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RESEARCH REPORT

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Does a local Alcohol Health Champion programme have a measurable impact on health and crime outcomes? A natural experiment evaluation of Communities in Charge of Alcohol (CICA) based on triangulation of methods

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Abstract

Background and Aim: Drinking alcohol may cause harm to an individual's health and social relationships, while a drinking culture may harm societies as it may increase crime rates and make an area feel less safe. Local councils in Greater Manchester, UK, developed the Communities in Charge of Alcohol (CICA) intervention, in which volunteers were trained to give alcohol-related advice to the public and taught how to influence policies to restrict when, where and how alcohol is sold. As part of a larger study, the aim of the current project is to measure the impact of CICA on health and crime outcomes at the lower super output (LSOA) geographical aggregation.

Design: Quantitative evaluation using four time series analytic methods (stepped-wedge design, and comparisons to local controls, national controls and synthetic controls) with findings triangulated across these methods. A cost–benefit analysis was carried out alongside the effectiveness analysis.

Setting and Participants: The general public in Greater Manchester, UK, between 2010 and 2020.

Measurements: The primary outcome of interest was alcohol-related hospital admissions. Secondary outcomes were accident and emergency (A&E) attendances, ambulance callouts, recorded crimes and anti-social behaviour incidents.

Findings: Triangulation of the results did not indicate any consistent effect on area-level alcohol-related hospital admissions, A&E attendances, ambulance callouts, reported crimes or anti-social behaviour associated with the implementation of CICA. The primary stepped-wedge analysis indicated an increase in alcohol-related hospital admissions following the implementation of CICA of 13.4% (95% confidence interval −3.3%, +30.1%), which was consistent with analyses based on other methods with point estimates ranging from +3.4% to 16.4%.

Conclusion: There is no evidence of a measurable impact of the Communities in Charge of Alcohol (CICA) programme on area-level health and crime outcomes in Greater

Manchester, UK, within 3 years of the programme start. The increase in alcohol-related hospital admissions was likely the result of other temporal trends rather than the CICA programme. Possible explanations include insufficient follow-up time, too few volunteers trained, volunteers being unwilling to get involved in licensing decisions or that the intervention has no direct impact on the selected outcomes.

KEYWORDS
alcohol, CICA, communities in control, evaluation, natural experiment, triangulation

INTRODUCTION

The misuse of alcohol is recognised to harm an individual's health and social relationships [1], and increasing evidence highlights the scale of alcohol's harm to others through second-hand effects [2, 3]. It may also harm society more generally, as urban areas can become less safe to visit [4] and crime may increase [5].The consumption of alcohol contributes significantly to health inequities. A so-called 'alcohol harm paradox' exists where alcohol harm is higher among those living in lower socio-economic communities, even when the amount of alcohol consumed is the same or less as those in more advantaged areas [6, 7].This is likely because of the prominence of binge drinking, lower access to health services, increased alcohol availability with fewer community assets and the accumulated effect of multiple risk factors (e.g. smoking and obesity) [8–10] in more deprived communities.

Alcohol outlet density has been shown to be associated with alcohol-related hospital admissions and crime [6], and both have been shown to decline faster in areas where more restrictive licensing policies are in place [11, 12]. In England, local authorities can address accessibility, serving practices and standards of operation of premises licensed to sell or supply alcohol using the regulatory framework of the Licensing Act 2003 [11].

Although the roles of community engagement on local alcohol licensing policy and licensing decisions have been discussed by governmental and non-governmental organisations, there is a paucity of published evidence on community engagement [12]. The World Health Organization highlights an important role for local communities in reducing alcohol harm [13]. In England and Wales, the sale of alcohol is subject to the Licensing Act 2003, which also recommends community involvement as part of the licensing decision-making process [14], and which can be achieved through, for example, statutory processes, consultation processes, representation or other relationship-focused initiatives. In 2014, the Greater Manchester Combined Authority (GMCA) agreed a 2014 to 2017 Alcohol Strategy, which included establishing a programme of activity to reduce alcohol-related harm. As part of this strategy, the GMCA and Greater Manchester Health and Social Care Partnership supported the launch of a new programme 'Communities in Charge of Alcohol' (CICA) in 2017 to train lay volunteers in all of GM's 10 local authorities. These volunteers completed the Royal Society for Public Health (RSPH) level 2 'Understanding Alcohol Misuse' course supplemented by additional training on alcohol licensing policy and became formal community 'assets' known as Alcohol Health Champions (AHCs) [15].They had two distinct roles: (1) to deliver brief alcohol advice to individuals in their communities; and (2) to help communities influence alcohol availability and strengthen restrictions in alcohol risk environments. The principle of community lay health champions has been well established [16], including for alcohol and the community's role in licensing [12, 17, 18], but the focus on AHCs has not previously been evaluated.

