Quasi-experimental evaluation of municipal ice cleat distribution programmes for older adults in Sweden

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ABSTRACT

Introduction Fall injuries caused by icy road conditions are a prevalent public health problem during winters in Sweden, especially in older populations. To combat this problem, many Swedish municipalities have distributed ice cleats to older adults. While previous research has shown promising results, there is a lack of comprehensive empirical data on the effectiveness of ice cleat distribution. We address this gap by investigating the impact of these distribution programmes on ice-related fall injuries among older adults.

Methods We combined survey data on ice cleat distribution in Swedish municipalities with injury data from the Swedish National Patient Register (NPR). The survey was used to identify municipalities that have distributed ice cleats to older adults at some point between 2001 and 2019. Data from NPR were used to identify municipality-level data on patients who have been treated for injuries related to snow and ice. We used a triple differences design—a generalisation of difference in differences—that compared ice-related fall injury rates before and after intervention in 73 treatment and 200 control municipalities, with unexposed age groups serving as within-municipality controls.

Results We estimate that the average ice cleat distribution programmes reduced ice-related fall injury rates by −0.24 (95% CI −0.49 to 0.02) per 1000 person-winters. The impact estimate was larger in municipalities that distributed more ice cleats (−0.38 (95% CI −0.76 to −0.09)). No similar patterns were found for fall injuries unrelated to snow and ice.

Conclusion Our results suggest that ice cleat distribution can decrease the incidence of ice-related injuries among older adults.

INTRODUCTION

Fall injuries that occur outdoors are often associated with environmental risk factors such as snow and ice, which are very common during winter in Nordic countries. As a result, ice-related fall injuries are a prevalent public health problem in Sweden.1 In previous studies, fall injuries have been found to be associated with weather conditions in interaction with individual characteristics such as high age.1 The risk of being injured in an ice-related fall increases with age,2 which implies a need for interventions targeting older adults. Research suggests that ice cleats can reduce the risk of ice-related fall injuries.2,4 Distributing ice cleats could, therefore, potentially complement other community interventions, such as clearing snow from roads and walkways.6,7

WHAT IS ALREADY KNOWN ON THIS TOPIC

⇒ Ice cleat distribution may be a cost-effective way to reduce the burden of ice-related fall injuries, but comprehensive impact evaluations are lacking.

WHAT THIS STUDY ADDS

⇒ We perform a comprehensive, quasi-experimental evaluation of 73 municipal ice cleat distribution programmes targeting older adults in Sweden. Our primary estimate suggests that ice-related injury incidence was 8.2% lower on average after ice cleat distribution.

HOW THIS STUDY MIGHT AFFECT RESEARCH, PRACTICE OR POLICY

⇒ Our study reaffirms previous evidence suggesting that distributing ice cleats to older adults may be an effective prevention measure in settings affected by snow and ice.

Over the past decade, about 25% of Sweden’s 290 municipalities have distributed and offered ice cleats to older citizens to help combat the seasonal rise in ice-related fall injuries that typically occur during the Swedish winters. Previous research suggests that exposure to these programmes is associated with greater ice cleat use among older adults, especially in municipalities with high distribution rates per citizen.8 Model-based economic evaluations have also found that ice cleat distribution is likely to be cost-effective.9,10 However, there is still a lack of comprehensive evidence on how the distribution of ice cleats impacts fall injury rates. To our knowledge, only one study has directly investigated changes in fall-related injury rates following an ice cleat distribution programme, and the estimates from this study are limited to a single city (Gothenburg, Sweden).7 While their results showed a short-term reduction, the programme in Gothenburg was also quite successful in reaching its target population (62% of all eligible citizens collected a pair of ice cleats). Meanwhile, process evaluation data indicate that municipalities have variations in programme designs, which can impact programme effectiveness in terms of reach.6 It, therefore, remains unclear whether these programmes have had an impact on ice-related fall injuries. While greater distribution rates seem to lead to larger increases in ice cleat use,5 it also remains unclear if these results translate to greater impacts on ice-related injury rates in municipalities with...
greater reach. In this study, we aimed to address these issues by conducting a comprehensive impact evaluation of the ice cleat distribution programmes on ice-related injury rates among older adults in Swedish municipalities.

**METHODS AND MATERIALS**

**Data collection**

**Intervention data**

In June 2019, we sent an electronic survey to all municipalities in Sweden (n=290) to collect data on ice cleats distribution programmes, with non-responding municipalities receiving up to 4 reminders (the final reminder was sent in October 2019). In the survey, we asked if the municipality had ever distributed ice cleats. If they answered yes, we collected data on implementation dates, targeted age groups, programme costs and how many ice cleats they distributed. Further details about the intervention data collection can be found in Holmberg et al.6

**Injury outcome data**

We used municipality-level data from the Swedish National Patient Register (NPR)11 to estimate the number of patients treated in inpatient care or at hospital-based outpatient physician visits for injuries related to snow and ice during the study period 2001–2019. Per our request, the National Board of Health and Welfare provided aggregated data on the number of patients with International Classification of Diseases, 10th revision (ICD-10) external cause code W00 (Fall due to ice and snow) stratified by municipality, year, month and age. To avoid double counting (eg, due to readmission), they only counted the same patient once per calendar year for the same diagnosis. To calculate rates per 1000 population, we combined these patient numbers with population data from Statistics Sweden.12

