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ABSTRACT

I examine the long-term mortality effects of an exogenous shock in alcohol availability using Census data, mortality data and data on distance to alcohol outlets. In 1969, Finland underwent significant changes in alcohol availability both via Minimum Legal Drinking Age (MLDA) and distribution channels. The geographical alcohol availability increased especially for the previously "dry" rural Finland. The main results suggest that reducing MLDA from 21 to 18 had a positive effect on alcohol-related deaths in the long term for the 18-19 year-olds that were immediately affected by MLDA. I do not find evidence that the reduction of physical alcohol availability in rural areas resulted into diminishing difference of alcohol-related death rates between urban and rural areas.

JEL Classification: I12; L43; R11

Keywords: Health Behavior; Deregulation; Regional Economic Activity

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1 Introduction

When consumers do not fully take account the negative health effects of their consumption behavior, it is common for a government to influence consumer choice with taxes, age-based restrictions, physical availability etc. Liberalization efforts of these types of paternalistic restrictions often spur public debate. Recently the increasing trend towards the legalization of cannabis in North and South America has increased concerns over the long-term health effects of these reforms. There is evidence which suggests that long-term use of cannabis initiated in adolescence is associated with e.g. substance dependence (Hoch, et al., 2015), and restricting legal cannabis availability improves academic performance (Marie & Zölitz, 2015). There is much firmer evidence on the long-term harms related excess alcohol drinking. These harms include working disability, hospital treatments, mental disorders and relatively early in life (WHO, 2014). For example in Finland causes of death that are directly linked to excess alcohol use are the leading cause of death among working aged people (Statistics Finland, 2012). Because of the wide array of social costs related to alcohol abuse, the relationships between availability of alcohol and different measures of alcohol-related harm have been studied extensively. Several studies suggest that early exposure to alcohol is associated with premature mortality (Cornelius, et al., 2008) and morbidity (Rohde, et al., 2007) and that reduction of minimum legal drinking age (MLDA) is related to short-term increased mortality (Carpenter & Dobkin, 2009) and morbidity (Conover & Scrimgeour, 2013) among the newly eligible age groups. On the other hand the causal evidence over the geographical availability and health measures are still inconclusive largely due to the problems related to endogeneity bias (Popova, et al., 2009).

This paper examines the long-term mortality effects of a massive alcohol reform. In 1969, Finland lowered Minimum Legal Drinking Age (MLDA) by three years and allowed alcohol to be sold in previously “dry” rural areas. I present evidence suggesting that the change in the MLDA increased relative risk of an alcohol-related death for the two youngest newly eligible birth cohorts (1950-1951) by 9-10 percentages. The analysis also indicates that the reductions in regional differences in geographical alcohol availability did not diminish regional differences in alcohol-related mortality.

I contribute to the alcohol studies in two ways. First, there are no prior studies that have examined the long-term health effects of an exogenous change in alcohol availability. Previous studies have focused on immediate effects of a change in alcohol availability on consumption, crime, traffic accidents and mortality. This study focuses on deaths related to chronic alcohol-related diseases (CARD) which serve as a reliable measure for the extent of alcohol-related problems within a population. I analyze how 1969 alcohol reform affected the long-term mortality patterns of young adults according to their year of birth (1940 to 1955). By this I aim to investigate whether individuals vary in their sensitivity to changes in the alcohol availability through age at exposure.

Second, the simultaneous radical change in geographical availability of allows also the possibility to study the long-term effects of the abolition of “rural prohibition”. From until the end of Finnish prohibition in 1932 to end of 1969 the rural municipalities were by and large left without off- and on-premise supply of alcohol. Only cities and market towns had access to state monopoly stores, Alkos. From 1969 on beer was allowed to be sold grocery stores all over the country and Alkos were founded to rural municipalities to effectively narrow the gap regional discrimination of alcohol availability. I study whether the urban-rural gap in CARD-rates narrow as age at reform decreases (birth cohort increases).

The estimation results suggest that the newly eligible birth cohorts that were the first cohorts to have legal access to alcohol have higher CARD-rates than elderly peers. This result supports the idea that the habit formation of alcohol consumption is associated with age. The age restrictions do not only prevent youth from risky health behavior in short term as prior evidence has shown but also in the long term. The higher CARD-rates of 18-19 year-olds support the evidence that early exposure to alcohol increases risk of alcohol dependence later in adulthood. This suggests that minimum purchase age restrictions are effective in protecting young adults from alcohol-related harm.

The differences in changes in geographical availability of alcohol are not statistically significant in explaining within cohort variation of CARD-rates. This does not support the idea that the reduction of urban-rural gap in geographical alcohol availability reduced urban-rural gap in long-term problem drinking. Increased legal alcohol availability appears to have affected whole Finnish population habits in a relatively uniform way. This leaves open the question how regional differences in socioeconomic factors and alcohol consumption culture (distribution and home production) were associated with the alcohol reform and the following change in alcohol consumption habits.

I perform the analysis using mortality data, individual-level data recording demographic characteristics in 1970, and pre- and post-reform GPS-coordinates for Alko outlets. These data allows the examination of differences in alcohol-related mortality risk between cohorts 1940-1955 (about 260,000 individuals) who were exposed to manifold increase in alcohol availability in their young adulthood.

The rest of the paper proceeds as follows. The next section provides the historical background and the details of the 1969 Finnish alcohol reform. Section 3 reviews the previous literature on alcohol availability and consumption. Section 4 describes the data and Section 5 discusses the empirical approach. Section 6 presents the results and section 7 concludes.

2 The reform

Finland has had relatively restrictive system for controlling the sale of alcoholic beverages during the 20th century. The Finnish parliament imposed prohibition the selling and manufacturing of alcohol in 1919. As the enforcement of the prohibition was weak from the start, bootlegging and home production became widespread. These illegal activities also produced negative externalities such as increased violence crime rates. The prohibition was repealed in 1932, and the monopoly right for legal distribution of alcohol was given state-owned company Alko. (Kallenautio, 1979)

Even though the prohibition was officially repealed in whole Finland, the dry era effectively continued for the rural area at least through the legal distribution channels. Alcohol outlets, Alkos, were only established to urban cities and some market towns. Salonen (2013) has suggested that the reasons for strong support for the strict alcohol policy in rural Finland were practical (enforcement of order) and paternalistic. The Alcohol Act also effectively restricted the founding Alko outlets by applying different voting rules between rural and urban municipalities over new Alko outlet establishments[1]. However, it should be noted that Finland rural population has a long history disobedience of alcohol policy restriction throughout the history. For example under Swedish rule in the 18th Century, when the local officials tried to heavily restrict alcohol consumption, it led to a marked increase in home production of liquor and consumption among rural population of Finland. (Mäntylä, 1995)

The dry era of rural Finland ended on 1.1.1969. Finland underwent two significant changes in alcohol laws. The New Alcohol Act lowered the Minimum Legal Drinking Age (MLDA) and allowed considerably easier access to alcohol compared to the previous years. The MLDA changed from 21 years to 18 years for medium beer and other non-spirits (less than 17 percent alcohol) and from 21

[1] Alko was allowed to establish Alko into cities and market towns if two thirds of town council member didn't oppose it. In rural municipalities Alko was allowed to conduct alcohol sales only if two thirds of town council members voted for it (Häikiö, 2007).

to 20 for spirits. The geographical accessibility of alcohol was eased in three ways. First, the act allowed the state alcohol retail monopoly, Alko, to grant licenses for medium beer to be sold in retail stores (from 0 in 1968 to 17000 licenses in 1969). Second, Alko increased the number of monopoly stores by 22 percent during 1969, practically all established in previously "dry" rural areas. Third, the number beer-licensed restaurants increased from 0 to 2716 and the number of liquor-licensed restaurant increased from 968 to 1101 (Häikiö, 2007). Prior to the reform only cities and market towns had alcohol-licensed restaurants.