The logic model of the intervention outlines how the AHCs' activities might impact on direct alcohol-related health outcomes (alcohol-related hospital admissions, accident and emergency [A&E] department admissions and ambulance call outs) and other alcohol-related outcomes (crime and anti-social behaviour) in the medium-to-long term [19]. The aim of this paper is to quantitatively evaluate the effectiveness of the CICA intervention on these health and crime related outcomes, with the hypothesis being that alcohol harms (hospital admissions and crime) are lower in areas with the CICA programme than in control areas.

METHODS

Design and setting

CICA was a complex community-level intervention. The intervention allocation was not amenable to complete conventional randomisation (as recognised in the complex interventions guidance) [20], and the evaluation was, therefore, considered to have a quasi-experimental design or natural experiment [21]. Specifically, the intervention areas were pre-selected as part of the Greater Manchester Alcohol Strategy 2014 to 2017, but partial randomisation was achieved because the order that the areas started implementing the intervention could be randomised by the research team and rolled out in a stepped-wedge manner.

Ethical approval for the evaluation was received from the University of Salford Research Ethics Committee (reference number: HSR1617-135) and the University of Bristol (reference number: 82762). The study was registered with the International Standard Randomised Controlled Trial Number registry (ISRCTN81942890) [22] and the protocol was published [19, 23].

The CICA intervention

The CICA Programme is a programme aimed at training volunteer residents to become AHCs that took place in Greater Manchester. AHCs

were aged 18+ years, recruited from the community and who received the accredited and standardised United Kingdom (UK) RSPH AHC 1-day training course. The course covered understanding factors that result in alcohol misuse; the personal and social impact of alcohol harms; how to have a conversation using the Alcohol Use Disorders Identification Test-C [24]; and how to offer informal advice. This was supplemented with a half-day licensing training session designed and co-delivered by local licensing officers covering The Licensing Act 2003; Statements of Licensing Policy; the role of 'responsible authorities'; the availability of public licensing registers; and how to influence licensing decision-making through reporting licensing issues, making 'representations' or objections [25]. Following this training, AHCs were able to go out into their communities and talk to family members, friends, and local residents to provide alcohol-related advice opportunistically and at organised community events [26]. To promote community licensing action, AHCs could get involved in the licensing process by talking to managers of licensed premises directly about concerns or use formal reporting process through the local licensing authority [27]. Following a cascade training model, training was delivered initially by the RSPH and subsequently by CICA coordinators who had already been trained (a 'train the trainers' model). Further details are presented elsewhere [28, 29].

Each local authority defined its own CICA intervention area by specifically selecting areas of concern with respect to alcohol-related impact. These areas were defined by pre-existing geographical communities defined by lower layer super output area (LSOA) boundaries [30]. The smallest intervention area encompassed one LSOA and the largest contained three LSOAs (midyear population estimates combined: 1600–5500 people). The fidelity of the CICA intervention in the study period was, unfortunately, less than what was originally envisioned. Originally it was planned that CICA would be rolled out in all 10 local authority areas (across 19 LSOAs), however, the programme was not implemented in one area (comprising one LSOA) in the study period and two other areas withdrew after 6 and 9 months. Complementary qualitative research further indicated that, during the study period, none of the AHCs had been involved in licensing through official channels. There were also not many licence applications in those areas, thereby limiting the opportunity to engage with the licensing system and make objections at all. Only four representations were made against new applications in the intervention period in one area (compared to 0 made pre-CICA), whereas eight representations were made against full variations in two areas in the intervention period (compared to two made pre-CICA) [27].

