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Impact of Fall Prevention Training for Home Care Staff on Fall Rates Among Elderly Clients

Schlussbericht

Autorinnen und Autoren
- Youssef Akladios (Health Economics Facility, University of Basel) MSc
- Nadine Schur (Health Economics Facility, University of Basel) PhD
- Matthias Schwenkglenks (Health Economics Facility, University of Basel) PhD, MPH

Projektleitende der Evaluation Gesundheitsförderung Schweiz
- Dr. Giovanna Raso

Zusatzmandat zur Evaluation des Projekts «StoppSturz» (2019-2023), in Zusammenarbeit mit Gesundheitsförderung Schweiz

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Wankdorfallee 5, CH-3014 Bern
Tel. +41 31 350 04 04
office.bern@promotionsante.ch

Avenue de la Gare 52, CH-1003 Lausanne
Tel. +41 21 345 15 15
office.lausanne@promotionsante.ch

www.gesundheitsfoerderung.ch
www.promotionsante.ch
www.promozionesalute.ch

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Authors: Youssef Akladios, Nadine Schur, Matthias Schwenkglenks

Health Economics Facility, Department of Public Health, University of Basel

Institute of Pharmaceutical Medicine (ECPM), University of Basel

Contact details:

Nadine Schur, PhD

Health Economics Facility

Department of Public Health

University of Basel

Tel +41 61 207 19 47

nadine.schur@unibas.ch

Prof Matthias Schwenkglenks, PhD, MPH

Health Economics Facility

Department of Public Health

University of Basel

Tel +41 61 207 19 49

m.schwenkglenks@unibas.ch

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Executive Summary

Falls among older adults remain a major public health concern in Switzerland, with considerable health, social, and economic implications. As part of the *StoppSturz* project, Spitex Zürich Limmat (SZL) implemented a structured fall prevention training program intended to enhance fall risk identification and the integration of preventive measures into routine home-care practice. The training program was delivered in two waves between 2022 and 2023 and included an e-learning module for all employees as well as in-person workshops for registered nurses, who were expected to disseminate key content within their teams.

This present evaluation aimed to assess whether the introduction of the training program was associated with a reduction in fall risk among SZL clients aged 65 years and older, over a two-year period. The study used a retrospective cohort design and relied on multiple operational datasets, including fall protocols, InterRAI assessments, staff training records, and client demographics. Because these datasets were not originally designed for research purposes, several assumptions (particularly regarding team assignment, fall documentation, and exposure classification) were required as a pre-requisite of combining them and constructing a coherent analysis dataset.

The primary analysis, based on falls documented in the fall protocol and fall anamnesis data, included 1,022 clients. These contributed 1,524 observations representing distinct time periods clients were cared for. Modified Poisson regression models showed no statistically significant reduction in fall risk for clients cared for by trained teams. Instead, the point estimates indicated a small, non-significant increase in fall risk (adjusted risk ratio [aRR] = 1.06, 95% confidence interval [CI] 0.92-1.22). Analyses using InterRAI-based fall data showed a similar pattern (aRR = 1.15, 95% CI 0.61-2.16), although these data were incomplete and subject to recall bias. Subgroup analyses by age and living situation did not indicate a protective effect. A statistically significant increase in fall risk was observed among clients younger than 80 years; however, this result likely reflects residual confounding and differences in documentation rather than a true adverse effect of training. These findings should be interpreted in

the context of substantial data constraints, such as inconsistent linkage between clients and teams or missing data, as these factors likely attenuated or distorted the measurable impact of the training program.

Overall, the evaluation did not find evidence that team-based fall prevention training reduced fall risk during the study period. Given the retrospective, observational nature of the analysis and substantial weaknesses of the available data basis, this result should not be interpreted causally. In addition, the fall prevention training, and preceding introduction of fall protocols, may have influenced the recording of falls, e.g. due to increased awareness. Thus, our observations do not confirm or exclude the possibility of beneficial effects of the fall prevention training. Future research using designs that allow for clearer attribution of effects, together with continued improvements in routine documentation practices, would enable more robust assessment of the impact of fall prevention measures. Ongoing investment in fall prevention remains important, particularly when embedded within broader, multi-component strategies in the home-care setting.