Besides physical availability and MLDA, Alko had also other means to control alcohol purchases. During and after Finnish-Soviet wars (1939-1945) increasing concerns over mass purchases of alcohol lead to introductions of purchase permits (viinakortti) in 1943 and buyer surveillance (ostajaintarkkailu) in 1946. Prior to the wars, Alko only controlled the amount one-time purchases. Purchase permits started to control the amounts of monthly purchases. These permits included information on ID, occupation and the history of alcohol purchases. Purchase permit also meant that customers had to be registered in the nearest Alko outlet to keep a record of their purchases. Off-premise purchases could only be done in this store. According to Häikiö (2007) one of the reasons for the introduction of purchase permits to control alcohol consumption was scarcity of labor during wartime and the need for more efficient use of workforce. The objective of buyer surveillance was to prevent alcohol purchases of "presumably" divergent alcohol consumers. Surveillance officers effectively served as security officers at Alko. They also gathered information from police on subjects of alcohol abuse and occasionally proceeded on house calls and interrogations of relatives and neighbors' of "presumed subjects". Buyer surveillance was discontinued in 1958 and purchase permit in 1971. Monthly limits of beer and wine purchases were already discountinued at the 1969 reform. (Häikiö, 2007).

The impact of easier access to alcohol on total consumption level was immediate. Within a year following the law change the recorded alcohol consumption increased by 46 percent in total alcohol consumption (Figure 1). The consumption of medium beer climbed up by 135 percent relative to the year before the reform. After 1969, alcohol consumption continued to rise steadily until the outbreak of the Finnish Recession of the early 1990's (Häikiö, 2007). As total alcohol consumption grew, also the prevalence of binge drinking increased during 1970's (Huhtanen et al., 2011). This relationship is reflected in Figure 2 which shows the trends in statistical alcohol consumption and alcohol-related mortality due to chronic causes. The latter is frequently used as a measure for the prevalence of problem drinking.

The change in availability of alcohol affected all Finnish people but especially the ones living in rural areas, and therefore about half of the Finnish population at the time. State monopoly stores, Alko outlets, were previously permitted only in cities or market towns. After 1.1.1969, alcohol became immediately more available in rural areas through the thousands of beer-licenses permitted to rural grocery stores and several new Alko outlets founded in rural areas. Virtually all municipalities (519 out of 521) got at least one grocery store that had a beer-license by March 1969, i.e. two months after the reform. This means that the abrupt change in increased geographical availability of beer was in general sense similar to all municipalities: from nearest Alko to municipality center. The change was the greatest in the sparsely populated regions in Northern Finland, where the reform effectively meant that driving distances to nearest beer-selling outlet decreased from over 400 kilometers into few kilometers. Häikiö (2007) claims that it was the increased availability of beer that relieved most of the suppressed demand for alcohol.

Figure 3 illustrates the change in geographical availability with pre- and post-reform locations of Alkos. It shows how people living in rural areas (especially in North Finland, Lapland) had inferior geographical access to Alkos compared urban population of Southern Finland prior to the alcohol reform. In 1969, Alko established liquor 22 new outlets, all but one of these were located in the rural area. In deciding the new Alko outlet locations, Alko used mathematical formula that aimed to effective reduce regional inequality of alcohol availability (Häikiö, 2007).

3 Related studies on alcohol

3.1 Alcohol consumption and alcohol-related deaths

Alcohol related harm is often split into immediate and chronic effects. Immediate effects include accidents related to single drinking occasions, whereas chronic effects due to long-term excess alcohol use include liver diseases. Chronic harmful effects of problematic alcohol use have increased in particular in the past decades (figure 2). The alcohol related deaths to chronic diseases have become a common cause of death for working-age population in Finland (Karlsson, 2009). These causes of deaths (CARDs) have been used as a proxy for a history of problem drinking (Paljärvi, et al., 2014).

Epidemiological studies demonstrated that the population-level incidence rate of liver cirrhosis mostly driven by the heavies alcohol consumers and thus reflect poorly present total consumption level. In Finland, liver cirrhosis deaths accounted for about 56 percent of CARDs in 2012. The mortality rate from liver cirrhosis is traditionally used as an indicator of the prevalence of chronic heavy drinking in a population (Bruun 1975 and Cook & Tauchen 1982).

Herttua et al. (2008) study the effect of Finnish alcohol tax cut of 2004 on alcohol-related deaths, including both chronic and contributory cause of death. Recorded total alcohol consumption increased by 10 percentages within a year following the tax cut. The authors find that alcohol-related mortality increased by 16% among men and by 31% among women and that it was mostly (82%) due to an increase in chronic causes, i.e. CARDs. The increase in absolute terms was largest among men aged 55–59 years and women aged 50–54 years. This research shows that a sudden increase in alcohol consumption may have a great impact on year-specific CARD-rates. As CARDs tend to develop after long period of alcohol abuse – decades of excess alcohol use may be needed (Skog, 1980) – the strong reaction in CARD-rates right after the tax cut first appears to contradict with common knowledge. However, just as Herttua et al. (2008) imply, it is likely that the hike in the alcohol mortality rates is most likely due to heavy drinkers that had already a long history of problem drinking.

3.2 Alcohol consumption and age

Both psychologists (see e.g. Greene, 1986) and economists (see e.g. Chaloupka, 1991) have found evidence of more present-oriented behavior for adolescents as compared to young adults. Cook (2007) argues that risks related excess consumption of alcohol is substantial especially among young people who are more likely to be psychologically immature. The neuroscience also suggests that the region of the brain that governs emotion and self-regulation is sensitive to disruptions into the early 20s (Dahl, 2004).

In a prospective cohort follow-up study, Wells et al. (2004) find that drinking habits in early adolescence increase the risk of various risky health behaviors, such as alcohol dependence. There are empirical evidence that alcohol use in early adolescence increases risk of both premature morbidity (Rohde, et al., 2007) and mortality (Cornelius, et al., 2008)as well as alcohol use disorders (Wells, et al., 2004).

There is vast literature on the impact of MLDA on alcohol consumption and alcohol-related harm. The empirical evidence suggests that there is inverse relationship between MLDA and alcohol consumption of young adults (Wagenaar & Toomey, 2002). One of the most prominent studies on the health impact of a change in MLDA has been done by Conover & Scrimgeour (2013). They evaluated the effect of reduction in New Zealand’s minimum legal drinking age (from 20 to 18) of alcohol on short-term morbidity. Their (difference-in-differences) results show a substantial increase (23%) in alcohol-related hospitalizations among those newly eligible to purchase liquor.

3.3 Alcohol consumption and geographical availability of alcohol

One can consider distance to alcohol outlet to reflect the transportation costs of purchasing alcohol. From intuitive perspective, transportation costs had relatively great role in consumer’s consumption decisions in the 1960’s and 1970’s as Alko heavily regulated the range of alcohol prices within Finland (Häikiö, 2007). Especially in the pre-reform Finland the time and money cost related to distance to Alko potentially formed great deal of the full cost of alcohol purchase. Full cost of alcohol refers to store price of alcohol supplemented with transportation costs (incl. individual value of time) following the logic of horizontal differentiation (Tirole, 1988). In this sense the magnitude of transportation costs relative to the full cost of alcohol were potentially very high in the rural areas. In 1968 the distance to nearest Alko outlet could be as high as 400 kilometers in sparsely populated Northern Finland whereas in the capital city, Helsinki, the distance to nearest Alko outlet was on average 1.4 kilometers.

There are no previous studies that address the long-term health effects of an abrupt change in geographical alcohol availability. This also follows from the fact besides the Finnish alcohol reform of 1969, there are no other comparable reforms that create such a sudden and massive space-based variation in alcohol availability within one country's borders accompanied with a change in MLDA at the same time. There are, however, series of studies that examine the association of alcohol availability on different health related measures in the short term.

Recent studies have used density of alcohol outlets (e.g. Connor et al, 2011, and Stockwell et al, 2011) or proximity (see e.g. Truong and Sturm, 2009 and Halonen et al., 2014) to alcohol outlets for studying regional and individual alcohol consumption levels in different contexts. These studies do not, however, address the potential endogeneity bias related to geographical determination in demand and supply of alcohol. It is likely that regions that have high alcohol consumption per capita attract alcohol-sellers not only the other way round.