To assess the risk of bias, we also collected corresponding data on the number of patients with external cause codes W01–W18 (Falls due to other specified causes unrelated to snow and ice) as a negative control outcome.15 We refrained from collecting mortality data because deaths due to falls on snow and ice are very uncommon in Sweden.6

**Study design**

We used a triple differences design14 to estimate the average impact of the ice cleat distribution programmes. Like a conventional difference-in-differences approach,15 the design controls for any time-invariant confounders by using preintervention data among the ages eligible for ice cleat distribution (‘eligible ages’), as well as national time trends by using concurrent outcome data from municipalities without ice cleat programmes (“control municipalities”). Our triple differences design also includes an internal control group consisting of within-municipality age groups that were ineligible for ice cleat distribution (‘ineligible ages’ defined here as 1–15 years younger than the age of eligibility), which allowed us to control for local time trends (eg, weather shocks and concurrent interventions). In control municipalities, we defined 65+ years as the eligible age group and 50–64 years as internal controls, as this is the most common age of eligibility for ice cleat distribution used by Swedish municipalities.6

**Statistical analysis**

For the statistical analysis, we constructed a panel dataset stratified by municipality, time, and eligible and ineligible age groups. We defined time intervals in winter periods (eg, 2003/2004) by aggregating data from 1 October to 30 April, as these months approximately capture the period at risk for ice-related injuries in Sweden (see online supplemental figure S1). The study period spans from the winter of 2001/2002 to 2018/2019 (18 winters). If a municipality implemented a programme during a given winter, eligible ages within that municipality were coded as treated from that period until the end of the study, reflecting the possibility that behavioural responses may persist even after the distribution has ended.

The programmes were implemented in different years (typically referred to as staggered adoption). It was recently discovered that two-way fixed effects models—the models typically applied in difference-in-differences studies—may be biased with this data structure.16 To estimate the impact of ice cleat distribution, we, therefore, applied an alternative imputation approach proposed by Borusyk et al,17 which does not suffer from bias due to staggered adoption.

The imputation-based method estimates the impact by first fitting a fixed effects regression model to not-yet-treated observations (ie, observations from the preperiod or unexposed groups). It then uses the estimated model to impute the expected counterfactual postperiod injury rates in all programme municipalities. It then calculates winter-and-municipality-specific impact estimates by taking the difference between the observed injury rates and the imputed counterfactual rates, and finally averages the estimates across programme municipalities and postintervention time points to estimate average effects. We performed the analysis in Stata V.17 (StatCorp), using the DID_IMPUTATION module,18 which performs the imputation and also accounts for within-municipality autocorrelation using cluster-robust standard errors. For further details, see online supplemental materials.

In our primary analysis, we aimed to estimate the average intention-to-treat effect19 of the programmes, which reflects the effectiveness of the programmes under ‘real-world’ conditions (including limited reach and adherence). In a secondary analysis, we also estimated the efficacy in a scenario where all targeted citizens collect a pair of ice cleats (ie, when the reach is perfect). To do this, we divided the municipality-specific impact estimates by municipality-specific reach before estimating the average programme impact, as proposed by Borusyk et al.20 Following Holmberg et al,6 3 we defined reach as the number of ice cleats distributed divided by the size of the postintervention target population in each programme municipality.

**Sensitivity analyses**

The validity of difference-in-differences analyses relies on the parallel trend assumption, which, in essence, means that the groups must have followed the same trend on the outcome in a counterfactual scenario without ice cleat programmes.20 To probe this assumption, we checked for differential pretrends visually. We also performed an F-test on time-specific placebo estimates up to 10 years before the implementation winter to assess if pre-existing differences jointly differed from zero, which if true would imply that any observed intervention impacts started occurring even before the intervention started (eg, due to non-parallel trends or anticipation effects).21 To further assess the risk of bias due to non-parallel trends, we performed a synthetic control analysis21 using the Bayesian dynamic multi-level latent factor modelling approach proposed by Pang et al,22 which relaxes the parallel trend assumption by modelling deviations from the national common trend using latent factors (see online supplemental materials for details). Finally, we conducted a falsification test by using fall injuries unrelated to snow and ice (ICD-10 codes W01–W18) as a negative control outcome.13
Patient and public involvement
No patients or members of the public were involved in the design of the study.