Introduction

Falls among older adults represent a substantial public health concern in Switzerland. Each year, approximately 92,000 people aged 65 years and older sustain fall-related injuries severe enough to require outpatient or inpatient medical treatment[1]. The risk of falling increases substantially with age, particularly among individuals with previous falls or multiple chronic conditions[2,3]. Beyond their immediate health consequences, falls often threaten the independence of affected individuals and impose a substantial burden on the healthcare system, with direct medical costs estimated at around CHF 940 million annually and total annual costs (including indirect effects) exceeding CHF 11.8 billion[1].

In response to the growing need for coordinated fall prevention, the *StoppSturz* project (www.stoppsturz.ch), supported by Health Promotion Switzerland, was launched to strengthen fall risk identification and prevention among older adults in routine home-care settings. A central element of the initiative is the provision of fall prevention training for healthcare professionals to support the early detection of fall risks and the implementation of preventive measures.

Whether such training leads to measurable reductions in fall risk in routine home-care settings remains unclear. Evidence from community-based interventions suggests that training alone may yield only modest effects, and implementation can vary substantially across settings.

The present evaluation therefore aimed to assess whether the introduction of a fall prevention training at Spitex Zürich Limmat (SZL) was associated with changes in the monthly frequency of falls. Using routinely collected care data, we examined whether fall risk differed before and after team training and compared patterns across alternative data sources.

Methods

Study design and setting

We conducted a retrospective cohort study to evaluate whether fall prevention training for staff at SZL was associated with a reduction in fall risk among clients aged 65 years and older during the period from January 2022 to December 2023. SZL is the largest home-care organization in German-speaking Switzerland and has participated actively in *StoppSturz* since 2021. The organization provides home-care and nursing services to approximately 6,500 clients annually and employs around 1,050 staff members working in self-organized, neighborhood-based teams.

Within the *StoppSturz* framework, SZL implemented a structured fall prevention training program aimed at strengthening fall risk identification and preventive practices in everyday care.

Intervention

The intervention consisted of a two-step training program:

- an **e-learning module** on fall prevention for all SZL employees, completed individually
- an **in-person workshop** delivered at team level and attended by registered nurses ("diplomierte Pflegefachpersonen")

The workshops were delivered in two waves over the study period (Figure 1). The first wave took place from April to June 2022 and included approximately half of the teams. The second wave took place from January to June 2023 and included the remaining teams. After completing the workshop, the registered nurses were expected to share the workshop content with their team members, and fall prevention measures were discussed as part of routine team and case discussions. All team members therefore received at least the e-learning and indirect training via internal discussions, while registered nurses received a more intensive training package. Thus, the intensity of training differed by professional role.

