



Evaluating the impact of health reforms in the Netherlands: Assessing the impact of an alcohol ban on sexually transmitted infections in national surveillance data*

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ARTICLE INFO

Article history:

Received 28 November 2018

Received in revised form 23 June 2019

Accepted 22 July 2019

Keywords:

Health reforms

Alcohol ban

Public awareness campaign

Sexually transmitted infections

Routine surveillance data

ABSTRACT

Background: On 1 January 2014, the minimum age to buy alcohol increased (16–18 years), accompanied by a public awareness campaign (NIX18). Decreases in alcohol consumption are associated with less risky sexual behaviour. This study analyzed the association between the health reforms and *Chlamydia trachomatis* infections (chlamydia) among young heterosexual people.

Methods: Chlamydia positivity rates, age, and gender from all STI-clinic attendees between 16 and 19 years old in the Netherlands of 2010 to 2016 were obtained. Interrupted time-series assessed immediate and gradual trends in chlamydia rates.

Results: Among the control group (18–19 year olds) chlamydia rates increased 0.5% each post-ban month (95% Confidence Interval [CI] 1.002–1.008, $p = .001$). Among 16–17 year olds there was no monthly increase post-ban (Rate Ratio 1.000, 95% CI 0.993–1.007, $p = .948$). In terms of confounders, only controlling for partner notification dissolved these time trends.

Conclusions: We found that chlamydia rates after the alcohol ban differed between 16–17 year olds and 18–19 year olds. This demonstrates that the health reforms might have affected this secondary outcome, but obtaining certainty using national surveillance data is difficult. Specific studies should be designed, as now changes in chlamydia over time could be explained by STI-clinic policy changes, by changes on an individual level including reduced alcohol consumption or most likely by the combination of these factors.

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1. Introduction

Changes in regulations, laws, policies aimed at improving population health usually have a broader impact than just the direct effects aimed for by the changes. For example, changes in alcohol laws and regulations are implemented to change alcohol buying behaviour and consumption and consequently the frequency of negative outcomes, such as hospital admission due to binge drinking. However, alcohol regulations aimed at decreasing alcohol intake probably also have secondary outcomes, such as reduced aggression and sexual risk behaviour. Evaluation of the efficacy of changed laws and regulations is mostly done by studies specifically

* Open Access for this article is made possible by a collaboration between Health Policy and The European Observatory on Health Systems and Policies.

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set up for this reason, and therefore these secondary and long-term outcomes are often not assessed directly.

Prototypically when on 1 January 2014, the minimum age to buy alcohol and the age for possession of alcohol in public spaces were increased from 16 to 18 years in the Netherlands [1], evaluations focused on alcohol buying behaviour and consumption [2,3]. To strengthen the effect of the law changes, a public campaign was launched simultaneously, called NIX18, to strengthen the social norms of people younger than 18 not to drink. This campaign was evaluated in terms of changes in attitudes and norms regarding alcohol consumption and drinking intentions [2].

The relation between alcohol and sexual behaviour has been intensively investigated. Most prominently hypothesized is that alcohol influences sexual behaviour via disinhibition [4–7], therefore reduction in alcohol consumption should reduce sexual risk behaviour. Furthermore, the relation between alcohol consumption and sexually transmitted infections (STI) seems well established, as supported in a systematic review [8]. In this study, we evaluated the impact of the alcohol ban and NIX18 campaign on sexual risk behaviour in the Netherlands using readily avail-

able surveillance data. These health reforms were not developed to prevent STI, and the data we used were not collected to evaluate this public health intervention. We used data from the Dutch STI-clinics for sexually transmitted infections and sexual behaviour for a period of 7 years, four years before and three years after the introduction of the ban and campaign. Among young heterosexual people, the most reported STI is *Chlamydia trachomatis* (further referred to as chlamydia). The overall positivity rate of chlamydia increased over the years; compared to heterosexuals of 18 and 19 years, we hypothesized that this increase will be reduced or absent among 16 and 17 year olds from the intervention (the alcohol ban) onwards (1 January 2014).