Outcomes

The primary outcome for this analysis was alcohol-related hospital admissions (narrow definition; i.e. where the main reason for admission to hospital was attributable to alcohol) obtained from the Office for Health Improvement and Disparities (then Public Health England) Local Alcohol Profiles for England [31]. Secondary outcomes were as follows: A&E attendances (weekdays and weekends separately) obtained from NHS Digital (Hospital Episode Statistics); ambulance callouts to the area (weekdays and weekends separately) provided by the North-West Ambulance Service; recorded crimes (violent crimes, sexual crimes and public order offence; weekdays and weekends separately) and anti-social behaviour incidents provided by the Greater Manchester Police Authority. All outcome data were collapsed into counts per month per LSOA. For most outcomes, data were available from January 2010 to March 2020, inclusive. For recorded crimes and anti-social behaviour incidents, data were available from January 2013 to March 2020 and January 2013 to June 2019, respectively. Where outcomes could be split by weekend/weekday, outcomes that happened between 9 AM to 5 PM Monday to Friday were deemed to be weekday occurrences, for which it was assumed that during this time period the impact of alcohol on any events would be minimal, and those that happened between 3 PM on Fridays and 3 PM on Sundays, which were deemed to be weekend occurrences for which it was assumed that any occurrences had a significant probability of being alcohol-related (note that there was some overlap in both measures). Time periods where it was much more difficult to assign a probability of the involvement of alcohol, notably weekdays 5 PM to 9 AM and Sundays 3 PM to Monday 9 AM, were not included in the analyses.

Selected covariate time series data were obtained from publicly available national statistics datasets from the Office for National Statistics [32]. These included area deprivation defined as the quintile of the English Index of Multiple Deprivation based an area's ranking compared to all LSOAs in England [33], average resident population age and LSOA resident midyear population sizes, which was used as the offset in statistical modelling. We also included month in the time series, and season, derived as January to March, April to June, July to September or October to December.

Statistical analysis

Following recommendations to improve causal inference from natural experiment evaluations by including triangulation of results based on different designs and analytic methods (among other recommendations) [34], four analyses were used to assess each of the eight outcomes. Additional details for each methodology, including how control areas were selected, are provided in the Supporting Information.

1. Stepped-wedge cluster analysis: each intervention LSOA changes from 'control' to 'intervention' in a staggered roll-out of the intervention, and therefore, serves as its own control [35]. Time series were analysed using mixed-effects negative binomial, to account for over-dispersion, regression models with midyear resident populations sizes as the offset. LSOA was fitted as a random effect (to account for repeated measures), and a pre/post-intervention variable was included as a dichotomous indicator. Covariates were included as fixed effects. Further, as preliminary analyses indicated there were non-linear average temporal patterns for all outcomes,

time (month) was modelled using b-splines [36]. As the results from stepped-wedge cluster designs can be susceptible to specifications of the secular trend [37], we also explored the impact alternative specifications—in particular a linear trend and random trends nested in each area (both correlated and uncorrelated to the random intercepts).

2. Comparison to local controls, which incorporate the same population, council policies and local context, for example, but might be susceptible to local idiosyncrasies not incorporated in the data: each intervention LSOA was matched to three comparable control LSOAs using propensity score matching (additional detail provided in the Supporting Information), based on an a priori selected set of confounding variables (baseline [for 2016, the year before the start of the CICA programme] LSOA population density, deprivation, average age and alcohol-related hospital admissions and crime rates). Potential matches were LSOAs from Greater Manchester, but not neighbouring the intervention LSOA to avoid spill-over effects. The analysis dataset contained monthly time series data for 19 intervention and 57 matched control LSOAs. Outcomes were analysed using controlled interrupted time series analysis (cITS) [38] using the same hierarchical growth modelling analytic method described above.
3. 3. Comparison to national controls, which should not be susceptible to the same local idiosyncrasies, but might differ in population and local context not captured in the data: this design was similar to the local controls analysis except the matched control LSOA were selected from all LSOAs in England outside of the Greater Manchester area. Matching was similarly conducted using propensity scores based on the 2016 values, although because of the large amount of data (there are 32 844 LSOAs in England) [39] a prior selection of areas was made based on similarities in the pre-2017 time series of alcohol-related hospital admissions (the primary outcome) using 'dynamic time warping', a statistical methodology that can be used for matching time series [40]. We only had national outcome data for alcohol-related hospital admissions and A&E admissions.
4. Synthetic control analysis [41, 42]: counterfactuals were based on the dataset including the intervention LSOAs and their matched local controls. Synthetic controls were developed using Bayesian structural time series [43–45]. This methodology minimises the impact of biases resulting from individual comparisons with controls through weighting, but remains reliant on whether the controls are appropriate. It was previously successfully used in other studies of the impact of local alcohol policies on health and crime [46–49]. Analyses were done for each intervention LSOA separately and the average intervention effect was estimated by combining the effects across all areas using meta-analysis.

The results for the different methods are presented in forest plots for direct comparison.

All analyses were done in *R* software (version 4.2.1). The MatchIt package was used for the propensity score matching and the dtw package for dynamic time warping. Mixed-effects models were done using the lme4 package and synthetic control analyses were done using the bsts and CausalImpact packages. Meta-analysis was done using the metagen package.

