

Supplementary Material for: Fatal Fire Incidents in Iceland, 1968–2025: A National Registry Study of Building Construction Period and Fire Mortality

2026-07-02

S1 Classification of Borderline Cases

Incidents were classified into two categories: fatal fires in structures (93 incidents, 116 deaths) and other fire-related deaths involving ships, vehicles, and other non-building settings (20 incidents, 29 deaths). The classification criterion, applied uniformly regardless of building age or construction year, was whether the fire originated in or spread through the building structure such that structural fire safety features (egress, compartmentation, detection, suppression) were relevant to the fatality outcome. This predefined rule (whether failure or success of structural fire safety features was materially related to the fatality) was applied consistently to all borderline cases.

Borderline cases required judgment. For example, a 2014 nursing home fire (building constructed 2001) where an elderly resident died was classified as “other” because the death resulted from a localized ignition (bedding) in a building whose fire safety systems functioned as designed; the fatality mechanism did not involve structural fire spread. Conversely, a criminal arson incident at Bræðraborgarstígur, Reykjavík, in 2020 (original construction year 1906, 3 deaths) was classified as a structure fire because fire spread through the building and structural safety features (egress routes, compartmentation) were material to the outcome [1]. The same classification would have applied regardless of the building’s construction year.

Because the relevance-based rule is narrower than the standard “any fatal fire in a building” convention (main text, Data and Methods), the paper accounts explicitly for the

Table S1: Fatal fires occurring inside permanent buildings but classified "other" under the relevance-based rule, with dates and recorded construction years. Only the 2014 case lies in the post-1998 cohort.

Year	Location	Setting	Construction year
1997	Böðvarsdalur	Other fire (in building)	not recorded
2010	Grundartangi	Industrial (factory) fire	not recorded
2014	Reykjavík	Nursing home (institutional)	2001

"other"-classified deaths. The 20 "other" incidents (29 deaths) are dominated by non-permanent-building settings: ship fires (seven incidents, 15 deaths), vehicle, motorhome, and camper fires (six incidents, including the 2013 Þjórsárdalur camper (hjólhýsi) fire), shed and tent fires (two incidents), and explosions (two incidents). The three "other"-classified deaths that occurred inside permanent buildings are listed in Table S1 with their dates and (where known) construction years. Only one, the 2014 nursing-home death in a building constructed in 2001, is relevant to the post-1998 residential cohort; it is exactly the case carried into the worst-case reclassification sensitivity (§S4 and main-text Results). Of the three in-building cases, the 1997 Böðvarsdalur death predates the post-1998 cohort, and the 2010 Grundartangi fatality was an industrial (factory) fire in a non-dwelling occupancy, excluded from the residential cohort regardless of construction year. The only fatal fire in a post-1998 residential building under the standard convention is therefore the 2014 nursing-home death (construction year 2001), which is the case already carried into the worst-case sensitivity. No in-building fatality in the post-1998 window has an unrecorded construction year within occupancies consistent with the dwelling person-years denominator: the only in-building post-1998-window fatality with an unrecorded construction year is the 2010 Grundartangi industrial (factory) fire, a non-dwelling occupancy that is excluded from the residential cohort regardless of construction year, since counting it against a dwelling person-years denominator would break numerator–denominator alignment. The standard-convention post-1998 residential count is therefore definitively one, and the adversarial worst case (two, adding the Stuðlar/Fossaleyni fatality) remains significant at both the death level (rate ratio 0.19, 95% CI 0.02–0.75) and the incident level (0.23, 95% CI 0.03–0.91).

Table S2: Overall burden of fatal fire incidents in Iceland (1968–2025).

Category	Incidents (n)	Deaths (n)	Deaths/incident (mean)
Structure fires	93	116	1.25
Other fire-related deaths	20	29	1.45
Total	113	145	1.28

Table S3: Fatal structure fires by building construction year. The 1996–2025 subset is reported because construction year is complete from 1996 onward. In both the full period and the 1996–2025 subset, zero fatal incidents occurred in buildings constructed after 1998.

Construction year	Full period (n = 93)		1996–2025 (n = 35)	
	Incidents	Deaths	Incidents	Deaths
1900–1919	7	13	4	6
1920–1939	3	3	1	1
1940–1959	18	19	13	14
1960–1979	11	12	11	12
1980–1997	6	8	6	8
Unknown	48	61	0	0
Total	93	116	35	41

S2 Overall Burden and Construction-Year Tables

S3 Person-Years Exposure Approximation: Full Derivation

Because no census-based housing-stock denominator stratified by construction period is available, person-years of exposure in pre-1998 versus post-1998 dwellings for 1999–2025 were derived from the observed annual dwelling-completions series (Statistics Iceland table IDN03001, Bygging íbúðarhúsa á öllu landinu, completed dwellings 1970–2021; range 565 in 2011 to 3,816 in 2020; 46,390 completed over 1999–2021). Completions for 2022–2025 (beyond the published series) were extrapolated from the 2019–2021 mean ($\sim 3,346$ /year). The cumulative stock was anchored to the documented ~ 2021 total of approximately 148,000 dwellings (Registers Iceland property registry), walked backward and forward with the actual completions, and split each calendar year at the 1998/1999 construction boundary; population was allocated by the post-1998 stock fraction. This yields a post-1998 stock fraction of approximately 31% in 2021 and post-1998 person-years of approximately 1.86 million over 1999–2025. The anchor is internally consistent: 148,000 minus

the 1970–2021 completions implies an end-1969 stock of approximately 50,500 dwellings, plausible for Iceland circa 1970. Varying the anchor across 140,000–155,000 changes post-1998 person-years from 1.97 to 1.77 million and the strict-classification upper bound from 0.15 to 0.17 per 100,000, leaving every conclusion intact. An earlier stepped completion-rate model (500/year pre-1940 rising to 3,500/year post-2010) gave 2.08 million post-1998 person-years, about 12% higher; that model was therefore mildly anti-conservative for the paper’s claim (overestimating post-1998 exposure lowers the reported upper bound), which is why the observed-completions denominator is used as primary here.