2. Materials and methods

2.1. STI clinic chlamydia data

In the Netherlands, young heterosexuals can obtain an STI test at their general practitioner (also known as family or primary care doctor/physician) or at STI clinics of regional public health services. We obtained and analyzed 2010–2016 data from STI consultations and chlamydia diagnoses of heterosexual people between 16 and 19 years at the STI clinics. STI clinics provide low threshold, free-of-charge STI/HIV testing and care, targeting high-risk groups, including people younger than 25 years of age and people notified for STI exposure by a sexual partner. Due to financial restrictions, populations at highest risk of STI are prioritized since 2015, such as people with symptoms or notified by partners. Regional public health services systematically record which tests are performed in each consultation, which diagnoses have been made (if any), and also information on demographic characteristics and risk factors, including sexual behavior [9]. However, data on having had unprotected condomless sex under the influence of alcohol was not available for the study period. Data from general practitioners was not included, as information on demographic characteristics and risk factors is mostly unavailable for STI consultations with general practitioners. Ethical approval for the study was not necessary following Dutch law as the study used pseudonymised patient data collected for routine surveillance [10]. Ethical approval for the study was not necessary following Dutch law as the study used pseudonymised patient data collected for routine surveillance [10].

2.2. Statistical analysis

We adopted an interrupted time-series analysis, as it is considered a strong design when randomization is impossible [11–13]. Shortly, interrupted time-series analysis can study effects of interventions using surveillance data, by measuring changes in the outcome considering previous trends. A condition is that these previous trends remain stable over time [14].

First, we assessed testing practice over time, we analyzed the number of chlamydia tests to compare the period before and after the ban, adjusting for seasonality and long-term trends over time. Subsequently, we analyzed chlamydia positivity rates (adjusting chlamydia diagnoses for number of tests done by including the number of tests as an offset in the models) of the period before and after the ban, adjusting for seasonality and long-term trends. Analyses were based on time-series of monthly STI clinic data in the Netherlands during the years 2010–2016. The series of monthly counts were assumed to follow a Poisson distribution allowing for overdispersion.

We adopted methods for interrupted time-series to assess the effect of the alcohol ban: we included an indicator variable that changed in January 2014, with a value of zero given to months before the ban and a value of one for the months following the ban

(as marker for the step change). We hypothesize that the intervention (alcohol ban and campaign) affects the outcome as a gradual change in the gradient of the trend (i.e., slope change) as opposed to an immediate effect of the alcohol ban (i.e., step change). Time was defined as a continuous variable from month 1 of 2010 to month 12 of 2016 and was included in the model to capture long-term trends in chlamydia rates over time. The gradual effects were studied with an interaction term between the indicator for the effect of the ban and time. We separately analyzed the data of people aged 16 and 17 years (at time of the STI clinic visit), the essential group as these people had to adjust their behaviour in relation to the alcohol ban. We used people aged 18 and 19 as controls, as they were allowed to buy and drink alcohol both before and after the ban. Due to the small number of events, it was not possible to stratify our data for age and gender simultaneously.

Potential confounders we modelled were seasonal variations in chlamydia rates using harmonic functions of time, including two sine/cosine pairs [11,15]. Other temporal changes, for instance in demographic characteristics of STI-clinic attendees due to stricter triage at the STI clinics, that could affect chlamydia positivity rates were included in the model as covariates. Variables we assessed were gender (monthly proportion of women), having a Dutch nationality (monthly proportion of people with Dutch nationality), symptoms (monthly proportion of people who experienced symptoms), or notification (monthly proportion of people who were notified by sexual partners).

Finally, to detect short-term and long-term effects of the alcohol ban and campaign on chlamydia positivity rates, the variable for the start of the intervention was adjusted by introducing a lag to be able to analyze delayed intervention effects in the models. We investigated lags of 3, 6, and 12 months. Analyses were performed using R software (RStudio version 1.1.383) and IBM SPSS for Windows (version 24.0.0.1).

