a specified time frame – were asked to assess the respondents' frequency of drinking beer, wine and spirits. The response categories were “never” (coded 0), “hardly ever” (1), “every month” (2) and “every week” (3). The answers were added and the resulting sum-score was the one and only measure on alcohol use in the study. However, since different drinking patterns may give the same score, the measure is unfit for assessing changes or stability in drinking over time. For instance, there will be no distinction between respondents who hardly ever drink beer, wine and spirits (1+1+1), and respondents who drink beer weekly but who never consume other alcoholic beverages (3+0+0).
- 2) Foxcroft et al. (1997; 2002; 2003); Graeff-Martins et al. (2008); Hanson (1996); Holmila et al. (2008) and Tresidder & Toumbourou (2007) are some of the researchers who have referred to Wilhelmsen et al.'s (1994) study in their own publications. Moreover, Tobler et al. (2000) included the study in their meta-analysis.
- 3) According to Google Scholar's registrations, the three articles (Tobler 1986; Tobler & Stratton 1997; Tobler et al. 2000) have been referred to 529, 393 and 291 times respectively. The search was carried out on the 11th of December 2009.
- 4) See <http://www.lifeskillstraining.com>

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