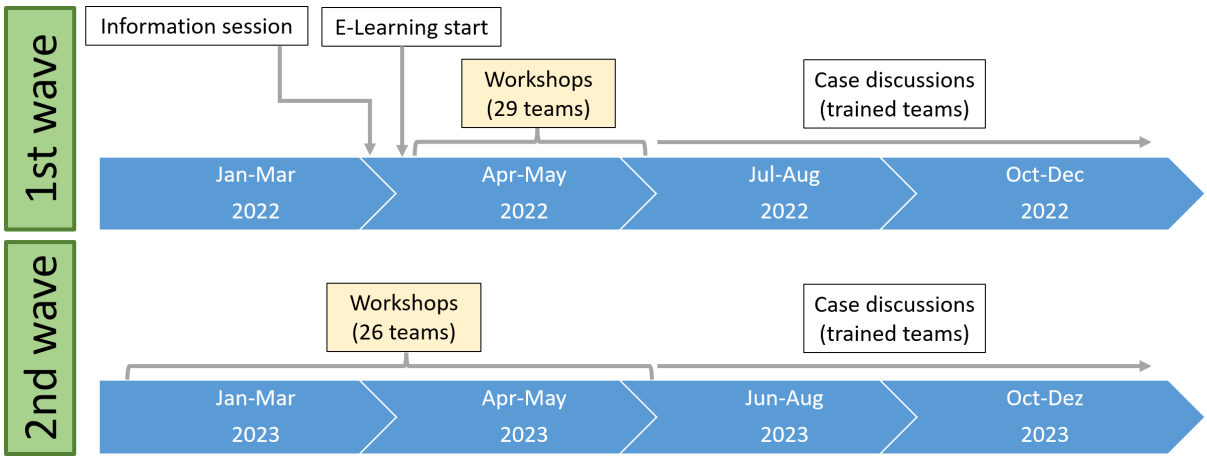


Figure 1: Intervention implementation and study time line

Study population

The study population comprised all clients aged 65 years or older who received home-care services from SZL at any time between 1 January 2022 and 31 December 2023. Clients were excluded if they were enrolled in SZL for fewer than 30 days during the study period or if key information required for analysis (e.g. age, sex, fall status, or team assignment) was missing. Clients entered and left care at different times, resulting in varying individual observation periods. For each client-month, exposure time was defined as the number of days enrolled within that month, allowing for partial months when clients entered or left mid-month.

Data sources

The evaluation was based on routinely collected operational data from SZL. In total, 11 separate Excel files were provided, each originating from a different administrative or clinical workflow: client demographic information, discharge records, employee demographic information, individual employee training participation, team membership lists, team-level training data, fall anamnesis documentation, fall protocols, InterRAI assessments, nursing diagnoses, and planned interventions. These datasets were constructed independently over time, often serving different operational, not research, purposes (such as clinical documentation, billing, training administration, team coordination). As a result, they differed in structure and completeness. A substantial part of the

methodological work therefore involved establishing a clear, reproducible linkage framework and harmonizing these heterogeneous sources.

Linking logic and structural constraints

Three identifiers were theoretically available for linking: client ID, employee ID, and team ID. However, these identifiers were not consistently present across all datasets, and crucially, there was no direct link between clients and teams. Therefore, team assignment had to be inferred indirectly via the employee associated with a fall protocol, fall anamnesis, or InterRAI assessment. This required a two-step linkage process (client → employee → team). This approach could only be applied when the responsible employee could be matched to a team in the team membership file.

For clients without any documented fall protocol, fall anamnesis, or InterRAI assessment during the observation period, no responsible employee could be identified and no team assignment was possible. As a result, exposure status (trained *vs*. untrained team) was more complete for clients with (several) documented contact events than for those with no or fewer recorded interactions.

The merging process was inherently iterative. Missing links and inconsistencies necessitated multiple rounds of cross-checking and refinement. Several linkage strategies (hierarchical, many-to-one, and one-to-many) were tested to arrive at a structure that maximized data retention while maintaining logical coherence.

Inconsistencies and missingness across datasets

When linking files, substantial inconsistencies were encountered. Some employee IDs appearing in fall protocols, fall anamnesis records, or InterRAI assessment data did not exist in the employee registry or team membership file, complicating efforts to determine team affiliation.

Variable completeness was also varied across datasets. Admission dates were contained in the client demographic file, whereas discharge dates were recorded separately and were missing for some

clients. InterRAI assessments, which included comorbidities and some functional information, were not performed regularly for all clients. Nursing diagnoses and planned interventions were recorded for some, but not all, clients, depending on care needs and documentation routines.

These inconsistencies required extensive manual review, iterative recoding, and application of rule-based logic during the harmonization process.

Handling of enrolment periods

Reconstructing client enrolment periods was particularly complex. Several clients had multiple admission dates, multiple discharge dates, or both, and in some cases the chronological order of entries did not align with expected care trajectories. To establish coherent observation windows, we applied a rule-based approach prioritizing temporal plausibility. If a client had a single admission and a single discharge date in the correct order, these dates were accepted as recorded. When multiple discharge dates but only one admission were present, the nearest discharge date occurring after the admission was selected. The same logic applied in reverse when multiple admission dates were present but only one discharge. For clients with several admission and several discharge dates, these principles were extended iteratively, pairing admission and discharge dates based on temporal proximity and correct sequencing to create realistic care episodes. Implausible combinations (e.g., discharge dates occurring before all recorded admissions, consecutive admissions without an intervening discharge) were flagged and resolved by selecting the most coherent date pairs. If no discharge date could be plausibly linked to an admission, the client was assumed to remain under care until the end of the study period.

Assumptions required for dataset construction

To construct a consistent dataset for analysis, several pragmatic assumptions were necessary:

1. **Continuous enrolment:** Clients without a recorded discharge date after harmonization were assumed to have remained enrolled throughout the study period.