3. Results

Table 1 shows summary data of the number of people tested, the number of people diagnosed with chlamydia, the number of people notified by a sexual partner, and the average monthly chlamydia counts and rates over the 84 months of the time-series for the subgroups. Between 2010 and 2016, STI clinics had 68,080 consultations with heterosexual people between 16 and 19 years old, of these, 38,317 (56.3%) consultations and 7,182 (53.2%) chlamydia diagnoses occurred before the alcohol ban.

Performing time-series analysis on the number of chlamydia tests showed that the number of tests increased between 2010 and 2016, and that this effect is larger among 18–19 year olds (Table 2). There is no sudden step change in tests for either age group. Among the control group (18–19 year olds) the increase in the number of tests after the ban gradually declines.

As can be seen in Table 3, the principal model for the total population with the outcome chlamydia infection, offset by testing numbers, showed an underlying trend of 0.2% increase in chlamydia rates per month (95% Confidence Interval (CI): 1.001–1.004; $p = 0.009$). There was a 22% immediate effect (negative step change) associated with the alcohol ban (95% CI: 0.658 - 0.924; $p = 0.004$). There do not seem to be any significant time effects investigating the data stratified for gender (Table 3). Although the direction of both the step and slope change are opposite among men and women, the confidence intervals are large suggesting this effect could be explained by the small number of monthly chlamydia cases among young men.

Results of the age stratified analyses are presented in Table 3 and Fig. 1 and showed that among the control group (18 and 19 year olds) there was 0.5% increase in chlamydia rates for each post-ban

Table 1
Descriptive data and mean monthly chlamydia diagnoses (Standard Deviation) for the duration of the time series (2010–2016).

Population	N chlamydia positive	N tested	N (%) notified	Monthly chlamydia count	Test population STI clinics	Monthly chlamydia rate
Total	13492	68080	9142 (13)	161 (46)	810 (200)	0.20 (0.02)
Age group						
16 and 17 years	2487	12679	1563 (12)	30 (11)	151 (47)	0.20 (0.04)
18 and 19 years	11005	55401	7579 (14)	131 (38)	660 (161)	0.20 (0.02)
Gender						
Men	2968	15516	2451 (16)	35 (13)	185 (50)	0.19 (0.04)
Women	10524	52660	6691 (13)	125 (36)	627 (153)	0.20 (0.02)

Table 2
Time, step change, and slope change in total number of chlamydia test risk ratios after the introduction of the alcohol ban compared with before the alcohol ban.

Population	Time RR (95% CI)	p-value	Step change RR (95% CI)	p-value	Slope change RR (95% CI)	p-value
Age group						
16 / 17 years	1.004 (1.000 - 1.007)	.031	0.985 (0.674 - 1.439)	.938	0.996 (0.989 - 1.002)	.143
18 / 19 years	1.010 (1.008 - 1.012)	<.001	1.124 (0.916 - 1.379)	.263	0.993 (0.990 - 0.997)	<.001

RR: rate ratio; 95% CI: 95% confidence interval. All result are based on a slope change model adjusted for seasonality, with period before the alcohol ban January 2010 to December 2013 and the period after the ban January 2014 to December 2016.

Table 3
Time, step change, and slope change in chlamydia risk ratios after the introduction of the alcohol ban compared with before the alcohol ban, for the age groups the analysis was done including no lag, a 3 month lag, a 6 month lag, and a 12 month lag.