RESULTS
We received a response from 228 (78.6%) out of the 290 municipalities invited to answer our survey (figure 1). Of these, 78 municipalities responded that they had distributed ice cleats. Five of these were excluded as they reported having distributed ice cleats to all ages, and therefore, cannot be analysed using our triple difference methodology. All remaining municipalities—that is, those who answered that they had not distributed ice cleats or did not respond to our survey—were assessed for eligibility to be included as controls. To validate the survey responses, we searched online for communications about ice cleat distribution programmes for all 290 Swedish municipalities (information was usually available on municipal websites or reported in local newspapers). This procedure identified 12 additional municipalities with distribution programmes. Two of these had participated in our survey but reported having no programme. Due to the inconsistency and lack of programme data, these 12 were all excluded from the study. The final study sample included 273 municipalities (73 with intervention, 200 controls; figure 1). As a sensitivity analysis, we also restricted the controls to those who responded to the survey (n=148).

Descriptive programme data
Table 1 contains descriptive data on the 73 included ice cleat programmes. The majority (84.9%) of programmes had set the age of eligibility for ice cleat distribution to 65+ years. Most programmes (78.1%) were implemented late in the study period (between 2015 and 2019), with a mean observation time of 14.5 winters before and 3.5 winters after intervention (see online supplemental figure S2 for exact data on implementation period per municipality). The programmes varied greatly in terms of...
reach, with a mean number of ice cleat pairs distributed per eligible citizen at 0.40 (min: 0.01, max: 1.08). This number was highly correlated with the number of ice cleats purchased (Spearman’s $r=0.89$, $p<0.001$; mean: 0.48 pairs per eligible citizen). Combining the cost data provided by the municipalities with population numbers, we estimate that the mean programme cost per eligible citizen was €3.069 in 2018 (table S1). All 73 municipalities provided the essential programme data required for our intention-to-treat analysis (table 1). Seven municipalities did not provide data on the number of distributed ice cleats and were therefore excluded from the (secondary) efficacy analysis.

**Descriptive injury data**

Our analysis is based on data from 132465 patients treated for injuries due to an ice-related fall (numbers by age group and intervention status are presented in table 2). Considering the entire study period, the mean incidence of ice-related falls in the eligible age groups was 2.84 per 1000 person-winters in control municipalities and 2.39 per 1000 person-winters in programme municipalities. The mean incidence increased over time in all groups (online supplemental figure S3), but the increase was smaller in eligible than ineligible ages within programme municipalities (table 2). Corresponding data on the negative control outcome can be found in table 2 and online supplemental figure S4.

**Estimated impact of ice cleat distribution**

The results from the triple differences analysis are presented in table 3. The primary analysis suggests an average intention-to-treat effect of $-0.24$ (95% CI $-0.49$ to $0.02$) ice-related fall injuries per 1000 person-winters, which corresponds to a $-8.2\%$ change. Scaling the estimates by municipality-specific reach implies that the impact under ideal conditions is $-0.38$ (95% CI $-0.76$ to $-0.09$) ice-related fall injuries per 1000 person-winters, which corresponds to a $-12.5\%$ change.

**Negative control analysis**

The negative control analysis showed no evidence of effects on injuries unrelated to snow and ice (table 2).

**Pretrends assessment**

There were no visual signs of pretrends (online supplemental figure S5) and the pretrends tests did not identify significant ‘effects’ before the start of the interventions (table 2).

**Sensitivity analyses**

The Bayesian synthetic control analysis, which is more robust to deviations from the parallel trend assumption, produced results that were similar to the primary analysis (table 2; see online supplemental files for detailed results). Restricting the control sample to municipalities that responded to our survey also had limited influence on the results (table 2).

**DISCUSSION**

This study aimed to investigate the average impact of Swedish municipal ice cleat distribution programmes on ice-related fall injuries among older adults. Using a quasi-experimental design, we found evidence suggesting that distributing ice cleats may reduce injury rates by about 8% with a mean of 3.5 years of follow-up in the average programme municipality and by 12.5% if one ice cleat pair is distributed per eligible citizen.

To our knowledge, this is the first comprehensive impact evaluation investigating injury outcomes following multiple ice cleat distribution programmes. Overall, our findings are consistent with previous research. In terms of injury impacts, Bonander and Holmberg also found evidence of a reduction in emergency department visits for ice-related falls following a distribution programme in Gothenburg, Sweden. We have also found an association between ice cleat distribution and increased ice cleat use among older adults living in municipalities with ice cleat programmes, and data from other studies suggest that
using ice cleats can reduce the risk of ice-related injuries.\textsuperscript{3,4} It, therefore, appears plausible that the reductions we observed in this study are caused by increases in ice cleat use. In fact, a population impact analysis using external data on estimated increases in ice cleat use\textsuperscript{8} and data on the effects of ice cleat use from a randomised trial\textsuperscript{13} yields estimates that are similar to the empirical estimates in our study (−0.196 (expected population impact) vs −0.235 (our empirical estimate) ice-related injuries per 1000 person-winters; see online supplemental file 1 for details).

Previous economic evaluations—one conducted alongside the impact evaluation from Gothenburg\textsuperscript{3} and the other a modelling study investigating hypothetical ice cleat programmes in all Swedish municipalities\textsuperscript{14}—have reported that the economic benefits of ice cleat distribution may outweigh the costs by about 10–90 times.\textsuperscript{7 9} In online supplemental file 1, we perform an updated cost–benefit analysis using the estimates from the present paper, finding a benefit-to-cost ratio of approximately 85 using the official estimates for benefits per pedestrian injury averted used by the Swedish Transport Administration.\textsuperscript{23} Using more conservative benefit estimates, the benefit-to-cost ratio decreases to approximately 10. Thus, our results support the conclusions from previous economic evaluations indicating that ice cleat distribution is likely to be cost-effective.

