Effects of opening off-premises alcohol outlets in rural Iceland on youth alcohol consumption

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Abstract

In Iceland, off-premises sales of alcohol are restricted to the state-owned alcohol monopoly ÁTVR. Since the early 1990's, ÁTVR has been opening new outlets in small towns in Iceland where there previously was no outlet. Many of the towns were, and are, isolated and far from the next outlet. The aim of this paper is to find if alcohol consumption of adolescents in towns without an outlet change following the opening of one.

The data set used covers 11 years (1997-2007) and 14 towns with a total of 4349 observations, where teenagers 14 and 15 years old answer questions about their alcohol and moonshine consumption. Using a difference-in-differences model, I find that following the opening of an outlet, alcohol consumption and moonshine consumption fall significantly. This runs contrary to common belief and earlier studies. The result also holds true for a subset of heavy drinkers (20 or more lifetime instances of alcohol use). Distance to the nearest outlet seems unrelated to alcohol consumption. The paper proposes five different reasons for this that can be tested using available data.

Contents

1 Introduction

A variety of methods has been used around the world to curb alcohol consumption. One of the approaches Nordic countries have used is constraining off-premises sales of alcohol to state-owned monopolies. Today, Sweden, Iceland, Finland and Norway limit sales this way, whereas Denmark has chosen to liberalize sales. In assessing the overall effects of these restrictions and attractiveness of the policies, it is central to estimate the effects it has on alcohol consumption.

In Iceland, off-premises sales of alcoholic beverages above 2.25% are restricted to the State Alcohol and Tobacco Company of Iceland, ÁTVR[1]. In 1990, ÁTVR had 26 outlets, 14 of which were outside the capital area. Since then, 22 outlets have been opened, the last one in Flúðir in 2009. The drive towards opening up new shops has had many reasons. One is of demand for better service, another of a more liberal view towards the ÁTVR and alcohol consumption in general. However, the monopoly is constrained by running a profitable business, *de facto* constraining each outlet to be profitable. When it began cooperating with shop owners across the country, it could dramatically lower the costs of running an outlet and thus open up new ones faster than before.

Since 1997, teenagers in 9th and 10th grades in all schools in Iceland have answered questionnaires about various aspects of their lives, including alcohol and moonshine consumption. However, as the data is sensitive, restrictions have been put on the usage of the data, as described further below. The data set comprised of more than 4200 observations from 14 schools in rural Iceland for 11 years, 1997–2007. The schools are grouped in three groups, those in towns that got a new outlet in 1999, those in towns that got a new outlet in 2000–2 and those schools that did not have an outlet in the period, used as a control group.

A priori, it is natural to assume that alcohol consumption will increase after the introduction of a new outlet. Similarly, it is a reasonable first guess that if alcohol consumption increases, consumption of moonshine or in general home-brewed alcoholic beverages will decrease.

[1] Off-premises sales refer to sales of closed containers of alcohol, whereas on-premises refers to sales of alcohol to be consumed on the premises, such as bars and restaurants.

Using a difference-in-difference model, the effects on alcohol consumption are estimated. The questions this paper tries to answer are:

1. What effects does opening a new off-premises outlet have on alcohol consumption among adolescents in rural Iceland?
2. If alcohol consumption increases, does consumption of moonshine decrease?

The rest of this paper is organized as follows. Chapter two describes the setting and background information. Chapter three covers theory and previous studies. Chapter four describes the data used and the empirical setup . Chapter five reports results and the paper is closed with a chapter of discussion and conclusions.

2 Setting

Iceland is a country of a relatively homogeneous population of around 300.000 people. Around 60% of the population lives in Reykjavík or its vicinity. Because of ease of measurement, focus here is on towns outside this capital area, and is referred to, somewhat inaccurately, as *rural Iceland*[2].

Alcohol consumption has been on the rise in Iceland for a long time, save for the last years following the economic meltdown. In general, Icelanders as their Nordic countrymen, drink seldom and much each time. In short, they binge drink more than other Europeans, especially Mediterraneans who drink often and little each time (Mäkelä et al., 2006).

In line with other Western countries, Iceland prohibited alcohol in 1915. The ban was lifted in 1935 but only for strong spirits and wine. The beer prohibition was not lifted until 1989. Alcohol policies continue to be restrictive. For example, all advertising of alcohol and tobacco is forbidden. Taxes on alcohol are similar to those in the other Nordic countries, which in international comparison are quite high (CEPS, 2009).

The State Alcohol and Tobacco Company of Iceland, ÁTVR, controls off-premises outlets (retail outlets) for alcohol above 2.25% strength by volume. The objectives are implicitly

[2]Here, vicinity of Reykjavík is defined as municipalities of Kópavogur, Hafnarfjörður, Garðabær and Seltjarnarnes. This definition varies over time; urban sprawl and better roads have increased the commuting area of Reykjavík.

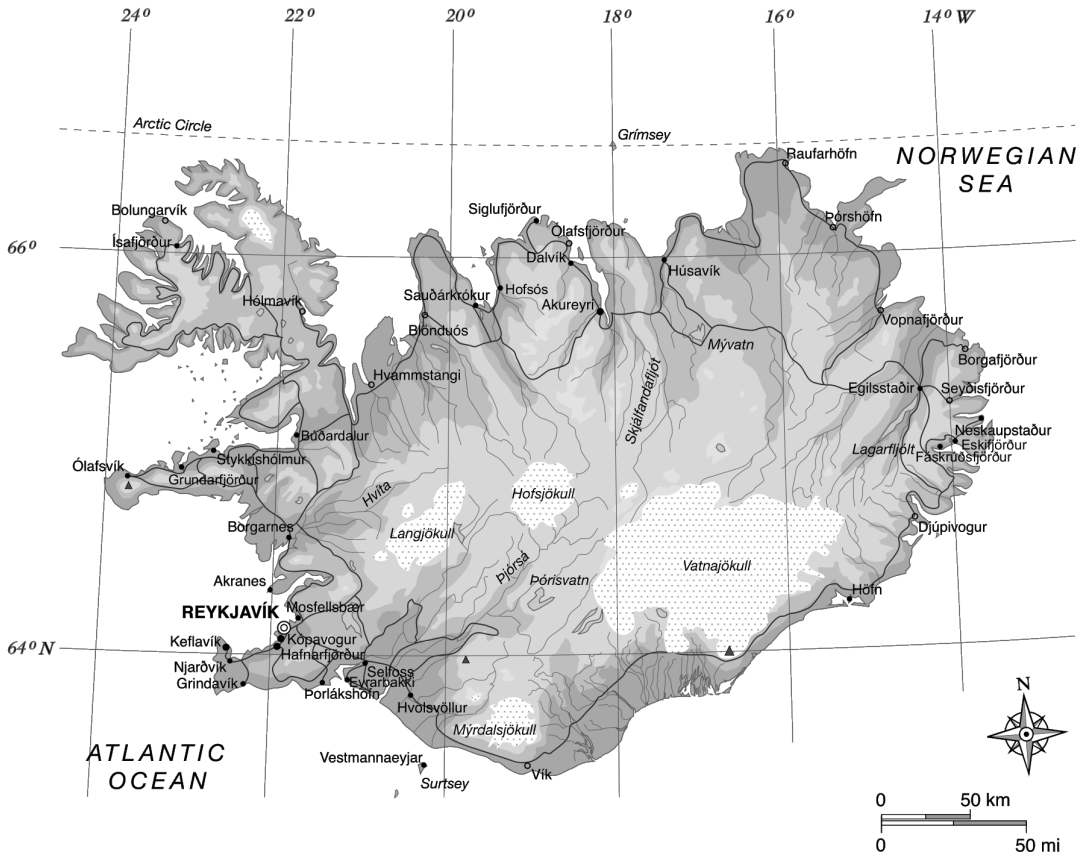

Figure 1: Map of Iceland. Based on a map from Wikimedia commons. Towns included in the study are listed in appendix A on page 37.