Population	Time RR (95% CI)	p-value	Step change RR (95% CI)	p-value	Slope change RR (95% CI)	p-value
Total	1.002 (1.001 - 1.004)	.009	0.780 (0.658 - 0.924)	.004	1.004 (1.001 - 1.007)	.005
Gender						
Men	1.009 (0.999 - 1.019)	.072	2.333 (0.782 - 6.961)	.129	0.983 (0.965 - 1.001)	.068
Women	1.004 (1.000 - 1.008)	.044	0.763 (0.490 - 1.190)	.233	1.003 (0.996 - 1.010)	.425
Age group (no lag)						
16 / 17 years	1.005 (1.001 - 1.009)	.016	0.921 (0.602 - 1.411)	.706	1.000 (0.993 - 1.007)	.948
18 / 19 years	1.002 (1.000 - 1.004)	.089	0.751 (0.625 - 0.904)	.002	1.005 (1.002 - 1.008)	.001
Age group (controlled for gender)						
16 / 17 years	1.005 (1.001 - 1.009)	.022	0.949 (0.616 - 1.462)	.812	0.999 (0.992 - 1.007)	.878
18 / 19 years	1.002 (1.000 - 1.004)	.099	0.751 (0.623 - 0.905)	.003	1.005 (1.002 - 1.008)	.002
Age group (controlled for symptoms)						
16 / 17 years	1.005 (1.001 - 1.009)	.014	0.936 (0.609 - 1.440)	.706	0.999 (0.992 - 1.007)	.867
18 / 19 years	1.001 (1.000 - 1.003)	.125	0.753 (0.627 - 0.905)	.003	1.005 (1.002 - 1.008)	.002
Age group (controlled for partner notification)						
16 / 17 years	1.003 (0.999 - 1.007)	.131	1.258 (0.806 - 1.964)	.313	0.995 (0.988 - 1.003)	.203
18 / 19 years	1.000 (0.998 - 1.002)	.966	0.899 (0.748 - 1.081)	.258	1.002 (0.999 - 1.005)	.221
Age group (controlled for nationality)						
16 / 17 years	1.005 (1.000 - 1.010)	.058	0.926 (0.597 - 1.436)	.731	1.000 (0.992 - 1.007)	.900
18 / 19 years	1.001 (0.999 - 1.004)	.318	0.747 (0.609 - 0.915)	.005	1.005 (1.002 - 1.009)	.006
Age group (3 month lag)						
16 / 17 years	1.004 (1.000 - 1.008)	.026	0.937 (0.583 - 1.506)	.787	1.000 (0.993 - 1.008)	.991
18 / 19 years	1.002 (1.000 - 1.003)	.053	0.718 (0.584 - 0.883)	.002	1.006 (1.002 - 1.009)	.001
Age group (6 month lag)						
16 / 17 years	1.004 (1.001 - 1.008)	.012	0.823 (0.479 - 1.411)	.478	1.002 (0.994 - 1.010)	.689
18 / 19 years	1.002 (1.000 - 1.004)	.014	0.658 (0.523 - 0.828)	<.001	1.007 (1.003 - 1.010)	<.001
Age group (12 month lag)						
16 / 17 years	1.003 (1.000 - 1.006)	.040	1.027 (0.444 - 2.377)	.950	1.000 (0.988 - 1.011)	.958
18 / 19 years	1.002 (1.000 - 1.003)	.018	0.598 (0.426 - 0.841)	.003	1.008 (1.003 - 1.013)	.001

RR: rate ratio; 95% CI: 95% confidence interval. All result are based on a slope change model adjusted for seasonality, with period before the alcohol ban January 2010 to December 2013 and the period after the ban January 2014 to December 2016.

month (slope change). In contrast and in line with our expectations, among the 16–17 year olds the increase in chlamydia rates that was apparent in the older age group was not found. In addition, exploring immediate effects findings show that among 18–19 year olds there was a significant step change, indicating that there was a 24.9% decrease in chlamydia positivity rate immediately after the ban. Among the 16–17 year olds there was no such sudden decrease.

The Durbin-Watson statistic for the 16–17 year old group showed no evidence of autocorrelation after controlling for seasonality (DW 2.29, $p = .741$), however for the control group (18–19 year olds) there was autocorrelation left (DW 1.46, $p < .001$; Fig. 1). Adjusting for the confounders of gender, having symptoms or nationality did not change the results. However, adding the proportion of people who were notified for potential STI exposure

by a sexual partner did significantly change the outcome. None of the time trends reached significance when adding STI notification as a covariate (Table 3). Investigating the effect of the alcohol ban introducing a lag of 3, 6, or 12 months resulted in similar yet slightly more pronounced findings compared to the model without lag (Table 3).

