

Remembering the Shogun Era: Long-Run Effects of a Catastrophe on Policymaking over 160 years*

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Abstract

We examine whether disaster experience shapes political attention and preparedness across generations. Lords' predetermined residence schedules provide a proxy for exposure to Japan's unexpected 1855 Ansei Edo earthquake, which we link to modern municipalities through historical village holdings. Greater exposure is associated with larger discussion responses to new earthquakes, including risk, evacuation, and communication, especially in consistently observed councils. Exposure also predicts preparations for outside assistance and shelter supplies. Differences in specifying tasks for outside disaster-response personnel emerge after 2018. Seismic-retrofit support shows no detectable differential change. Preserving experience may therefore contribute to public preparedness across generations.

Keywords: policy responsiveness, disaster preparedness, experience effects, historical persistence
JEL codes: D83, H75, H84, N45, Q54

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1 Introduction

History can repeat itself when the memory of a tragedy fades across generations. A large literature shows that experiencing or observing a disaster changes behavior around the time of the event. Such evidence leaves open whether the experience of one generation also shapes public decisions made by distant future generations. Records, books, and word of mouth can carry memories beyond the original witnesses. If those memories affect how later generations respond to danger, preserving disaster experience may contribute to future preparedness.

This paper asks whether experiencing a catastrophe changes political attention and public policy across generations. We study Japan’s 1855 Ansei Edo earthquake, which struck unexpectedly while local lords followed a predetermined schedule of alternating residence between their domains and Edo, now Tokyo. Some lords and their traveling retinues were scheduled to be in Edo at that time, while others were scheduled to be in their home domains. We use scheduled residence as a proxy for direct earthquake experience and link the lords’ village holdings to present-day municipalities. Council records from 2018–2023 and preparedness records from 2025 then allow us to look for a historical association more than 160 years after the catastrophe.

Municipalities with a larger share of their historical village links tied to lords scheduled to be in Edo show a greater discussion response to new earthquakes and are more likely to have made concrete disaster preparations. The discussion evidence is clearest in municipalities observed consistently during regular council months (March, June, September, and December); estimates in the broader sample are smaller and less precise. The policy differences appear in preparations for receiving outside personnel and stocking shelter supplies, with the positive difference in specifying tasks for outside disaster-response personnel emerging after 2018. We do not detect a comparable difference in municipal support for strengthening buildings against earthquakes. These patterns are consistent with inherited experience remaining relevant to political attention and preparedness long after the original witnesses are gone.

Section 2 explains what we compare and what we measure. The historical comparison uses predetermined attendance in Edo, rather than destruction in the lords’ home territories. We map the lords’ village holdings to 1,104 present-day municipalities. Historical exposure ranges from zero when all included links are to home-scheduled lords to one when all are to Edo-scheduled lords. Council records measure the monthly share of observed councillors mentioning earthquakes while questioning local officials. Fixed vocabulary and context rules classify these remarks into nine overlapping topics. Municipal records measure arrangements for receiving outside personnel, shelter supplies, and access to seismic-retrofit support.

Section 3 asks whether historical exposure appears in baseline earthquake discussion, responses to new earthquakes, and the subjects councillors raise. We find no clear positive association in months without a major national earthquake in the current or preceding six months. We then compare changes after new earthquakes across historical exposure, accounting for persistent municipal differences, common monthly changes, and observed municipal characteristics. Among 205 municipalities observed in every regular council month in 2018–2023, the earthquake-mention share rises 4.60 percentage points more one to three months after an earthquake for an exposure difference from zero to one. In the full sample of 604 municipalities, the estimate is 1.72 points, with a 95 percent confidence interval including zero. Applying the same comparison to the nine topics shows larger increases in discussion of risks and damage, evacuation and shelter life, and residents’ preparedness, drills, and communication in the 205-municipality sample. Estimates for building and infrastructure safety, administrative plans, and lessons from past disasters are close to zero; these exploratory topic comparisons vary in precision with the sample and regional monthly controls.

Section 4 asks whether historical exposure also appears in actual preparedness, and when differences emerged. We compare municipalities within the same prefecture, Japan’s regional administrative unit. Moving from zero to full exposure is associated with 9.8–12.6-percentage-point higher probabilities in 2025 of specifying tasks for outside disaster-response personnel, designating workspace for incoming personnel, and stocking bedding for evacuees with limited mobility. The positive difference in specifying tasks for outside personnel emerged after 2018, during a national effort to strengthen preparedness. From 2018 to 2025, this preparation increased 16.6 percentage points more across the same exposure contrast; intermediate annual records corroborate the updating pattern. No analogous association is detected for access to seismic-retrofit support. These parallel associations do not estimate the causal effect of council speech on policy.¹

Section 5 considers preserving disaster experience as an investment in future preparedness. If transmitted experience informs public decisions, benefits can extend beyond those preserving or hearing the accounts, strengthening the economic rationale for supporting testimony and its transmission across generations. The analysis leaves open which ways of preserving experience lead later generations to undertake concrete preparations, since it does not identify the transmission process.

Related literature. The paper contributes to the literature on learning from disasters by examining responses long after the original witnesses are gone. Existing studies document changes in insurance demand, household choices, corporate policy, and political institutions

¹Appendix C.3 separately relates prior policy discussion to subsequent preparedness reports.

when people experience or observe a disaster (Lin, 2020; Filipski et al., 2019; Dessaint and Matray, 2017; Belloc, Drago and Galbiati, 2016). Closest to our analysis of political attention, Wyss and Chiru (2026) examine parliamentary speeches following Germany’s 2021 floods and find that legislators representing constituencies with prior flooding experience are more likely to participate in the flood debates. Their evidence connects local disaster histories to which representatives speak when a new disaster occurs. We examine links to a catastrophe more than 160 years earlier and consider both the content of council discussion and practical preparations.