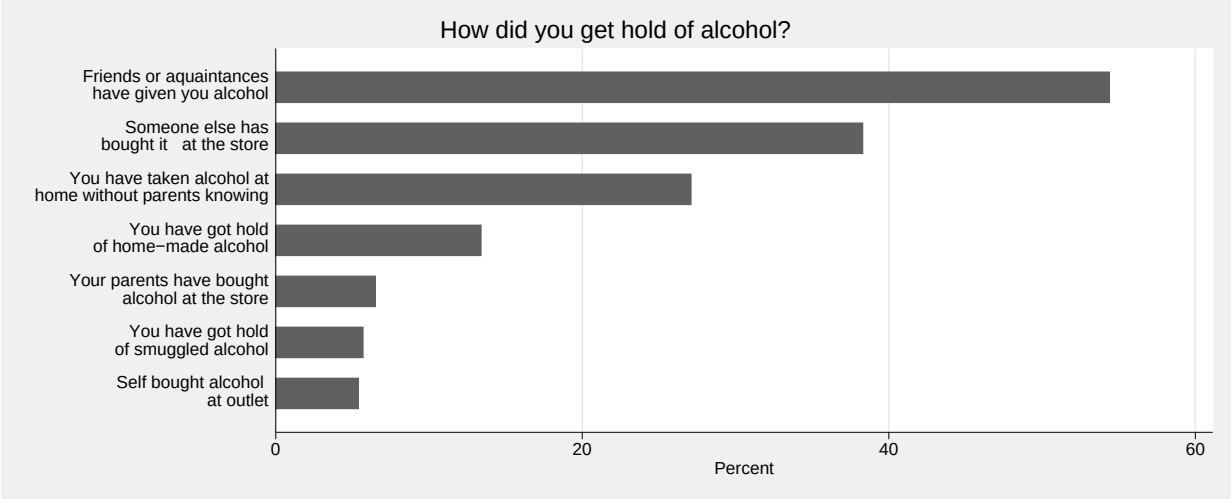

Figure 2: How youth got hold of alcohol. Respondents could answer positively to more than one choice so the sum does not necessarily have be 100%. (Lýðheilsustöð, 2007)

to earn a profit and promote responsible alcohol consumption. Thus, outlets are only opened where it is deemed profitable.

In 1990, 14 outlets were outside the capital area and 11 within it. In 2010, 36 outlets were found in rural communities.

In 1987, ÁTVR started to experiment with opening small outlets. In the village Ólafsvík, it rented a small space adjacent to a store with children's clothes. By striking a cooperation deal with the store owner, it could cut its operating costs significantly. This model has since been followed and many outlets opened. The scale of the sales is little, and some stores are open as little as one hour a day. ÁTVR has however firmly noted that no compromises have been made as for controlling if under-age people can buy alcohol (Vinbudin.is, 2007).

The minimum age for buying any alcohol is 20 years by the day. Over half of those asked in the Icelandic part of the European School Survey Project on Alcohol and Other Drugs (ESPAD) in 2007, were given alcohol by friends or acquaintances. Almost 40% had someone, except for parents, buy them alcohol. A quarter stole alcohol from their parents, see figure on page 7 (Lýðheilsustöð, 2007).

The minimum age for getting a driver's license is 17, at which age the vast majority indeed gets its license. The group of focus here is adolescents aged 14 and 15. Not

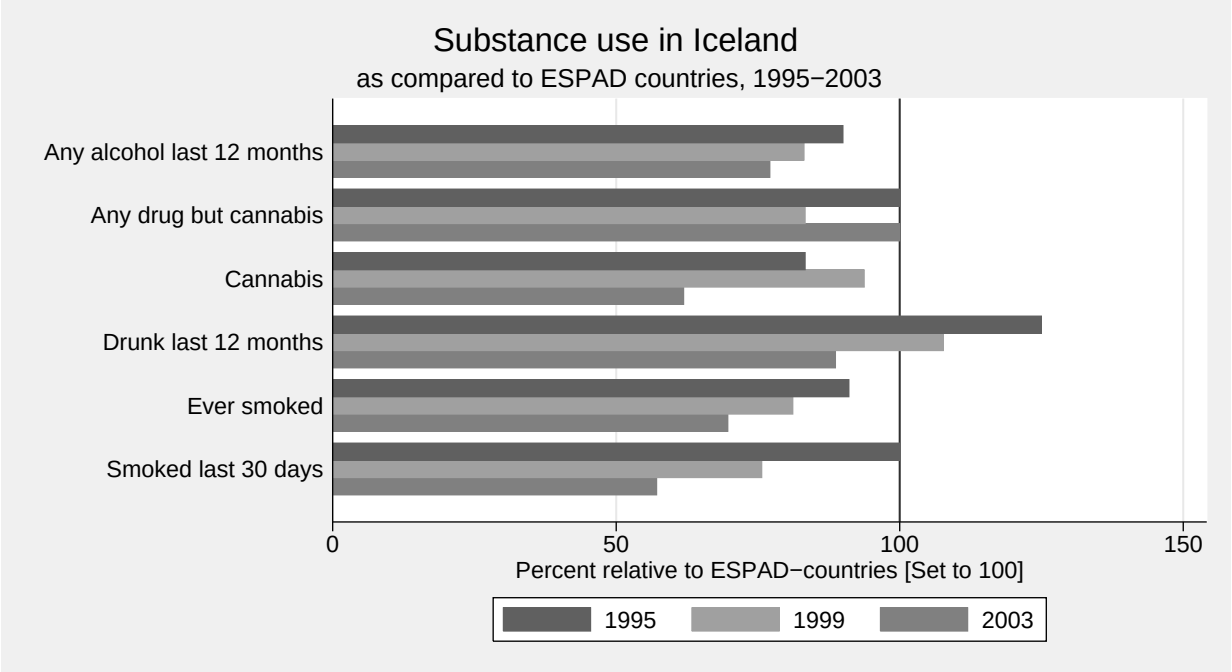

Figure 3: Proportion of substance use for nine substance-use behaviors among Icelandic students compared to the average use of students in 34 other countries participating in the European School Survey Project on Alcohol and other Drugs (ESPAD). Sources: Hibell et al., 1997, 2000, 2004, own calculations.

having a driver's license, they have limited mobility. Public transportation between towns is little or non-existent, further exacerbating the low mobility.

Since the early 1990's, alcohol consumption among adolescents has fallen significantly (Sigfúsdóttir et al., 2009). This is illustrated in two figures. One is figure 3, where the average for ESPAD-countries (mainly EU-countries) is normalized to 100. Between 1995 and 2003, relative consumption decreased in all but the catch-all category 'drugs other than cannabis'.

The other figure is number 4 on the next page showing the general trends in consumption. The data in the latter figure is the data used in the analysis in this paper, and will be described better in later chapters. Sufficient to say, number of times teenagers have drunk alcohol or moonshine, or gotten drunk in their lifetime, has been on the decline in recent years.

This trend has been attributed to prevention programs (Sigfúsdóttir et al., 2009).

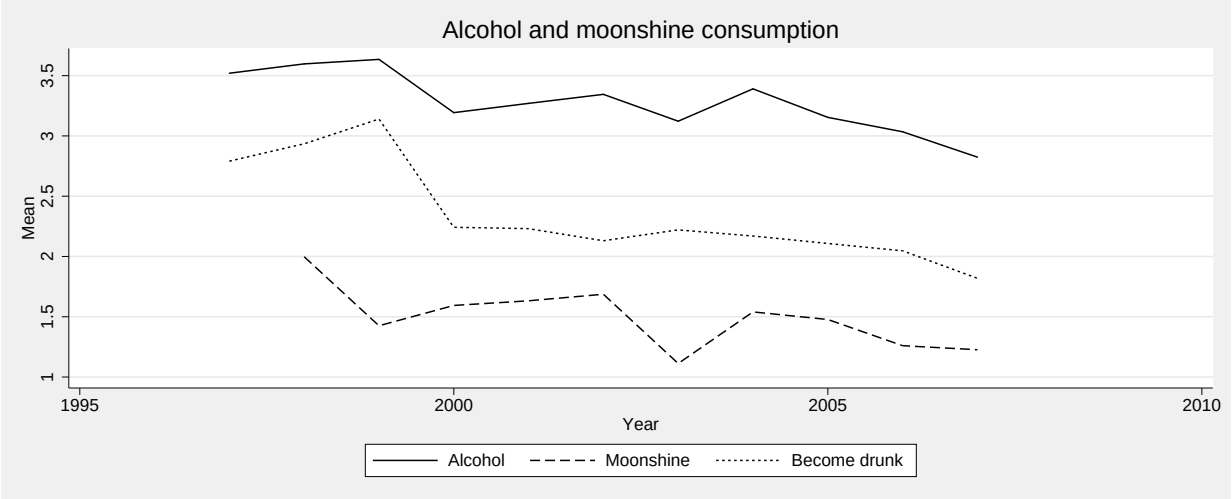

Figure 4: General trends in consumption. Unit of measurement is a discrete scale from 1–7. All variables refer to lifetime consumption.

3 Theory and previous studies

3.1 Consumption should rise: availability theory

Availability theory is a set of linkages used to try to find if there is a connection between the general notion of availability of alcohol on the one hand, and social problems and health on the other. As a first approximation, standard availability theory does a good job. In its purest form, it links availability to alcohol-related problems.