Popova et al. (2009) examine the literature on the effect of alcohol outlet densities on alcohol consumption and alcohol-related harm. In their review, they find that increased alcohol outlet density is associated with higher consumption and alcohol-related problems. But again, as the authors point out, these studies suffer from an endogeneity problem between the density of alcohol outlets and alcohol consumption.

Perhaps the closest match to this study's design is Johansson et al. (2014). They use the exogenous variation arising from a policy reform to analyze the relationship geographical variation in access to cheap alcohol and cross-border health. Johansson et al. (2014) conduct a micro-level register study on the effect of the 2004 Finnish alcohol tax on mortality, alcohol-related illnesses and work absenteeism in Sweden in regions near the Finnish border. Using polynomial model for sickness absence with respect to distance to Finnish border, they find that workplace absenteeism increased by 9% for males and by 15% for females living next to the Finnish border. The authors did not, however, did not find statistically significant mortality or morbidity effects on the Swedish following the 2004 Finnish alcohol tax reform.

4 Data & Methods

In this section, I present the data sources (4.1-4.3) and the sampling strategy (4.4) used in the estimation phase. Figure 4 will present the timeline of some relevant historical events with regard to alcohol availability in Finland and the time periods for the data collected for this study.

4.1 Cause of death registry

Registry data on causes of death is from Statistics Finland for birth cohorts from 1940 to 1955. The cause of death data set includes personal identification key, cause of death and date of death. The cause of death used in statistics is determined according to the selection and application rules of the International Classification of Diseases (ICD-10) compiled by the World Health Organisation (WHO). The alcohol-related death data is a 95% random sample on individuals coded with alcohol being their primary (or underlying), i.e. CARD, cause of death during the years 1971-2013[2].

This study focuses on the birth cohorts that were young adults or adolescents at the time of the reform. Given the differences in how birth cohorts' lifespans overlap with the availability of CARD data, the mortality analysis will be conducted for a fixed age interval. I examine alcohol-related mortality due to chronic causes within the age intervals of 31-58 for cohorts 1940-1955. This interval includes nine birth cohorts that were not affected by the change in MLDA (1940-1948), three cohorts (1949-1951) that were affected that were immediately affected by the new MLDA and four birth cohorts (1952-1955) that gained legal access to alcohol at same age as the youngest of the immediately affected, i.e. birth cohort 1951. The inclusion of birth cohorts 1952-1955 allows us to distinguish the alcohol mortality cohort trend from the MLDA effect.

There is a two-year gap between the start of the reform and the mortality data. Table 3 shows how many CARDs by cohort are missed when we have this timing gap. This may leads attrition bias. However, this bias will be relatively small as deaths due to alcohol-related diseases before age 31 are fairly rare (2,5 percent on average of total CARDs).

[2] This registry has data already starting 1969. Unfortunately for years 1969 and 1970 there are no comprehensive ID information that could be used to link different registries. The dataset also includes other causes of death for the control group. These include also deaths with alcohol as a contributory cause. This data is from 1987 onwards.

4.2 Background variables: Census 1970 data

The mortality data is matched with Census 1970 data to provide background information for the individuals that suffered CARD. To offer counterparts for the CARD-events, 20 percent Census 1970 random sample was drawn from the population of non-events (non-CARDs) in cohorts 1940-1955. This type of sampling strategy, choice-based sampling, is discussed in section 4.4.

The set of background variables taken from Census 1970 include gender, date of birth, municipality of residence, residential coordinates and completed education. Year of birth will used as a measure for age at exposure (age treatment) to alcohol reform.

This study uses background information from Census 1970 as measures for the pre-reform individual characteristics. As Census 1970 was conducted at the end of 1970, there is a gap of almost two years between the ideal pre-reform background characteristics and Census 1970 data. This shortcoming opens up a possibility for the end of 1970 data being outcomes of the alcohol shock in 1969. We are dealing with relatively young population which was 18 to 29 year-olds at time of the reform. It could be that e.g. educational choice were affected by the increased introduction of alcohol within the two years which in turn may give a misleading view of the pre-reform background of study subject. I thus focus on controlling for the likely time-invariant characteristics (mainly regional residential information) that are unlikely to be affected by reform within two years.

I obtain household coordinates from Census 1970 regarding the end of year 1970. These coordinates are actually residential building information included in

Census 1970 for which GPS coordinates can be obtained if these buildings still exist. GPS coordinates were obtained for more than 98 percent of sample's observations. This information isn't accessible for researchers directly due to data security reasons. The building coordinates were used by Statistics Finland for distance calculation from residential address to nearest Alko outlet. As we use Census 1970 for distance calculation, there is again timing mismatch of two years between individual house coordinates and between pre-reform locations. Because of labor movement this will lead to measurement error. The labor movement at this period is unlikely random to be correlated with treatment variables for geographical availability and socio-economic background. Measurement error of this type will result in to downward bias in the coefficient estimates (Cameron & Trivedi, 2005).

4.3 Data on changes in geographical alcohol availability

This study considers two types of "treatment" with regard to the change in geographical alcohol availability in 1969. First treatment is the change in the availability beer ("beer treatment") and the second is the change in the availability of liquor ("liquor treatment").

The changes in geographical alcohol availability were formed by using Euclidean distance measure between Census 1970 residential data and nearest pre- and post-reform Alko. Distance data was calculated by Statistics Finland with GIS (Geographical Information System). Alko address data was obtained from Alko Yearbooks and Alko offices. Alko outlet data with original addresses was partially obtained from Alko Yearbook 1968 and Alko address archives of The Hotel and Restaurant Museum. These addresses were then updated to match todays' addresses with Google searches, city hall inquiries and Alko office inquiries.

To address the dramatic change in availability of beer resulting from the alcohol reform, I create a binary variable *rural*. Because I only have access to pseudo-municipality code instead explicit municipality name for direct identification of rural and urban municipalities, I exploit the distance data to form the binary variable. As all of the biggest cities and market towns basically had an Alko store, the average distance of their population to nearest Alko in 1968 is substantially lower than the one of rural municipalities. I use rationale to form a crude measure for the change in beer availability, D_i, the following way:

$$D_r = \begin{cases} 0, & \text{if distance to Alko in 1968 } \leq 10km \\ 1, & \text{if distance to Alko in 1968 } > 10km \end{cases}$$

Basically, for rural municipalities, this meant that the nearest beer off-premise sales changed from the nearest Alko outlet to municipality center. For urban municipalities this practically meant no change. D_r will also capture the changes in on-premise alcohol sales as the regional change was similar to beer availability.

Estimating the effects of the changes in liquor availability, I transform the change in Euclidean distance to Alko measure in to three discrete variable taking values for values:

$$\Delta A_i = \begin{cases} 0, & \text{if change in distance to Alko } < 0{,}5km \\ 1, & \text{if } 0{,}5km \leq \text{ change in distance to Alko was } \leq 10km \\ 2, & i\text{f change in distance to Alko was } > 10km \end{cases}$$

This change was, again, confined to rural municipalities. The introduction of Alko outlet to rural municipalities reduced in some cases the Euclidean distance to nearest Alko by over 300 km.

4.4 Sampling strategy: Choice-based sampling

In register-based studies, the usual data collection strategy is random sampling. When one of the values of the binary dependent variable, Y, is rare in the population, random selecting separately within categories of Y may save considerable resources in data collection (King & Zeng, 2001). In econometrics this type of data collection strategy is known as choice-based or endogenous stratified sampling and in epidemiology as a case-control design (Breslow, 1996).

This study applies choice-based sampling strategy in studying the mortality outcomes among a specific subset of birth cohorts that were young adults at the time of the reform. The reason for this choice isn't cost-efficiency but the constraints posed by the Population Census Act of Finland, which restricts the scope of sample and the indirect identification individual persons[3]. The strategy used here is to select all possible rare events (actually 95% random sample of CARDs) and randomly selected large population of non-events (20% Census 1970 sample of non-CARDs).