Using this observed-completions denominator, the 1999–2025 period yields approximately 6.86 million person-years in pre-1998 dwellings and 1.86 million person-years in post-1998 dwellings. During this period, 38 structure-fire deaths occurred in pre-1998 buildings (construction year complete for this window), yielding a fatality rate of 0.55 per 100,000 person-years (95% CI 0.39–0.76). Zero deaths occurred in post-1998 buildings during the study period (incidence rate: 0.00 per 100,000 person-years; rate ratio not estimable). The Poisson 95% upper confidence bound for the post-1998 rate, conditional on this denominator, is 0.16 per 100,000 person-years (one-sided), well below the pre-1998 rate. If the true rate in post-1998 buildings equalled the pre-1998 rate, the expected death count would be approximately 10.3. The probability of observing zero under that assumption is 3×10^{-5} , confirming that the zero-fatality observation is statistically anomalous and not an artifact of low exposure. Even at one-tenth of the pre-1998 rate, the expected count would be approximately 1.0, yielding roughly a 36% probability of observing zero.

Because fatalities cluster within incidents (mean 1.25 deaths per structure-fire incident), the death-level Poisson calculation slightly overstates the independent information content. The calculation was therefore repeated at the incident level, which is conservative with respect to clustering. The 32 fatal structure-fire incidents in pre-1998-cohort buildings over 6.86 million person-years imply a rate of 0.467 incidents per 100,000 person-years; applied to the 1.86 million post-1998 person-years, the expected number of post-1998 fatal structure incidents under rate equality is approximately 8.7, so the probability of observing zero incidents is approximately $e^{-8.7} \approx 1.7 \times 10^{-4}$. The conclusion is unchanged: the observed zero remains highly improbable under the null of equal risk, even after collapsing correlated deaths to independent incidents.

The dwelling-stock model is approximate and may overestimate post-1998 exposure if second homes, vacancy, or demographic concentration in older stock are substantial; however, even halving the denominator raises the strict-classification upper confidence bound only to 0.32 per 100,000, still below the pre-1998 rate. The proportional allocation of population to building stock assumes uniform occupancy density across construction eras. Because older housing stock is frequently subdivided for higher-density, lower-income occupancy, the pre-1998 person-years denominator is likely underestimated, and the post-1998 denominator overestimated. The magnitude of this distortion cannot be quantified with current data. Consequently, the comparative safety gap between the pre-1998 and post-1998 cohorts reported in this study is likely inflated by this unmeasured density gradient. This gap-inflating gradient is partly offset by family-formation sorting into newer stock, which raises occupancy – and hence the modeled person-years – in post-1998 dwellings; the net direction of the occupancy bias is therefore not identified from the available data.

Worst-case reclassification of borderline cases. Two cases sit near the structure/other boundary in a way potentially relevant to the post-1998 cohort: the 2014 nursing-home death in a building constructed in 2001 (classified “other”; §S2) and the 2024 Stuðlar/Fossaleyni case (recorded construction year 1996; extension unregistered). To ensure that no headline conclusion depends on their adjudication, a deliberately adversarial scenario was constructed in which both are counted as post-1998 structure-fire deaths. This yields two deaths across the approximately 1.86 million post-1998 person-years, a rate of 0.108 per 100,000 (one-sided 95% exact-Poisson upper bound 0.34 per 100,000, using the same convention as the zero-event bound). The 2024 Stuðlar/Fossaleyni death carries a recorded construction year of 1996 and is therefore among the 38 pre-1998 deaths; moving it to the post-1998 cohort in this worst case removes it from the pre-1998 numerator, giving a pre-1998 reference of 37 deaths (0.54 per 100,000), a negligible change. The 2014 nursing-home death is classified “other” and is not among the 38, so its reclassification does not alter the pre-1998 count. For comparability the rate ratios above are reported against the unchanged 38-death pre-1998 reference; using 37 changes them only in the third decimal. This remains approximately five-fold below the pre-1998 rate of 0.55 per 100,000, and the upper bound stays below it. The primary finding is therefore robust to the most unfavorable classification of both borderline cases.

Occupancy alignment of numerator and denominator. The denominator is modeled

dwelling person-years, so the occupancy of the numerator was checked. Of the 38 pre-1998-cohort deaths (1999–2025, recorded construction year ≤ 1998), 36 occurred in ordinary dwellings and 2 in converted commercial-to-residential premises (Funahöfði and Stangarhyl, both 2023); restricting to ordinary dwellings gives 0.52 per 100,000, essentially unchanged from 0.55. The two post-1998 borderline cases are an institutional occupancy (the 2014 nursing home) and an unauthorized unregistered extension (Stuðlar/Fossaleyni), neither an ordinary dwelling. A strictly dwelling-restricted comparison therefore records 36 pre-1998 dwelling deaths against zero post-1998 dwelling deaths, so the occupancy mismatch does not manufacture the contrast.