1. As the availability of alcohol in a community increases, the mean consumption of its population also increases.

2. As the mean alcohol consumption in a population increases so the number of heavy drinkers increases.

3. Heavy drinking is associated with adverse health and social outcomes and as the number of heavy drinkers in a population increases, so too does the level of alcohol-related health and social problems (Livingston et al., 2007, p. 216).

However, as has been pointed out (Stockwell and Gruenewald, 2004), the links are not always strong; greater availability does not always lead to more drinking, and more drinking does not always wreak havoc or even cause troubles.

There have been attempts at refining or expanding the theory. An attempt by Stockwell and Gruenewald (2004) is an example. Their definition is in four parts, but the first is of interest here, corresponding to the first in the original version:

> 1. Greater availability of alcohol in a society will increase the average consumption of its population when such changes reduce the "full price" of alcohol, i.e. the price plus the convenience costs of obtaining them (Stockwell and Gruenewald, 2004).

In the revised edition, the real price of alcohol is its retail price plus the convenience costs of obtaining them. Under-age adolescents have to get the alcohol through different canals than adults, by stealing, getting their parents or older friends to buy alcohol for them or by any other means (see again figure 2 on page 7).

According to the availability theory, increased availability will increase average consumption. This can be expected to happen when an outlet opens in town. It will simply be easier and less costly to get hold of alcohol.

3.2 Consumption should fall: five hypothesis

However, there are forces working in the opposite direction. When real price is considered as opposed to the crude measure of availability, there is uncertainty as for if a new outlet in a town—and thus better access for adults—translates into better access for those under age. Here, I present five hypothesis for why this could happen. The list is by no means exhaustive and untested, but illuminate the complexity of the question at hand.

1. Stocks and stealing

As can be seen on figure 2 on page 7, 27% of respondents in the ESPAD research answer that they got their alcohol from their parents without the parents knowing about it. In other words: more than a quarter stole alcohol at home. How this relates to opening outlets requires a set of reasonable generalizations and assumptions.

1. Adults respond to a new outlet by buying alcohol more frequently and in less quantity each time[3]

2. In towns without an outlet, adults will have bigger stocks of alcohol at home. When an outlet opens in town, stocks at home will shrink.

3. When stocks are big, stealing is easier because of less oversight by parents.

Therefore: it is harder to steal alcohol when there is an outlet in town.

The ESPAD data has not been broken down by towns, but doing so would reveal if this hypothesis is true, that is, if there is a significant difference in ways of obtaining alcohol depending on location or existence of outlet in town.

A variant of this story concerns the consumption patterns of adults. If adults move from moonshine to legal alcohol, the stocks of moonshine at home will fall. If it is the case that moonshine is more easily stolen, because of higher quantities owned or different storage methods, it will be further more difficult to steal. Point 4 below examines a different externality of this move of adults from illicit to licit alcohol.

Looking again at figure 2 on page 7, it can be seen that most get their alcohol from friends. The effect of less stealing does then not have to be widespread to have far-reaching effects on the supply of alcohol for groups of friends.

2. Small tightly knit communities

The setting for this research is small towns. Not only is the capital area excluded entirely from the research, bigger rural towns are also in effect excluded as they would all have an outlet before 1997. The population of towns in the data set is listed in appendix A on page 37, and varies from 238 to 2701. This means that these communities are all tightly knit and employees at the counter will have a good chance of spotting suspicious buying patterns and know personally its customers.

The effect can take two major forms. One is that an adult who buys suspicious amounts or has a strange mix of beverages can be spotted or shys away from buying because of the risk. The other form is, that it is more difficult for teenagers to get away with

[3]No data is readily available for Iceland, but this has been established by Norwegian research (Hauge and Amundsen, 1994). It is reasonable to assume it to hold in Iceland as well.

buying alcohol. Obviously, even if you look mature, it is harder to pass as a 20 year old if everyone in town knows you are still in 10th grade. This effect is not as pronounced when alcohol has to be bought in another town, but can nevertheless exist to different degrees.

3. Post orders

In small towns, post order is one of the ways to get alcohol. In short, you call the nearest outlet and order. A day or two later you pick it up at the post office or the truck drops it of at your house. Personal or anecdotal evidence suggests that it is somewhat easier for adolescents to buy alcohol this way, using a name and i.d. of an acquaintance (who may or may not be involved in the ordering and pick up).

When an outlet opens in town, post-orders disappear completely. Not even rarities or long-tail products are sent by post, but delivered at the outlet instead. As in point 2, the tightly knit community should inhibit this. No reliable data is available here so the size or indeed the existence of this effect is disputable.

4. Moonshine prices rise

If adults move from illicit to licit alcohol, production of moonshine can be expected to fall. Price, real or nominal, can then be projected to rise. Assuming (unreasonably, but instructively for the argument) that real alcohol prices are unchanged, total consumption is likely to fall because of budget constraints[4].

5. *Love for variety or quality differences*

People love variety. Or maybe people like the alcohol at the store better than the garage-made moonshine from the next-door neighbor. However the reason, if real costs of obtaining licit alcohol falls, we still might see a fall in *total consumption.* The reason is that in monetary terms, licit alcohol is more expensive. Cash-strapped teenagers might start buying more of it and thus have less money to buy moonshine. This theory predicts a slight increase in consumption of licit alcohol but a bigger fall in moonshine

[4]Baruch and Kannai (2001) claim to have established that the Japanese drink shochu, similar to moonshine, is a Giffen-good. Whether moonshine is a Giffen-good for teenagers in rural Iceland is an open question and not of discussion here.

consumption, and more importantly, a fall in number of times adolescents have gotten drunk. A substitution from quantity to quality, in short.

3.3 Earlier studies

To get an overview of the earlier studies it is useful to divide the attention in two; research methods and questions asked. Three main research strategies can be employed to estimate the relationship between outlets and alcohol consumption, namely cross-sectional studies, natural experiments and time series analysis.

When it comes to the question asked, it will obviously vary across societies, interest of researchers and, alas, availability of data. The first dimension is the group studied. Different groups have different reactions to varying alcohol availability. Men do not necessarily change their consumption patterns the same way women do, adolescents do not adjust the same way as older people. The reasons for these differences are sometimes clear, sometimes not. Mobility is likely to cause different adjustment mechanism to changes in alcohol availability. Thus, it is invalid to use data on changes in alcohol consumption among adults and extrapolate upon teenagers who do not have a driver's license or are immobile. The distinction between casual drinkers and heavy drinkers is also of interest as it has been shown that heavy drinkers react in other ways to changes in availability.

Another dimension is whether variation in density of off-premises outlets is studied, if on-premises outlets are the focus, or a mixture of both. Studies can for example look at effects of dismantling alcohol monopolies or of off-premises sales being liberalized.

The third dimension along which studies vary is the type of drinks, such as if the sales of beer is liberalized and allowed in regular stores, or if total beer prohibition is lifted, as in Iceland in 1989.

Evidently, the field is quite wide, and radically different effects to be expected depending on settings. Here, it is better to refer to two papers summarizing the research in the field, Livingston et al. (2007) taking a wide view on the topic and Room (2002) summarizing the research done so far in the Nordic countries. More specifically, Mäkelä et al. (2002) summarize this narrower field of research in the Nordic countries.

In this study, we will be studying adolescents, using a quasi-natural experiment to study the effects of variation in existence of off-premises outlets.

Of most interest here are two lines of research. One is from the 1950's in Finland, the other from Norway, described in turn.

3.3.1 Finland

According to laws passed in 1932, the sale of beverages with more the 2.25% alcohol was constrained to the Finnish State Alcohol Monopoly, which only set up stores in cities and market-towns. These restrictions applied only to off-premises sales; on-premises sales were allowed subject to a regular restaurant license. As the cities and market-towns were relatively few, rural areas were many quite far away from the next outlet. However, this was nothing new, the rural areas had been effectively *dry* since 1902 (Kuusi, 1957). In 1951, ideas arose about lifting the restrictions somewhat and allowing for beer to be sold in grocery stores in rural areas. As expected, a fierce debate ensued as for the expected results of such a policy change. It was then decided to assess the real effects by conducting an experiment.

Simply put, the alcohol consumption was to be measured in a number of small towns, before a randomized part of which would get more liberal rules regarding beer and wine sales (and later spirits). After the effect, alcohol consumption would be measured again. Three towns were affected, two towns acted as controls.