**Strengths and limitations**

A key strength of our study is the large sample of intervention and control municipalities combined with high-quality register data on injury rates from the Swedish NPR.\textsuperscript{11} Using a triple differences design,\textsuperscript{14} we were able to control for (1) national time trends, (2) time-invariant unobserved confounders and (3) time-varying unobserved confounders that influence eligible and ineligible ages equally (eg, local weather shocks). Our data also passed several bias checks, including synthetic and negative control analyses.

Our study also has some limitations. First, our primary intention-to-treat estimate was imprecise. It seems unlikely that these programmes would be harmful considering previous research,\textsuperscript{3 5 7 8} but still worth noting the upper bound of the 95% CI is consistent with a small increase in risk (0.02 ice-related injuries per 1000 person-winters; table 2).

Our design also relied on younger, ineligible ages as internal controls. If there were spillover effects in terms of increased ice cleat use in these ages, our estimates might be biased towards a null effect. However, Holmberg\textsuperscript{et al.} found no evidence of spillovers on younger ages in terms of ice cleat use.

Another limitation is that our injury data do not cover injuries treated in primary care, as these are not reported to the NPR. The NPR, while generally deemed to be of good quality,\textsuperscript{11} may also fail to capture all ice-related injuries treated in outpatient or outpatient care, the same injury may be double counted despite efforts to reduce such risks, and data quality may change over time. However, we see no reason to suspect that any of these issues would be unique to any of the comparison groups in our study.

Despite efforts to validate our programme data, our results may still be susceptible to exposure misclassification bias. We suspect that the effectiveness results, which are based on a binary exposure classification, should be less susceptible to these problems than the efficacy estimates, which also rely on correct data about the number of distributed ice cleats.

Finally, our study is observational, and we cannot rule out the possibility of residual confounding that uniquely affects ice-related injuries among older adults in municipalities with distribution programmes. Our data are also ecological, and we cannot guarantee that the patients who drive the observed reductions are those who participated in the programmes.
Future perspectives
Important avenues for future research include studying similar interventions in other contexts, preferably with a randomised design. Longer periods of follow-up time after intervention (ours was, on average, 3.5 years) may also allow for a more precise estimation of the longevity of the impact.

As expected, our results suggest that greater reach leads to greater impact. In a previous process evaluation, we found that the strongest determinant of high reach was simply how many ice cleats the municipality purchased; on average, the municipalities which participated in our survey reported that 9 out of 10 ice cleats purchased were eventually distributed. Thus, it appears that those that aim high are usually able to achieve higher reach, but more in-depth analyses of other determinants of successful implementation are still needed to enable rational decision-making about the optimal design of ice cleat distribution programmes, including the most (cost-effective) means of communication and distribution.

CONCLUSION
Distributing ice cleats may be a useful and cost-effective complement to winter road maintenance for reducing the incidence of ice-related fall injuries among older adults.

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Contributors CB and JG conceptualised the study, acquired funding and managed the project. CB and RH performed data collection and management. EE and CB conducted the statistical analyses. EE drafted the initial manuscript with critical input to the interpretation of the results and approved the final version.

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Competing interests None declared.

Patient and public involvement Patients and/or the public were not involved in the design, conduct, or reporting, or dissemination plans of this research.

Patient consent for publication Not applicable.

Ethics approval This study was approved by the Regional Ethics Board in Uppsala (Dnr 2018/480; with addendum (DNR 2021-10338) approved by the Swedish Ethical Review Authority).

Provenance and peer review Not commissioned; externally peer reviewed.

Data availability statement Data are available on reasonable request. Data may be obtained from a third party and are not publicly available. This study used data from two sources. Our programme survey data are non-sensitive and will be shared with anyone on reasonable request. The injury outcome data, although aggregated, were classified as sensitive personal data by the National Board of Health and Welfare due to the fine-grained aggregation into cells with few patients. These data must, therefore, be handled in accordance with the Swedish Ethical Review Act (SFS 2003:460) and the European Union’s General Data Protection Regulation (GDPR; 2016/679). Researchers interested in gaining access to this part of the data must first apply for ethical approval from the Swedish Ethical Review Authority. With the appropriate approvals in place, the data can then be ordered directly from the National Board of Health and Welfare or by contacting the corresponding author.