All model results are presented as percentage change as a result of the intervention, alongside 95% CIs and *P* values; or Bayesian credible intervals and posterior probability *P* values [50] for the synthetic control analyses. We graphically assessed the pre-intervention parallel trends assumption for the cITS models using local (Figure S1) and national controls (Figure S2), which indicate that, whereas this assumption is met for the local control models, this is not the case for comparison to the national controls. The latter analyses should, therefore, be interpreted with caution. We further checked for zero-inflation, and where this was observed, we reanalysed using zero-inflated negative binomial mixed-effects models (using the glmmTMB package). CIs largely overlapped with the original models and inferences about the CICA programme effectiveness were the same, so we present only results from the original models.

With the exception of the synthetic control analyses (see below), the 10th LSOA in which the intervention was not rolled out and its matched controls (see below) were kept in the dataset and served as an additional control set. For the synthetic control analyses, which were conducted at LSOA level before combining them to obtain the average effect, this LSOA was not included.

Costs were applied to health and non-health outcomes following the effectiveness analysis to value the potential fiscal return of CICA.

RESULTS

Average monthly outcome counts in intervention LSOAs stratified as pre- and post-intervention are presented in Table 1. For all outcomes except anti-social behaviour, the monthly incidence was higher post-intervention than pre-intervention. LSOA outcome counts are small for ambulance callouts, crime and anti-social behaviour, so any absolute impact from the CICA intervention would similarly be expected to be small.

The results of all statistical analyses are shown in in Figures 1–5. There was little evidence of an impact of the CICA programme on alcohol-related hospital admissions. In fact, all analyses hinted at an increase in admissions in the LSOAs post-intervention compared to the controls of 3% to 16%, although 95% CIs included null or a reduction in admissions in all but one (comparison to local controls) comparisons. Results for ambulance callouts to the areas were contradictious and indicated fewer callouts in the stepped-wedge analysis (−15.5% and −5.2% for weekend and weekday callouts, respectively) and for the local control comparator (−4.2% and −0.6%, respectively), but increased callouts when compared to the synthetic control (+19.4% and +24.3%). There was little relative difference between weekend and weekday callouts. A&E admissions increased post implementation with 0.3% to 14.4% for weekends and ranged −0.2% to +14.3% for weekdays, depending on analytic design, compared to control conditions, but CIs only excluded the null for comparisons to local controls or to the synthetic control. Incidence of reported crimes in weekends

TABLE 1 Average monthly outcome counts for intervention LSOAs pre- and post-intervention.

	Pre-intervention			Post-intervention		
Outcome	Median	IQR	Range	Median	IQR	Range
Alcohol-related hospital admissions	118	(60, 205)	(2, 896)	134	(67, 203)	(2, 896)
Ambulance weekend	4	(2, 7)	(0, 45)	5	(3, 8)	(0, 45)
Ambulance weekday	4	(3, 7)	(0, 57)	6	(3, 9)	(0, 65)
A&E weekend	19	(15, 24)	(4, 49)	21	(17, 27)	(4, 60)
A&E weekday	25	(20, 31)	(5, 66)	28	(23, 35)	(5, 96)
Crime weekend	2	(1, 3)	(0, 45)	4	(2, 6)	(0, 45)
Crime weekday	0	(0, 0)	(0, 12)	2	(1, 4)	(0, 41)
Anti-social behaviour	3	(2, 5)	(0, 43)	2	(1, 4)	(0, 43)

Abbreviations: A&E, accident and emergency; LSOAs, lower layer super output area; IQR, interquartile range.

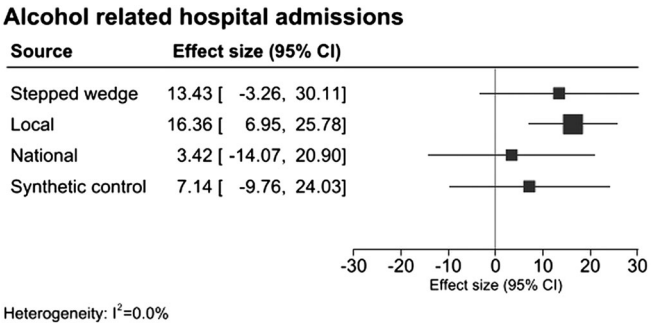

FIGURE 1 Average effect (%) and approximate 95% CI or credible interval (synthetic control) of Communities in Charge of Alcohol (CICA) intervention for alcohol-related hospital admissions (narrow definition).

and weekdays was also higher post implementation compared to controls, although point estimates were only meaningful for weekends (+3.4% to +31.1%, depending on design) and not for weekdays, because of low incidences in the latter. CIs for weekdays included null and reductions in incidence, in the stepped-wedge design and in comparison with local controls, but not when compared with the synthetic controls. The CICA intervention was associated with a reduction in anti-social behaviour incidence in the stepped-wedge analysis (−15.6%; [95% CI =] −31.2%, −0.01%) and synthetic control analysis (−39.3%; 95% CI = −45.0%, −33.6%), but not when compared to trends in local control LSOAs (+1.6%; 95% CI = −9.0%, +12.3%).