For those who already did consume alcohol, volume of drinking increased by 10–40% depending on municipality. Stronger even were the effects on frequency, which rose even more. Interestingly however, is the fact that infrequent drinkers and abstainers were unaffected by the liberalization. That stretched as well to boys and girls aged 15–19 to a certain extent, further strengthening the difference between the effect on frequent drinkers and infrequent ones. Kuusi notes, "this conclusion is a natural one. It is understandable that the abolishment of restrictions on the sale of alcohol causes reaction—if any—primarily among those users of alcohol who have previously been forced to adjust their use to the existing sales restrictions."(Kuusi, 1957)

3.3.2 Norway

As in other Nordic countries, Norway limits its off-premises sales to state monopoly outlets. These have become more widespread in smaller towns in the last four decades and have inspired or triggered a line of research as for the effects this has had.

In research spanning 1961–91, adolescents were not included (Mäkelä et al., 2002, Hauge and Amundsen, 1994) but in 1999, adolescents were (Horverak, 2004). All studies have in common being conducted in towns with considerable travel distances to other outlets, but with beer available in grocery stores.

For adults, the opening of outlets did not increase the proportion of alcohol consumers nor the total consumption of alcohol, but shifted consumption from moonshine to legal spirits, and increased wine consumption slightly. People also tended to buy wine and spirits more often, but in smaller quantities each time with total consumption not increasing. Where beer had been available before the outlet opened, its consumption did decrease somewhat (Mäkelä et al., 2002). In 1999, heavy drinkers (those drinking more than a bottle of wine and a bottle of spirits per month) did increase their consumption significantly, men by drinking more spirits, women by drinking more wine (Horverak, 2004).

Among people aged 16-17, there was no significant increase alcohol consumption. The share reporting they usually drank wine rose. As expected, there was a marked shift from illegal spirits to spirits bought at Vinmonopolet, but no change was found in beer consumption (Horverak, 2004).

Of interest is that in the 1991 research intoxication frequency increased somewhat among women, as did total consumption among women and older people. This, Hauge and Amundsen (1994) hypothesize, has to do with mobility. Those groups that traditionally have been less mobile are thus affected differently than those who are mobile—namely adult men.

However this hypothesis is not consistent with the overall finding that the group by far least mobile, that is adolescents below driving age, do not seem to be affected all that much by the changes. I dare to speculate that refining the hypothesis somewhat would

solve this problem. By assuming that adolescents get their alcohol through adult men, the hypothesis holds. How reasonable this assumption is, however, remains an open question.

3.4 In general

However, what Mäkelä et al. (2002) point out about the Norwegian studies hold also true for other similar studies; the evaluation has only assessed possible short-term effects, and hence the long-term effects of increased availability may differ from those observed here.

It is important not to take these results too far. A more drastic change, such as the elimination of wine retail monopoly can have a considerable effect to increase alcohol consumption (Wagenaar and Holder, 1995). The studies have to be taken for what they are and in what context they are conducted.

4 Data and empirical setup

4.1 Alcohol consumption

In 1997, The Icelandic center for social research and analysis (ICSRA) started gathering yearly data on lives and conditions of all adolescents in Iceland, in class 9 and 10 (14 and 15 years old). Similar researches had been done before, but not yearly. A significant portion of the questions are asked every year, but every three years, an extended version is used with more wide-ranging and in-depth questions. The questionnaires are given to teenagers at school. What makes the undertaking valuable is that 73–90% do answer the questionnaires. The reasons for not participating are mostly that teenagers were not at school the particular day, or the school for some reason did not participate (Sigfúsdóttir et al., 2008)[5]. The researchers claim the drop-out can be regarded as completely random.

The subset of data used here are observations spanning 11 years, from 1997 through 2007, summarized in table 1 on the following page. 14 towns and villages are in the

[5]Participation was 73% in 2007 according to correspondence with authors.

Number of observations	4349
Number of years	11
Number of schools with new outlets in 1999	5
Number of schools with new outlets in 2000–2002	4
Number of control schools	5

Table 1: Summary of data

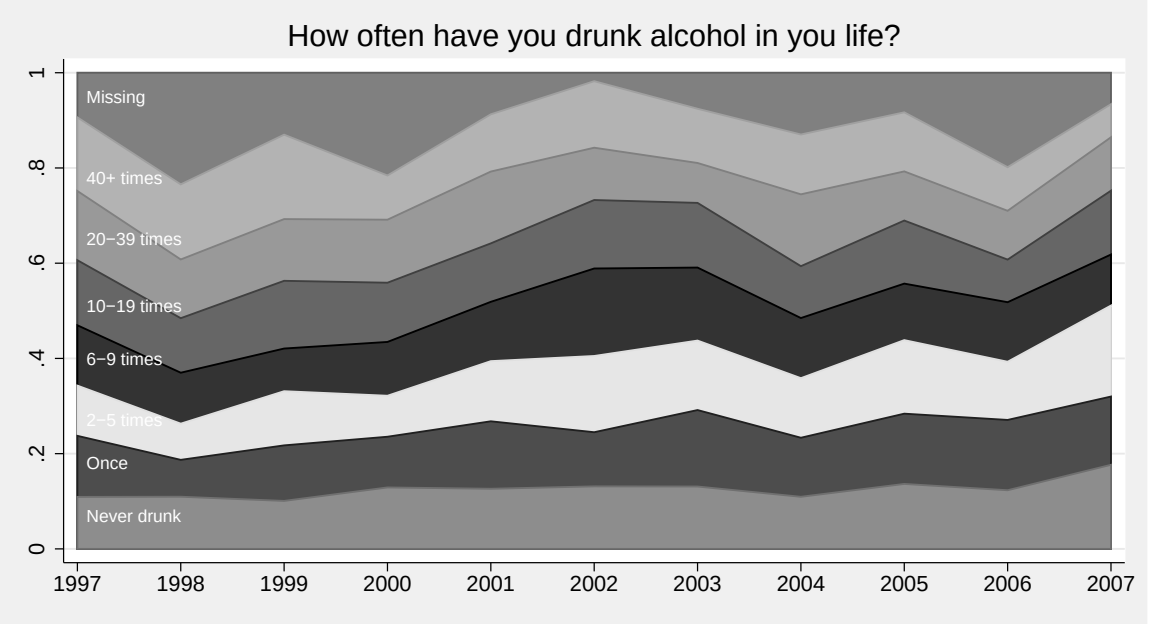

Figure 5: Reported alcohol drinking in the data set.

data set, five of which getting a new outlet in 1999, four getting outlets in the years 2000 through 2002 and five never getting an outlet, thus serving as a control.

Due to the sensitive nature of the data, several restrictions had to be put in place before the research could start. As already noted, the four towns getting an outlet in the years from 2000 through 2002 are grouped into one group. Importantly, a set date had to be put for the outlet starting year, and was assumed to be 2001, the year in the middle.

Alcohol consumption is measured with the question "How often have you used alcohol in your life?" The answers are coded 1=never; 2=once; 3=2–5 times; 4=6–9 times; 5=10–19 times; 6=20–39 times; 7=40+ times. The data is summarized graphically in figure 5.

Moonshine consumption is measured using a question with the same wording *mutatis mutandis*. The data is summarized in figure 7 on the following page.

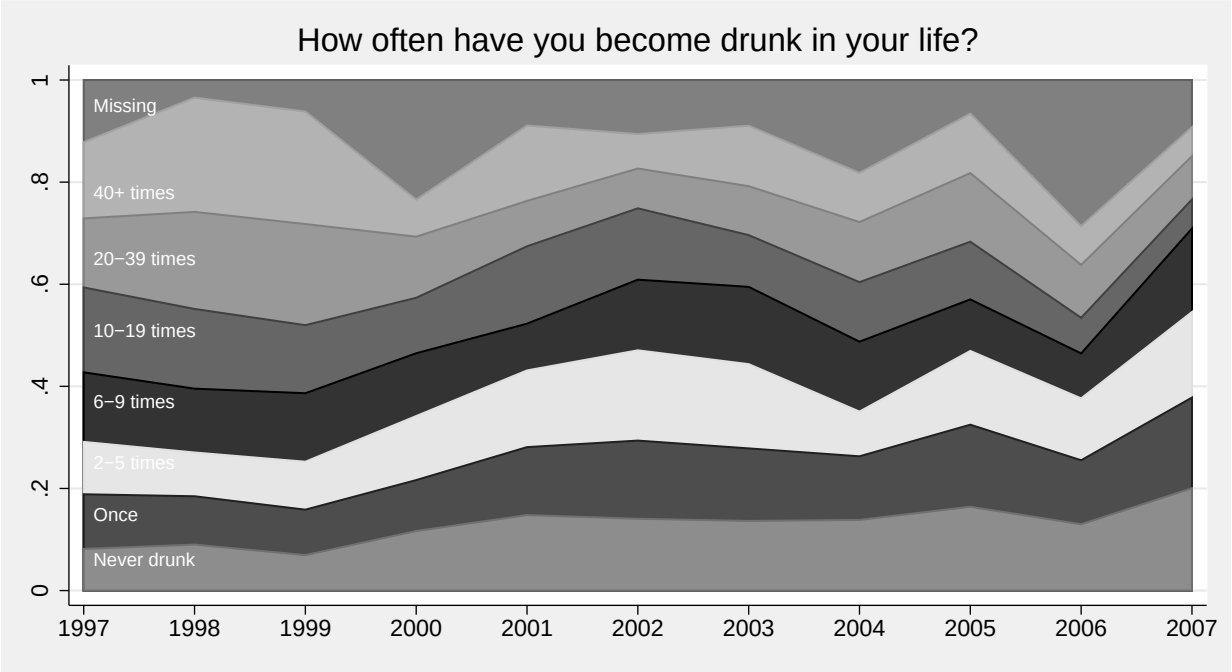

Figure 6: Reported number of times getting drunk.