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REFERENCES


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Online Supplementary Appendix

This Appendix contains additional output and analyses to support the conclusions of Eklund et al. “A quasi-experimental evaluation of municipal ice cleat distribution programs for older adults in Sweden”

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SUPPLEMENTARY FIGURES

**Figure S1.** Total number of patients from 2001 to 2019 treated for falls due to snow and ice (ICD-10 external cause code W00) in outpatient or inpatient care according to data from the Swedish National Patient Register, by calendar month. The shaded period (October-April) is time interval used to define a winter period in our study.
Figure S2. Timing of ice cleat distribution in each programme municipality (n = 73) by winter period relative to the beginning of the study (1 = 2001/2002; 18 = 2018/2019).
Figure S3. Trends in the incidence of ice-related fall injuries (ICD-10 external cause code W00) per intervention status and age range.
**Figure S4.** Trends in the incidence of fall injuries unrelated to snow and ice (ICD-10 external cause codes W01-W18) by intervention status and age range.
Figure S5. Event study plot showing triple differences impact estimates on ice-related fall injury incidence per 1,000 person-winters up to ten winters before and ten winters after the implementation of the ice cleat distribution programmes. Due to staggered adoption, the number of programme municipalities that contribute with data varies by time point, as detailed in parentheses in the labels on the x-axis. The pre-intervention coefficients were estimated to assess differential pre-trends (evidence of trends in pre-intervention coefficients or significant pre-intervention coefficients may be a sign of bias). The plot also includes post-intervention coefficients for reference, but we caution against interpreting variation in these given that the number of programme municipalities contributing with data drops off quickly after the first post-intervention period (variations over time may be due to the changing composition of the sample).
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TRIPLE DIFFERENCES METHODOLOGY, ADDITIONAL DETAILS

This section contains additional details about the statistical methodology used to estimate the impact of ice cleat distribution programmes in our study. We used a generalized version of difference-in-differences, referred to as triple differences (or difference-in-difference-in-differences). In a regression framework, the triple differences model can be expressed as follows [1,2]:

\[ Y_{igt} = \alpha_{ig} + \alpha_{it} + \alpha_{gt} + \tau D_{igt} + \epsilon_{igt} \]  

where \( Y_{igt} \) is the outcome variable (injury incidence per 1.000 person-winters) in municipality \( i \), age group \( g \), and winter \( t \); \( \alpha_{ig} \) are municipality and age group-specific fixed; \( \alpha_{it} \) are municipality and time-specific fixed effects; \( \alpha_{gt} \) are age group and time-specific fixed effects; \( \tau \) is the estimated average treatment effect on the treated; \( D_{igt} \) is an intervention dummy coded for treated observations and 0 otherwise, and \( \epsilon_{igt} \) is the error term. In our case, treated observations are defined as post-intervention time points in eligible age groups within programme municipalities.

The regression-based triple differences model in Equation 1, and its standard difference-in-differences representation (without an internal control group), has recently been shown to be biased when units implement the intervention at different times (also known as staggered adoption) if treatment effects are heterogeneous [2,3]. The bias occurs due to a previously unknown problem relating to improper comparisons where early adopters (municipalities that implement early in the study period) may inadvertently serve as controls for late adopters (municipalities that implement late in the study period).

Borusyak et al. [2] recently proposed a simple way to avoid this problem using imputation. The idea builds on the potential outcomes framework, where it is typically conceptualized that each unit has two potential outcomes: one potential outcome with an ice cleat distribution program, \( Y(1)_{igt} \), and one without, \( Y(0)_{igt} \). The causal effect of the program for unit \( i \), group \( g \), and time \( t \), is then given by \( Y(1)_{igt} - Y(0)_{igt} \), and the average treatment effect on the treated (ATT) is given by taking
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expectations over the post-intervention period in the treatment group, i.e., \( E[Y(1)_{igt} - Y(0)_{igt}|D = 1] \), which is our target quantity.

Assuming counterfactual consistency [4], we can write \( Y_{igt} = Y(1)_{igt} \) for all post-intervention observations in the treatment group. That is, we assume that in these periods and groups (where \( D = 1 \)), the realized outcome, \( Y_{igt} \), is the potential outcome under the treated state, \( Y(1)_{igt} \). However, when \( D = 1 \), \( Y(0)_{igt} \) is missing must be imputed to estimate the ATT.

The imputation-based estimator exploits the idea that in all other periods and groups (where \( D = 0 \)), we observe the potential outcome under the untreated state, \( Y(0)_{igt} \). The imputation estimator can be described in the following steps:

1. Subset the data to untreated observations only (i.e., when \( D = 0 \)) and estimate a regression \( Y(0)_{igt} = a_{ig} + a_{it} + a_{gt} + \epsilon_{igt} \) to obtain estimates of all fixed effects terms in Equation 1.
2. For each treated observation (i.e., when \( D = 1 \)), estimate the missing potential outcome by setting \( \hat{Y}(0)_{igt} = a_{ig} + a_{it} + a_{gt} \).
3. For each treated observation (i.e., when \( D = 1 \)), estimate unit-specific treatment effects by setting \( \hat{t}_{igt} = Y_{igt} - \hat{Y}(0)_{igt} \).
4. Estimate the ATT by taking the average of \( \hat{t}_{igt} \) over all treated observations (i.e., when \( D = 1 \)).