[3] According to the current interpretation of Population Census Act, Statistics Finland is allowed to give access to a maximum 10% sample of Census 1970 and 1980 population. Also considerable selection of variables is required to make sure that it is not possible to (indirectly) identify individuals.

The estimation of choice-based sample would generally lead to inconsistent estimation of regression parameters. An appropriate weighting estimation would achieve consistency. In the estimation phase, this study uses sampling weights as suggested by Manski and Lerman (1977) to weight up the zeros to population level.

5 Empirical strategy

I evaluate the impact of age and change in distance to nearest liquor store during the year 1969 on the risk of dying due to chronic alcohol disease. The basic linear probability model[4] that I estimate is

$$y_i = a + \varphi D_c + \gamma D_r + \pi(D_c * D_r) + \mu \Delta A_i + \delta(\Delta A_i * D_c) + \boldsymbol{X}_i + \varepsilon_i, \qquad (1)$$

where y_i is a binary variable taking the value 1 in the occurrence of a CARD between the ages of 31 and 58[5] and zero otherwise. D_c denotes the set of birth

[4] Here I focus on LPM because non-linear models are known to be difficult to interpret when interaction terms are involved (Ai & Norton, 2003). Wooldridge (2002) also point out that, even though LPM is not constrained to 0-1 interval, this not as problematic when most of the explanatory are discrete and take on only a few values, which is the case in this paper.

[5] This homogenous age interval to study cohort differences in death rates due to chronic alcohol-related diseases is determined by choice of age cohorts and availability of detailed mortality data. We examine cohorts born between year 1940 and 1955 with mortality data over a time period of 1971-2013.

cohort categories or dummies and D_r dummy variable for a rural municipality. ΔA_i expresses the individual-specific measure for the change in the availability of alcohol between dates 31.12.1968 and 31.12.1969. Individual control variables are encompassed in $\boldsymbol{X}_i$, and ε_i summarizes the unobservable factors.

The parameters of greatest interest in this study are φ, π and δ. In the ideal situation these parameters capture effects of the age-related shock and also the interaction of the age and two types of shocks on geographical availability on alcohol-related mortality. I now ask the following questions:

1. Did the reduction of MLDA increase the CARD-risk? That is, is φ higher for birth cohort group 1949-1951 compared to 1945-48? Is the CARD-risk of the new eligible birth cohort above the alcohol mortality trend?
2. Did the decrease of urban-rural gap in legal distribution channels lead to a decrease in urban-rural gap in CARD-rates?

The first question aims to find evidence of the interplay between habit formation and MLDA. Does it matter whether the MLDA is 18 or 21 from the health perspective? This may have great policy relevance depending on social preferences over paternalistic policies.

The second question addresses the impact of differential geographical availability. The Finnish young adults witnessed liberalization of alcohol policy from different pre-reform perspectives both via age and geographical availability. If one assumes that age matters for habit formation on alcohol consumption in general and so does distance to the nearest alcohol outlet, then it is natural to assume that the young are more sensitive to changes in geographical alcohol availability than the old. According to this logic, the effect of a change in alcohol availability should be seen in the interaction term of age and geographical alcohol availability measures.

To be able to give causal interpretation for the estimates for φ, π and δ one must build on the following identifying assumptions:

I. The birth cohort, or age at the time of the change in alcohol availability in 1969, is uncorrelated with unobserved individual characteristics prior to the reform:

$$Cov(D_c, \varepsilon_i | \boldsymbol{X}_i, D_r, D_c) = 0, \tag{2}$$

II. Living a rural municipality prior to the reform is uncorrelated with unobserved pre-reform individual characteristics:

$$Cov(D_r, \varepsilon_i | \boldsymbol{X}_i, D_c, \Delta A_i) = 0, \tag{3}$$

III. The change in the distance to the nearest alcohol outlet in 1969, is uncorrelated with unobserved individual characteristics prior to the reform:

$$Cov(\Delta A_i, \varepsilon_i | \boldsymbol{X}_i, D_r, \Delta A_i) = 0, \tag{4}$$

In short these assumptions mean when that when individuals' observed characteristics are controlled for, the three "treatment components" are uncorrelated with individual characteristics prior to the reform. Thus if we assess the first assumption for example, birth cohort is considered to account for all relevant variation between individuals when observable characteristics are fixed.

However, it is possible that the unobservable factors, ε_i, may include some confounding factors that are associated with probability of CARD. First, it can be argued that perhaps other policy shocks took that place around the same time as the alcohol reform may affect the plausibility of using birth cohort as a treatment variable for CARD. There are some policy shocks that partially coincide with the young adulthood period of the same birth cohorts that I use for 1969 alcohol reform analysis. Two of the potential confounding reforms are laws on gender equality of labor income in 1962 and abortion law in 1970. Changing social norms regarding gender equality since 1960s might be reflected alcohol consumption of females (Karlsson & Österberg, 2010). In 1965 the 6-day working week was reduced to 5 days which might affect alcohol consumption habits for both genders by increasing free time and thus the opportunity for excess alcohol consumption. If one of these other reforms were to affect alcohol consumption habits of the young adults, one might think that alcohol consumption would increase the following year. From figure 1 it becomes evident that the year following the alcohol reform (1969) saw clearly the biggest annual change in alcohol consumption between years 1960-1979 and none of these other reforms mentioned are followed by exceptionally high changes in alcohol consumption. Thus, following this piece of evidence, it is difficult to see how any of the other reforms would affect alcohol consumption habits in the long term in any significant way.

Second, the late 60's and early 70's involved an extensive internal migration due to late urbanization following the rapid automatization of agriculture (Waris, 1974). Kallio (1982) also emphasizes the importance of the urbanization process in the decline of homogenous cultural norms in rural Finland. Thus, migration is perhaps one element that might affect the urban-rural gap of alcohol-related problems especially if migration rates differ between cohorts. Once an individual is detached from his original living environment it could be that some of his unobserved inherent characteristics eventually lead him to self-select into "wet" living environment.

Third, and perhaps most importantly, the home production of alcohol and regional tastes towards home production of alcohol may vary greatly between different areas. Thus it can be that the legal geographical alcohol availability doesn't reflect the real urban-rural gap in alcohol supply. Given the long history of home production of alcohol in rural areas (see e.g. Mäntylä 1995), it very well may be that illegal alcohol production has effectively substituted the relative unavailability of the legal alcohol products, especially among the risk group of alcohol users. The effect of treatment thus could be confounded if there is variation between cohorts (trends) in illegal alcohol consumption and substitution from illegal to legal alcohol consumption after the reform vary by cohort after observed characteristics are controlled for. Unfortunately reliable statistics on the magnitude of home production of alcohol is not available.

Whereas the first identifying assumption is very likely to be consistent with reality, the two latter are may be problematic. Table 2 shows evidence that people in the urban and rural areas are somewhat different with regard to population size and education. Prior research has shown that low education is associated with alcohol-related mortality (Mäkelä, 1999). Thus it is possible that relatively high increase in geographical alcohol availability in the rural areas could be associated disproportionately high rate of reduction in the urban-rural gap in cohort CARD-rates. Given this evidence, one should consider magnitudes of the effect regarding the change in geographical alcohol availability with possible upward bias in mind.

To assess the impact of reducing the MLDA more closely I examine whether the CARD-rates of the newly eligible rise above the cohort alcohol mortality trend. As I now include a continuous trend variable I turn to use the logit model which more suitable for the analysis of binary outcomes with continuous predictors. The basic logit model that I estimate is

$$y_i = 1[{y_i}^* = a + c + \varphi_1 D_{1949} + \varphi_2 D_{1950} + \varphi_3 D_{1951} + R_i + \varepsilon_i > 0], \qquad (5)$$

where ${y_i}^*$ is a latent variable and $1[\cdot]$ is an indicator function that defines the binary outcomes, here CARD. It takes on the value one if the event in brackets is true, and zero otherwise. Cohort CARD-trend is captured in c, D_{1949} to D_{1951} refer to dummy variables for all newly eligible birth cohorts separately and R_i encompass regional fixed effect controls such as municipality fixed effects or pre-reform distance to Alko.