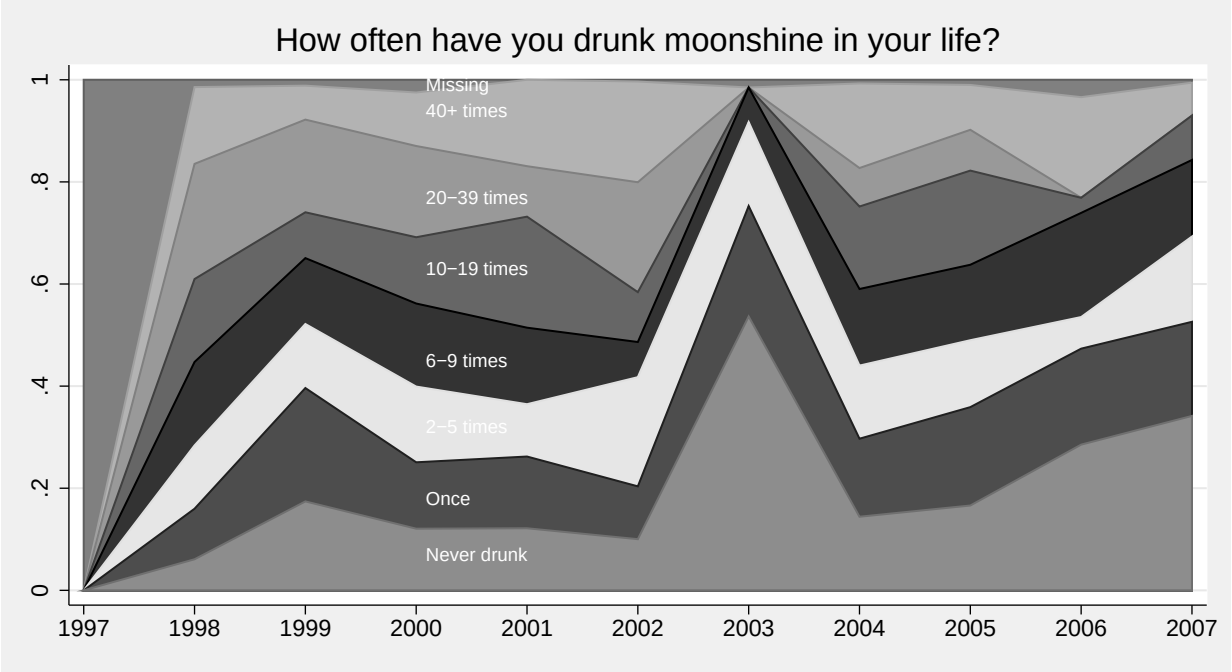

Figure 7: Reported moonshine drinking in the data set.

VARIABLES	(1) N	(2) mean	(3) sd	(4) min	(5) max
alcohol	4,277	3.284	2.075	1	7
drunk	4,288	2.331	1.859	1	7
moonshine	3,827	1.532	1.170	1	7
class	4,349	0.533	0.499	0	1
opening	4,349	1.902	0.839	1	3
distance	4,349	0.734	0.796	0	3
year	4,349	2,002	3.205	1,997	2,007

Table 2: Descriptive statistics

The third dependent variable is "How often have you become drunk in your life?". The answers are coded the same way as the alcohol consumption. This variable is plotted in figure 6 on the previous page.

The data set has a few irregularities worth mentioning. First, the 1999 and 2003 questions on moonshine consumption were harmonized with the ESPAD research project. The wording of the question changed these years, causing noticeable kinks in the otherwise relatively smooth moonshine consumption line. Year 1999, no data was gathered in 9th grade.

Table 2 shows descriptive statistics for the data set. Figures 8 on the next page, 9 on the following page and 10 on page 21 show the evolution of the consumption over the period grouped by the three groups, that is the control group, the group where an outlet opened in 1999 and the pupils where outlets opened in 2000–2.

4.2 Outlets

A list of alcohol outlets and their opening years. The data was provided by ÁTVR. The list is included as an appendix A on page 37. The appendix lists the towns included in the sample.

4.3 Distances

The third data set is distances in kilometers from each school to the nearest outlet. These were collected by me using a list of road distances by the Icelandic Road Administration.

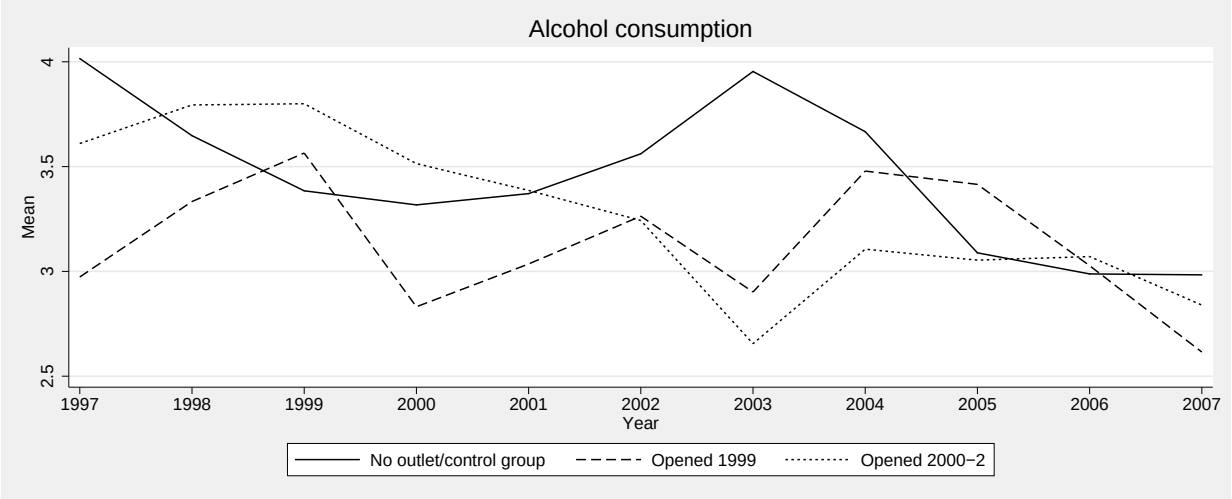

Figure 8: Alcohol consumption by group.

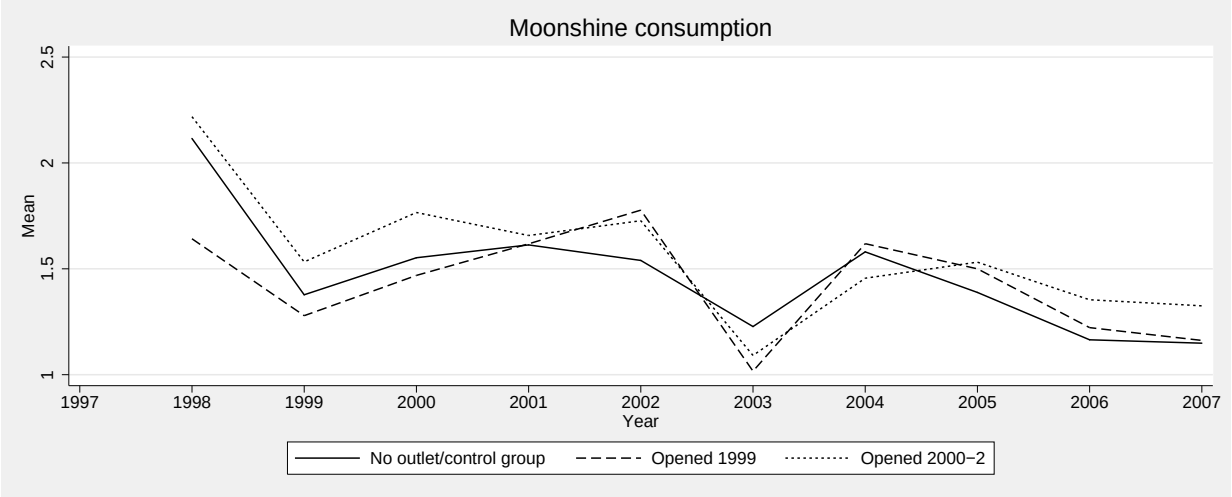

Figure 9: How many have you drunk moonshine in your life? Grouped by when and if outlets were opened in town.