Intentional-fire sensitivity. Arson is not code-preventable in the way accidental ignition is, and intentional fires cluster in older, socially marginal stock, which could inflate the cohort contrast for non-code reasons. The registry records two incidents as confirmed arson in the 1999–2025 pre-1998 cohort (five deaths; Selfoss 2018 and Bræðraborgarstígur 2020), both in pre-1998 buildings. Excluding them leaves 33 pre-1998 deaths over the 6.86 million pre-1998 person-years, a rate of 0.48 per 100,000, still above every post-1998 one-sided upper bound (strict 0.16, standard 0.26, worst case 0.34). If the 2025 Reykjavík fire (2 deaths, construction year 1967, under criminal investigation for suspected arson) is subsequently confirmed as intentional, the arson-excluded pre-1998 count falls to 31 deaths (approximately 0.45 per 100,000), still above every post-1998 upper bound. Because passive protections (egress, compartmentation) remain material even in intentional fires, this exclusion is conservative rather than definitive; either way, intentional fires do not manufacture the cohort contrast.

Joint denominator $\times$ classification sensitivity. Table S4 crosses the three classification counts (0, 1, 2 post-1998 deaths) with the modeled and a halved post-1998 denominator, comparing each one-sided 95% upper bound with the pre-1998 rate. The density-gradient bias described above reallocates population between cohorts rather than removing it, so halving the post-1998 denominator must be paired with a coherent increase in the pre-1998 denominator: the approximately 0.93 million person-years subtracted from the post-1998 stock accrue to the pre-1998 stock, raising it to approximately 7.79 million and lowering the pre-1998 point estimate to 0.49 per 100,000 person-years (95% CI 0.35–0.67). Reported on the same two-sided exact conditional rate-ratio scale as main-text Table 2, the halved-and-reallocated denominator yields a rate ratio whose 95% CI excludes 1 only in

Table S4: Joint sensitivity: post-1998 one-sided 95% upper bound (per 100,000 person-years) crossing classification (deaths) with the modeled and halved denominators. Modeled column compared with the pre-1998 point estimate 0.55 (95% CI 0.39–0.76); halved column with the coherently reallocated pre-1998 point 0.49 (95% CI 0.35–0.67), since person-years removed from the post-1998 stock accrue to the pre-1998 stock. Bold bounds exceed the applicable pre-1998 point estimate.

Post-1998 deaths	Observed-compl. denom. (1.86M)	Halved + reallocated (0.93M)
0 (strict)	0.161	0.323
1 (standard)	0.256	0.511
2 (worst case)	0.339	0.678

the strict zero-death cell (RR 0, 95% CI 0–0.85); the standard (RR 0.22, 95% CI 0.005–1.3) and worst-case (RR 0.45, 95% CI 0.05–1.8) cells include 1. Statistical significance is therefore not claimed to survive an adversarial halving of the denominator; what is preserved throughout is the 2- to 5-fold point-estimate separation (post-1998 point 0.11 and 0.22 against the reallocated pre-1998 0.49), and at the modeled (primary) denominator every rate-ratio CI excludes 1 under all three classification conventions (Table 2). The one-sided upper bounds crossing classification with denominator are tabulated in Table S4 for reference.

S4 Sensitivity Analysis for Missing Construction Year

Construction year is missing for 48 of 93 structure-fire incidents (52%), raising the question of whether unrecorded post-1998 buildings could contain fatal incidents. The temporal distribution of all missing-construction-year incidents to assess whether any could plausibly fall in the post-1998 cohort. All 48 incidents with missing construction year occurred between 1970 and 1995. Because a building that burned before 1999 cannot have been constructed after 1998, the missing data are logically irrelevant to the post-1998 finding: regardless of how missing values are imputed, the observed zero in post-1998 buildings is unaffected. The missingness is missing-not-at-random but in a direction that strengthens rather than undermines the key finding. Missing construction-year data therefore cannot bias the post-1998 comparison. This immunity applies specifically to the post-1998 zero; it does not extend to the full-period construction-year distribution (Table S3), where 61 of 116 structure-fire deaths have unknown construction year, so the full-period cohort profile (unlike the post-1998 finding) should be read as incomplete rather than definitive.

S5 Interrupted Time Series Analysis: Full Specification and Robustness

To move beyond purely descriptive assessment, segmented regression models were fitted using generalized linear models with a Poisson distribution and log-link, with the natural logarithm of the annual population as an offset [2, 3]. Because the 1998 measure is a dwelling-construction regulation, the primary outcomes are structure-fire deaths and structure-fire incidents; the combined structure-plus-non-structure death series is retained only as a secondary descriptive comparison (its early, maritime-dominated non-structure deaths belong to unrelated safety regimes). The model specified an intervention at 1999 (the effective year of Byggingarreglugerð nr. 441/1998):

$$\log(E[Y_t]) = \log(\text{pop}_t) + \beta_0 + \beta_1 \cdot \text{time} + \beta_2 \cdot \text{post1999} + \beta_3 \cdot \text{time_since_1999}$$