As outlined in the Analyses Plan, [51] we conducted additional sensitivity analyses for the stepped-wedge design to assess whether there might be a lag between the start of the implementation of the CICA intervention and any measurable effects. The results for 6-month and 12-month lags are presented in Table 2. Effect estimates vary considerably between the unlagged, 6-month lagged and 12-month lagged results and sometimes differ in direction of change. However, results are imprecise and CIs largely overlap. For the interpretation of Table 2, it should again be noted that the relative effects presented do not do justice to the absolute effects, which, because of the low incidence, are generally small. Nonetheless, there is no indication of a pattern that would indicate that lagged effects differ from the null effects observed in the main analyses. We further explored the impact of different specifications of the secular trends on the inferences. Results are presented in Table S1 and indicate minimal differences between the different specifications.

Additional sensitivity analyses were conducted for the synthetic control analyses by specifying different priors. The results are presented in Table S2 and indicate that inferences are only marginally sensitive to choice of priors.

The total cost of CICA implementation was £115 065 with two-thirds of the cost being attributable to initial training (including design and development, delivery, qualification and assessment fees). In terms of returns, the cost–benefit analysis did not show a positive fiscal return on investment with respect to health and non-health outcomes. Such a finding is not unexpected given the low intensity cost of the intervention and, for health effects, the limited timeframe of the analysis.

DISCUSSION

The CICA programme introduced the first alcohol-focused champion role of its kind. The training of dedicated community lay AHCs to reduce alcohol harm at a hyperlocal level, and doing this across nine city region local authorities, provided an opportunity to evaluate CICA as a natural experiment.

Analyses indicated no consistent evidence of a measurable impact of the CICA intervention on rates of alcohol-related hospital admissions, A&E admissions or ambulance callouts to the area, nor to reported crimes or anti-social behaviour incidents, within the 3-years of follow-up. In fact, effects on alcohol-related hospital admissions, A&E admissions and reported crimes were higher in the intervention areas post implementation compared to the pre-intervention period or to controls, although mostly with 95% CIs including null or opposite effects. It is implausible that the CICA interventions caused such effects, and possibly this should be considered an artefact of the non-random allocation of the programme. Only for anti-social behaviour incidence there is some evidence of

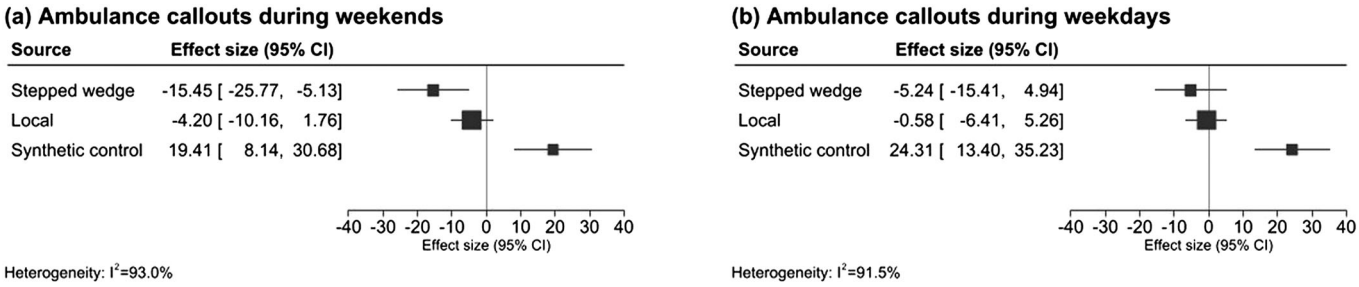

FIGURE 2 Average effect (%) and approximate 95% CI or credible interval (synthetic control) of Communities in Charge of Alcohol (CICA) intervention for ambulance callouts during (a) weekdays and (b) weekends.