6 Results & Discussion

6.1 Descriptive statistics

Table 1 reports Census 1970 average characteristics for CARD's (ones) and non-CARD's (zeros) for cohorts 1940. The most important difference between the two groups is high representation of men in mortalities. Men account over 80 percent of CARDs. Also there were a greater fraction of blue collar workers and singles in CARDs than in the comparison group. The means of distance variables are all smaller in the comparison group. This is driven by the fact that the greater fraction of CARD's has lived in urban areas compared to non-CARDs. This notion is backed by the higher average among the home municipality sample population for CARDs vs. non-CARDs. Table 1 also reports Census 1970 average characteristics for zeros and ones separately for both sexes. There no apparent differences between genders in which background characteristics associate with CARD. Women appear to change municipality of residence more often than men. Also non-movers have higher rate of CARD than movers, although this data omits those that have deceased or moved abroad during the follow-up. Also one must also recall the critique over the comparability of the data between birth cohorts discussed in Section 4.2.

Table 2 reports Census 1970 average characteristics of birth cohort 1940 with regard to distance "treatment" variables. The oldest birth cohort is used to provide a general view of the differences in treatment categories as they are most likely to have completed education. The people living in rural Finland are less educated and are less likely to change municipality of residence within five years. These variables are not used in estimation phase as they are suspects of being outcomes of the reform.

Table 4 describes the cohort differences in some relevant background characteristics. Younger cohorts are more likely live in rural areas in Census 1970, and are more likely to change municipality of residence. Table 4 also describes the CARD-rates by cohort for movers and non-movers between Censuses 1970 and 1975. Among the youngest cohorts the non-movers have considerably higher rates of CARD. In the older cohorts there is not much difference in CARD-rates between movers and non-movers.

Figure 5 show graph of mean CARD-rates by birth cohorts according urban/rural residential status in 1970. We can see that the CARD-rates do not converge as year of birth increases. CARD-rates also show a very steep rise between cohorts 1949 and 1950.

6.2 Estimation results & discussion

Table 5 shows the estimation results of CARD risk with model (1). In Column 1, CARD is regressed on four birth cohort categories using only gender as a control variable. Cohorts are categorized the following way

$$D_c = \begin{cases} 1, & \text{if } 1940 \leq \text{cohort} \leq 1944 \\ 2, & \text{if } 1945 \leq \text{cohort} \leq 1948 \\ 3, & \text{if } 1949 \leq \text{cohort} \leq 1951 \\ 4, & \text{if } 1952 \leq \text{cohort} \leq 1955 \end{cases}$$

As reference group I use i) men ii) born in 1945-1948 iii) living in urban area in 1970 iv) with zero change in distance to Alko at the reform. The estimation results should be interpreted correspondingly. CARD-rates of birth cohorts group 1949-51 differ significantly from 1945-1948. The birth cohorts 1949-51 have 0.2-0.3 percentage points higher risk of CARD depending on different specifications. For birth cohorts 1940-44 this risk is lower 0.2 percentage points vs. reference group. For birth cohorts 1952-56 CARD-risk is 0.11-0.12 percentage points higher compared to the newly eligible cohorts. The higher CARD-risk is expected because cohorts 1949-1950 had legal access to alcohol at the mean ages of 20 and 19 respectively compared to age 18 of cohorts 1951 onwards.

The main effect of “rural” is statistically significant and can be interpreted the people living in rural area in 1970 to have 0.24-0.25 percentage lower risk of CARD. This only tells the mean difference between the rural and urban area CARD-rates for cohorts 1945-48.

The more relevant coefficients for our analysis are the interaction terms between cohort and rural variable and then the interaction term between cohort and change of distance to Alko. Neither of the interaction terms appears to be driving within cohort CARD risk in general. Because the interaction isn’t statistically significant in any of the cases, not one piece of evidence is found of the diminishing urban-rural gap.

The interaction between liquor availability and birth cohorts is significant in few cases. Cohorts 1940-44 that had proximity reduced 0.5-10 km, have approximately 0.33 percentage points lower CARD-risk vs. reference group. This is goes according to the logic of reducing urban-rural gap. The reduction of nearest distance to Alko could be regarded more for younger cohort groups vs. elderly cohorts if the usual interaction between habit formation and age is assumed. Thus lower CARD-risk among the older cohorts that we affected by "liquor treatment". However, this would be expected also for cohort 1940-44 and 10+ km change to Alko-distance but the sign is opposite of the expected and estimate is not statistically significant. According to the rationale presented in Section 5, if differences changes in geographical availability of alcohol would affect cohort-specific CARD-risk, it would be seen as negative sign in cohort category 40-44 and positive in 49-51 and 52-55 when interacted with measures for the change in beer and liquor availability. This, however, is not the case generally and the signs for in many of the cases part from the logic of second hypothesis presented in Section 5.

If cohort-rural and cohort-ΔA –interactions would have shown clear evidence of reduction of urban-rural gap, the second hypothesis of this study may be regarded plausible. For most part, I do not find evidence of the decrease of urban-rural gap in legal distribution channels leading to a decrease in urban-rural gap in CARD-rates. As we see from Figure 5, there appears not to be even slightest reduction of urban-rural gap in CARD-rates by birth cohort. Thus I conclude that the measures for changes in legal alcohol availability used in this study do not capture significant impact on within-cohort CARD-trend. The results suggest that if increased legal alcohol availability had had a positive effects on CARD-risk through age at exposure, then this was relatively uniform within cohorts and thus irrespective of differences in changes of relative geographical availability.

The reform changed legal alcohol distribution channels quite heterogeneously across Finnish regions. So where does the above-mentioned zero-effect arise from? There's at least two possible reasons for this. First, the results open the question of whether there ever was a true “urban-rural gap” in alcohol availability for the “group at risk”. It might be that those individuals that had the highest inherent tendency of forming addictive behavior and highest demand for alcohol found a way to acquire alcohol even in the presence of relatively high transportation costs of legal alcohol. The zero result might thus be explained by widespread pre-reform home production culture in the rural areas and high rate of substitution between legal and illegal alcohol consumption following the reform. The urban-rural gap in alcohol-related mortality may in fact reflect something other than gaps in alcohol-distribution channels such as cultural differences between the rural and urban areas. These cultural differences might be reflected in tables 2 and 3 which show that the urban and rural population differed from each other in many ways.

Second, the zero effect of urban-rural mortality gap reduction may also arise from measurement error of legal alcohol availability. As stated before, this will increase standard errors and decrease t-statistic values of the LPM estimates. The measurement error might arise from migration within two years of reform and using simple Euclidean distance instead of road travelling distances.

Nevertheless, the evidence suggests that there is potentially an MLDA effect. However, one might consider this to be partially driven by alcohol-related cohort death trends. I take a closer look at cohort effects in auxiliary analysis (Tables 6-8) in which I estimate equation (5). Only odds ratios and confidence intervals for linear cohort trend and birth cohort dummies 1949, 1950 and 1951 are reported. Column (1) reports baseline model and column (2)-(5) includes different types of regional controls.

From table 6 it becomes clear that over cohorts 1940-1955, there is a statistically significant linear cohort trend in CARD-rates. Being born one year later is associated with about 4 percent increase in risk of dying due to chronic alcohol-related causes. Among the newly eligible birth cohort under the reduced MLDA, the then 18-19 –olds (cohorts 1950-51) have around 9-10 percent risk of CARD above the trend. These estimates are fairly stable regardless whether we use for municipality fixed effects[6], municipality size or pre-reform distance to Alko as regional controls.

Tables 7 and 8 report similar analysis separately for men and women and also by rural/urban status. It shows that males have an increasing mortality of

[6] Due to perfect prediction of failure in small municipalities, municipalities with sample population under 200 were deleted in the regression with fixed municipality effects.

about 4 percent per year. The male birth cohorts 1950-51 have 7.6% and 9.6% higher CARD-risk above the trend. This suggests that the reduction of MLDA is likely to have had an impact on the young men's alcohol consumption behavior in the long term. For women, the cohort mortality trend is steeper: 5.5% increase in urban areas and 7 percent increase in rural areas per year. A bit surprisingly the estimates for birth cohort 1950 are the highest (15.1 percent above trend for all women) among the newly eligible women after MLDA reduction. The birth cohort 1951 is associated with 4.8 percent increase in CARD above the trend. Although the estimates for birth cohort 1950 are large in magnitude, none of the newly eligible cohort estimates are statistically significant at 5 % level. This is probably due to relatively low prevalence of CARDs among women.