2. **Separate fall documentation streams:** Fall protocol and fall anamnesis records were treated as independent data sources because they originate from different processes. In cases where dates from both sources were closely aligned, suggesting possible overlap, these entries were interpreted as multiple fall events occurring within a short interval rather than as duplicate documentation of the same event.

3. **Classification of clients without falls:** Clients who could not be linked to either fall protocol or fall anamnesis records were assumed not to have experienced a fall during the study period. This assumption was necessary because missing documentation may reflect a true absence of falls, incomplete reporting, or data linkage issues, which could be related to team training status, reporting routines, client recall, or other contextual factors.

4. **Team training status definitions:** Because not all caregivers involved in a client's day-to-day care appeared in fall protocols, fall anamneses, or InterRAI assessments, individual-level training status could not be reliably linked to all clients. Thus, team-level training was used as the primary exposure indicator. A team was classified as "trained" from the date on which the team completed the team-level training. All team members received at least the e-learning and team discussions, while registered nurses additionally participated in the workshop. Training intensity therefore varied within teams.

Harmonization and quality assurance

After mapping identifiers across all datasets, extensive quality checks were performed to identify duplicates, contradictory records, and implausible values. Categorical variables were standardized, free-text fields were cleaned, and date formats were aligned across sources.

Construction of client-monthly observations

Once the datasets were merged, monthly observations were created for each client for every month they were enrolled. For each client-month, team assignment was taken from the most recent responsible employee information. A binary exposure variable was created to indicate whether the client's assigned team had completed the fall prevention training prior to the start of the month. Fall events were then mapped (i.e. whether a fall occurred or not) to the corresponding month, and an offset term reflecting the number of days at risk in each month was calculated for each client. This produced a structured panel dataset with repeated observations clustered at the client level.

The final analytic file reflects an iterative process of cleaning, linkage, and the application of transparent, pragmatic assumptions designed to address structural gaps and inconsistencies in the original documentation.

Outcomes

The primary outcome was the occurrence of at least one fall in a given month, defined as any event documented in the fall protocol or fall anamnesis dataset. For this study, a fall was defined as an event in which a person comes to rest on the ground, not as the result of a major medical event (e.g. stroke or myocardial infarction). Clients without any documented fall in the fall protocol and fall anamnesis dataset were assumed to have remained fall-free.

For the secondary analysis of InterRAI data, falls were identified from items that record whether the client experienced a fall during a specified recall period (i.e. the previous 30 days or between 31 and 90 days, depending on the InterRAI question used). Primary, when available, falls in the last 30 days were used for the analysis. Otherwise, we used falls within the 90-day window. For clients indicating a fall, a random imputed date was generated within the relevant recall window as substitute for the real but unknown event date to assigning the fall to the client-month.

Statistical analysis

We used modified Poisson regression with robust standard errors to estimate the association between team training and the monthly risk of falls among clients. This approach is well suited for binary outcomes and allows direct estimation of risk ratios while accounting for non-Poisson variance structures, unlike a traditional Poisson regression which may overestimate the error of relative risk. Client and team-level cluster-robust covariance matrices (sandwich estimators) were used to account for repeated monthly observations per client and nesting of clients within teams. An offset term, defined as the natural logarithm of the number of days at risk in each month, was included to account for varying exposure time (i.e. differences in the duration of care for each client in a given month).

Covariates in the adjusted models included client age (continuous), sex, living situation (living alone *vs*. with partner and/or others), and calendar season. Season was treated as a categorical variable with eight levels representing spring (March to May), summer (June to August), autumn (September to November), and winter (December to February) for each of the two study years (2022 and 2023).

Two main models were specified:

- **Model 1 (primary analysis):** Outcome based on falls documented in the fall protocol or fall anamnesis data. Clients without a fall protocol and fall anamnesis entry during the study period were classified as having no falls.
- **Model 2 (secondary analysis):** Outcome based on falls derived from InterRAI data, using the generated fall dates as described above.

Subgroup analyses were conducted by living situation (living alone *vs*. with partner/others) and by age group (<80 vs ≥80 years). In addition, a sensitivity analysis was performed that applied a 30-day lag time after the recorded team training date such that the first client-month following the training was still classified as "untrained." This lag was intended to account for the time required to implement preventive practices and to reduce misclassification of falls occurring immediately after team training.