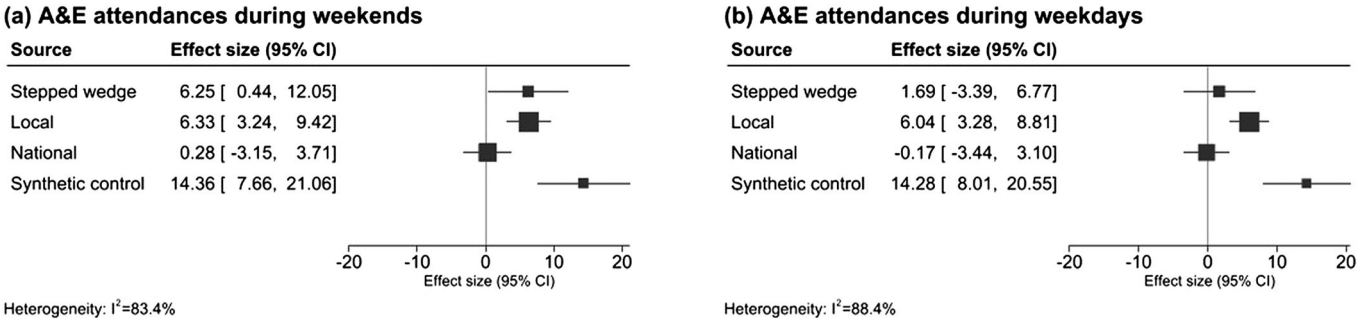

FIGURE 3 Average effect (%) and approximate 95% CI or credible interval (synthetic control) of Communities in Charge of Alcohol (CICA) intervention for accident and emergency attendances during (a) weekends and (b) weekends.

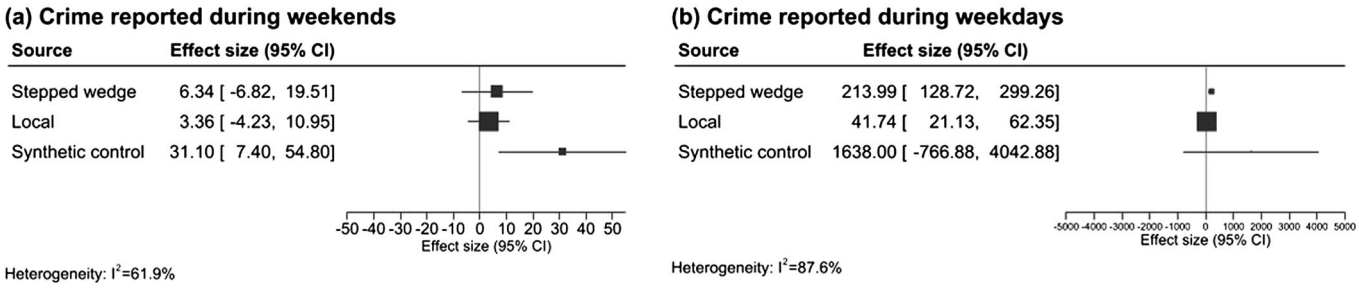

FIGURE 4 Average effect (%) and approximate 95% CI or credible interval (synthetic control) of Communities in Charge of Alcohol (CICA) intervention for reported crimes during (a) weekends and (b) weekdays.

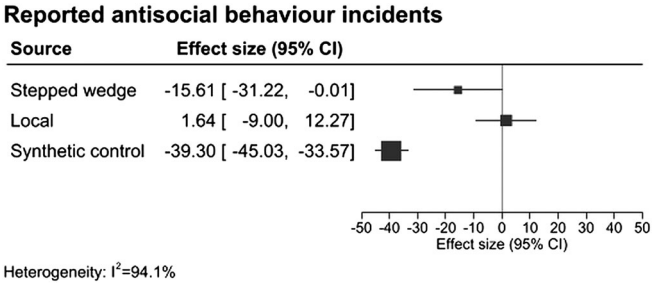

FIGURE 5 Average effect (%) and approximate 95% CI or credible interval (synthetic control) of Communities in Charge of Alcohol (CICA) intervention for reported anti-social behaviour incidents.

an effect in the hypothesised direction (i.e. a reduction in incidence compared to pre-intervention time period or controls). However, across the different analytic designs the strength that such an effect might exist is weak.

There always remains the possibility of bias because in evaluations of natural experiments the allocation of the intervention is not fully under the control of the research team. Indeed, the allocation of CICA intervention areas was decidedly not 'as-if random' [34]. We attempted to minimise potential bias: (1) by design, because we were able to influence randomisation of the sequence of implementation; and (2) analytically, by matching CICA intervention areas to control areas that were comparable with respect to alcohol harm indicators before the start of the CICA programme using propensity scores. In

TABLE 2 Sensitivity analyses for stepped-wedge analysis.