In short, the results from the auxiliary analyses provide support for the notions of Dahl (2004) regarding the age sensitivity of self-regulation. The 18-19 year-olds of year 1969 exhibit clearly higher CARD-risk within a fixed age interval. The estimates are in concordance with the first hypothesis of the study especially among men. Among men, the effect is the higher the younger the newly eligible were at the time of the reform. These results suggest that 18-19 year-old men were especially vulnerable for the reduction of MLDA in the long term. In other words, the young men have shown a tendency towards long-term addictive behavior towards alcohol when exposed to easy access to alcohol at an earlier age. For females this relationship is in the same direction but not as clear as for men. This is possibly because of changes in norms with regard to gender equality in the 1960's and 1970's.

7 Conclusions

Liberalization of Finnish alcohol policy created a situation in which different birth cohorts were exposed legal alcohol access at different age. This historical episode allows the study of the impact of mortality effects of the reform in the long-term. The findings indicate that having easy access to alcohol at an earlier age increases the risk of problem drinking proxied by alcohol-related deaths due to chronic causes.

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Figure 1. Percentage change in alcohol consumption in 1960s and 1970s

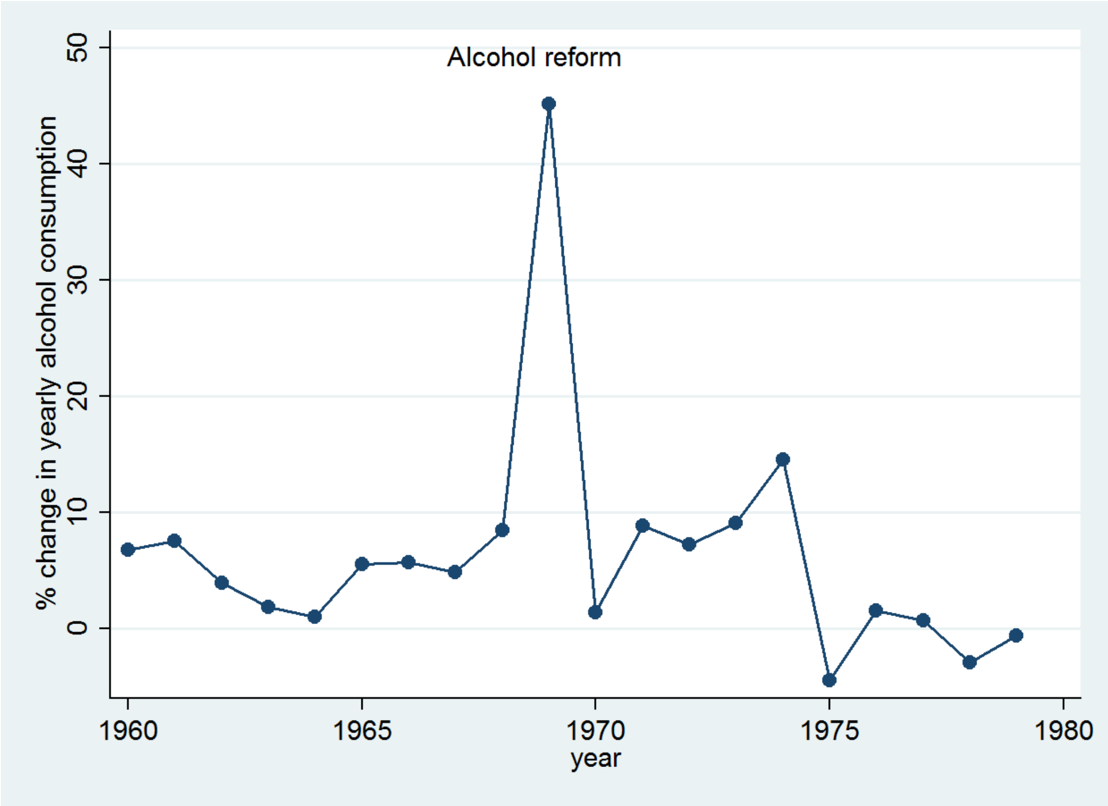

Figure 2: Chronic alcohol-related deaths and alcohol consumption in Finland between years 1969 and 2013. Source: Statistics Finland