All statistical analyses were conducted using R (version 4.4.0)[4].

Ethical considerations

Anonymized client data from routine care were used for this study. The evaluation was conducted in accordance with SZL's institutional data protection policies. The Ethics Committee of Northwestern and Central Switzerland (Ethikkommission Nordwest- und Zentralschweiz, EKNZ) reviewed the study and confirmed that no formal ethics approval was required, issuing a waiver (BASEC-No. Req-2022-00465).

Results

Study population

A total of 1,022 unique clients were included in the primary analysis, contributing 1,524 observations. These represented distinct time periods during which clients were cared for. The mean age was 83.8 years (range: 65-103), and 36.9% were male (**Table 1**). Most clients (60.6%) lived alone, and the mean duration of care provided by SZL during the study period was 448 days.

Table 1: Characteristics of clients included in the analysis (n = 1,022)

Characteristics	Results
Mean age (range), years	83.8 (65-103)
Age proportion, %	
< 80 years	28.3
≥ 80 years	71.7
Male, %	36.9
Living situation, %	
Alone	60.6
With partner and/or others	39.4
Mean duration of care provided by SZL during study period, days	448
Median duration of care provided by SZL during study period (interquartile range)	449 (223-729)
Proportion of falls, %	
0	11.2
1	72.2
2	7.8
≥ 3	8.8
Mean monthly fall rate per client	0.12
Median monthly fall rate (interquartile range)	0.08 (0.05-0.14)

Based on fall protocol and fall anamnesis records, 88.8% of the clients that could be included in the analysis had at least one documented fall during the observation period. This high proportion reflects the two-year observation window. The mean monthly fall rate per client was 0.12 falls per month. The distribution of the falls can be found in **Figure 2.**

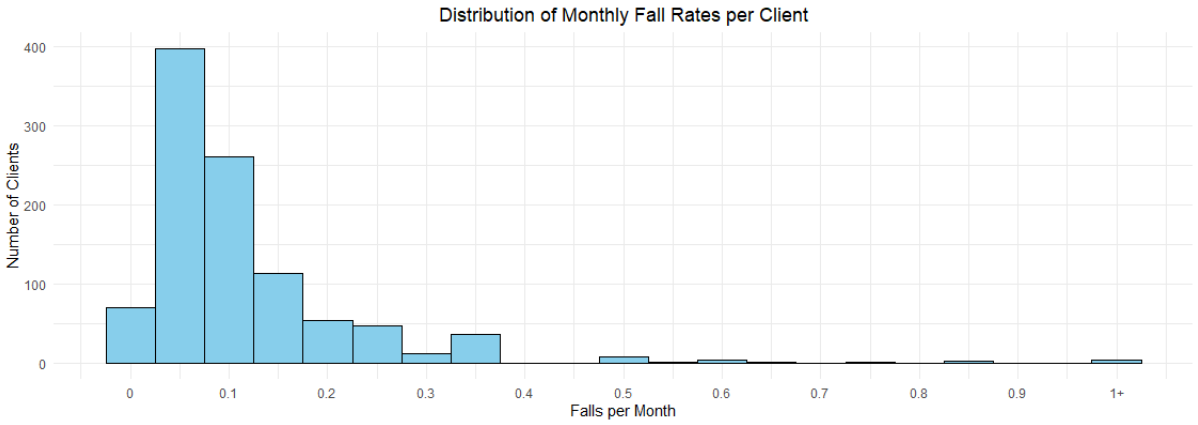


Figure 2: Distribution of monthly fall rates per client

Model 1 – Primary analysis

In the primary model using fall protocol and fall anamnesis data, team training was not associated with a statistically significant reduction in the risk of falls even after adjusting for sex, age, living situation and season (adjusted risk ratio [aRR] = 1.06, p = 0.41). The estimates suggested a small non-significant increase in documented falls among clients cared for by trained teams. Female clients had a slightly lower risk of falls compared with males (aRR = 0.91, p = 0.02), and clients living with others had a modestly higher risk of recorded falls than those living alone (aRR = 1.10, p = 0.03). Seasonal patterns did not demonstrate a consistent trend. Full results are shown in **Table 2**.