4. Discussion

We saw a gradual increase in chlamydia positivity rates after the ban, accounting for time trends before the ban, among 18–19 year olds. In contrast, this gradual increase was not observed among 16–17 year olds. This could indicate that the alcohol ban indeed reduced sexual risk decisions, and diminished the increasing trend of chlamydia positivity rates among 16–17 year olds as seen in

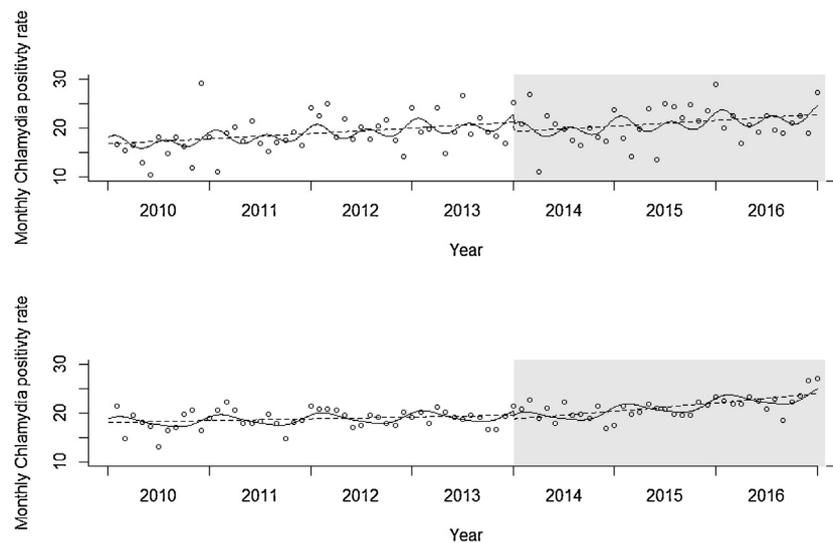


Fig. 1. Chlamydia rates (multiplied by 100) among STI clinics attendees in the Netherlands during the period 2010–2016. Observed (circles) and predicted (solid lines) rates among young people aged 16 and 17 (top), young people aged 18 and 19 (bottom). Dashed lines represent the deseasonalised trend of chlamydia rates before and after the introduction of the national alcohol ban (grey block).

the years before the ban. In other words, reduced alcohol intake may have prevented sexual risk behaviour and ongoing increases in chlamydia positivity rates. Stopping an increase of 0.5% does not seem like a big deal, but this reflects preventing a monthly increase, thus over a prolonged period this could have a substantial effect on the chlamydia epidemic. We also saw an immediate change among 18–19 year olds, we do not believe this is an immediate effect of the alcohol ban, but we have no data to investigate this sudden decrease

These findings were nullified when adjusting for notification by sexual partners. This could be due to effects directly associated with the alcohol ban; changes in alcohol consumption could have reduced the proportion of people notified among the 16–17 year olds (i.e., decreased risk behaviour due to decreased alcohol consumption leads to decreased notification and decreased chlamydia rates). However, other time trends, such as stricter triage could have increased the number of people notified in this age group. Triage criteria officially only changed a year after the alcohol ban, though. Both explanations would lead to a relative increase in notifications in the older age group, explaining why adding the proportion of people notified nullifies the time-effects on chlamydia positivity rates, as people who are notified have a larger risk for chlamydia. In the surveillance data we used we cannot disentangle these effects.

From a policy perspective, this emphasizes the importance of partner notification, since the quantity of partner notification represents an indicator of risk for STI. Partner notification is very effective as secondary prevention of STI (preventing transmission by testing and treating people). If reducing sexual risk behaviour indeed can be achieved in certain age groups through the introduction of an alcohol ban, this should be regarded as a primary prevention serving public health.

