

Supplemental Appendix

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

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A Historical Exposure and Design

A.1 Scheduled and Documented Presence

The location audit assesses the schedule as a proxy for direct earthquake experience. It deliberately examined 41 lords for whom useful historical documentation seemed most likely; it is not a random sample. The sources classify 20 cases: seventeen match the scheduled location and three do not. Table S1 also reports cases without decisive evidence.

The audit does not supply a population-wide accuracy estimate. Because verified presence is unavailable for the full roster, the regressions remain scaled to scheduled exposure. Interpreting their coefficients as effects of direct experience requires the identifying assumptions. The measured home-geography, domain-size, and Edo-damage checks appear in Table 1; failure to detect imbalance does not establish equivalence or rule out unmeasured differences.

A.2 Municipal Exposure Construction

We connect the villages recorded in *Kyudaka_agrivillage* v2.01 to modern municipalities through their 2015 agricultural-settlement identifiers. If an identifier appears with multiple five-digit municipality codes, we use the most frequent code. Recorded holders are matched to the fixed roster of 187 alternating domains; 179 have at least one usable village record connected to a modern municipality. Nonalternating domains, nondaimyo holders, and unmatched holders do not enter the exposure denominator.

Each usable village record contributes total weight one, divided equally among its recorded holders. Let $\mathcal{V}_m$ be the records assigned to municipality m and $\mathcal{H}_v$ the holders of village v . Domain j receives weight

$$W_{mj} = \sum_{v \in \mathcal{V}_m} \frac{\mathbf{1}\{j \in \mathcal{H}_v\}}{|\mathcal{H}_v|}.$$

Summing these weights over the Edo-scheduled and home-scheduled domains gives V_m^E and V_m^H in equation (1). Thus $E_m = V_m^E / (V_m^E + V_m^H)$ counts historical record shares, not land area or population. It summarizes many-to-many historical connections rather than continuous administrative descent.

The mapping yields positive alternating-domain weight for 1,127 municipalities. Requiring municipality codes to link to both 2010 and 2025 in the seismic-retrofit-support panel leaves the common policy sample of 1,104 municipalities in 44 prefectures. Table S3 reports the domain, municipality, and village counts. Figure S1 displays the geocoded village links:

Table S1: Audit of Scheduled and Documented Location

Audit category	Domains
Domains selected for document search	41
Domains with classifiable realized location	20
Classifiable locations agreeing with the schedule	17
Classifiable locations disagreeing with the schedule	3
Domains with no usable location evidence	17
Domains with uncertain or conflicting evidence	4

Documented disagreement IDs: H379, H395, H408.

Notes: The audit sample is purposive and was selected to maximize the chance of finding usable location evidence. Agreement and disagreement are defined only among cases for which the available sources permit a location classification. The audit evaluates the schedule as a proxy for direct earthquake experience; its match rate is not a population-wide accuracy estimate.

color identifies the scheduled group of the recorded holder, not the village’s location during the earthquake. Mixed villages have eligible holder links in both groups; villages without an eligible alternating holder are shown for context.

Table S2 summarizes the distribution of exposure in the common policy sample.

Table S2: Descriptive Statistics for Historical Exposure

Variable	N	Mean	SD	Min	Max
Historical exposure share (E_m)	1,104	0.509	0.465	0.000	1.000

Notes: Statistics weight each of the 1,104 municipalities in the common policy sample equally. Historical exposure E_m is the share of included village-record weight linked to Edo-scheduled domains. SD is the sample standard deviation; there are no missing observations in this sample.

Table S3: From the Fixed Domain Roster to the Municipal Sample

Sample component	Count
<i>Panel A. Municipal analysis</i>	
Alternating domains in the fixed schedule sample	187
Home-due domains	94
Edo-due domains	93
Alternating domains with mapped village mass	179
Modern municipalities in the analytic sample	1,104
Modern prefectures	44
Modal-holder clusters	163
<i>Panel B. Historical village map</i>	
Village records in the holder crosswalk	66,581
Villages with plotted coordinates	66,517
Villages with any alternating-domain holder	38,730
Home-due holders only, plotted	19,667
Edo-due holders only, plotted	18,910
Both schedule groups, plotted	126
No alternating-domain holder, plotted	27,814

Notes: The fixed roster contains the alternating domains classified by their 1855 attendance schedule. A domain enters the municipal mapping when at least one eligible village record can be assigned to it. Municipal exposure is observed when the municipality has positive record weight from at least one alternating domain. “Village mass” in the generated table means this summed record weight; it is neither land area nor population. The modal-holder cluster count refers to the policy-regression assignment described in Appendix C.1.