where Y_t is annual fire deaths, pop_t is the population offset, time is calendar year minus 1968, post1999 is an indicator for years ≥ 1999 , and time_since_1999 is the number of years since 1999 (zero before 1999). This is the standard segmented (Wagner) parameterization: coding the post-intervention trend as years since the intervention makes β_2 the level change at the 1999 breakpoint, β_1 the pre-intervention secular trend, and β_3 the change in slope thereafter. (Coding the post-intervention trend instead as $\text{time} \times \text{post1999}$ with time measured from 1968 would make β_2 the vertical gap between the two segments at the arbitrary 1968 origin rather than the discontinuity at 1999; the years-since parameterization above avoids that misinterpretation.) Because building regulations affect fire mortality through gradual stock turnover rather than instantaneously, β_2 is in any case treated as a descriptive breakpoint rather than a causal intervention effect; the net post-1999 annual trend is $\exp(\beta_1 + \beta_3) \approx 0.932 \times 1.084 \approx 1.01$ per year, i.e. approximately flat. A stock-turnover dose model was deliberately not fitted (annual fire mortality regressed on the post-1998 dwelling-stock fraction), although the requisite stock-share series was constructed for the person-years denominator. Over the 27 post-intervention years the post-1998 stock share rises smoothly and near-monotonically from approximately 1% to 37%, so as a time-series covariate it is nearly collinear with the linear calendar-time trend (Pearson $r \approx 0.98$ between the annual post-1998 stock share and calendar year over

1999–2025); a dose coefficient estimated on this series would be statistically fragile and could not be cleanly separated from the secular decline, giving the appearance of a mechanism test without its substance. The mechanism-aligned evidence for the stock-turnover interpretation is instead the construction-period cohort comparison (main-text Table 2), which contrasts fatal-fire rates across the standing stock directly rather than inferring turnover from a near-trend covariate. A secondary model added an intervention at 1982 (Brunavarnalög nr. 74/1982). Overdispersion was assessed by the Pearson chi-squared dispersion ratio. Two distributional robustness checks were fitted: a negative binomial model with the dispersion parameter α estimated jointly by maximum likelihood (statsmodels’ discrete NegativeBinomial, NB2 parameterization), and a quasi-Poisson model in which the Poisson standard errors are scaled by the square root of the Pearson dispersion ratio. Residual autocorrelation was assessed by the Durbin–Watson statistic on Pearson residuals. Because the intervention is a dwelling-construction regulation whereas the primary series combines structure and non-structure deaths, the model was additionally re-estimated on structure-fire deaths and on structure-fire incidents as sensitivity analyses. Model comparison used Akaike’s Information Criterion (AIC). Incidence rate ratios (IRR) with 95% confidence intervals are reported. Analyses were performed in Python 3 using statsmodels [4]; the corresponding script (revision_analyses.py) is included in the reproducibility repository.

The Poisson ITSA model on the combined series (AIC = 216.3; Pearson dispersion ratio = 1.15, indicating mild overdispersion) shows a significant pre-1999 declining trend in fire mortality (β_1 : IRR per year = 0.932, 95% CI 0.909–0.955, $p < 0.001$), no significant level change at the 1999 breakpoint (β_2 : IRR = 1.29, 95% CI 0.58–2.86, $p = 0.54$), and a positive slope-change term (β_3 : IRR per year = 1.084, 95% CI 1.037–1.132, $p < 0.001$). The net post-1999 annual trend, $\exp(\beta_1 + \beta_3) \approx 0.932 \times 1.084 \approx 1.01$ per year, is approximately flat: the decline levelled off after 1999 at a historically low rate rather than reversing, so the positive slope-change term should not be read as a rise in the absolute rate. There is thus no detectable discrete step at 1999, only a pre-existing decline that flattens.

A negative binomial model with the dispersion parameter estimated by maximum likelihood returned $\hat{\alpha} = 0.045$ (95% CI including zero), indicating approximate equidispersion, with AIC = 217.8 (only 1.6 points above the Poisson AIC of 216.3, as expected for a

nested model at low dispersion). It reproduced the non-significant 1999 level change (IRR = 1.28, 95% CI 0.56–2.95, $p = 0.56$). A quasi-Poisson model with dispersion-scaled standard errors gave essentially the same estimate (IRR = 1.29, 95% CI 0.55–3.03, $p = 0.56$). The absence of a significant discrete step at 1999 is therefore stable across all three distributional treatments. (Holding the dispersion parameter fixed at $\alpha = 1.0$ rather than estimating it would, for these near-equidispersed data, grossly overstate the variance and inflate the AIC; the maximum-likelihood estimate is therefore used throughout.) The Durbin–Watson statistic on the Poisson Pearson residuals is 2.95. A value above 2 indicates mild negative first-order autocorrelation (here $r \approx -0.48$), not the positive autocorrelation that would deflate standard errors and inflate the significance of the trend; the statistic’s reference distribution is only approximate for Pearson residuals from a Poisson GLM at $n = 58$, but the direction is unambiguous and is conservative for the secular-decline inference. Because $\hat{\alpha} \approx 0.045$ lies close to the lower boundary of the dispersion parameter, the negative-binomial AIC and likelihood-ratio comparisons against the nested Poisson are boundary cases and are reported as such. The pre-existing declining trend is robust throughout.