The paper also connects research on historical persistence with work on how experience survives beyond individuals. Studies of health, reconstruction, communities, and commemoration document the persistence of historical experiences (Caruso and Miller, 2015; Gignoux and Menéndez, 2016; Hornbeck and Keniston, 2017; Fouka and Voth, 2023; Charnysh and Peisakhin, 2022). Research on organizational learning explains how records and routines can preserve information after individuals leave (Levitt and March, 1988; March, Sproull and Tamuz, 1991). Connecting family history to legislative behavior, Feigenbaum, Palmer and Schneer (2025) show that US legislators with immigrant parents or grandparents are less likely to speak negatively about immigration. Their close-election analysis also finds that electing legislators with immigrant ancestry shifts the tone of immigration speeches in a more favorable direction. We bring these questions to disaster preparedness 160 years after the original catastrophe, using a historical comparison defined by scheduled attendance in Edo rather than local destruction.

2 Background and Data

2.1 Alternate Attendance and the 1855 Earthquake

During the Edo era, domain lords had to spend part of their time in Edo, traveling there with substantial retinues to serve the Shogun under *sankin-kotai*, the alternate attendance system. The lords’ wives and heirs were required to remain in Edo, and each domain maintained an estate and permanent staff there. Lords and their traveling retinues stayed at these estates while in Edo. The system kept the Shogun informed about, and imposed large travel and residence costs on, political elites (Tsukahira, 1966; Jansen, 2000; Vaporis, 2008). The attendance schedule followed a predetermined rotation: one group was due in Edo in odd-numbered years and the other in even-numbered years. The rotation could pair lords from nearby domains on opposite schedules (Fujii, 1989, p. 18).

On November 11, 1855, a catastrophic urban disaster, the *Ansei Edo earthquake*, struck

Table 1: Scheduled Groups, Home Geography, and Edo Exposure

	Home-due	Edo-due	Edo minus home	<i>p</i> -value	<i>N</i>
<i>Panel A. Predetermined home-domain characteristics</i>					
Distance to Edo (km)	446.9	435.0	-12.0	0.747	176
Log assessed domain size	10.614	10.621	0.007	0.963	184
Home-region composition (joint)	—	—	—	0.689	187
<i>Panel B. Recorded Edo-estate exposure</i>					
Coverage in damage compilation	90.4	95.7	5.3	0.261	187
Any death recorded	44.7	43.8	-0.9	0.937	174
Main residence destroyed (share)	24.9	22.2	-2.7	0.610	174
Any structural collapse	78.8	84.3	5.4	0.366	174
Assigned seismic intensity	5.67	5.61	-0.06	0.437	173

Notes: Differences are Edo-due minus home-due. Panel A reports fixed-count random-label diagnostics for predetermined domain characteristics. Panel B reports the same diagnostic for recorded Edo-estate exposure; percent outcomes are multiplied by 100, while seismic intensity remains in its original units. Samples use all available observations for each row. Failure to reject a difference is not an equivalence result.

without warning. With an estimated magnitude of about 7, the earthquake and ensuing fires burned roughly 1.5 square kilometers of Edo and killed at least 7,000 people (Cabinet Office, Government of Japan, 2004, p. 1). Because the attendance schedules had already been set, lords could not choose their scheduled location in anticipation of the shock. We use scheduled presence as a proxy for whether a lord and his traveling retinue experienced the earthquake in Edo. We refer to domains whose leaders were due in Edo as the *Edo-scheduled group* and those whose leaders were due at home as the *home-scheduled group*. Our reconstruction assigns 93 alternating domains to Edo and 94 to their home domains when the earthquake struck (Center for Open Data in the Humanities, 1855).²

Although the earthquake’s timing was plausibly random, the scheduled groups could differ in their home territories. We find no detected imbalance in measured home geography or domain size. Table 1 compares home-region composition, castle-to-Edo distance, and recorded domain size. Castles in the Edo-scheduled group were 12 kilometers closer to Edo on average, with a random-label *p*-value of 0.747. The joint random-label *p*-value is 0.689 for home-region composition, and the *p*-value for log domain size is 0.963. We also compile recorded Edo-estate damage for both groups (Takemura, 1995; Earthquake Research Institute, University of Tokyo, 1985). Table 1 also shows no detected differences in recorded deaths, destruction of the main residence, structural collapse, or seismic intensity.

²Historical records let us assess how well the schedule measures actual location. We searched domain histories, contemporary diaries, official disaster reports, and archival earthquake records for a purposive sample of 41 lords. Twenty yield a classifiable location. Seventeen documented locations agree with the schedule and three differ. Appendix A.1 reports the audit and its limitations.

2.1.1 From Historical Villages to Municipal Exposure

To assign the 1855 schedule classifications to modern municipalities, we use the villages recorded among each domain’s late-Edo holdings. The source combines village-holder records based on the National Museum of Japanese History’s *Kyūdaka Kyūryō Torishirabe-chō* database with municipality codes derived from the Ministry of Agriculture, Forestry and Fisheries’ 2015 Agricultural Settlement Boundary Data (Honda, Natsume and Nemoto, 2024; National Museum of Japanese History, 2026; Ministry of Agriculture, Forestry and Fisheries, 2015).