Outcome	Stepped wedge Effect (95% CI)	6 months lag Effect (95% CI)	12 months lag Effect (95% CI)
Alcohol-related hospital admissions	13.43% (−3.26, 30.11)	−10.36% (−23.83, 5.49)	−5.73% (−21.03, 12.54)
Ambulance weekend	−15.45% (−25.77, −5.13)	−5.93% (−16.68, 6.21)	23.29% (8.40, 40.24)
Ambulance weekday	−5.24% (−15.41, 4.94)	−8.06% (−18.36, 3.53)	19.44% (5.27, 35.52)
A&E weekend	6.25% (0.44, 12.05)	3.71% (−2.37, 10.18)	+1.81% (−4.56, 8.60)
A&E weekday	1.69% (−3.39, 6.77)	0.98% (−4.45, 6.72)	7.57% (−1.59, 10.73)
Crime weekend	6.34% (−6.82, 19.51)	−3.37% (−15.23, 10.15)	−7.23% (−20.16, 7.78)
Crime weekday	213.99% (128.72, 299.26)	54.95% (24.41,92.98)	−17.40% (−33.94, 3.29)
Anti-social behaviour	−15.61% (−31.22, −0.01)	2.87% (−16.82, 27.22)	42.46% (13.76, 78.39)

Abbreviation: A&E, accident and emergency.

addition, we aimed to avoid any issues of spill-over by not including any directly neighbouring LSOAs [52]. We further followed recommendations to improve causal inference by comparing against different sets of controls and using different analytic methods [34]. Triangulation aims to compare results with different potential sources of bias (and different data generation processes, which we were not able to incorporate here) [53]. Here, triangulation included four methods: the stepped-wedge design with internal controls, matched local control areas, national matched control areas and synthetic controls. Furthermore, the strength of inference from Bayesian analyses is also dependent on the specification of the priors in this study; here, sensitivity analysis with different priors indicates results are relatively insensitive to the specification of priors. This triangulation of results is an important strength of the current study, as comparison of these results indicates significant differences depending on what method was used. Although directions of effects were similar in most cases, the effect sizes differed significantly. Given that each method is acceptable for the evaluation of natural experiments, using only one of the methods would have resulted in plausible, but erroneous conclusions on impact. Having the directly comparable evidence from the complementary analyses enabled inferences across those results, and thereby strengthen our causal conclusions. We believe this approach guarded against incorrect inferences that would have likely occurred had we relied on only one of the evaluation designs or analytic methods.

The CICA intervention was delivered at a relatively low cost, but we were not able to estimate the fiscal returns of delivery and returns to population health given the low intensity of the delivered programme and its relatively short follow-up.

This study has a number of further strengths. The dataset was relatively large and should have been able to detect relatively small impacts on any of the outcomes, if these existed. The time series spanned 10 years, which benefitted the accurate modelling of the temporal patterns. The time point of when the intervention was implemented in each area was randomised by the researchers and followed by all but one area, which is likely to have minimised bias resulting from areas considered being most 'at risk' having CICA implemented first, for example.

There were also a number of limitations that should be noted. Unfortunately, we were not able to obtain all data for all comparisons, and as a result, comparisons to national controls could not be done for ambulance callouts, reported crimes incidents and anti-social behaviour. Although the researchers were able to randomise the sequence when the implementation of the intervention started in each area, they had no influence on the selection of the intervention areas themselves. This is likely to have impacted on the evaluations, and the observation that several of the analyses indicated an increase in negative health and crime outcomes, indicates that some bias is likely to have been present despite the successful matching of the local and national control areas to the interventions using propensity scores. Although datasets were reasonably large, the number of events recorded per LSOA per month was small for most outcomes. Aggregation over time or over geography could have improved this, but would have resulted in analyses less specific to the intervention areas and less accurate over time. It is further worth mentioning that the COVID-19 pandemic had some impact on this evaluation. The follow-up period stopped early in February 2020 because of the pandemic, and as a result, the last area in which the intervention was implemented only had a 9-month follow-up (rather than 12). Assessment of pre-intervention parallel trends indicated these were not parallel for some of the national controls; unfortunately, we were not able to retrospectively obtain the data for alternative control areas. This evaluation may have further benefitted from triangulation of results using a different dataset from a different source and with a different data generation process [53], but unfortunately, this was not possible.