To match the outcome to the intervention, the model was re-estimated on structure-only series. The secular decline persisted for structure-fire deaths (IRR per year = 0.948, 95% CI 0.924–0.973, $p < 0.001$) and structure-fire incidents (IRR per year = 0.959, 95% CI 0.932–0.987, $p = 0.004$). The 1999 level change was non-significant on both outcomes: IRR = 0.91 (95% CI 0.39–2.16, $p = 0.83$) for structure deaths and IRR = 0.84 (95% CI 0.33–2.11, $p = 0.71$) for structure incidents. The robust and reproducible signal across every outcome definition is thus the pre-existing secular decline; no discrete step at 1999 is detected. A two-intervention model adding a 1982 breakpoint showed no improvement over the single-intervention model ($\Delta\text{AIC} = +3.4$). Because the post-1998 dwelling stock share was near zero in 1999, an instantaneous 1999 level change could not in any case be a causal effect of Byggingarreglugerð nr. 441/1998 under a stock-turnover mechanism; the paper’s inferences rest on the robust secular trend and the cohort comparison, not on the breakpoint term. The full coefficient set for every outcome series and distributional treatment is given in Table S5. Figure S1 presents the ITSA model fit for the combined and structure-fire series with observed counts, fitted values, and the extrapolated pre-1999 trend.

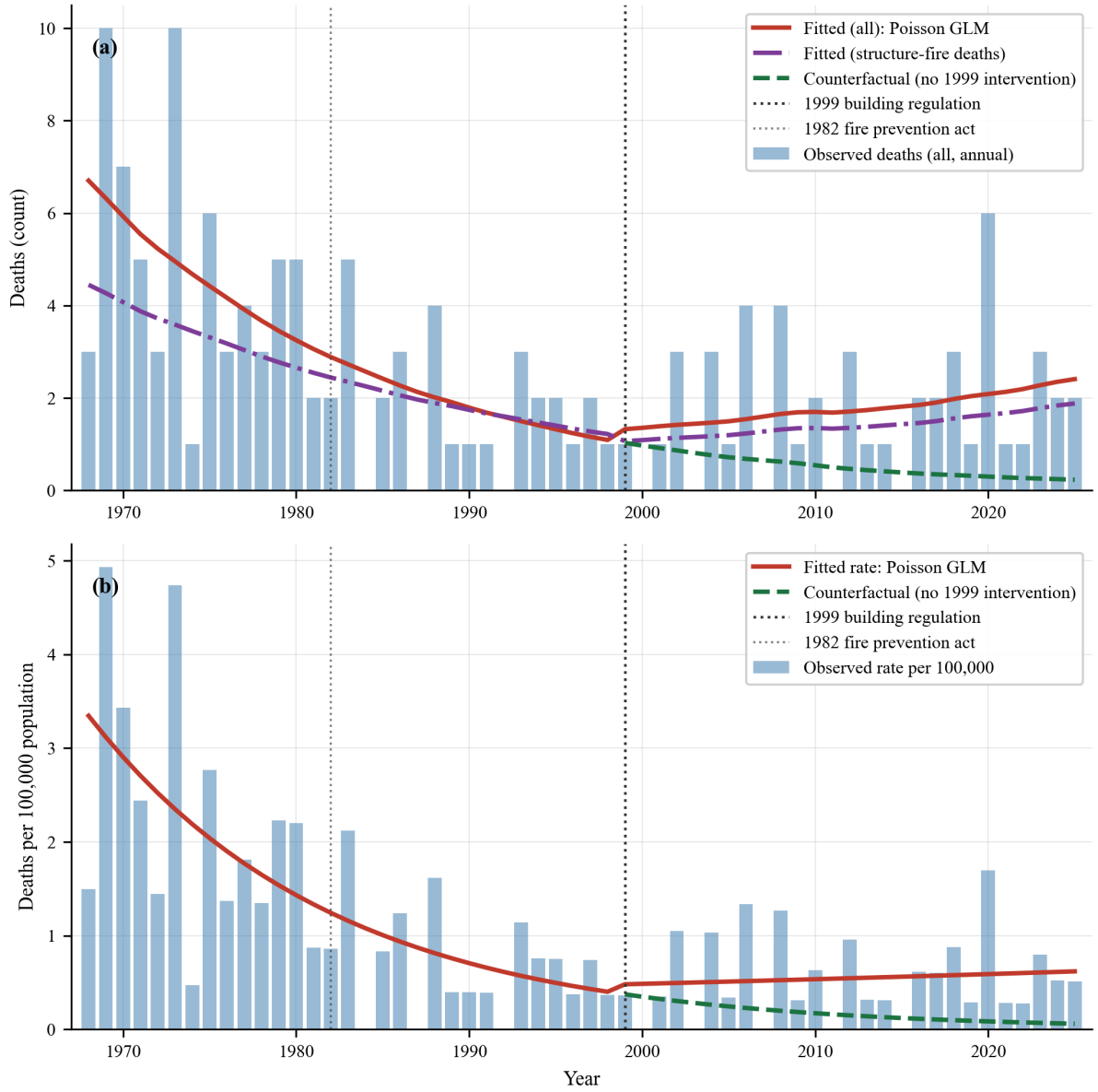


Figure S1: Interrupted time series analysis of annual fire-related deaths in Iceland (1968–2025). Upper panel: observed annual deaths (all fatal fires) with fitted values from the segmented regression model, the fitted structure-fire-deaths trend (dash-dot), and the extrapolated pre-1999 trend (dashed). Lower panel: corresponding mortality rates per 100,000 population. Vertical lines indicate the 1982 fire prevention act and the 1999 building regulation. The displayed fit uses the preferred Poisson model; the maximum-likelihood negative binomial and quasi-Poisson fits are materially identical (see text). Under the standard segmented parameterization there is no significant level change at the 1999 breakpoint (combined-series IRR = 1.29, $p = 0.54$); the figure shows a pre-existing decline that flattens after 1999 rather than a discrete step, consistent with the cohort finding being a property of the building stock rather than of a datable intervention.