The agricultural-settlement data identify each village’s present-day municipality. We match recorded holder names to the fixed roster of 187 alternating domains. 179 domains are recorded as holders of at least one village that can be assigned to a modern municipality.

Municipal exposure is the share of a municipality’s recorded village holdings tied to Edo-scheduled domains. For municipality m , let V_m^E be the sum of record shares assigned to Edo-scheduled domains and V_m^H the sum assigned to home-scheduled domains. We define

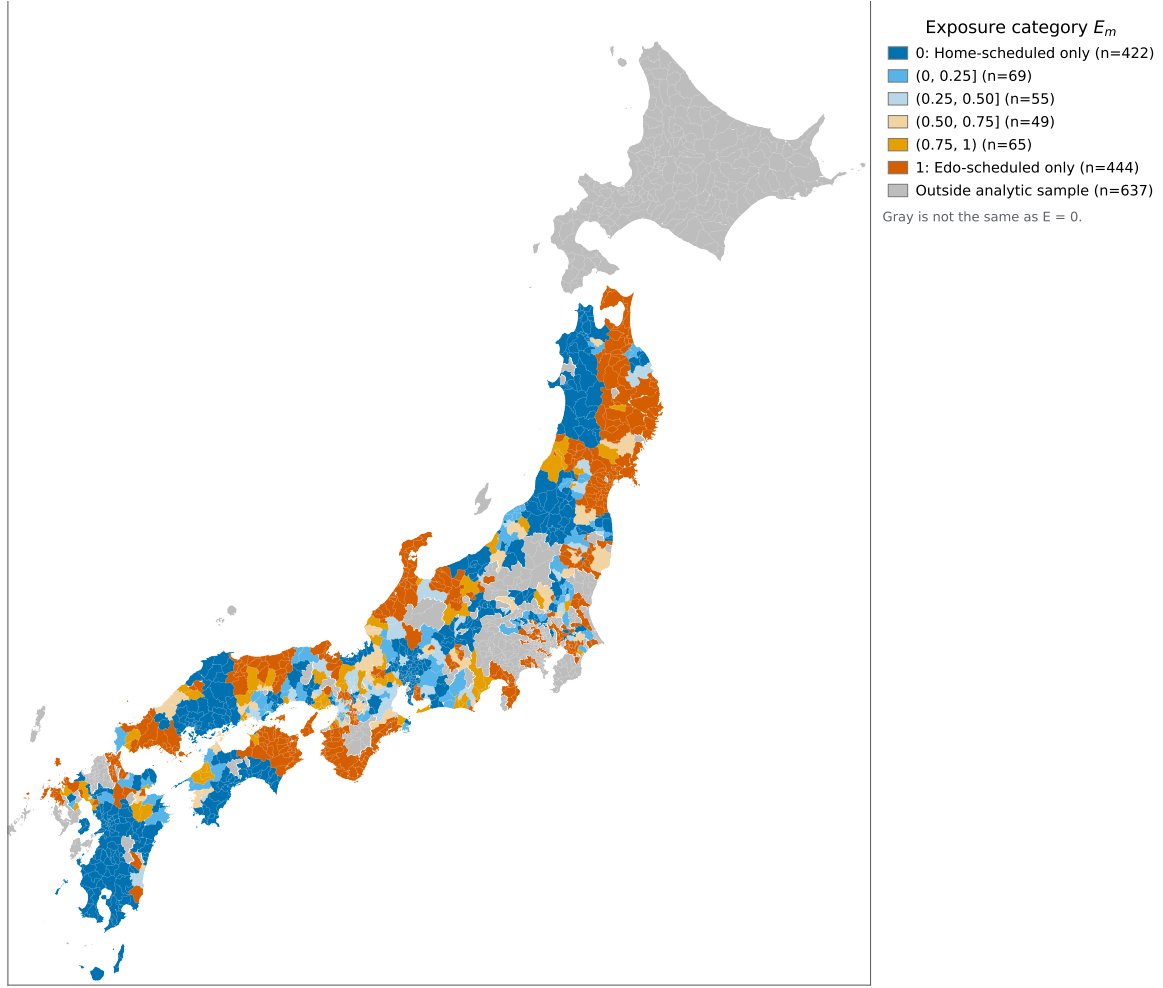
$$E_m = \frac{V_m^E}{V_m^H + V_m^E}. \quad (1)$$

Exposure is zero when every included record share is tied to the home-scheduled group and one when every included record share is tied to the Edo-scheduled group. Nonalternating domains and nondomain holdings do not enter the denominator.³ This process yields a positive total weight from alternating domains, $V_m^H + V_m^E > 0$, for 1,127 municipalities. We retain municipalities whose codes link to both 2010 and 2025 in the existing housing seismic-retrofit-support panel described below. This leaves a common policy sample of 1,104 municipalities in 44 prefectures. Appendix A.2 documents the village mapping and sample flow and illustrates the exposure calculation with Kurashiki.

Descriptive Statistics. Exposure averages 0.509 in the 1,104-municipality policy sample. Its distribution is concentrated at the endpoints: 422 municipalities have $E_m = 0$, 444 have $E_m = 1$, and 238 have intermediate exposure (Appendix Table S2).

Figure 1 maps this variation, distinguishing the endpoints and intermediate exposure ranges; gray denotes municipalities outside the analytic sample.

³These excluded holdings include domains whose lords were permanently resident in Edo or did not switch between the Edo and home groups in odd and even years. We also exclude villages controlled directly by the shogunate, by *hatamoto* retainers, by temples and shrines, by court or imperial households, and holder names that cannot be matched to the fixed domain roster. None of these holdings contributes to either V_m^H or V_m^E .