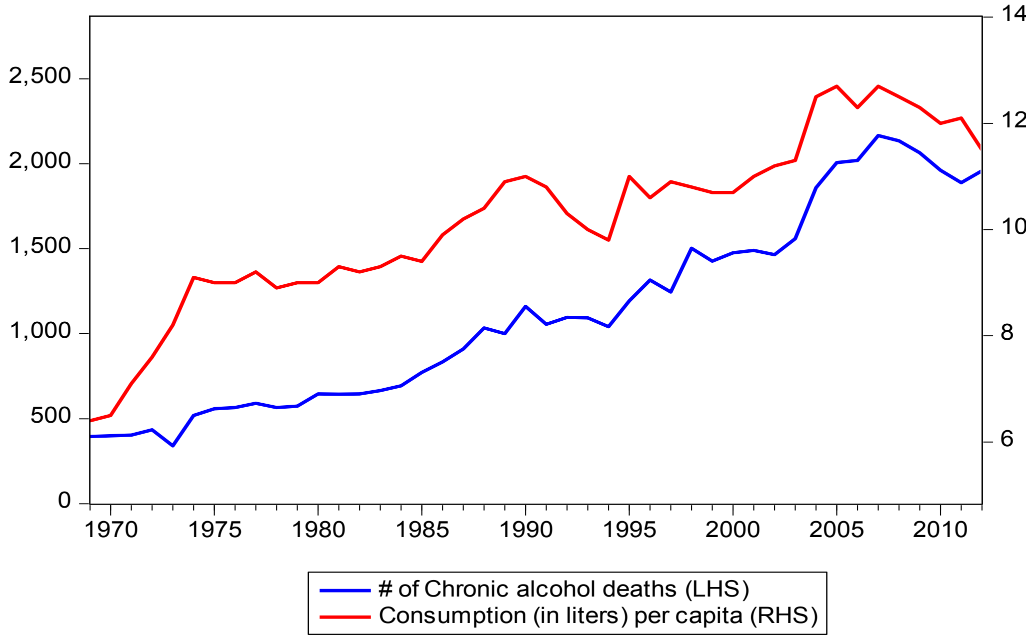

Figure 3: Alko outlets before and after 1969 alcohol law change

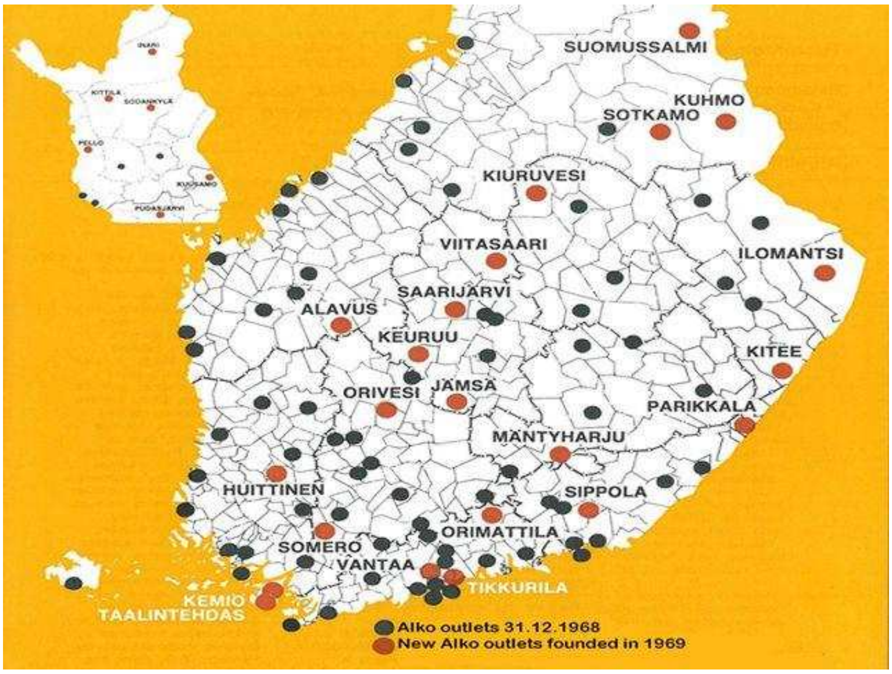

Figure 4. Timeline of events and measurement

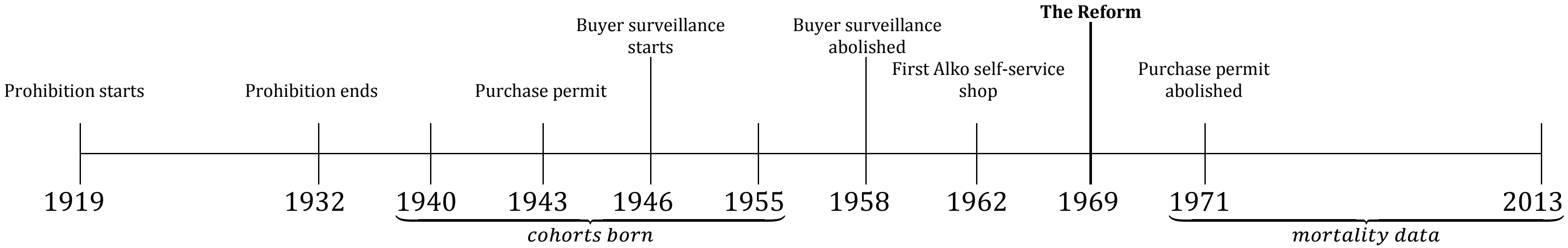

Figure 5. Urban-rural gap of CARD-rates for 1940≤ cohorts ≤1955

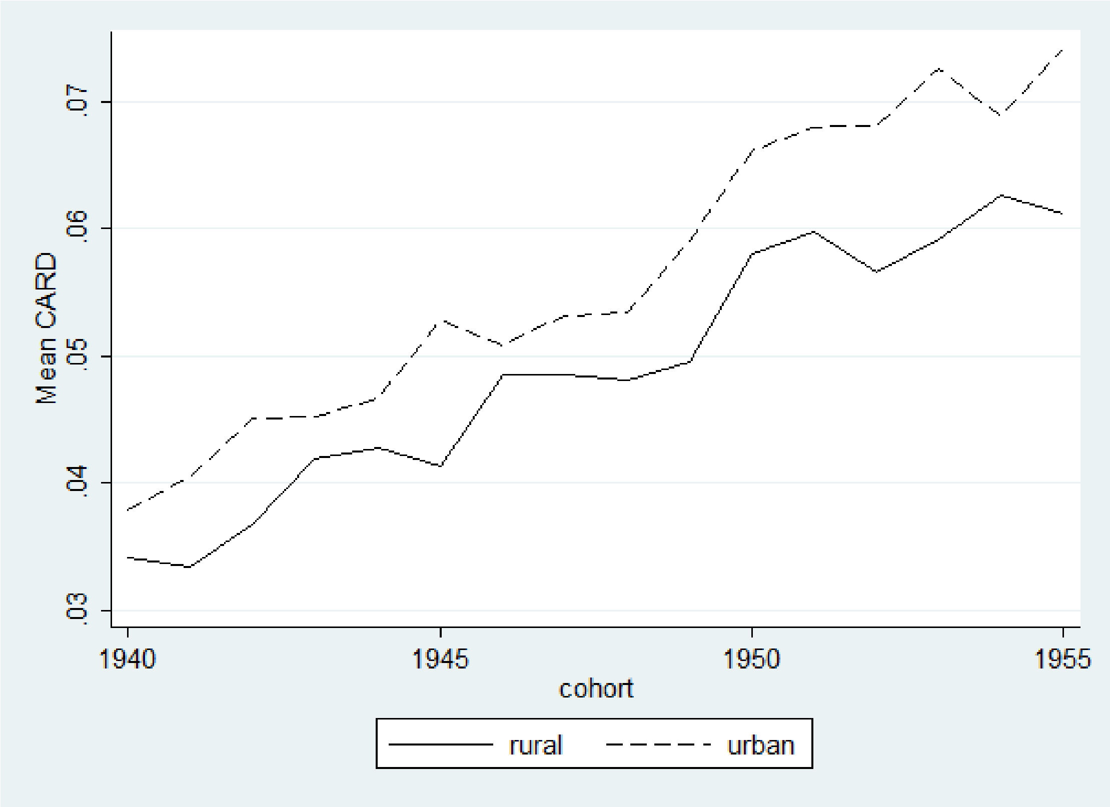

Table 1: Census 1970 Characteristics: Sample Means (cohorts 1940-1951)

	All		Men		Women	
	0	1	0	1	0	1
Age at Census 1970	24.02	23.49	24.03	23.53	24.02	23.35
Cohort	1945.98	1946.51	1945.98	1946.49	1945.98	1946.67
Sample pop. in hometown	6491	7214	5951	6645	6988	9456
# of children	1.572	1.586	1.706	1.669	1.45	1.32
Dist.to Alko 1968 (km)	15.1	14.18	16.19	14.821	13.86	11.356
Dist.to Alko 1969 (km)	12.02	11.15	13.04	12.026	11.01	9.181
Δdistance (km)	3.31	2.87	3.444	2.936	3.028	2.277
Sex						
Male	0.507	0.817				
Female	0.493	0.183				
Education						
upper secondary level	0.3268	0.2792	0.3415	0.2894	0.3118	0.234
low/high level tertiary	0.1281	0.0663	0.1129	0.0632	0.1437	0.0802
unknown	0.5451	0.6545	0.5456	0.6474	0.5445	0.6858
Labor movement						
1970 -1975	0.331	0.29	0.3163	0.2821	0.3453	0.323
1970 -1980	0.407	0.37	0.3927	0.3647	0.4214	0.3926
Observations	178191	12404	91592	10277	88953	2308

Note: Ones refer to individuals that have died due to a chronic alcohol-related disease (CARDs) during 1971-2013 at 31≤age≤62. with and without deaths due to accidental poisoning. Zeros refer to comparison group (non-CARDs).

Table 2: Background characteristics by treatment variable categories for birth cohort 1940

	All		Change in distance		
	Urban	Rural	0 km	0,5-10 km	over 10 km
Sample pop. in hometown	10306.2	945.1	7571.1	5999.2	543.4
Distance to Alko-68 (km)	2.6	36.6	9.9	22	60.2
Distance to Alko-69 (km)	2.5	28.6	9.9	18.2	24.9
Δdistance (km)	0.1	8	0	3.8	35.5
CARD	0.08	0.066	0.075	0.064	0.075
Education					
upper secondary level	0.231	0.199	0.223	0.181	0.209
low/high level tertiary	0.223	0.114	0.196	0.138	0.09
unknown	0.547	0.687	0.581	0.68	0.701
Labor movement					
Census 1970 vs.1975	0.229	0.207	0.221	0.229	0.194
Census 1970 vs.1980	0.299	0.251	0.286	0.272	0.233
Observations	7265	3948	9644	607	837

Table 3: CARD attrition because of timing between the reform and Census

Cohorts	# of CARDs in 69-70	Relative share per 100k
1955	1	1.192605844
1954	1	1.185129001
1953	2	2.376138227
1952	0	0
1951	0	0
1950	2	2.296988527
1949	3	3.345882994
1948	4	4.417400139
1947	3	3.288812086
1946	0	0
1945	5	6.212527271
1944	7	10.727998
1943	3	4.776933488
1942	3	5.948399736
1941	5	6.794149527
1940	4	7.540883684