Other studies on health reforms focused on increased taxes on alcohol, making alcohol more expensive and possibly less available to certain groups [16–18]. These studies suggested that pricing policies could be an effective way to reduce alcohol consumption and related harm. We instead focused on the unavailability of alcohol in certain subgroups as prevention policy, specifically changes in minimum legal drinking age. In line with our findings, another study showed that drinking-age changes reduced gonorrhoea rates among the age group subject to the drinking-age regulation [18]. Taxes on alcohol affect everybody, whereas changes in drinking-age

only affect small subgroups. Therefore, the total national chlamydia rates might not show an effect.

4.1. Challenges in the evaluation of public health interventions

A previous study by Weatherly and colleagues [19] identified four methodological challenges that apply to the assessment of cost-effectiveness of public health interventions. We posit that these methodological challenges also apply to evaluations of the impact of (secondary) outcomes of public health interventions. First attribution of effects, the health reforms, the alcohol ban and campaign, were not the only variables that changed in time. A condition of the analysis is that trends in time remain stable, and in real-life this seldom can be guaranteed. During the study period there were some policy changes at the publicly funded STI-clinics. In 2015, a financial ceiling was implemented restricting the number of people who can be tested in a given year. As a result, many STI-clinics had to apply stricter triage to serve those people most at risk, e.g. people being notified for STIs by a sexual partner. Despite the delay of this policy change (one year after the ban), these and other changes in time could have influenced our outcomes. Interrupted time-series analyses do not exclude these kinds of explanations. Randomized controlled trials are the golden standard for evidence of the effectiveness of public health interventions (e.g. [20]). In this particular case, an RCT would not have been feasible, whereas interrupted time-series analysis was a feasible, proportionate, and informative evaluation method.

The second methodological challenge refers to measuring and valuing outcomes [19]. In the policy preparation stage, if sexual health was important enough it could have been arranged to measure alcohol consumption among STI clinic attendees in the months before and after the policy implementation. This provides a measure of the primary outcome (alcohol consumption), and an opportunity to link individual alcohol consumption pre- and post-ban to both sexual behaviour and STI.

The third methodological challenge of the evaluation public health interventions are the intersectoral costs and consequences [19]. The effect of decreased alcohol consumption on STI rates is a type of spillover effects. Moreover, as with most public health interventions the alcohol ban and campaign have wide-ranging impacts. Determining more exact figures on the exact size of the effect and causality could contribute to the successful evaluation

of this intervention, through more optimistic (and possibly more accurate) cost-effectiveness studies. This could pave the way politically or financially for an additional campaign, stricter enforcement of the alcohol ban, or other public health interventions.

The final methodological challenge in the evaluation of public health interventions is considerations of equity [19]. The alcohol ban is a policy implication that supports equity, because the ban is applicable to all people from all subgroups within this age category. In contrast to policy interventions, such as tax increases, which affect people from certain populations with less disposable income more [17]. In the current study, it was not possible to analyze meaningful sub-groups due to the limited number of monthly chlamydia diagnoses; therefore, it was not possible to evaluate equity consideration related to this public health intervention.

4.2. Strengths and limitations

A strength of this study is that we have data of all STI-clinics in the Netherlands, and thus on all chlamydia diagnoses performed there. Nevertheless, there are some limitations related to the data. As mentioned above, we did not have any data on alcohol consumption pre- and post-ban on an individual level and the numbers were too small to assess subgroups. Furthermore, this data is not representative for all young heterosexuals, we could not include data from young people testing at the GP or ordering their test online, the latter being a method for testing that is increasingly used in the Netherlands [21]. People visiting the STI clinic are usually regarded as at high risk for STI, but it is unknown how alcohol consumption factors into this characterization. We believe it is plausible that there are individual characteristics rendering a person at risk for both STI and problematic alcohol consumption, such as for instance sensation seeking.