To estimate efficacy, we replace \( \hat{t}_{igt} \) with \( \frac{\hat{t}_{igt}}{R_i} \) in Step 4, where \( R_i \) is the number of ice cleats distributed per eligible citizen in municipality \( i \) (see Section 5.2 in Borusyak et al. [2]).

The imputation process solves the improper comparisons problem by only using untreated and not-yet-treated observations for model fitting. For more advanced statistical details (e.g., estimation of standard errors), please refer to reference [2].
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COST-BENEFIT ANALYSIS

This section details a back-of-the-envelope cost-benefit analysis using the effect estimates from our study.

According to our program survey, the average incremental cost of ice cleat distribution is €3.069 (31.28 SEK, 2018) per eligible citizen. For simplicity, we assume that this investment takes place initially at the program's start. To monetize the effect on injuries, we presume a monetary benefit per averted injury of €38,576 (393,198 SEK, 2018). This number, which is derived from external data [5–7], reflects the sum of avoided societal costs excluding productivity loss (€3,592 [36,612 SEK, 2018] [5,7]) and the willingness to pay (WTP) per averted quality-adjusted life year (QALY) loss associated with a pedestrian fall injury (QALY loss per injury [5]: 0.1488; WTP per QALY [6]: €235,178 [2,397,081 SEK, 2018]).

We assume that the program lasts 3.5 years, which is the average length of the post-intervention period in our empirical data. For simplicity, we assume that the effect on injury rates (0.0002351 prevented injuries per person-year according to our triple differences model) is evenly distributed over this period.

After monetizing the effect estimate and applying a discount rate of 3.5% per year for future benefits (recommended by the Swedish Transport Administration [8]), we obtain an estimated total benefit of €30.39 (309.8 SEK, 2018) per eligible citizen for the average ice cleat distribution program. Subtracting the initial investment implies a net present value of €27.32 per person (278.5 SEK, 2018; benefit-to-cost ratio: 9.9). Thus, the benefits seem to outweigh the costs from a (Swedish) societal perspective. This was also true in 94.75% of 10,000 Monte Carlo simulations accounting for sampling

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1 Our own calculation based on Table 25 in Olofsson et al [5], which contains data up to 6 months after an average pedestrian fall injury in a Swedish context. They provide a different total loss QALY estimate per person (1.387), which is based on extrapolation of the QALY loss from the year of injury to the average life expectancy in their sample. This is the official estimate currently used for economic analyses by the Swedish Transport Administration [8]. However, given the short data collection period, we take a conservative stance and assume that the health-related quality of life has returned to normal after 12 months. Re-calculation by applying the trapezoid rule [9] under this assumption which yields our conservative QALY loss estimate (0.1488). We note that using the official QALY loss estimates in our cost-benefit analysis implies a considerably larger benefit-to-cost ratio (84.65), which is very close to the model-based estimates provided in Bonander et al [7] (mean benefit-to-cost-ratio: 87), who also used the official QALY loss estimates.
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uncertainty in the effects and program cost estimates, assuming a normal distribution for the effect and a gamma distribution for costs.

**Replication code for R**

```r
# Seed for reproducibility
set.seed(201398)

# Avg. length of post-period in empirical study
post.period <- 3.5

# Conversion to Euro
conversion_euro = 0.09811

# QALY loss due to injury from IHE
hrq <- c(0.918,0.204,0.563,0.678,0.796,0.918)
# Last point assumes return to normal at 12 months

# Calculate QALY loss
time_diff <- c(0.002739726,0.035616438,0.126027397,0.335616438,0.5)

qaly_inj_base <- (hrq[1]+hrq[2])*time_diff[1]*0.5 +
(hrq[2]+hrq[3])*time_diff[2]*0.5 +
(hrq[3]+hrq[4])*time_diff[3]*0.5 +
(hrq[4]+hrq[5])*time_diff[4]*0.5 +
(hrq[5]+hrq[6])*time_diff[5]*0.5

qaly_healthy_base <- 0.918

# Modified benefit assuming conservative QALY loss
wtp_p_inj = 3324751
q_loss1 <- 1.387

q_loss2 <- qaly_healthy_base-qaly_inj_base
# Our conservative QALY loss assuming return to normal at 12 months

wtp_qaly <- 3324751/1.387

wtp_modified <- q_loss2*wtp_qaly

# Healthcare costs (subtracting production loss) from IHE report
hc_cost <- 36612

# Average treatment effect estimates
effect <- (-.2350829/1000)
effect_se <- ((0.0151396/1000)-(-.4853054/1000))/3.92
dist_prevented <- -rnorm(10000,effect,effect_se) # Flip sign to get injuries prevented
```

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benefit_per_prevented <- (wtp_modified+hc_cost)*conversion_euro
benefit <- (-effect)*benefit_per_prevented

dist_benefit <- dist_prevented*benefit_per_prevented

# Average cost per person
cost_mean <- 31.28006*conversion_euro #From our survey data
cost_se <- 3.860791*conversion_euro #From our survey data
cost_alpha <- (cost_mean/cost_se)^2

cost_beta <- (cost_se^2)/cost_mean

cost <- cost_mean

dist_cost <- rgamma(10000,shape=cost_alpha,scale=cost_beta)