Table S5: Full interrupted time series coefficients (incidence rate ratios, 95% CI) for every outcome series and distributional treatment. β_1 = pre-1999 annual trend; β_2 = level change at the 1999 breakpoint (standard segmented coding); β_3 = post-1999 slope change. No β_2 is significant in any specification.

Series / model	β_1 trend/yr	β_2 level	β_3 slope/yr
Combined, Poisson	0.932 (0.909–0.955)***	1.29 (0.58–2.86)	1.084 (1.037–1.132)***
Combined, ML NegBin	0.932 (0.908–0.956)***	1.28 (0.56–2.95)	1.084 (1.035–1.135)**
Combined, quasi-Poisson	0.932 (0.908–0.956)***	1.29 (0.55–3.03)	1.084 (1.034–1.136)***
Structure deaths, Poisson	0.948 (0.924–0.973)***	0.91 (0.39–2.16)	1.064 (1.014–1.117)*
Structure incidents, Poisson	0.959 (0.932–0.987)**	0.84 (0.33–2.11)	1.048 (0.994–1.105)

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

S6 Seasonality

Fatal incidents occur year-round but concentrate in the heating season. October–March accounts for 66 of 113 fatal incidents (58%) and 84 of 145 deaths (58%) (Figure S2). The test is a binary comparison of the October–March heating season against April–September, not a twelve-cell monthly goodness-of-fit test; the expected heating-season share is 49.9% of days rather than exactly one half. This yields $\chi^2 = 3.2$ (df = 1, $p = 0.07$) for incidents and $\chi^2 = 3.6$ (df = 1, $p = 0.056$) for deaths, a consistent directional pattern that falls short of conventional significance, reflecting the limited statistical power of 113 events. The concentration of fatalities in October–March is consistent with established seasonal patterns across Northern Europe, attributed to heating demand, extended indoor exposure, and reduced nighttime escape margins [5], and may be particularly pronounced in the Nordic context due to sustained winter heating and extended polar darkness.

S7 Non-Structure Fatal Incidents

Non-structure incidents contribute 29 deaths across 20 incidents. Maritime incidents are prominent: ship fires account for 15 deaths across seven incidents, and vehicle fires account for three deaths across three incidents. The non-structure deaths are concentrated in the earlier decades of the study period (18 of 29 deaths occurred before 1990), suggesting that improvements in maritime and occupational fire safety may have contributed to the overall decline alongside the residential improvements. The small number of events precludes formal trend analysis for this subgroup.

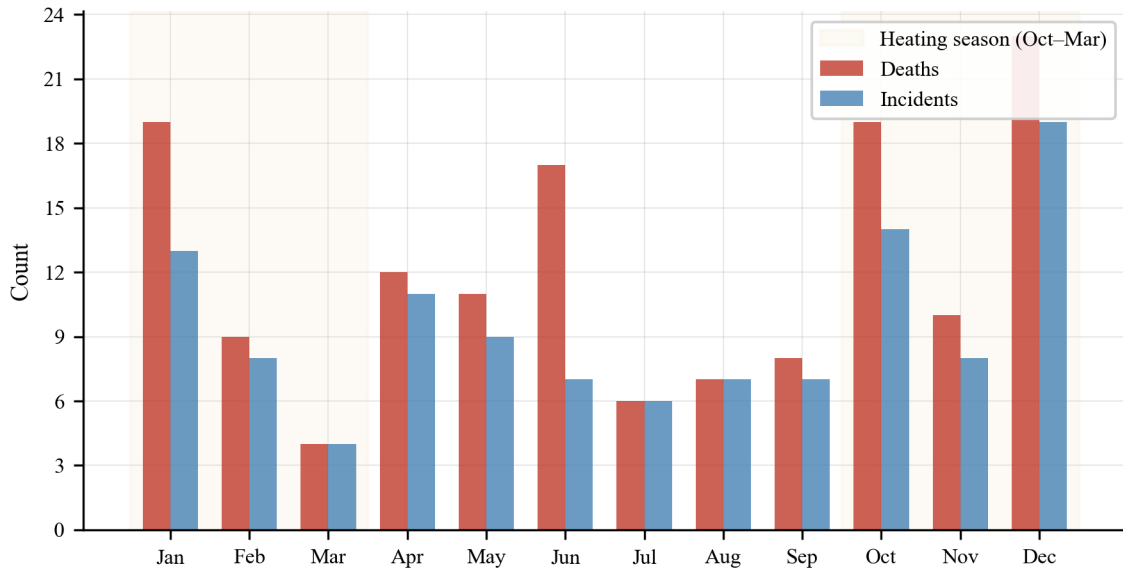


Figure S2: Monthly distribution of fatal fire incidents (deaths and incident counts). The October–March heating season accounts for 58% of both incidents and deaths. The consistent heating-season concentration suggests that indoor exposure and heating-related ignition sources remain persistent risk factors.