Table S4: Historical Domain Weights for Kurashiki

Recorded domain holder	Record weight	Share of included weight	Scheduled location
Okayama	45.5	51.4%	Home
Ikusaka (?)	16.5	18.6%	Home
Okada	12.0	13.6%	Edo
Kamogata (?)	10.0	11.3%	Edo
Niimi	2.0	2.3%	Edo
Matsuyama (Okayama)	1.5	1.7%	Edo
Niwase	1.0	1.1%	Home
Total	88.5	100.0%	

Notes: Record weight is the sum of the holder’s shares of usable village records assigned to present-day Kurashiki. A village with n recorded holders contributes $1/n$ to each holder. The question marks in the source labels for Ikusaka and Kamogata indicate the underlying transcription’s qualified domain attribution. Scheduled location refers to the lord and traveling retinue’s location under the pre-earthquake attendance schedule, not verified actual location for every domain.

The example of Kurashiki. Kurashiki’s included village records link it to seven alternating domains. Table S4 gives their record weights and scheduled locations. Four Edo-scheduled domains contribute 25.5 units and three home-scheduled domains contribute 63.0, so

$$E_{\text{Kurashiki}} = \frac{25.5}{63.0 + 25.5} = \frac{25.5}{88.5} \simeq 0.288.$$

Thus 28.8 percent of its included record weight is linked to Edo-scheduled domains. All seven holders enter the exposure measure. The separate use of the largest contributing holder for policy-regression clustering is explained in Appendix C.1.