In addition, there are limitations regarding the public health intervention. The alcohol ban and NIX18 campaign were evaluated in separate studies. According to a study investigating alcohol purchasing, enforcing regulations works, as buying alcohol did decrease after the alcohol ban, from 78 percent (2011) to 10 percent (2015) among the 16 to 17 year olds [3]. However, painting a different picture, an interview study showed that many 16 to 17 years olds drank alcohol in the past month (54%), and 83% indicated to have ever drunk alcohol [2]. Moreover, only 47 percent of the pre-18 year olds indicated to intend not to drink before their eighteenth birthday [2]. Taken together, only half of the young people form the intention to not to drink until they are 18 years old, and they might not be able to behave in accordance with their intention, the well-known intention-behaviour gap [22], or have answered questions partly in a social desirable way.

Even though the enforcement of alcohol purchases by young people seems successful, alcohol still is largely available through secondary supply [23]. Therefore, parents who are the target group for the NIX18 campaign should be targeted even more, as they are one of the most important suppliers of alcohol to people younger than 18 years [3]. Positively, 73% of parents do believe it is normal if people younger than 18 do not drink [2]. Nevertheless, only 13 percent of the parents find underage drinking unacceptable. Hence, if the public health intervention only has moderate effects on actual drinking behaviour it is to be expected that its effects on subsequent sexual risk behaviour may also be limited.

4.3. Implications

In the Netherlands, parts of the (public) health care system and policy departments responsible for the development of preventive plans operate in a stratified manner. The alcohol ban and public awareness campaign were treated as issue-specific, whereas secondary (health) outcomes exceeded this specific issue. In our

view, policies and policy evaluations could be improved, by using a more integrated approach. An example of an integrated approach is the HiAP (health in all policies) approach, coordinating action across health and non-health sectors [24]. During HiAP implementation, policies may be issue-specific, but a HiAP approach focuses on addressing systemic considerations (i.e., quality of life). Similarly, closely related (health) outcomes could have been integrated in this public health intervention. Some outcomes lend themselves better for an integrative approach, for instance drinking and driving, which has been investigated mostly in the context of the United States [16]. In the Netherlands, distances are much smaller, therefore drinking and cycling is emphasized more [25]. The big difference is that drinking and driving is bound by law and enforced. In the Netherlands, drinking and cycling (or drinking and having unprotected condomless sex for that matter) are not enforced. Therefore, an outcome such as sexual health is less easily evaluated.

As outlined above, evaluating health interventions in real-life settings is extremely difficult, as human decision making and behaviour is complex and determined by multiple personality factors that can interact with a multitude of environmental influences. Planned studies naturally focus on the primary outcomes of studies, and even then coming to a strong conclusion can be difficult. It is possible to assess effects of health intervention in routine collected surveillance data, but as this data is not collected for this research purpose researchers have to accept the shortcomings of the data. Accounting for the methodological challenges as discussed above could have aided in the evaluation though. Therefore, if policymakers are interested in the effect of changes on secondary outcomes these studies are best planned in advance, to be able to obtain information before and after the intervention is implemented, and measure all relevant information.

4.4. Conclusion

Taken together, the current research hypothesizes that the alcohol ban and NIX18 campaign should also have positively affected sexual risk behaviour, but this effect is difficult to establish in routinely collected data. If a secondary outcome is important enough, policymakers could in the development of public health interventions account for the methodological challenges in evaluating public health outcomes. Does the alcohol ban and campaign affect sexual risk behaviour, as reflected in chlamydia positivity rates, or could other factors such as changes in testing policies, or individual characteristics better explain the results? We believe the most likely explanation is that it is not one of these factors but a combination of the above.

CRedit authorship contribution statement

C. Den Daas: Conceptualization, Methodology, Formal analysis, Visualization, Writing - original draft. **F. Van Aar:** Data curation, Writing - review & editing. **B.H.B. Van Benthem:** Writing - review & editing, Supervision.

Declaration of Competing Interest

None.

Acknowledgements

We would like to thank Liesbeth Sluis and her colleagues for their valuable input on the manuscript. In addition, we thank all Public Health nurses and physicians of the STI clinics for their contribution to the data collection and medical microbiology laboratories for STI diagnostics.

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