S8 Institutional Sustainability and Emerging Risks: Extended Analysis

The data presented in this study document declining fire mortality during a period of active institutional development. Whether those gains can be sustained is not answerable from the registry alone, but contextual evidence suggests that institutional capacity is a relevant factor. Young [6] emphasizes that regime effectiveness is dynamic: institutional arrangements that produce results in one period may lose efficacy if supporting conditions erode.

Institutional transfers and capacity erosion. The fire safety portfolio has undergone repeated transfers: from Brunamálastofnun (established 1969) to Mannvirkjastofnun, and subsequently to Húsnæðis- og mannvirkjastofnun (HMS). These transfers resulted in fragmented record-keeping, diminished institutional memory, and no systematic registry linking fire incidents to building characteristics. Government action plans have repeatedly proposed improvements without confirmed funding. A 2021 plan listed 23 emergency services measures, of which 15 required capital investment, yet none identified a funding source [7]. The gap between stated policy objectives and resourced implementation

remains substantial [8]. In 2020, the relocation of the fire prevention division from Reykjavík to Sauðárkrókur prompted complete staff turnover, confirming practitioner warnings of capacity loss [9].

The Stuðlar investigation. The fatal fire at Fossaleyni 17 (October 2024, 1 death) provides the most concrete documented example of institutional weakness producing fatal consequences. The HMS investigation [10] found that the Capital Region Fire and Rescue Service (SHS) ceased physical site inspections from 2022 onward, replacing them with paper-based self-reports from the building owner, despite outstanding violations never remedied. A 191 m² building extension completed in 2004 had operated without legal authorization for 20 years: undetected by inspection, unregistered in the national property registry, and absent from fire insurance records. At the time of the fire, compartment doors were not closed, electronic escape routes failed, smoke vents did not open, and the evacuation plan was not followed. HMS concluded the fatality could have been prevented had prescribed measures been operational. This case illustrates the distinction between regulatory outputs (legislation enacted, standards adopted) and regulatory outcomes (lives protected). More broadly, the Stuðlar case demonstrates that modern prescriptive codes are highly effective when enforced but physically meaningless when institutional enforcement fails: a building that exists outside the regulatory framework receives none of its protections, regardless of the date of construction. Nominal regulatory compliance status, as recorded in the property registry, is an unreliable proxy for actual physical fire protection when enforcement mechanisms have degraded.

Unauthorized residential use and arson. Recent fatalities at Funahöfði and Stangarhyl (both 2023) involved buildings designed for commercial use but occupied as dwellings, lacking residential fire safety provisions. Three confirmed or suspected arson incidents account for seven deaths: Kirkjuvegur, Selfoss (2018, 2 deaths) [11]; Bræðraborgarstígur, Reykjavík (2020, 3 deaths, 1906 building) [1, 12]; a residential fire in Reykjavík (2025, 2 deaths). All occurred in older buildings involving interpersonal conflict or housing precarity among vulnerable populations, a combination that prescriptive codes alone cannot address.

Data quality as institutional indicator. The Gjöll registry was compiled because existing official statistics lacked incident-level traceability and contained documented errors (e.g.,

2009 recorded as zero fire deaths despite a confirmed fatality at Kljáströnd) [13]. The registry approach, with source references for each incident and a public-facing research interface, supports auditable trend analysis and reproducible research [14].

All three patterns, institutional erosion, unauthorized use, and arson, disproportionately affect vulnerable populations. The remaining fatal fire burden is now concentrated among those least served by the regulatory apparatus: individuals in older housing, unauthorized conversions, and marginalized circumstances. The benefits of Iceland’s fire safety progress accrue primarily to those living in compliant, regulated housing; those with the fewest resources face the greatest residual risk [13]. The transition from writing new building codes to enforcing existing ones and addressing the socioeconomic determinants of fire risk represents the next frontier of fire safety policy.

S9 Completeness Reconciliation Against the National Cause-of-Death Series

To assess ascertainment completeness without individual-level linkage, the registry’s annual fire-death counts were reconciled against Statistics Iceland’s official cause-of-death series for exposure to smoke, fire and flames (ICD-10 X00–X09; table MAN05302 [15]), available from 1996. The official total sums ICD-10 codes X00, X04, X06, X08 and X09, the five codes in the X00–X09 block with recorded deaths over 1996–2024 (codes X01–X03, X05 and X07 record no deaths and are absent from the published table); the corresponding per-code counts are 23, 1, 4, 3 and 5 respectively. Over 1996–2024 the registry documents 50 fire deaths against 36 in the official X00–X09 series, with exact agreement in 13 of 29 years.

The two counts are not expected to coincide, and the direction of each discrepancy is informative. The registry exceeds the official count in 12 years. This excess is expected and explicable: the registry captures the full fatal-fire burden, including intentional fire deaths (arson and homicide, for example the 2018 Selfoss and 2020 Bræðraborgarstígur incidents), which are coded to assault or undetermined-intent categories outside X00–X09; non-building fire deaths (ships and vehicles); and deaths the official series omits, most conspicuously the 2009 fatal fire at Kljáströnd, recorded in the registry but absent from

the official zero-deaths tally for that year. The official series exceeds the registry in only four years (1996, 1997, 2000 and 2003), each by a single death and concentrated in the earliest part of the window; these are residual under-ascertained cases that the aggregate comparison cannot adjudicate at the individual level, and that the registry’s disclosed early-decade under-ascertainment already anticipates.