Boundary source: Ministry of Land, Infrastructure, Transport and Tourism, National Land Numerical Information, Administrative Area Data (N03), January 1, 2025; edited by the author. Exposure: authors' construction.

Figure 1: Schedule-Based Historical Exposure Across Modern Municipalities

Notes: The map colors the 1,104 municipalities in the analytic sample by the share of mapped village-record weight tied to Edo-scheduled domains. Exposure ranges distinguish the two endpoint values from four intervals in the interior of the unit interval. Gray municipalities are outside the analytic sample and are not coded as zero exposure. Designated-city wards are aggregated to their parent municipalities; Tokyo's 23 special wards remain separate municipalities. The Izu Islands, Ogasawara Islands, and Okinawa are omitted because none contains a municipality in the analytic sample. Municipality boundaries are based on the Ministry of Land, Infrastructure, Transport and Tourism's National Land Numerical Information, Administrative Area Data (N03), January 1, 2025 (Ministry of Land, Infrastructure, Transport and Tourism, 2025a).

2.2 Council discussion of earthquakes

We use municipal council minutes to measure elected representatives’ public discussion of earthquakes. We seek general-question records from plenary sessions in the common policy sample of 1,104 municipalities during 2018–2023. The period spans the policy analysis’s 2018 benchmark and the 2020 national guidance discussed below. We collect HTML and PDF minutes directly from municipal and council publication systems, using meeting calendars and indexes to locate records by date and meeting type. The downloaded corpus contains 98,421 documents from 831 municipalities.

The analysis retains councillors’ own general questions and follow-up remarks for which the meeting date, speaker, and type of proceeding can be identified. Administrative replies, procedural interventions, and other categories of questions are excluded, as are duplicates and unresolved text.⁴

The resulting observed sample contains 144,817 councillor-months in 604 municipalities, covering 13,341 municipality-months in 44 prefectures. Approximately 92% of these municipality-month observations fall in March, June, September, or December. To assess the role of changing municipality and meeting-month coverage, we restrict observations to these four calendar months and retain municipalities with an observed outcome in all 24 such months of 2018–2023. This regular-month sample contains 205 municipalities and 4,920 municipality-months.

Our main measure is the *earthquake-mention share*: the fraction of observed general-question speakers whose own recorded remarks contain either *jishin* (earthquake) or *shinsai* (earthquake disaster) at least once during the month. This measure includes references to past disasters and commemorations as well as discussion of preparedness; it does not require an explicit expression of concern or a policy request.⁵

Topics within earthquake discussion. We use rule-based natural-language processing to classify what councillors discuss in their earthquake-related remarks. AI-assisted readings of source passages informed nine overlapping categories and the vocabulary used to identify them. Appendix Table S6 summarizes each category. The rules assign a topic when its vocabulary appears in the same or nearby sentences as an earthquake reference or an earthquake-specific expression. We refined the vocabulary and context rules using a separate calibration sample and fixed them before estimating the topic-specific responses.

⁴Source-specific processing identifies speaker and meeting boundaries. We exclude 126 PDF documents with known mixed-layout problems; other partially parsed documents contribute only the eligible remarks that can be identified.

⁵A month with observed general-question speakers but no matching remarks has a zero share, whereas a month without a usable speaker denominator is missing.

Classification was checked against AI-assisted readings of a small, stratified held-out sample after the rules were frozen. The topic shares are proxies for recorded discussion, not exhaustive accounts of its content. Appendix B.1 details the procedure and checks.

We calculate a *topic-specific speaker share* for each category, using the same speakers and municipality-month unit as the overall earthquake-mention share. A councillor counts once toward a topic if at least one eligible passage on that topic is detected in their own remarks during the month. We divide this count by all observed general-question speakers in that municipality and month. Because a councillor may discuss several topics, the nine shares overlap and do not sum to the overall earthquake-mention share. An observed month with no detection has a zero topic share; a month without an observed speaker denominator remains missing.

Descriptive Statistics. The earthquake-mention share averages 21.39% across all observed municipality-months and 21.43% in the regular-month sample. The corresponding average numbers of observed general-question speakers per municipality-month are 10.86 and 10.72. Appendix Table S5 reports overall mentions, the nine topics, and observation coverage together.

Risk and damage recognition and evacuation and shelter life are the most frequently detected topics, averaging 5.91% and 5.90% of observed speakers in the full sample. The regular-month sample has similar means across the nine categories.

2.3 Municipal Preparedness

As measures of policy reform, we focus on three preparations promoted in national guidance after 2018. The first records whether a municipality has specified which disaster-response tasks it will carry out with outside personnel; the second records whether it has designated workspace for those personnel; and the third records whether it stocks bedding for evacuees with limited mobility. The Cabinet Office’s 2020 receiving-support guidance and template ask municipalities to plan the tasks and working arrangements needed to receive outside personnel, while separate shelter guidance urges advance stocks of cardboard beds (Cabinet Office, Government of Japan, 2020; Cabinet Office, Government of Japan and Fire and Disaster Management Agency, 2020). We measure the first two preparations using municipal records from the Fire and Disaster Management Agency, and the third using the Gender Equality Bureau’s 2025 municipal preparedness survey (Fire and Disaster Management Agency, 2026; Gender Equality Bureau, Cabinet Office, Government of Japan, 2025). The records also show whether municipalities had specified tasks for outside personnel in

2018, allowing us to measure subsequent changes. Appendix C.1 details the annual policy records and their alignment.