Table 4: Some relevant birth cohort characteristics

	Rural		Change in distance			Moved 70-75	
cohort	0	1	0	1	2	0	1
1940	0.643	0.357	0.870	0.055	0.076	0.779	0.221
1941	0.644	0.356	0.866	0.056	0.078	0.763	0.237
1942	0.662	0.338	0.879	0.047	0.074	0.744	0.256
1943	0.663	0.337	0.876	0.055	0.069	0.724	0.276
1944	0.667	0.333	0.876	0.051	0.074	0.707	0.293
1945	0.669	0.331	0.878	0.053	0.068	0.682	0.319
1946	0.659	0.342	0.875	0.054	0.071	0.659	0.341
1947	0.644	0.356	0.870	0.053	0.077	0.631	0.369
1948	0.620	0.380	0.861	0.055	0.083	0.618	0.382
1949	0.581	0.419	0.855	0.055	0.090	0.608	0.392
1950	0.552	0.448	0.847	0.060	0.094	0.608	0.392
1951	0.537	0.463	0.842	0.059	0.099	0.618	0.382
1952	0.522	0.478	0.834	0.059	0.107	0.642	0.358
1953	0.510	0.490	0.831	0.060	0.109	0.680	0.320
1954	0.494	0.506	0.826	0.058	0.115	0.725	0.275
1955	0.492	0.509	0.825	0.058	0.116	0.784	0.216
Total	0.592	0.408	0.855	0.056	0.089	0.679	0.321

Note: Table reports rates of living in rural area, change

Table 5: Linear probability model for CARD-rate (ref. urban cohort49-51)

		1		2		3	
		Estimate	[S.E]	Estimate	[S.E]	Estimate	[S.E]
Female		-0,0153***	[0,0002]	-0,0154***	[0.0002]	-0,0154***	[0.0002]
Cohort							
40-44		-0,0021**	[0,0003]	-0,0023**	[0.0003]	-0,0022**	[0.0003]
49-51		0,0021**	[0,0003]	0,0027**	[0.0004]	0.0028**	[0.0004]
52-55		0,0032**	[0.0003]	0,0038**	[0.0004]	0.0040*	[0.0019]
Rural				-0,0024**	[0.0004]	-0,0025**	[0.0004]
Cohort*rural							
40-44				0,0006	[0.0005]	0,0006	[0.0006]
49-51				-0,0008	[0.0006]	-0,0008	[0.0007]
52-55				-0,0006	[0.0006]	-0,0006	[0.0006]
ΔA							
small ΔA						0,0032*	[0.0010]
large ΔA						0,0001	[0.0008]
Cohort*ΔA							
40-44	small ΔA					-0,0036*	[0.0013]
	large ΔA					0,0004	[0.0011]
49-51	small ΔA					-0,0019	[0.0015]
	large ΔA					-0,0001	[0.0012]
52-55	small ΔA					-0,0033*	[0.0014]
	large ΔA					0,0004	[0.0011]
Constant		0,0189***	[0.0002]	0,0198***	[0.0003]	0,0197***	[0.0003]
Obs		260574		260574		260331	
R^2		0,0052		0,0053		0,0054	

Note: Weighted Linear probability (WLS) estimates with heteroskedasticity robust standard errors for CARD taking value one if the person was died due to alcohol disease or accidental poisoning when 31≤age≤58. Only gender as a control variable. Urban cohorts 1945-48 population with no change to nearest Alko used as reference group. ***, ** and * refer to statistical signicance at least at the 0.1%, 1% and 5 % respectively.

Table 6: Logistic estimates (odds ratios) for CARD-rate

	1		2		3		4		5	
	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI
c	1.04***	[1.036,1.044]	1.04***	[1.036,1.045]	1.043***	[1.039,1.047]	1.042***	[1.038,1.046]	1.043***	[1.039,1.047]
D_{1949}	1.007	[0.943,1.075]	1.016	[0.949,1.087]	1.007	[0.943,1.076]	1.006	[0.942,1.074]	1.007	[0.943,1.076]
D_{1950}	1.101**	[1.032,1.174]	1.093**	[1.022,1.168]	1.103**	[1.035,1.177]	1.102**	[1.034,1.175]	1.103**	[1.035,1.177]
D_{1951}	1.087*	[1.019,1.159]	1.081*	[1.011,1.156]	1.09**	[1.022,1.162]	1.089**	[1.021,1.161]	1.09**	[1.022,1.162]
Municipality FE	no		yes		no		no		no	
Mun. size	no		no		yes		no		yes	
D. to Alko 1968	no		no		no		yes		yes	
# of mun.	446		276		446		446		446	
Obs.	260574		240941		260574		260574		260574	

Note: ***, ** and * refer to statistical signicance at least at the 0.1%, 1% and 5 % respectively.

Table 7: Logistic estimates (odds ratios) for CARD-rate for men

	All		Urban men		Rural men	
	OR	95% CI	OR	95% CI	OR	95% CI
Cohort trend	1.037***	[1.032,1.041]	1.041***	[1.035,1.047]	1.037***	[1.029,1.044]
Birth cohort 1949	1.01	[0.939,1.087]	1.052	[0.956,1.156]	0.959	[0.854,1.078]
Birth cohort 1950	1.076*	[1,1.157]	1.111*	[1.008,1.223]	1.048	[0.939,1.17]
Birth cohort 1951	1.096*	[1.019,1.178]	1.11*	[1.007,1.224]	1.089	[0.977,1.214]
Obs.	137193		78582		58611	

Table 8: Logistic estimates (odds ratios) for CARD-rate for women

	All women		Urban women		Rural women	
	OR	95% CI	OR	95% CI	OR	95% CI
Cohort trend	1.055***	[1.045,1.065]	1.055***	[1.042,1.067]	1.071***	[1.054,1.089]
Birth cohort 1949	0.949	[0.81,1.11]	0.978	[0.811,1.178]	0.873	[0.649,1.174]
Birth cohort 1950	1.151	[0.995,1.332]	1.134	[0.947,1.358]	1.188	[0.925,1.524]
Birth cohort 1951	1.048	[0.903,1.216]	1.045	[0.866,1.26]	1.067	[0.835,1.364]
Obs.	123381		76353		47028	

Note: ***, ** and * refer to statistical signicance at least at the 0.1%, 1% and 5 % respectively.

APPENDIX:

A1: Data on chronic alcohol-related deaths

This study only deals with alcohol-related deaths as an underlying cause. Statistics Finland has data on this from 1969 onwards. Causes of death in 1969 and 1970 suffer from high degree of attrition and thus we limit the statistical analysis to deal only with mortality data 1971 onwards. I do have data also on other causes of deaths 1971 onwards and alcohol as a contributory cause of deaths 1987 onwards for the 20% Census 1970. But these data are not in the focus of this research.

The classification of causes of death used in the statistics has changed a number of times. Since 1996, causes of death have been coded according to the ICD-10 classification (International Statistical Classification of Diseases and Related Health Problems). Between 1987 and 1995, the data were coded using the national classification of diseases 1987 and from 1969 to 1986, the international classification ICD-8 was in use. (Statistics Finland, 2014)

Table A1 reports ICD codes for causes of death which Statistics Finland uses in its reporting for alcohol-related deaths as an underlying cause. These include e.g. mental and behavioral disorders due to use of alcohol, alcoholic liver diseases, alcoholic pancreatitis. Accidental poisoning by/and exposure to alcohol (X45, E851 and E860) are most prominent cause of death in the list which could be argued that it doesn’t reflect history of problem drinking. To account for this, regression results and sample means are reported for CARDs with and without alcohol-deaths due to accidental poisoning.

Table A1: ICD codings of alcohol-related deaths as an underlying cause

Statistics Finland coding	1996 - ICD-10	1987-1995 ICD-9	1969-1986 ICD-8
41 Alcohol related diseases and accidental poisoning by alcohol	F10, G312, G4051,G621, G721, I426,K292, K70, K860,K8600, 0354, P043,X45	291, 303, 3050, 3575, 4255,5353, 5710-5713,5770D-5770F,5771C-5771D,7607A,7795A, E851 (E860)	291, 303, 5710, 577(males); E860

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