For the 1999–2025 window on which the post-1998 construction-cohort finding depends, the two series are closely concordant. The only years in which the official count exceeds the registry (2000 and 2003) each involve a single death in the early post-intervention period, when the post-1998 dwelling-stock share was negligible (approximately 1–3%), so they are very unlikely to bear on the post-1998 cohort finding. The full year-by-year table is deposited as `icd10_reconciliation_1996_2024.csv` in the reproducibility repository and is regenerated by `reconcile_icd10.py`, which queries the Statistics Iceland PxWeb API directly.

S10 Extended Limitations

This study is descriptive and subject to the following limitations.

First, small sample size and rare-event instability. The registry contains 145 deaths across 58 years. This is small by epidemiological standards. Decade-level estimates are sensitive to individual high-casualty incidents. For example, the Bræðraborgarstígur arson in 2020 (3 deaths) accounts for 20% of the 2020–2025 death total. Readers should interpret decade-level comparisons as indicative trends, not precise estimates.

Second, fatal incidents only. Trends in non-fatal fire incidence and property damage cannot be assessed. This limits evaluation of whether regulation reduces fire occurrence versus fire lethality.

Third, incomplete ascertainment. Annual death counts were cross-checked against the official national fatal-fire tallies (fire authority, 1968–2018); the two diverge in several of the earliest years, so completeness before the mid-1990s cannot be fully quantified and under-ascertainment is plausible in those years when archival sources may be incomplete. For the 1996–2025 window on which the post-1998 finding depends, every incident carries both a media and an official-investigation source, and completeness is expected to be

near-total.

Fourth, missing construction-year data. Construction year is missing for 48 of 93 structure-fire incidents (52%). All missing cases are concentrated in pre-1996 incidents. The sensitivity analysis demonstrates that all 48 occurred between 1970 and 1995. It is logically impossible for any to represent post-1998 buildings.

Fifth, modeled denominators. The post-1998 finding relies on a modeled housing-stock denominator. It does not use census-derived dwelling counts stratified by construction period. Until dwelling-stock data cross-tabulated by construction year become available, the separation of improved building design, reduced exposure time, and sociodemographic differences remains approximate. The model relies solely on cumulative dwelling additions. It omits structural attrition (demolition, abandonment). This effectively models an immortal building stock, systematically overestimating the surviving volume of older structures in recent decades. This bias inflates the pre-1998 person-years and therefore understates the pre-1998 rate, so it is conservative for the cohort contrast: it works against the reported gap, in the opposite direction to the occupancy-density gradient above. The two acknowledged denominator biases thus run in opposite directions.

Sixth, building-age survivorship bias. The comparison between pre- and post-1998 buildings is not age-standardized. A building constructed in 1997 has had 28 years to accumulate risk factors. A 2020 building has had only 5.

Seventh, temporal confounding. The decline in fire mortality coincided with several concurrent trends. These include secular reductions in smoking prevalence, improvements in emergency medical services, and changes in household composition and heating technology. The ITSA cannot separate these trends from the effect of building regulation.

Eighth, missing individual-level data. Key determinants are not captured as structured variables. These include smoke alarm presence and operability, alcohol involvement, smoking, and mobility limitations. Approximately 60% of U.S. home fire deaths occur in dwellings without working smoke alarms [16]. Alarm operability is a stronger predictor of survival than building age [17]. This data gap is a substantial constraint on causal inference. Individual-level risk factors consistently identified in other jurisdictions (alcohol impairment, living alone, advanced age, impaired mobility) cannot be assessed from the Gjöll registry [18–20].

Ninth, the renovation blindspot. The binary construction-year variable fails to capture permitted major renovations. Pre-1998 buildings subjected to substantial renovation must meet modern fire safety standards for the scope of the renovation. The registry classifies an unquantified number of functionally modernized structures as older stock. This conflates the original construction date with current physical and regulatory reality. An unknown portion of the pre-1998 safety record may reflect post-1998 compliance applied retroactively through the permitting process.

Tenth, construction year as proxy. Construction year serves as a proxy for multiple correlated features. These include building materials, electrical systems, fire safety equipment, and occupant demographics. It is not a direct causal mechanism. The post-1998 threshold captures a bundle of regulatory and technological changes that cannot be disentangled in this design. Additionally, the physical completion date ignores regulatory permit lag. Buildings completed between 1999 and 2001 were likely designed and permitted under pre-1998 codes. This temporarily contaminates the post-1998 cohort with older regulatory standards.

Eleventh, zero-numerator constraint. Under the strict convention the post-1998 cohort has zero events, so the rate-ratio point estimate is degenerate (0); inference in that cell rests on the exact conditional upper bound (Table 2), and the precision of the strict-convention comparison is correspondingly limited.

Twelfth, sociodemographic confounding. The ITSA's declining secular trend masks unmeasured sociodemographic shifts across the 58-year study period. These include de-institutionalization of psychiatric and elderly care, relocating high-risk individuals from monitored institutional settings into the general housing stock. They also include population aging, as fire fatality risk scales non-linearly with advanced age due to cognitive decline and impaired mobility. Changes in alcohol consumption patterns, a primary co-factor in fire fatalities internationally, are also unmeasured. These factors dynamically alter the population baseline of vulnerability independently of building regulations.

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