Municipal access to housing seismic-retrofit support provides a more capital-intensive comparison outcome. We use existing observations for access in 2010 and 2025, the change between those years, and adoption by 2025 among municipalities without access in 2010 (Ministry of Land, Infrastructure, Transport and Tourism, 2025*b*). The measure records whether residents can access a municipal or prefectural support program; it does not measure completed retrofits, construction expenditure, or the seismic performance of the housing stock.

Descriptive Statistics. The three promoted preparations were present in 70.9, 61.9, and 78.1 percent of municipalities in 2025, respectively (Appendix Table S16). The share of municipalities that had specified tasks for outside personnel rose from 10.2 percent in 2018 to 70.9 percent in 2025, while access to seismic-retrofit support rose from 63.9 percent in 2010 to 93.8 percent in 2025. These are unadjusted sample summaries.

3 Earthquake Discussion in Municipal Councils

This section studies councillors’ discussion of earthquakes. We distinguish differences in the level of earthquake discussion from differences in the response to new earthquakes. The first comparison asks whether councillors in more historically exposed municipalities discuss earthquakes more in months without a recent major national earthquake. The second asks whether their discussion changes more after a new event, which can bring preparedness back to the political agenda even far from the affected area.

We use a common national earthquake calendar for 2018–2023 to define a new “event.” A major national earthquake is an event group in the Japan Meteorological Agency (JMA) records with maximum observed seismic intensity of at least 6-lower somewhere in Japan. Nine event groups meet this criterion, as verified against the JMA’s published list and monthly counts (Japan Meteorological Agency, 2023). JMA sometimes groups nearby earthquakes whose intensities cannot be separated, so the count refers to event groups. Appendix B.2 lists the event dates.

The comparison period consists of months with no qualifying earthquake in the current or preceding six months. We use these months to measure baseline discussion and as the reference category against which discussion after a new event is compared. They can still include weaker earthquakes, foreign earthquakes, and discussion of earlier disasters. Let Y_{mt} denote the earthquake-mention share defined in Section 2.2 for municipality m in month t ,

and let E_m be its historical exposure from equation (1). Let D_t^h indicate the event month, one to three months later, or four to six months later, with $h \in \{0, 1-3, 4-6\}$. These national-event bins are mutually exclusive: each new qualifying event resets the clock.

We use the two council samples described in Section 2.2. The full sample contains 604 municipalities with usable observations in at least one month. The regular-month sample retains the same 205 municipalities in every March, June, September, and December from 2018 through 2023, giving 24 common observation months. We use the available baseline months within each sample for the first comparison and all of its observed months for the event-response analysis.

3.1 Baseline Discussion

We first ask whether earthquake discussion is persistently higher where historical exposure is greater. Restricting the observations to the baseline months defined above, we compare municipalities within prefectures while adjusting for common year-month changes:

$$Y_{mt} = \mu_{p(m)} + \lambda_t + \gamma E_m + \delta' Z_m + \varepsilon_{mt}. \quad (2)$$

The coefficient γ measures the difference in earthquake-mention levels associated with a one-unit difference in historical exposure during these months. Municipality fixed effects would absorb the time-invariant exposure, so this comparison uses prefecture fixed effects $\mu_{p(m)}$. The main table includes the attributes Z_m ; Appendix Table S7 also reports specifications without them, using identical observations within each sample.

The vector Z_m contains log alternating-domain village-record weight; the shares of mapped weight assigned to alternating domains, nondomain holders, and always-Edo domains; castle-town status; log mean municipal revenue in 2011–2015; and two $\log(1 + n)$ counts of recorded local strong-shaking event groups, in 1919–2010 and 2011–2017. The eight attributes are standardized across the 604 municipalities and retain those values in both samples. They enter this regression in levels.

For this comparison and the response regressions below, standard errors are clustered by prefecture and year-month. We use CR1 finite-sample adjustments and t tests with the smaller cluster count minus one as the degrees of freedom. Appendix B.2 gives the sample-specific inference details.

Results. We do not find a statistically distinguishable positive association between historical exposure and earthquake mentions outside the recent-event windows. Panel A of Table 2 uses 5,028 observations from 597 municipalities; seven of the 604 municipalities have no ob-

Table 2: Historical Exposure, Baseline Earthquake Mentions, and Responses to New Earthquakes

	Full council sample (1)	Regular-month sample (2)
<i>Panel A. Baseline levels: coefficient on historical exposure E</i>		
Historical exposure	0.37 (1.27)	-3.01 (1.94)
Observations	5,028	1,845
Municipalities	597	205
Prefectures	44	43
Year-months	25	9
Mean outcome (%)	18.51	18.47
<i>Panel B. Responses: coefficients on exposure $\times$ event window</i>		
Months 1–3 after event	1.72* (0.98)	4.60*** (1.21)
Months 4–6 after event	1.13* (0.65)	2.57*** (0.91)
Observations	13,341	4,920
Municipalities	604	205