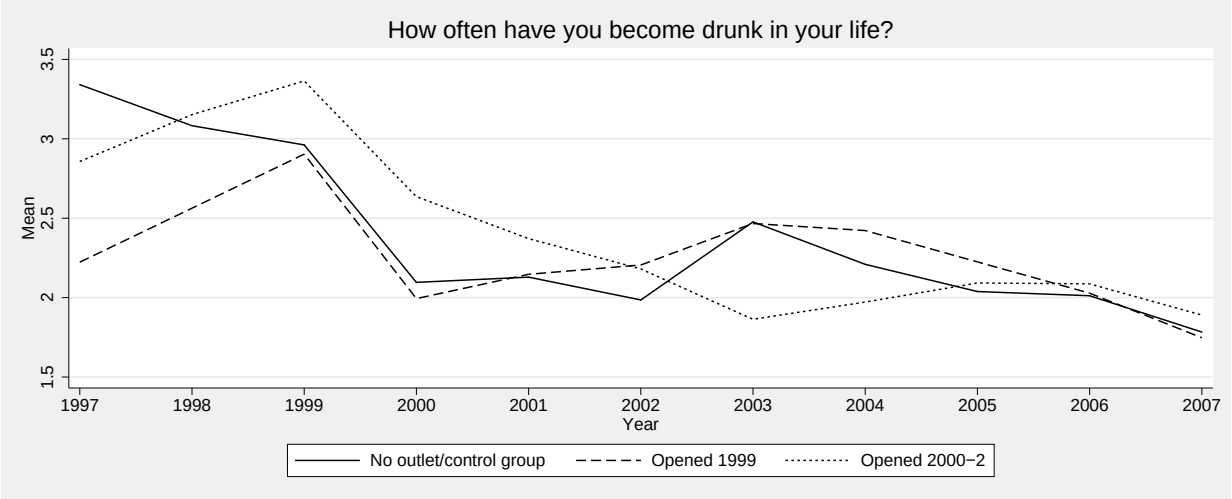

Figure 10: How often have you become drunk in your life? Divided by treatment groups.

	Outlet in 1999	**Outlet in 2000–2002**	**No outlet**	**Total**
1–49 km	1,237	761	1,337	**3335**
	70.16%	60.93%	100%	
50–99 km	192	237		**429**
	10.89%	18.98%		
100+ km	334	251		**585**
	18.94%	20.10%		
Total	**1763**	**1249**	**1337**	**4349**

Table 3: Distribution of distances between different groups of schools. Percentages refer to columns.

The list was reduced to a 3-degree scale for privacy reasons. The codes are: 0: 0 km; 1: 1–49 km; 2: 50–99 km; 3: 100+ km. The control group, with schools assigned randomly, did not have any variation in distance (when schools with missing years had been erased). More precisely, all did fall within the category of 1–49 km.

It is worth stressing that the distances are not on individual level but on school or community level. The towns are small and students living outside towns are both few and likely to be in most connection with the town where they go to school, rather than another town.

No school had a distance code 0 in year 1997. When an outlet opened in town, *distance* was recoded to 0 in 1999 and later for the group that got an outlet in 1999. *distance* was set to 0 in 2001 and later for towns that got an outlet in the year span 2000–2002.

4.4 Empirical setup

Ideally, the identification methods are two, the existence of an outlet in town (see 4.5) and the distance to nearest one (see 4.6) These two can then be combined (see 4.7).

The main problem with the difference-in-differences method of analysis is the control group and treated group have to have parallel trends. Only so can the difference in differences after the treatment be attributed to the treatment and not any unrelated random event. The best way to tackle the problem is to have data covering a long period before the treatment and estimate the correlation.

Here, two years pass before the first treatment takes place which is not a very long time. Figure 8 on page 20 shows alcohol consumption. The lines are constructed by calculating the mean of the count-variable answers for each group each year. The figure shows that in 1997—two years before any outlet had been opened—there was a considerable difference between the control group (the whole line) and the other groups, especially the group that got outlet in 1999. The overall trend seems similar for all groups, that is a slight decrease in consumption, but the way there seems jumpy. In 2003, there is extra divergence between the groups.

Figures 9 on page 20 and 10 on the preceding page convey a different picture, plotting moonshine and an indicator of how often respondents have become drunk respectively. Both indicate a somewhat stronger correlation between all the groups. Alas, given the data, there is not much to do except assume the control group works sufficiently well as such.

On a more subtle level, it is worth mentioning that the group that got an outlet in 1999 works as a control group for those getting an outlet in 2000–2002.

Another issue is the potential reverse causality; that is whether the decisions to open a new outlet are influenced by youth drinking. The push towards opening up new outlets can be seen as a mixture of many factors. More alcohol consumption has increased the expected sales of new outlets. Demand of good service on behalf of people in small towns has been on the rise, manifesting itself in formal requests from municipality councils to ÁTVR to open up a new outlet in their town. Upon receiving such a request,

ÁTVR, estimates expected profitability and possible partners. The decision is then taken based on information gathered in the process, where adolescent drinking is not one of the factors (Vinbudin.is, 2007). It is easy to draw up a potential line of effect, where a community of big drinkers will both be more likely to push for an outlet and have high incidence of adolescent drinking. A bigger data set is required to rule this theory out, but it is assumed here to be weak or non-existent.

Furthermore, an endogeneity problem could potentially arise if decision where to move or settle is dependent upon availability of alcohol, or more precisely whether an outlet is in town or not. In other words: in deciding where to resettle, availability of alcohol could influence the decision of where go. This would then take the form of people moving after the treatment takes effect—that is after 1999-2002, as before the 1999, none of the towns had an outlet. The mechanisms through which this possible effect would have on adolescent alcohol consumption are even further down a thinkable chain of events.

Not to be neglected either, is the potential—or even likely—clustering of consumption. Schools are not full of independent youth going about their business in vacuum. Rather, schools are full of friendships and influences across classes and cohorts. It would have been of big importance to be able to correct for this, such as by clustering each school. Sadly, the data set does not permit that, as different schools have been combined precisely to make them indistinguishable from one another. This is worth noting, however, both to have in mind when interpreting the results, but also to bear in mind when and if a better data set can be used.

Even though the dependent variable is discrete, it will be treated as if it were continuous. The other method—of recoding the data into binary variable of never drunk alcohol/drunk alcohol at least once—was considered. Doing that would have been throwing out the baby with the bath water, as very much variation would have been lost.

4.5 Existence of outlet

To estimate the relationship between outlets and consumption, the difference-in-differences method can be used. In its most simple form, it takes panel data and uses two dummy variables and their interaction term. One dummy variable identifies the treatment group from the control group, *treated* below. The other dummy indicates whether the effect has taken place or not, *after* below.

$$Y = \beta^0 + \beta^1 treated + \beta^2 after + \beta^3 \times treated \times after + \beta^4 class + \underbrace{[\beta^5 m\_99\_03]}_{\text{only for moonshine}} + \mathbf{D} + \epsilon \quad (1)$$

treated is a dummy, taking value 1 for the treatment group and 0 for control group. Parameter β_1 indicates difference between treatment and control group before the treatment takes place. *after* is a dummy taking value 1 after the opening of an outlet and 0 otherwise. β_2 indicates the difference for both groups between the time before and after the treatment. β_3 is the parameter of interest, that is the interaction between *treated* and *after*.

A full set of year-dummies **D** is included in the regression, as well as a *class* variable taking 1 for 10th grade and 0 for 9th grade. A dummy is included in the regressions for moonshine, *moonshine in 99&03*. The dummy takes a value 1 for years 1999 and 2003, otherwise value 0. See chapter 4.1.

4.6 Distance to outlet

The second approach to identifying the effected is to use distance to the nearest outlet as the main independent variable. As noted earlier, *distance* is coded 0–3, where 0=outlet in town; 1: 1–49 km.; 2: 50–99 km.; 3: 100+ km. The variable will be treated as a continuous one. Here, when an outlet opens in town, the outlet distance drops down to 0. As towns are lumped together in the data set, and geographically spread apart, there is no intermediate effect in the data set, that is, never does a distance drop from say 3 to 1 due to a new outlet opened in a neighboring town.