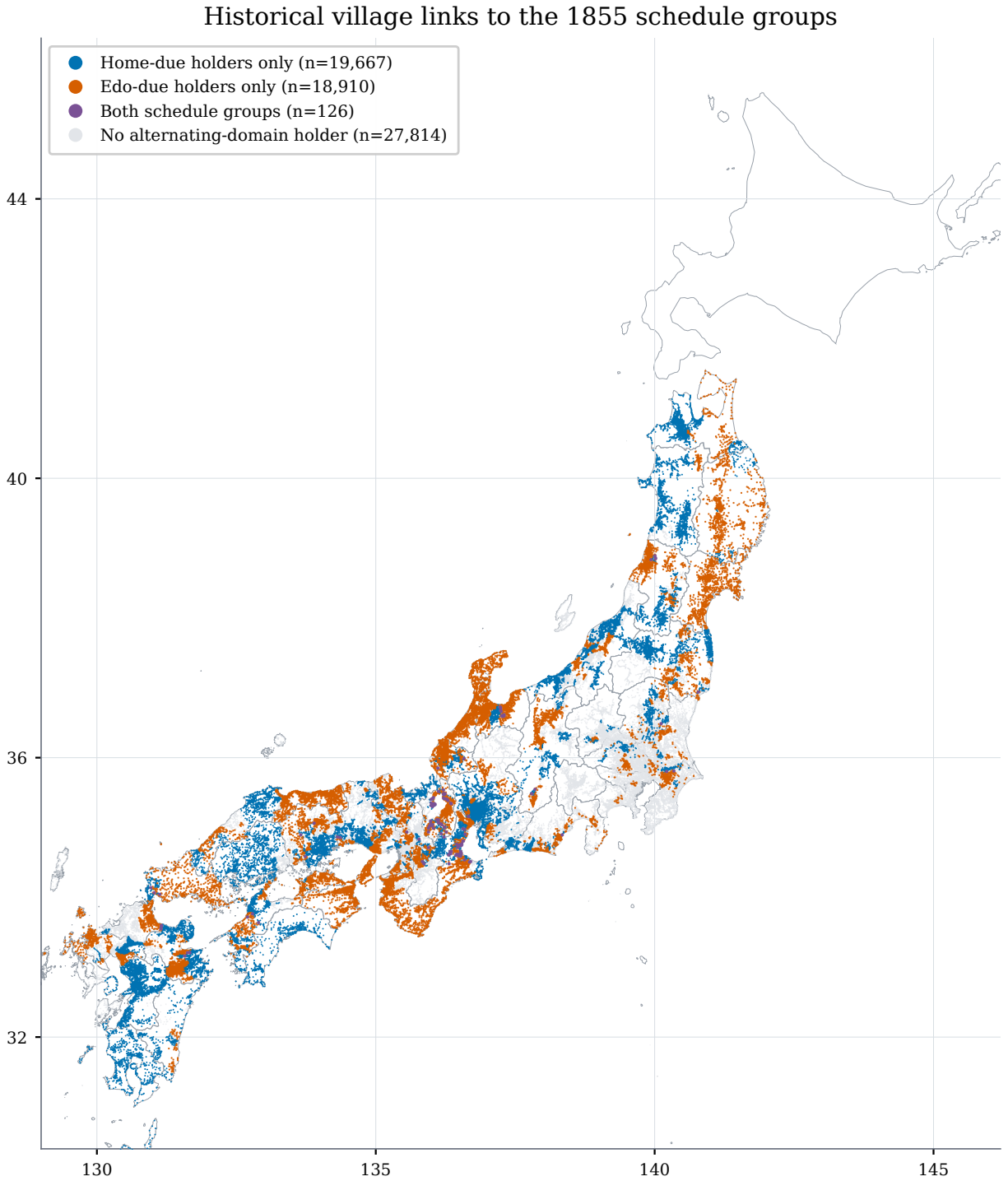


Figure S1: Historical Villages Linked to Lords Due in Edo or at Home

Notes: Points are geocoded historical villages in the holdings data. Edo-due and home-due refer to the 1855 schedule of each village's recorded eligible holder. Mixed villages have record shares in both alternating groups. A village with several recorded holders contributes equal shares to those holders. Villages without an eligible alternating-domain holder are displayed for context but do not enter the exposure denominator. The schedule governed where lords and traveling retainues were due to be, not where the villages were located.

B Council Discussion

B.1 Council Records and Topic Classification

The council analysis distinguishes the materials downloaded from the remarks that can be used to construct an outcome. The collection sought general-question records for 1,104 municipalities over January 2018–December 2023 and downloaded 98,421 documents from 831 municipalities. Of these, 30,959 documents contribute at least one retained general-question turn. The usable corpus contains 2,042,005 councillor turns, grouped into 146,168 question blocks and 144,817 questioner-month observations. Eligibility requires a resolved meeting date, a councillor identity, and a general-question classification. The 205-municipality sample requires a usable monthly denominator at each of the 24 designated regular-session months; this condition does not establish complete retrieval of every councillor’s remarks. Neither sample is a census of all discussion in the target municipalities.

Table S5 reports speaker shares, discussion topics, and observation coverage in the two council samples.

Construction of discussion topics. The nine categories in Table S6 describe distinct aspects of earthquake discussion. The classification guide defines their content, inclusion and exclusion rules, and examples; it permits multiple labels when a passage addresses more than one subject.

Classification is local to passages because a councillor’s remarks can contain several unrelated subjects. We restrict the automated search to the 31,071 existing question blocks that satisfy the original literal earthquake-keyword rule. Each block groups a councillor’s eligible remarks within a dated source record; its constituent speaking turns are split into sentences. Fixed vocabulary and exclusion rules detect topic expressions and connect them to an earthquake reference or an earthquake-specific term, such as seismic resistance or liquefaction. The connection must occur in the same sentence or within two sentences in the same speaking turn, with a maximum distance of 500 normalized characters. Context does not extend through detected transitions to a new question or propagate recursively from another context-linked sentence. Separate rules screen condolences, institutional names, and incidental fiscal or project lists. A stricter version requires the earthquake and topic expressions to occur in the same sentence.

We developed and checked the rules through AI reading of source passages, separately from estimation of the exposure coefficients. An initial set of 30 existing audit blocks informed the codebook and its boundaries. We then selected one target sentence from each of 80 additional blocks and fixed a split of 40 calibration sentences and 40 held-out audit

Table S5: Descriptive Statistics for Council Discussion of Earthquakes

Variable	Full sample		Regular-month sample	
	Mean	SD	Mean	SD
<i>Panel A. Speaker shares (%)</i>				
Any earthquake mention	21.39	20.43	21.43	20.62
At least one of the nine topics	15.38	16.61	15.38	16.50
General disaster preparedness	1.14	3.74	1.12	3.63
Risk and damage recognition	5.91	9.85	5.93	9.84
Building and infrastructure safety	4.83	8.57	4.88	8.42
Evacuation and shelter life	5.90	9.22	5.94	9.11
Supplies, rescue, and disaster medicine	2.65	6.06	2.65	5.99
Administrative plans and implementation	2.58	5.84	2.46	5.58
Preparedness, drills, and communication	4.95	8.34	4.98	8.30
Recovery and reconstruction	4.59	10.45	4.49	10.67
Lessons from past disasters	1.59	4.66	1.50	4.32
<i>Panel B. Speakers per municipality-month</i>				
General-question speakers	10.86	5.36	10.72	4.85
Speakers mentioning earthquakes	2.32	2.43	2.33	2.53
<i>Panel C. Sample coverage</i>				
Municipality-months		13,341		4,920
Municipalities		604		205
Observed councillor-months		144,817		52,736
Observed months per municipality	22.09	5.85	24.00	0.00