Notes: The outcome is the percentage of observed distinct general-question speakers whose own remarks mention earthquake or earthquake disaster. Estimates and standard errors are percentage points for an exposure difference from zero to one; municipality-months receive equal weights. Qualifying national events reach JMA intensity 6-lower somewhere in Japan, and event windows restart at each new event. Baseline months have no qualifying event in the current or preceding six months. Panel A uses only baseline observations, includes prefecture and year-month fixed effects and eight historical, fiscal, and prior-shaking attributes in levels, and excludes municipality fixed effects. Seven of the full sample’s 604 municipalities have no observed baseline months. Table S7 reports all four baseline specifications. Panel B uses all observed months in the full sample and all 24 March, June, September, and December dates of 2018–2023 for the fixed 205-municipality sample. Each column comes from one regression including municipality and year-month fixed effects, exposure interactions with year and calendar month, event-month terms, the eight attributes interacted with all three event windows, and three local-shaking indicators. Its coefficients measure changes relative to baseline months, whereas Panel A reports exploratory conditional level associations from different regressions; coefficients across panels must not be added to infer post-event levels. Parentheses report two-way CR1 standard errors clustered by prefecture and year-month; Panel A counts all identified fixed effects in the finite-sample adjustment. Two-sided t tests use the smaller cluster count minus one degrees of freedom (24 and 8 in Panel A); stars denote unadjusted p -values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

served baseline months. The controlled estimate is 0.37 percentage points for an exposure change from zero to one, with a standard error of 1.27.

The controlled estimate in the regular-month sample is -3.01 percentage points and is also statistically indistinguishable from zero. The 205 municipalities contribute 1,845 observations over only 9 baseline months. The comparisons leave open whether historical exposure is associated with a persistent difference in discussion, motivating the separate test of responses to new earthquakes.

3.2 Responses to New Earthquakes

We next compare changes in discussion after a new earthquake, absorbing permanent municipal differences in the level of mentions. We estimate the difference in this response across levels of historical exposure using

$$Y_{mt} = \alpha_m + \lambda_t + \sum_h \beta_h E_m D_t^h + \sum_h \theta'_h Z_m D_t^h + \rho' L_{mt} + E_m a_{y(t)} + E_m c_{r(t)} + \varepsilon_{mt}. \quad (3)$$

Municipality fixed effects α_m absorb permanent local differences, and year-month fixed effects λ_t absorb common monthly changes, including the average response to each national event. The controls allow municipalities with different historical composition, fiscal resources, and recent earthquake experience to respond differently to the same national events. Every attribute in Z_m is now interacted with all three event windows. The vector L_{mt} adds indicators for recorded local shaking of intensity 6-lower or above in the current month, the preceding one to three months, and the preceding four to six months. Appendix B.2 details the coverage of local shaking records. The interactions $E_m a_{y(t)}$ and $E_m c_{r(t)}$ allow the exposure gradient to vary across years $y(t)$ and calendar months $r(t)$. The coefficients β_h therefore measure how the exposure gradient in mentions differs in each event window from the reference period, conditional on these adjustments. We report coefficients in percentage points for a one-unit difference in E_m .

The response regressions use 13,341 municipality-months in 44 prefectures and 65 observed year-months in the full sample, and 4,920 observations in 43 prefectures and 24 year-months in the regular-month sample.

Aggregate responses. The post-event estimates associate greater historical exposure with a larger change in earthquake mentions relative to the reference period, with the clearest evidence in municipalities observed in every regular-session month. Panel B of Table 2 reports the estimates. In the full sample of 604 municipalities, the share of questioners mentioning earthquakes changes by an additional 1.72 percentage points in months one to three for a one-unit difference in historical exposure, relative to the reference period. Its 95 percent confidence interval includes zero. The estimate for months four to six is 1.13 percentage points.

The difference is larger when the same municipalities are observed in all regular-session months. The second column uses the 205-municipality regular-month sample. The estimated additional increase is 4.60 percentage points in months one to three, with a standard error of 1.21. The estimate for months four to six is 2.57 percentage points, with a standard error of 0.91.

Topics of discussion. The topic analysis asks what councillors discuss when their earthquake-related remarks respond to a new event. We apply the event-response comparison to each of the nine topics defined in Section 2.2. Let $\mathcal{Q}_{mt}$ be the set of all observed general-question speakers in municipality m and month t . For speaker $i \in \mathcal{Q}_{mt}$, let $q_{imt}^{(k)}$ equal one if the classification rules detect at least one passage on topic k in that speaker’s eligible earthquake-related remarks during the month. The *topic-specific speaker share* is

$$Y_{mt}^{(k)} = \frac{1}{|\mathcal{Q}_{mt}|} \sum_{i \in \mathcal{Q}_{mt}} q_{imt}^{(k)}. \quad (4)$$

A zero denotes no rule-detected passage on that topic in the covered remarks, while months without an observed speaker denominator remain missing.

For each topic, we replace Y_{mt} in equation (3) with $Y_{mt}^{(k)}$ and estimate all coefficients separately. The topic coefficient $\beta_h^{(k)}$ measures how the exposure gradient in the share of all observed speakers discussing topic k differs after a national earthquake from the reference period. The event calendar, controls, equal municipality-month weights, and clustering remain unchanged.

Table 3 shows statistically significant positive responses in four topics at the 5% level in the regular-month sample. Among the 205 municipalities, the estimated additional response in months one to three is 2.082 percentage points for risk and damage recognition, 1.779 for evacuation and shelter life, 1.700 for residents’ preparedness, drills, and communication, and 1.191 for general disaster preparedness without a specific field.

Appendix B.3 reports geographical decompositions and checks using alternative samples, fixed effects, clustering, and topic definitions.

The aggregate and topic estimates are consistent with historical experience shaping the attention elected officials give to earthquakes when new events occur, particularly their discussion of risks facing residents and preparations for disaster response. These public remarks do not establish that residents’ beliefs changed or that a proposal was implemented. Section 4 examines whether the same historical exposure is also associated with concrete municipal preparations.