Local AHCs as an intervention to engage communities in brief advice and statutory processes to affect alcohol availability in their local area have, to date, not been implemented elsewhere, so these results cannot be directly compared with results from other evaluations. However, there are some observations that we can make in relation to the null findings of this evaluation. Given the individualised approach of providing brief advice (the second role of AHCs), its impact on population statistics was, therefore, expected to be minimal. Further, we had hypothesised that likely impacts on 'hard' public health outcomes such as those included in this study (in contrast to

'softer', more difficult to measure, outcomes such as impacts on the 'experience' of the neighbourhood, feelings of belonging to a community and feelings of safety) were always going to be small at best. Moreover, the fidelity of the CICA programme was diminished for various reasons. During the period of follow-up none of the AHCs had been involved in licensing through official channels [27], which may have been because, compared to providing brief advice, this is the more difficult role of AHC. Interviews with AHCs and stakeholders indicated that a community 'voice' (through AHCs) in isolation has little opportunity to influence licensing outcomes and that collaboration with other 'voices' (i.e. the various responsible authorities) to influence decision-making is needed [27]. Equally, there was only minimal opportunity to engage with the licensing system resulting in only few representations made against applications in the intervention period [27].

Furthermore, in our logic model [19], we hypothesised impacts on health and crime indicators might be measurable in the medium-to-long term, and possibly, the 3-year follow-up from the implementation start date of the programme was insufficiently long. At different phases of the process evaluation, findings showed that the time, resource and overall infrastructure needed to develop and maintain champion capacity were underestimated [28, 29]. Additionally, considering the previous point, even if the AHC had engaged with the licensing system, it still would have been difficult to fundamentally change the alcohol environment of a community. Finally, it was also flagged by some licensing leads in the intervention areas that the scale of the CICA project might not have been sufficient (yet) to achieve measurable impact [27].

It might have been unrealistic to expect any measurable impacts within the confines of the current evaluation, given the limitations highlighted above. However, in an evaluation with a substantially longer follow-up it would have been increasingly difficult to confidently infer that any effects (if observed) would directly result from the implementation of the CICA programme. However, theoretically, if the initial group of local AHCs start engaging with the licensing regime in an area, and engaging the wider community in this, and if training of additional champions in subsequent rounds were to be maintained, it might be that eventually its population impact could be measurable.

To our knowledge, CICA is the first alcohol-focused champion role of its kind to be implemented and evaluated across multiple intervention sites. This evaluation has demonstrated the benefits of using triangulation of analysis methods for evaluating natural experiments; had we relied on just a single one of our four standard approaches we might have come to different, plausible, but erroneous, conclusions. In conclusion, our evaluation does not provide compelling evidence of a measurable impact of the CICA programme on health and crime in Greater Manchester within the first 3 years of its implementation. This conclusion tallies with the evidence from the complementary process evaluation that described that there might have been too few AHCs recruited and trained, insufficient engagement of the AHCs with the licensing system, the follow-up might have been too short or the scope of the CICA intervention might have been too small to be observable quantitatively yet. The CICA intervention might also not directly impact on the selected outcomes.

AUTHOR CONTRIBUTIONS

Lauren J. Scott: Data curation (equal); formal analysis (equal); investigation (equal); methodology (equal); writing—original draft (equal); writing—review and editing (equal). **Mira Hidajat:** Data curation (equal); formal analysis (equal); project administration (equal); writing—review and editing (equal). **Elizabeth J. Burns:** Funding acquisition (equal); investigation (equal); writing—review and editing (equal). **Cathy Ure:** Investigation (equal); writing—review and editing (equal). **Suzy C. Hargreaves:** Investigation (equal); writing—review and editing (equal). **Suzanne Audrey:** Investigation (equal); writing—review and editing (equal). **Margaret Coffey:** Investigation (equal); writing—review and editing (equal). **Susan Hare:** Investigation (equal); writing—review and editing (equal). **Noemia Siqueira:** Formal analysis (equal); investigation (equal); project administration (equal); writing—review and editing (equal). **Steve Parrott:** Conceptualization (equal); funding acquisition (equal); investigation (equal); supervision; writing—review and editing (equal). **Penny Cook:** Conceptualization (equal); funding acquisition (lead); methodology (equal); supervision (equal); writing—review and editing (equal). **Frank de Vocht:** Conceptualization (equal); formal analysis (equal); funding acquisition (equal); investigation (equal); methodology (equal); writing—original draft (equal); writing—review and editing (equal).

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DECLARATION OF INTERESTS

The authors declare no conflicts of interest.

DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available from OHID, Greater Manchester Ambulance Service and Greater Manchester Policy Authority. Restrictions apply to the availability of these data, which were used under license for this study. Data can be requested from the above organisations.

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