Notes: Panels A and B weight municipality-months equally; SD denotes the sample standard deviation. Every share uses all observed general-question speakers in the municipality and month as its denominator. Speakers are counted once per topic and month. Topics overlap; the “at least one” row is their union. Panel C reports coverage counts and the mean and SD of observed months across unique municipalities. The regular-month sample contains municipalities observed in all 24 March, June, September, and December dates of 2018–2023. Missing months are not assigned zeros; regular-month coverage does not establish complete retrieval of all meetings or speakers. Topic detection uses fixed vocabulary and bounded context within the original earthquake-keyword-positive remarks.

Table S6: Topics of Earthquake Discussion

Topic	Content covered
General disaster preparedness	Preparedness needs, priorities, or budgets without a specific domain.
Risk and damage recognition	Expected shaking or inundation, vulnerability, casualties, and observed damage.
Building and infrastructure safety	Seismic safety, lifelines, protective structures, and safer facility locations.
Evacuation and shelter life	Evacuation routes and assistance, shelter access, management, and living conditions.
Supplies, rescue, and disaster medicine	Stocks and delivery of essential supplies, rescue, emergency medicine, and disaster health.
Administrative plans and implementation	Disaster plans, continuity of operations, staff responsibilities, and coordination of outside assistance.
Preparedness, drills, and communication	Training, education, community preparedness, warnings, and communication of risk information.
Recovery and reconstruction	Housing and livelihood support, reconstruction, recovery finance, and disaster debris.
Lessons from past disasters	Lessons drawn from past earthquakes and preservation or transmission of disaster experience.

Notes: The categories overlap: a remark may discuss more than one subject. A past earthquake's name, an anniversary, or condolences alone do not establish discussion of lessons or transmission. Appendix B.1 details the classification rules and treatment of unmatched text.

sentences before reading them. Selection covered all five source formats and deliberately included detected topics, possible omissions, difficult context connections, and a small random component. Reviewers interpreted each target using surrounding text without seeing its predicted labels, historical exposure, or regression results. Calibration readings supported limited rule revisions; the revised rules were frozen before the held-out readings and topic regressions. In the held-out set, the complete measured topic set matched the AI reference judgment for 25 of the 39 sentences with a determinate judgment; one remained uncertain. These small, stratified AI audits document errors and ambiguities rather than establish accuracy against human-coded ground truth or population-wide precision and recall. The measures may miss unfamiliar expressions or implicit connections and may attach nearby discussion to the wrong earthquake context.

We retain earthquake-mentioning councillors for whom none of the nine topics is detected as a diagnostic residual. This residual is neither a measured “other substantive topic” category nor a finding that their remarks lack substantive content. Together with the share discussing at least one detected topic, it accounts for the original earthquake-mention share. The separate “other substantive topic” category in the reading codebook (T99) is not an estimated outcome.

B.2 Event Calendar, Controls, and Inference

The nine event groups, identified by the date and epicentral area of their largest earthquake, are: 18 June 2018 (northern Osaka Prefecture); 6 September 2018 (central and eastern Iburi, Hokkaido); 3 January 2019 (the Kumamoto region of Kumamoto Prefecture); 21 February 2019 (central and eastern Iburi, Hokkaido); 18 June 2019 (off Yamagata Prefecture); 13 February 2021 (off Fukushima Prefecture); 16 March 2022 (off Fukushima Prefecture); 19 June 2022 (the Noto region of Ishikawa Prefecture); and 5 May 2023 (off the Noto Peninsula). The June 2019, March 2022, and May 2023 groups include nearby earthquakes whose observed seismic intensities JMA reports jointly.

These nine national event groups define mutually exclusive bins for the event month, months one to three, and months four to six. The reference category has no qualifying event in the current or previous six months, and a new event resets the clock. Observations in a window therefore do not form separate before-and-after comparisons for each physical earthquake. In the regular-month sample, only six distinct year-months fall in the one-to-three-month category and four in the four-to-six-month category. Many municipality-month observations do not constitute many independent national shocks.