Table 3: Topics in council responses to major earthquakes

Topic	Full sample		Regular-month sample	
	1–3 months	4–6 months	1–3 months	4–6 months
General disaster preparedness	0.477 (0.333)	0.022 (0.258)	1.191*** (0.297)	-0.010 (0.338)
Risk and damage recognition	0.386 —	-0.098 (0.482)	2.082*** (0.611)	0.957 (0.845)
Building and infrastructure safety	-0.378 (0.408)	-0.288 (0.361)	0.021 (0.635)	-0.338 (0.534)
Evacuation and shelter life	0.439 —	-0.310 —	1.779*** (0.533)	-0.031 (0.789)
Supplies, rescue, and disaster medicine	0.542* (0.291)	0.174 (0.201)	1.317* (0.718)	1.054** (0.488)
Administrative plans and implementation	-0.130 (0.142)	0.088 (0.148)	-0.072 (0.363)	0.235 (0.522)
Preparedness, drills, and communication	0.315 (0.360)	-0.132 (0.456)	1.700** (0.607)	0.527 (1.066)
Recovery and reconstruction	0.230 —	0.626** (0.248)	1.124 —	1.341 —
Lessons from past disasters	0.156 (0.212)	0.333 (0.285)	0.124 (0.345)	-0.427 (0.508)
Municipalities	604	604	205	205
Municipality-months	13,341	13,341	4,920	4,920

Notes: Coefficients are percentage-point differences in the post-earthquake response for historical exposure increasing from zero to one. The outcome is the share of all observed general-question speakers in a municipality-month with the named detected topic; the denominator is not restricted to earthquake speakers. A speaker is counted once per topic and month. Topics overlap and do not partition earthquake mentions. Parentheses report two-way CR1 standard errors clustered by prefecture and by year-month, in percentage points. A dash in place of a standard error indicates that the estimated two-way variance is negative; the standard error is undefined and is not replaced by zero. Stars denote unadjusted two-way clustered p -values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. No multiple-testing adjustment is applied, and undefined inference receives no stars. Regressions weight municipality-months equally and retain municipality fixed effects, exposure interactions with year and calendar month, the fixed eight municipal attributes interacted with event periods, and local strong-shaking indicators. Periods are measured from the latest national earthquake reaching JMA intensity 6 lower; reference months have no qualifying event in the current or preceding six months. Both samples use municipality and national year-month fixed effects. The regular-month sample requires observations in all 24 March, June, September, and December months in 2018–2023; the full sample uses every observed month. Classification is a fixed rule-based proxy, not exhaustive semantic coding.

4 Municipal Disaster Preparedness

The council estimates in Section 3 suggest that historical exposure is reflected in the issues councillors raise after major earthquakes. We next ask whether differences associated with the same historical experience also appear in tangible preparations for a future disaster. Defining responsibilities, assigning workspace, and stocking supplies go beyond mentioning earthquakes in a council meeting.⁶

We use 2018 as the benchmark for a period in which the national government encouraged municipalities to strengthen disaster preparedness. During this period, national guidance promoted plans for receiving outside disaster-response personnel and advance stocks for shelter operation (Cabinet Office, Government of Japan, 2020; Cabinet Office, Government of Japan and Fire and Disaster Management Agency, 2020). We ask whether municipal responses to these common policy priorities differed with historical exposure. The evidence has three parts: differences in three administrative preparations in 2025, differential updating after 2018 in whether tasks for outside personnel have been specified, and no detectable differential change in the more capital-intensive policy of seismic-retrofit support.

4.1 Preparedness in 2025

For each focal policy outcome, we compare municipalities in the same prefecture that differ in the share of historical village-record weight tied to Edo-scheduled domains. For outcome Y_m , we estimate

$$Y_m = \theta E_m + \lambda_{p(m)} + \varepsilon_m, \quad (5)$$

where $\lambda_{p(m)}$ is a prefecture fixed effect. For a level outcome, θ compares predicted outcomes between exposure values of zero and one. The three 2025 comparisons use 1,104 municipalities in 44 prefectures. We cluster standard errors by the alternating domain contributing the largest share of each municipality’s mapped village-record weight. This convention captures dependence through the principal historical holder, but not all overlapping links through smaller holdings. Appendix C.1 explains the cluster construction and its limitations.

Historical exposure is positively associated with each of the three preparations in 2025. Panel A of Table 4 reports the estimated differences. Moving historical exposure from zero to one is associated with a 12.6-percentage-point increase in the probability that a municipality has specified which disaster-response tasks it will carry out with outside personnel. The cluster-robust standard error is 3.8 percentage points. The other two estimates are also positive. Moving historical exposure from zero to one is associated with a 10.6-percentage-point

⁶Appendix C.3 examines whether prior discussion of receiving support and shelter beds is associated with subsequent preparedness reports.

Table 4: Historical Exposure and Municipal Disaster Preparedness

Outcome	Historical exposure coefficient	Observations	Clusters
<i>A. Preparedness in 2025</i>			
Tasks for outside personnel specified	0.126*** (0.038)	1,104	163
Designated receiving workspace	0.106*** (0.034)	1,104	163
Bedding for limited mobility	0.098*** (0.029)	1,104	163
<i>B. Tasks for outside personnel specified</i>			
2018 level	-0.040* (0.023)	1,104	163
2018–2025 change	0.166*** (0.041)	1,104	163
Adoption by 2025 if absent in 2018	0.132*** (0.039)	991	157
Mean 2019–2024 change from 2018	0.085*** (0.031)	1,104	163
<i>C. Seismic-retrofit support</i>			
Access in 2010	-0.014 (0.029)	1,104	163
Access in 2025	0.001 (0.014)	1,104	163
Change, 2010–2025	0.015 (0.028)	1,104	163
Access by 2025 if absent in 2010	-0.006 (0.031)	399	88