The regression can be expressed as follows:

$$Y = \beta^0 + \beta^1 distance + \beta^2 class + \beta^3 class + \underbrace{[\beta^4 m\_99\_03]}_{\text{only for moonshine}} + \mathbf{D} + \epsilon \quad (2)$$

A full set of year-dummies **D** is included in the regression, as well as a *class* variable taking 1 for 10th grade and 0 for 9th grade and a dummy for moonshine data irregularities.

4.7 Existence of outlet and distance

The two identification methods can be combined into one. Thus, a difference-in-differences model. In our case, the model estimated is

$$Y = \beta^0 + \beta^1 \times treated + \beta^2 \times after + \beta^3 \times treated \times after + \beta^4 class + \beta^5 distance + \underbrace{[\beta^6 m\_99\_03]}_{\text{only for moonshine}} + \mathbf{D} + \epsilon \quad (3)$$

A few points are worth commenting. First, the regression has a dummy variable called *class*, taking value 0 for 9th class (14 year olds) and 1 for 10th class (15 year olds). As 10th graders drink more than 9th graders, the dummy clears out possible imbalances in cohort sizes which could otherwise distort the results.

Secondly, *distance* is included in the regression as a continuous variable even though it is a coded variable, see chapter 4.3.

Third, a dummy is included in the regressions for moonshine, *moonshine in 99&03*. The dummy takes a value 1 for years 1999 and 2003, otherwise value 0.

Fourth, a full set of dummies for year effects, **D** is included in the regression.

VARIABLES	(1) alcohol	(2) moonshine	(3) drunk
treated	0.521***	0.316***	0.533***
	(0.112)	(0.0766)	(0.0980)
after	0.486***	0.380***	0.797***
	(0.183)	(0.107)	(0.160)
treated*after	-0.439***	-0.320***	-0.585***
	(0.143)	(0.0919)	(0.125)
class	0.854***	0.306***	0.849***
	(0.0634)	(0.0380)	(0.0556)
moonshine in 99&03		-0.729***	
		(0.0963)	
Constant	2.713***	1.608***	1.964***
	(0.125)	(0.0773)	(0.109)
Observations	4,277	3,777	4,246
R-squared	0.058	0.060	0.098

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Regression results difference in difference. Year dummies suppressed from table.

5 Results

5.1 Existence of outlet

The other identification method is using a difference-in-differences for the existence of outlet. The results from the regression is shown in table 4. The regression was run with three different dependent variables: alcohol consumption, moonshine consumption and how often respondents had gotten drunk. A full set of year-dummies was included in the regression but dropped from the output. Other control variables are *class* and a dummy to correct for irregularities in moonshine question wording, *moonshine in 99&03*.

The control group has lower consumption as measured by all three dependent variables, as shown by treated. The estimator of interest, *treated*after*, is significant and negative, inviting the interpretation that an outlet opened in town lowers consumption. This is discussed further in chapter 6 on page 31.

VARIABLES	(1) alcohol	(2) moonshine	(3) drunk
class	0.856***	0.303***	0.850***
	(0.0636)	(0.0381)	(0.0558)
distance	0.0219	-0.0122	-0.0156
	(0.0481)	(0.0291)	(0.0421)
m_99_03		-0.704***	
		(0.0964)	
Constant	3.038***	1.832***	2.351***
	(0.119)	(0.0722)	(0.104)
Observations	4,277	3,777	4,246
R-squared	0.052	0.055	0.090

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Regression results using distance as identification method. Full set of year dummies suppressed from table.

5.2 Distance to outlet

The natural first assumption is that proximity to alcohol outlets, measured in distance in kilometers to the nearest one, should influence availability and thus consumption. Here, the data available is grouped in distances into four categories ranging from 0 indicating an outlet in town to 3 for more than 100 kilometers to next outlet. An OLS regression was run with a set of control variables.

The results from the regression are presented in table 5. As can be seen from the regression results, *distance* is insignificant and thus has no effect or correlation with alcohol consumption, moonshine consumption or number of times respondents have become drunk.

5.3 Combination

The results from a regression based on equation 3 on page 25 can be seen in table 6 on the next page. The dummy for existence of outlets, *treated*, is significant and positive, meaning that towns with outlets have more consumption overall. More important, though, is the difference-in-differences estimator, here denoted by *treated*after*. It

VARIABLES	(1) alcohol	(2) moonshine	(3) drunk
treated	0.505***	0.334***	0.569***
	(0.118)	(0.0803)	(0.103)
after	0.489***	0.376***	0.788***
	(0.183)	(0.107)	(0.160)
treated*after	-0.400**	-0.364***	-0.676***
	(0.170)	(0.109)	(0.149)
class	0.854***	0.305***	0.847***
	(0.0634)	(0.0380)	(0.0556)
distance	0.0270	-0.0302	-0.0626
	(0.0640)	(0.0406)	(0.0559)
moonshine in 99&03		-0.727***	
		(0.0964)	
Constant	2.686***	1.640***	2.026***
	(0.139)	(0.0882)	(0.122)
Observations	4,277	3,777	4,246
R-squared	0.058	0.060	0.099

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Regression results difference in difference combined with distance to nearest outlet. Year dummies suppressed from table.

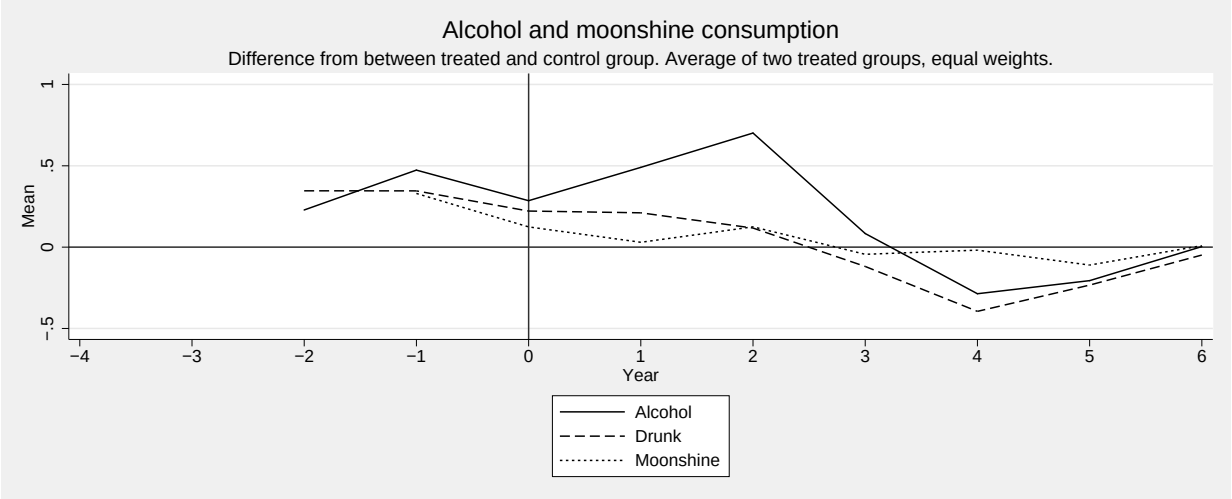

Figure 11: Alcohol and moonshine consumption

is significantly negative for all three dependent variables. *class* is highly significant, showing that many begin drinking in 10th grade. Distance is insignificant.

The point of *treated* and the difference-in-differences estimator can be seen more intuitively in figure 11. As can be seen in the figure, treated groups do consume more before the treatment, but start catching up with the control group until four years after the opening of the outlets, when they consume even less than the control group.

5.4 Triple-interaction

Doing the same with a triple-interaction variable (*treated*after*distance*) changes the results in no significant way; the interaction term for group and effect is still negative and significant in all cases. The regression table is therefore redundant.

5.5 Heavy drinking

It is interesting to see if heavy drinking increases when an outlet opens in town. Kuusi (1957) and Horverak (2004) found out that teenagers did not increase consumption when an outlet opened in town. However, both did also find out that among adults who did drink more than average before the outlet opened, drank more after the fact. Heavy-drinking teenagers could then fall into either category and either increase

VARIABLES	(1) heavy_alc	(2) heavy_moon	(3) heavy_drunk
treated	0.0758***	0.0105	0.0626***
	(0.0227)	(0.00889)	(0.0175)
after	0.0708**	0.0187	0.126***
	(0.0351)	(0.0138)	(0.0271)
treated*after	-0.0589*	-0.0163	-0.0909***
	(0.0327)	(0.0128)	(0.0253)
distance	-0.000169	0.00264	-0.0139
	(0.0123)	(0.00482)	(0.00949)
class	0.128***	0.0114**	0.0901***
	(0.0122)	(0.00478)	(0.00941)
moonshine in 99&03		-0.988***	
		(0.0126)	
Constant	0.114***	0.983***	0.0717***
	(0.0268)	(0.0105)	(0.0207)
Observations	4,277	4,277	4,277
R-squared	0.040	0.803	0.050

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Heavy drinking

their consumption (with their heavy-drinking adult counterparts) or not change their consumption (with their classmates).