Table 2: Effect of team training on falls – Fall protocol and fall anamnesis data (Model 1 - Primary model)

	RR	CI	p-value	aRR	CI	p-value
Team training completed	1.06	0.95-1.18	0.28	1.06	0.92-1.22	0.41
Age				1.00	0.99-1.00	0.10
Sex						
Male				1		
Female				0.91	0.85-0.99	0.02*
Living Situation						
Alone				1		
With Partner/Others				1.10	1.01-1.19	0.03*
Season						
Spring 2022				0.87	0.64-1.18	0.37
Summer 2022				1.04	0.82-1.33	0.75
Autumn 2022				0.92	0.67-1.27	0.62
Winter 2022				1		
Spring 2023				0.95	0.72-1.25	0.72
Summer 2023				0.79	0.60-1.05	0.11
Autumn 2023				0.98	0.73-1.31	0.90
Winter 2023				0.93	0.72-1.20	0.56

Abbreviations: RR (Risk Ratio), aRR (adjusted Risk Ratio), CI (Confidence Interval).
Covariates: age, sex, living situation, yearly season.
***p < 0.001, **p < 0.01, *p < 0.05.

Model 2 – Secondary analysis

Results from the secondary model based on InterRAI-derived fall information were consistent with the primary analysis. Clients cared for by trained teams showed a 15% higher risk of falls, though the association between team training status and fall risk was not statistically significant (aRR = 1.15, p = 0.65; **Table 3**). No statistically significant associations were observed for sex, age, or living situation in this model. Seasonal variation was more pronounced, likely reflecting the smaller sample and the recall-based nature of the InterRAI fall variable.

Table 3: Effect of team training on client falls – InterRAI data (Model 2 - Secondary model)

	RR	CI	p-value	aRR	CI	p-value
Team training completed	0.92	0.64-1.32	0.67	1.15	0.61-2.16	0.65
Age				1.01	0.98-1.04	0.62
Sex						
Male				1		
Female				0.82	0.59-1.14	0.25
Living Situation						
Alone				1		
With Others				1.09	0.74-1.62	0.66
Season						
Spring 2022				0.88	0.45-1.70	0.70
Summer 2022				0.91	0.50-1.66	0.76
Autumn 2022				1.01	0.54-1.89	0.98
Winter 2022				1		
Spring 2023				0.73	0.32-1.66	0.45
Summer 2023				0.71	0.32-1.58	0.40
Autumn 2023				0.65	0.29-1.46	0.30
Winter 2023				0.22	0.08-0.63	0.01**

Abbreviations: RR (Risk Ratio), aRR (adjusted Risk Ratio), CI (Confidence Interval).
Covariates: age, sex, living situation, yearly season.
***p < 0.001, **p < 0.01, *p < 0.05.

Subgroup analyses

Subgroup analyses did not suggest differential associations between team training status and documented falls. Among clients living alone, the association remained non-significant (aRR = 1.04, p = 0.60), as it did for those living with others (aRR = 1.10, p = 0.51) (**Table 4**). When stratified by age group, clients younger than 80 years showed a statistically significant increase in fall risk associated with team training status (aRR = 1.40, p = 0.01), whereas no association was found among those aged 80 years or older (aRR = 0.96, p = 0.56) (Table 5). Given smaller subgroup sizes and limited adjustment for baseline fall risk, these findings should be interpreted cautiously.

Table 4: Effect of team training on fall risk by living situation

	Clients who live alone			Clients who lived with partner/others		
	aRR	CI	p-value	aRR	CI	p-value
Team training completed	1.04	0.90-1.21	0.60	1.10	0.83-1.44	0.51

Abbreviations: RR (Risk Ratio), aRR (adjusted Risk Ratio), CI (Confidence Interval).
Covariates: age, sex, yearly season.
***p < 0.001, **p < 0.01, *p < 0.05.

Table 5: Effect of team training on fall risk by age group

	Clients under 80 years			Clients 80+ years		
	aRR	CI	p-value	aRR	CI	p-value
Team training completed	1.40	1.09-1.79	0.01*	0.96	0.82-1.11	0.56

Abbreviations: RR (Risk Ratio), aRR (adjusted Risk Ratio), CI (Confidence Interval).
Covariates: age, sex, living situation, yearly season.
***p < 0.001, **p < 0.01, *p < 0.05.