Notes: Each row reports the coefficient on historical exposure from a separate regression with prefecture fixed effects. Coefficients are in shares for an exposure change from zero to one. Panel A uses 2025 preparedness indicators; Panel B examines whether tasks for outside personnel have been specified; Panel C examines access to seismic-retrofit support. Adoption rows are restricted to municipalities without the outcome in the indicated baseline year. The annual mean in Panel B averages the six changes from 2018 observed in 2019–2024. Standard errors clustered by modal alternating domain are in parentheses. Stars use the unadjusted cluster-robust tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

increase in designated receiving workspace and a 9.8-percentage-point increase in bedding for evacuees with limited mobility. Their cluster-robust standard errors are 3.4 and 2.9 percentage points, respectively.

4.2 Updating after 2018

Of the three preparations, specifying tasks for outside personnel is the only one observed on a comparable basis in both 2018 and 2025. We therefore use this outcome to distinguish a difference already present at the start of the period from differential updating thereafter.

We apply equation (5) separately to the 2018 indicator, its change between 2018 and 2025, and adoption by 2025 among municipalities that had not specified tasks for outside personnel in 2018. The coefficient thus compares initial preparedness or subsequent updating across the same exposure contrast, from zero to one, within prefectures. The adoption regression uses a smaller sample selected on baseline policy status; all three retain clustering by the largest-contributing alternating domain.

Panel B of Table 4 shows no positive initial difference in 2018. Moving historical exposure from zero to one predicts a -4.0-percentage-point difference in the share of municipalities that had specified tasks for outside personnel, with a standard error of 2.3 percentage points. On the other hand, updating from 2018 to 2025 differs sharply by historical exposure. Over this period, the share of municipalities that had specified tasks for outside personnel increased by 16.6 percentage points more between exposure values of zero and one, with a standard error of 4.1 percentage points. Among municipalities that had not specified tasks for outside personnel in 2018, moving from zero to full exposure is associated with a 13.2-percentage-point increase in adoption by 2025, with a standard error of 3.9 percentage points.

The intermediate annual records show the same updating pattern. For each municipality, we average its six annual changes from the 2018 level over 2019–2024 and use that average as the outcome in equation (5). Full historical exposure is associated with a 8.5-percentage-point larger change. The annual mean appears in Panel B of Table 4; Appendix C.2 reports the supplementary annual inference and sensitivity checks.

Taken together, the repeated records locate the positive 2025 difference in post-2018 updating rather than in a higher 2018 starting level. All municipalities faced the same national effort to strengthen preparedness for receiving outside assistance, but the share specifying tasks for outside personnel increased more where historical exposure was stronger. This pattern is consistent with historical experience shaping municipal responsiveness to a new policy demand.

4.3 Seismic-Retrofit Support

We use seismic-retrofit support to ask whether the association extends to a more physical, capital-intensive form of preparedness. We apply equation (5) to access in 2010 and 2025, its change between those years, and adoption among municipalities without support in 2010.

The comparisons retain prefecture fixed effects and clustering by the largest-contributing alternating domain.

Panel C of Table 4 shows no detectable association for these outcomes. Historical exposure predicts -1.4 percentage points of access in 2010, 0.1 percentage points in 2025, and a 1.5 -percentage-point change between 2010 and 2025. The change estimate has a standard error of 2.8 percentage points. Among municipalities without support in 2010, the conditional adoption estimate is -0.6 percentage points. They show no detectable differential response in this more physical, capital-intensive policy over the observed period, in contrast to the three administrative preparations measured in 2025.

5 Conclusion

This paper links historical exposure to the 1855 Ansei Edo earthquake to political attention and municipal preparedness in contemporary Japan. Municipalities with stronger historical exposure show larger increases in earthquake-related council discussion following new earthquakes, with the clearest evidence among consistently observed councils. The additional discussion concerns earthquake risk, evacuation, and communication. Historical exposure is also associated with concrete preparations for receiving outside assistance and providing shelter supplies. The difference in specifying tasks for outside disaster-response personnel emerges through greater updating after 2018, whereas seismic-retrofit support shows no comparable differential change. Together, these findings connect historical disaster experience with both political attention and practical preparedness more than 160 years later.

Preserving experiences of catastrophe may therefore be understood as an investment in future preparedness. Efforts to transmit such experiences already take diverse forms, including hibakusha testimony and successor programmes in Hiroshima, Holocaust survivors and their descendants speaking in British schools, and tsunami knowledge passed through songs and storytelling on Indonesia's Simeulue Island.⁷ Our findings suggest that the potential value of transmitting experience extends to the public policies communities adopt. When transmitted experience informs collective preparations, its benefits can reach people beyond those who preserve or hear the accounts. These potential social returns strengthen the economic rationale for supporting institutions that record testimony, train successors, and sustain opportunities to share experiences across generations. This study does not identify the transmission process itself, leaving an important question for future research: which ways of preserving experience lead subsequent generations to undertake concrete preparations?

⁷For these initiatives, see Hiroshima Prefectural Government, <https://www.pref.hiroshima.lg.jp/site/hiroshimaforpeace-en/hibakusha-family-member-legacy-successors.html>; Holocaust Educational Trust, <https://www.het.org.uk/education/outreach-programme>; and UNESCO, <https://www.unesco.org/en/articles/when-culture-becomes-first-responder-times-disaster?hub=66941>.

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