# Discount rate
d <- 0.035

# Calculate base case results
npv.list=cost.list=benefit.list=list()

for (t in 1:4) {
  if (t == 1) {
    npv.list[[t]] <- (benefit-cost)
    benefit.list[[t]] <- benefit
    cost.list[[t]] <- cost
  } else {
    npv.list[[t]] <- (benefit/((1+d)^(t-1)))
    benefit.list[[t]] <- (benefit/((1+d)^(t-1)))
    cost.list[[t]] <- 0
  }
}

if (t == 4) { #Half benefit final year to account for 3.5 yrs of post-period data
  npv.list[[t]] <- npv.list[[t]]*0.5
  benefit.list[[t]] <- benefit.list[[t]]*0.5
}

npv.res <- sum(do.call("rbind",npv.list))
benefit.res <- sum(do.call("rbind",benefit.list))

cost.res <- sum(do.call("rbind",cost.list))
bca.res <- benefit.res/cost.res
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base.res <- c(benefit.res,cost.res,npv.res,bca.res)

# Probabilistic sensitivity analysis (PSA) function
sim.fun <- function(b,c) {
  npv.list=cost.list=benefit.list=list()
  for (t in 1:4) {
    if (t == 1) {
      npv.list[[t]] <- (b-c)
      benefit.list[[t]] <- b
      cost.list[[t]] <- c
    }
    else {
      npv.list[[t]] <- (b/(1+d)^(t-1))
      benefit.list[[t]] <- (b/(1+d)^(t-1))
      cost.list[[t]] <- 0
    }
    if (t == 4) { #Half benefit final year to account for 3.5 yrs of post-period data
      npv.list[[t]] <- npv.list[[t]]*0.5
      benefit.list[[t]] <- benefit.list[[t]]*0.5
    }
  }
  npv.res <- sum(do.call("rbind",npv.list))
  benefit.res <- sum(do.call("rbind",benefit.list))
  cost.res <- sum(do.call("rbind",cost.list))
  bca.res <- benefit.res/cost.res
  sim.res <- data.frame(benefit.res,cost.res,npv.res,bca.res)
  return(sim.res)
}

# Loop the PSA function
sim.list <- list()
for (i in 1:10000) {
  sim.list[[i]] <- sim.fun(b=dist_benefit[[i]],c=dist_cost[[i]])
}