Sensitivity analyses

A sensitivity analysis applying a 30-day lag following team training yielded results similar to the primary model. The association between team training status and fall risk remained non-significant (aRR = 1.10, p = 0.24), suggesting that short-term transitional effects did not materially alter the findings (Table 6).

Table 6: Sensitivity analysis with 30-day lag after team training

	aRR	CI	p-value
Team training completed	1.10	0.94-1.29	0.24

Abbreviations: RR (Risk Ratio), aRR (adjusted Risk Ratio), CI (Confidence Interval).
Covariates: age, sex, living situation, yearly season.
***p < 0.001, **p < 0.01, *p < 0.05.

Discussion

This evaluation examined whether a structured fall prevention training program for home-care staff at SZL was associated with a reduction in fall risk among elderly clients. Across all models, we did not observe a statistically significant decrease in fall risk among clients cared for by trained teams. In fact, clients cared for by trained teams showed a non-significant trend toward more documented falls across both primary and secondary analyses (Model 1: aRR = 1.06; Model 2: aRR = 1.15). While the strength and direction of effect estimates varied by subgroups, no model provided evidence for a protective effect of the intervention as implemented during the study period. These findings align with our initial assumption that reported fall risk might increase following team training due to heightened staff awareness and more complete reporting of fall incidents.

Interpretation of findings in light of data structure

The absence of a protective team training effect should be interpreted in the context of the complex data environment in which this study was conducted. The operational datasets used for the analysis stemmed from multiple documentation processes, differed in completeness and granularity, and lacked a unified relational structure. As a result, several analytical assumptions were required to construct a coherent client-level longitudinal dataset. These circumstances introduced several pathways through which the estimated team training effects may have been diluted, distorted, or rendered statistically non-detectable.

First, the absence of a direct link between clients, employees, and teams required a multi-step inference process to assign exposure status. For clients without fall protocol, fall anamnesis, or InterRAI records, no responsible employee could be identified, and those clients could not be assigned to a team. As a result, exposure status was more complete for clients who experienced documented contact events, and less complete for those with fewer interactions. This asymmetric availability may

have introduced bias if the probability of fall documentation was itself associated with training status, workload, or reporting routines within teams.

Second, the construction of enrolment periods required multiple assumptions, as clients frequently had multiple or inconsistently ordered admission and discharge dates. Although a rule-based temporal plausibility approach was applied (e.g., selecting the nearest plausible admission–discharge pair), this introduced uncertainty regarding time under observation. Although this approach maximized internal coherence, remaining discrepancies in admission and discharge recording may have led to misclassification of the exposure window or the time during which clients contributed to trained *vs*. untrained periods.

Third, assumptions had to be made regarding fall classification. Clients without any linkable fall protocol or fall anamnesis record were assumed not to have fallen. This assumption was necessary, but it may have introduced outcome misclassification if falls occurred but were undocumented or could not be linked during dataset harmonization. Such underreporting is plausible in home-care settings and may depend on contextual factors such as client recall, employee familiarity with reporting systems, or evolving documentation practices during the rollout of training. If documentation practices improved post-training, this may have inflated reported falls in the trained group and biased estimates toward an increased risk after team training – a direction consistent with our findings.

Fourth, exposure was defined at the team level, assuming that all team members were trained from the date the team-level training was completed. While all team members received e-learning at some point in time and participated in internal case discussions, only registered nurses participated in the additional workshop component. Consequently, training intensity varied within teams. Clients classified as exposed may have received most of their care from staff with differing levels of training, and the degree to which workshop-trained nurses interacted with specific clients was not captured in

the datasets. Such within-team heterogeneity likely biased training effects towards no measurable effect.

Fifth, fall outcomes themselves reflect two very different documentation processes. Fall protocol data and fall anamnesis data capture current events and contain exact fall dates. Possible overlap between these independent datasets was principally resolved by using a binary fall outcome per client-month. In contrast, InterRAI data record falls within a recall period and do not specify event dates. Hence, the data are subject to recall bias and may underreport minor or unobserved falls. The need to assign a random date within the recall window introduced noise and reduced temporal precision. Furthermore, inconsistencies in InterRAI completion (both in timing and frequency) introduced further uncertainty. These structural differences between the incident-based fall data and the InterRAI records limit the comparability of results derived from these sources.