To find out, I recoded the three dependent variables as dummies. The code was 1 if respondents answered they had drunk alcohol, had drunk moonshine or had become drunk in their lives 20 times or more often, respectively. The results of the regression are printed in table 7. As before, the difference-in-differences estimator is denoted *treated*after*. It turns out to be significantly negative for alcohol and times drunk but insignificant for number of times respondents had drunk moonshine. Number of heavy drinkers decreases or in any case does not increase.

The results held same signs or were insignificant when using other cut-off numbers for the dummy variable. None was significant with a positive sign.

	Opened in 1999		Opened 2000-2002		Opened 2000-2002 (excl. 2001)	
	F-test	p-value	F-test	p-value	F-test	p-value
Alcohol	9.99	0.087	1.12	0.331	1.31	0.316
Drunk	13.11	0.069	3.43	0.114	2.05	0.225
Moonshine	.	.	0.71	0.446	0.40	0.592

Table 8: Parallel trends tests

5.6 Parallel trends

One of the assumptions of difference-in-differences regressions, is that the treated group and the control group have the same trend before the treatment incurs or takes place. A clear drawback of this study is that the period before the first effect is short. Establishing whether the trends are similar before the effect is thus hard, and ultimately a matter of judgment.

However, a formal test is possible as well. Table 8 presents F-tests. They test whether the trends before the treatment of each group is significantly different from the trend of the control group. A low p-value (< 0.05 for example) would indicate that the trends are significantly different from one another. As can be seen from the table, there is no p-value lower than 0.05. That indicates that we do not have enough evidence to claim that the trends are different from one another at this level of significance. However, absence of evidence is not evidence of absence.

Noteworthy is that no values are given for moonshine for the group that got an outlet in 1999. That is because there is no data for 1997, and only 1998 and 1999 available. There simply is not enough degrees of freedom to do the test. The test is even conducted *including* 1999, even though the outlets opened that year (when in the year in respect to the data collection is not known). The tests for the group where an outlet opened in 2000-2002 are conducted both with and without data for year 2001.

6 Discussion and conclusions

With only a small subset of the available data. A more fine-grained data set with more observations would have improved the reliability of the results. With more data, more

questions could have been answered. However, there are good reasons to see the results presented here as reliable.

When using distance-to-outlet as identification method, there is no relationship between the distance and adolescent alcohol or moonshine consumption. For researchers, there is always bias towards finding significant result. Here, it can be argued that the lack of significant correlation in either direction is of much interest. However, the data set leaves much to be desired. There is a lack of variation in the control group (all schools in control group fall within the 1–49 km group), and the variation has been boiled down from a continuous variable to a discrete one with few steps.

The most important and interesting result of this paper is the negative relationship between outlets and alcohol consumption. Using a difference-in-differences model, alcohol consumption (however measured) drops in response to a new alcohol store in town. This goes contrary to prior research and general assumptions.

Standard theory and research indicates that when an off-premises outlet is opened, consumption of illicit alcohol decreases while consumption of licit alcohol increases, giving a negligible net effect in terms of alcohol-liters. Here, this is not the case; both moonshine and alcohol consumption falls.

As Mäkelä et al. (2002) point out, most of the research hitherto conducted has been short-term. Usually, consumption has been measured a year before the outlets opened and again a year later. Possibly, a third observation is made under the transition period. All these thus measure short-term effects. The method presented here, and the underlying data set, allows for long-term analysis.

Looking at figure 11 on page 29 it is obvious that the negative effect is most pronounced in year after the outlet opened. It is possible that the reason why the results of this study differ from other similar studies (Horverak, 2004, Kuusi, 1957) is indeed the time horizon here. Just as likely is that the differences between the results presented here and previous research stems from other factors. Potential such factors are many. After all, this paper is the first on Iceland, and differences in results could be because of some unmeasured characteristics of Iceland. Differences in how data has been gathered

can also be the reason for discrepancies, possibly bringing systematic biases into the data-gathering process.

The treated groups had more alcohol consumption in the beginning of the time series. There is not enough data available to establish why this could be. If it is a coincidence, it is possible that the towns that had above-average consumption in 1997–8 bounced back because of more prevention programs. The mechanisms at play are many and no way of telling apart.

However, during the analysis for this paper, different specifications were tried, with and without control variables and using other designs. Although not reported here in detail, they all conveyed a similar result as the ones reported in this paper. The result is also robust for heavy drinkers, as reported in chapter 5.5 and using a tripe interaction approach, as reported in chapter 5.4. The internal validity of the results is thus high.

It is tricky to interpret or translate coefficients into something meaningful, as the dependent variables are not continuous variables but rather discrete with differing intervals. Around the mean of roughly 3, a coefficient of 0.5 in either direction translates into roughly 2 instances of lifetime alcohol consumption (moonshine consumption or number of times gotten drunk for the other two variables).

Many factors have been found to correlate with alcohol consumption. Among such factors are socio-economic status, gender and more. Such background data for each observation is available in the original data set but not used here. Given the relative homogeneity of the rural Iceland, it is unlikely that schools differ systematically in this respect and that such a difference would be correlated with outlets being opened. My judgment is thus that there is little chance of an omitted variable bias on the individual level.

It is important to stress what the results in paper *do not* say—the external validity is limited. They do not say that liberalization of alcohol policies, such as abolishing the monopoly, will cause a decline in alcohol consumption among teenagers. They do not apply to circumstances where new liquor-stores are opened up in towns or cities where there previously were stores. And the results say nothing about changes in on-premises alcohol sales.

Given the wealth of data available, there are plenty of further research questions that can be addressed. The most obvious next step is to repeat the research with a bigger subset of the existing data.

Both in the ICSRA data set used here, and the ESPAD data set, data is available on how teenagers get hold of their alcohol. Using that data, it is fairly easy to establish if there are significant differences in the channels by which adolescents reach alcohol in town with and without an outlet respectively. This paper establishes that total alcohol consumption falls (or does not rise at least), but which, if any, of the possible mechanisms mentioned in chapter 3.2 is correct?

To sum up: there seems to be a negative relationship between outlets opening and adolescent alcohol consumption. Referring to this relationship as causal in the strictest sense is jumping to conclusions, and there are a number of shortcomings to the data set and method used in this paper. The relationship is counter-intuitive and not entirely in line with previous research. The results are interesting and strong enough to merit a closer look with a better data set, keeping an eye on the possible mechanisms behind the results. The final answer is looming in existing data—it is just a question of extracting it.

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A Outlets

Town	Year opened	Included	Population 2007
Reykjanesbær	<1990		
Mosfellsbær	<1990		
Akranes	<1990		
Ólafsvík	<1990		
Ísafjörður	<1990		
Sauðárkrókur	<1990		
Siglufjörður	<1990		
Akureyri	<1990		
Seyðisfjörður	<1990		
Neskaupstaður	<1990		
Höfn	<1990		
Selfoss	<1990		
Hveragerði	<1990		
Vestmannaeyjar	<1990		
Húsavík	1991		
Egilsstaðir	1992		
Borgarnes	1993		
Stykkishólmur	1994		
Blönduós	1994		
Patreksfjörður	1997		
Grindavík	1999	Yes	2,701
Dalvík	1999	Yes	1,404
Þórshöfn	1999	Yes	391
Vopnafjörður	1999	Yes	561
Fáskrúðsfjörður	1999	Yes	612
Búðardalur	2000	Yes	238
Hvammstangi	2000	Yes	1,167
Hvolsvöllur	2000		
Grundarfjörður	2001	Yes	870
Djúpivogur	2002	Yes	367
Þorlákshöfn	2003		
Vík í Mýrdal	2003		
Hólmavík	2004		
Kirkjubæjarklaustur	2004		
Reyðarfjörður	2005		
Flúðir	2009		
Ólafsfjörður	-	Yes	923
Hofsós	-	Yes	172
Bolungarvík	-	Yes	902
Eyrarbakki	-	Yes	585
Eskifjörður	-	Yes	1,063

Table 9: List of towns outside capital area with outlets and/or included in the data set. Population 2007 used as that is the last year of data collection. Population may or may not include outskirts or rural surroundings. Sources: ÁTVR, Statistics Iceland.