sim.df <- do.call("rbind",sim.list)
benefit.lower <- quantile(sim.df[,1],0.025)
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```r
benefit.upper <- quantile(sim.df[,1],0.975)
cost.lower <- quantile(sim.df[,2],0.025)
cost.upper <- quantile(sim.df[,2],0.975)
npv.lower <- quantile(sim.df[,3],0.025)
npv.upper <- quantile(sim.df[,3],0.975)
bcac.lower <- quantile(sim.df[,4],0.025)
bca.upper <- quantile(sim.df[,4],0.975)
prob.costbenefit <- mean(sim.df$npv>0)
```
SYNTHETIC CONTROL ANALYSIS

This section details a sensitivity analysis to assess if our main estimates are sensitive to non-parallel trends by running a synthetic control analysis. Synthetic controls are a generalization of the difference-in-differences framework that can handle situations where pre-intervention trends diverge across units.

To implement the method, we applied the Bayesian dynamic multilevel latent factor model framework proposed by Pang et al. [10]. For our purposes, the benefits of this framework are three-fold: (i) it provides easily interpretable credible intervals for the effect estimates, (ii) it accepts outcomes among younger ages as time-varying covariates with municipality-specific coefficients, (iii) it allows for coefficient shrinkage on time-varying covariates to avoid overfitting, which is important when including noisy outcomes as covariates. The method helps handle situations with non-parallel trends in addition to estimating municipality and time fixed effects. In practice, this is done by subsetting the data to not-yet-treated observations and estimating latent time-varying factors and constant municipality-specific factor loadings; municipalities with similar factor loadings share similar trends. The observed counterfactual outcomes are then imputed based on the model.

We used the `bpCausal` package for R to run the analysis [10]. The package uses Markov Chain Monte Carlo (MCMC) algorithm to estimate parameters and perform model selection. Our model included the incidence of ice-related fall injuries per 1,000 person-winters in the treated age range as the outcome variable; a post-intervention treatment dummy, coded as one after the intervention in treated municipalities and zero otherwise; and the incidence of ice-related fall injuries per 1,000 person-winters in the negative control ages as a time-varying covariate. Following Pang et al. [10], we allowed for up to 10 latent factors. We also allowed the time-varying covariate to have a common (constant) fixed effect, municipality-level random effects, and time-level random effects. Coefficient shrinkage was used on all effects and on the factor loadings to assist with model selection and avoid overfitting. Priors on the shrinkage were set to Gamma(0.001, 0.001), as recommended by Pang et al. [10]. We performed 50,000 MCMC runs, discarding the first 5,000 runs as a burn-in period.
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The results are presented in Figure S5. We note that the pre-trends are consistently close to zero, implying that the method successfully handled non-parallel trends in the pre-intervention period. The average post-intervention estimate is -0.220 (95% credible interval: -0.445, 0.004) ice-related fall injuries per 1,000 person-winters, which is very similar to our primary triple differencing estimate (-0.235 [95% confidence interval: -0.485, 0.015]). Thus, our initial estimates appear robust to non-parallel pre-trends.

Figure S6. Estimated time-varying effects (incl. pre-trends) relative to the implementation of ice cleat programs using Bayesian synthetic controls. The number of program municipalities contributing to each time point varies, as shown in the bar chart above the plot, due to time-varying adoption dates. The mean estimate is the average of all unit- and time-specific post-intervention effect estimates (early post-intervention years contribute the most to this average due to the higher number of program municipalities contributing with data in those periods).
EXPECTED IMPACT BASED ON EXTERNAL DATA

This section details a calculation of the expected impact of ice cleat distribution programs based on external data sources. We use this methodology to assess the plausibility of the estimates obtained in our main analysis.

We used data from two external sources to conduct a population impact analysis [11] to quantify the expected average impact in the 73 program municipalities included in our study. The first source is a randomized controlled trial evaluating the effects of ice cleat use among older adults in the US [12]. The other is an observational study investigating the impact of ice cleat distribution programs in Sweden on ice cleat use [13].

We applied the population impact analysis formula detailed in Heller et al. [11] to estimate the expected number of ice-related injuries prevented per 1.000 person-winters. The estimate is given by:

\[ y_0 \left( \frac{\Delta(1/RR - 1)}{1 + \Delta(1/RR - 1)} \right) \]

where \( y_0 \) is the mean incidence rate per 1000 person-winters before implementation (obtained from our data); \( \Delta \) is the average causal effect of ice cleat distribution programs on ice cleat use, expressed as a probability difference (0.075; obtained from [13]); and \( RR \) is the average causal risk ratio associated with ice cleat use (0.45; obtained from [12]). We performed 10,000 Monte Carlo simulations to assess uncertainty in the expected impact.

The results are reported in Table S1. According to the impact analysis, we can expect an effect of -0.1959 ice-related injuries per 1,000 person-winters with a 0.075 probability increase in ice cleat use and a causal risk ratio of 0.45. The expected impact estimate is close to the empirical estimate from the present study (-0.2350), suggesting that the empirical estimate is within a plausible range.
Table S1. Comparison of the empirical estimates of the effects of ice cleat distribution programs on ice-related injury rates among older adults in Sweden from the present study to estimates based on population impact analysis using external data.

<table>
<thead>
<tr>
<th>Input data</th>
<th>Mean</th>
<th>SE</th>
<th>Lower 95%</th>
<th>Upper 95%</th>
</tr>
</thead>
<tbody>
<tr>
<td>Change in ice cleat use (probability difference, Δ)</td>
<td>0.075</td>
<td>0.0169</td>
<td>0.042</td>
<td>0.108</td>
</tr>
<tr>
<td>Risk reduction associated with ice cleat use (RR)</td>
<td>0.45</td>
<td>0.23</td>
<td>0.85</td>
<td></td>
</tr>
<tr>
<td>Baseline injury rate per 1,000 person-winters (γ₀)</td>
<td>2.326</td>
<td>0.055</td>
<td>2.217</td>
<td>2.434</td>
</tr>
</tbody>
</table>

Expected effect based on external data

Effect per 1,000 person-winters (rate difference)            | -0.1959| 0.1189| -0.4845   | -0.0245   |

Empirical estimates from the present study

Effect per 1,000 person-winters (rate difference)            | -0.2350| 0.1277| -0.4853   | 0.0151    |

Notes: SE = Standard error. 95% confidence intervals for expected effects were estimated using Monte Carlo simulations with 10,000 replicates, assuming a normal distribution on all parameters except the relative risk, RR, which was simulated assuming a log-normal distribution.

Replication code for R

```r
## Set seed for reproducibility
set.seed(102398123)

## Define input parameters
# RR (McKiernan)
lnRR = log(0.45)
seRR = (log(0.85)-log(0.23))/3.92
# Baseline rate (our data)
baseline_rate <- 2.325701
baseline_rate_se <- .0552909
# Change in use (Holmberg et al)
change_in_use <- .0752676
change_in_use_se <- .0168791

# Perform impact analysis, base case
impact_derived <- baseline_rate*((change_in_use*(1/exp(lnRR)-1))/(1+change_in_use*(1/exp(lnRR)-1)))
# Simulate uncertainty
sim_change <- rmorm(10000,change_in_use,change_in_use_se)
```
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```r
sim_logrr <- rnorm(10000, lnRR, seRR)
sim_rate <- rnorm(10000, baseline_rate, baseline_rate_se)
sim_impacts <- sim_rate * ((sim_change*(1/exp(sim_logrr)-1)) / (1+sim_change*(1/exp(sim_logrr)-1)))
quantile(sim_impacts, c(0.025, 0.975))
```
REFERENCES