Finally, although we adjusted for key demographic and contextual factors, some important confounders could not be included, particularly comorbidities, medication profiles, functional status, or baseline fall risk. These variables were inconsistently documented (as the InterRAI records varied in timing and frequency) and therefore could not be incorporated into multivariable models. If clients with higher baseline fall risk were more likely to receive more preventive measures from trained teams, this could have led to additional bias.

Another factor that may have influenced the observed patterns relates to the fall protocol itself, which was introduced shortly before the start of the study period. As a newly implemented documentation tool, reporting practices were likely subject to a learning curve, with potential fluctuations in completeness, accuracy, and consistency during the early phases of adoption. It is possible that falls were recorded more frequently at the beginning due to heightened attention and motivation, or conversely less frequently if staff were still becoming familiar with the system or unsure about reporting thresholds. Over time, documentation fatigue or perceived administrative burden may also

have contributed to changes in reporting behaviour. Furthermore, there is the possibility that minor, moderate, and severe events were captured at different rates across teams and periods. These implementation dynamics may have introduced time-dependent variability in the outcome measure independent of the training intervention.

Together, these factors suggest that the estimated effects of team training must be interpreted with caution. The expected directions of possible biases are not uniform: while improved documentation after training may have inflated reported fall risk in the trained group, exposure misclassification due to imperfect linkage would have tended to attenuate true effects. The combined impact of these mechanisms likely contributed both to the lack of statistical significance and to the absence of a protective effects observed in the analyses.

Comparison with existing evidence

Existing literature has shown that fall prevention interventions in community settings typically yield only modest, nonsignificant reductions in fall risk despite comprehensive staff training[5]. Stronger evidence exists for individualized risk assessments, home modifications, and sustained organizational support alongside education[6]. A 2019 Cochrane systematic reviews concluded positively on some types of exercise programs[7]. A large intervention effect attributable solely to staff training, the intervention underlying our study, would not necessarily be expected. Our findings are therefore not inconsistent with the broader evidence base and likely reflect a combination of the modest expected effect size and the structural limitations inherent to routine operational data.

Implications for practice and evaluation

Although this study did not detect a significant reduction in falls attributable to the fall prevention training program, the evaluation offers valuable insights for future implementation and monitoring of fall prevention strategies. Most notably, the analysis underscores the importance of consistent, standardized fall documentation and clearer linkage between clients, staff, and teams. Improved

integration of operational datasets, regular completion of clinical assessments, and more explicit documentation of caregiving responsibilities would substantially increase the ability to evaluate training effectiveness.

Furthermore, given that teams differed in training intensity due to their internal composition, future fall prevention initiatives may benefit from standardizing the depth of training across all professional groups or explicitly capturing which staff members provide care to which clients. Embedding structured follow-up procedures after training could also strengthen the integration of preventive practices into everyday care.

Conclusion

This evaluation did not identify a statistically significant association between team-based fall prevention training and reductions in fall risk among elderly clients receiving home-care services from SZL. While the training represented a meaningful organizational investment in staff development, the estimated effects were small and did not indicate a measurable reduction in falls during the observation period.

Given the retrospective, observational nature of the analysis and the reliance on routine operational data, the findings should be interpreted with appropriate caution. The study design did not establish a basis for causal conclusions. In addition, the fall prevention training, and preceding introduction of fall protocols, may have influenced the recording of falls, e.g. due to increased awareness. Resulting variation in documentation practices and implementation intensity across teams may have influenced the observed results. Furthermore, the weaknesses of the data basis, complex process of harmonizing multiple independent operational datasets, and need for several analytical assumptions limit the extent to which conclusions can be drawn. Our observations do not confirm or exclude the possibility of beneficial effects of the fall prevention training.

Future research using designs that allow for clearer attribution of effects, together with continued improvements in routine documentation practices, would enable more robust assessment of the impact of fall prevention measures. Continued attention to fall prevention remains important, particularly when training is integrated into broader, multi-component strategies aimed at reducing fall-related harm and supporting older adults to remain safely at home.

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