The eight fixed covariates are log alternating-domain village-record weight, the share of

mapped weight assigned to alternating domains, the share assigned to nondomain holders, the share assigned to always-Edo domains, castle-town status, log mean municipal revenue over the available years of 2011–2015, and two $\log(1 + n)$ counts of recorded local strong-shaking event groups in 1919–2010 and 2011–2017. They are standardized across the unique 604 municipalities and kept at those values in all subsamples. They enter the controlled baseline regressions in levels and the response regressions interacted with all three national event windows. The local-shaking controls in the response regressions indicate a matched record of intensity 6-lower or above in the current month, preceding one to three months, and preceding four to six months. Those windows can overlap, unlike the mutually exclusive national-event bins. Missing station-to-municipality matches and the limited strong-shaking catalogue mean that an indicator of zero does not establish an absence of local shaking or damage.

All council regressions weight municipality-months equally. Topic regressions retain the same event windows, controls, and exposure adjustments as the aggregate response regressions, with 2018 and March omitted from the exposure-by-year and exposure-by-calendar-month interactions. Two-way inference clusters by prefecture and year-month, uses CR1 adjustments, and takes the smaller cluster count minus one as the degrees of freedom. A negative estimated two-way variance leaves its standard error, confidence interval, and p -value undefined; it is not replaced by zero. Appendix B.3 compares alternative clustering conventions.

The baseline analysis uses this same reference category. It retains 597 of the 604 municipalities, with 5,028 observations across 25 year-months; the other municipalities have no usable observations in the reference category. Restricting the existing regular-month sample to the same category retains all 205 municipalities, with 1,845 observations across 9 year-months. Both the controlled and uncontrolled regressions use exactly the same observations within each sample. The two-way inference consequently uses 24 and 8 degrees of freedom, respectively. The baseline extension was specified after the earthquake-response results had been examined; all four level specifications are reported, and the exercise is exploratory. Baseline months are not an untreated period preceding the historical exposure.

Table S7 reports these four baseline specifications, including the controlled columns summarized in the main text.

B.3 Additional Council Results

Calendar and municipality composition. Table S8 separates the choice of observation months from the municipalities represented. It estimates the full roster, the 205-municipality

Table S7: Historical exposure and baseline earthquake mentions

	Main comparison		Regular-month sample	
	(1)	(2)	(3)	(4)
Historical exposure E	0.00 (1.52)	0.37 (1.27)	-4.62 (3.29)	-3.01 (1.94)
Municipal attributes	No	Yes	No	Yes
Prefecture fixed effects	Yes	Yes	Yes	Yes
Year-month fixed effects	Yes	Yes	Yes	Yes
Observations	5,028	5,028	1,845	1,845
Municipalities	597	597	205	205
Prefectures	44	44	43	43
Year-months	25	25	9	9
Mean outcome (%)	18.51	18.51	18.47	18.47

Notes: The outcome is the percentage of observed distinct general-question speakers whose recorded remarks mention earthquake or earthquake disaster. Baseline months have no qualifying national earthquake in the current or previous six months; qualifying events reach JMA intensity 6-lower somewhere in Japan. Columns (1)–(2) use 597 of the original 604 municipalities; seven have no observed baseline months. Columns (3)–(4) use baseline observations within the fixed 205-municipality regular-month sample. Coefficients and standard errors are percentage points for an exposure difference from zero to one. All observations receive equal municipality-month weights. Parentheses report two-way CR1 standard errors clustered by prefecture and year-month, counting all identified fixed effects in the finite-sample adjustment; two-sided t tests use the smaller cluster count minus one degrees of freedom. The regular-month baseline sample has only nine year-months. Controlled columns add the levels of the eight existing historical, fiscal, and prior-shaking attributes. Municipality fixed effects are not included. These are exploratory conditional level associations, not estimates of the causal effect of historical exposure or of an earthquake.

regular-month roster, and the remaining municipalities, each using either all observed months or only March, June, September, and December in 2018–2023. Only the 205-municipality panel is complete over the selected 24 months. Missing months are never filled with zeros, and the municipal attributes retain their original standardization. Holding the roster fixed changes the calendar; holding the calendar fixed compares municipality groups. These separate estimates do not jointly test equality between groups or identify an effect of selection into the regular-month sample.

Table S8: Calendar and municipality composition: months 1–3

Topic	Full sample		Regular-month roster		Remaining municipalities	
	All months	Same 24 months	All months	Same 24 months	All months	Same 24 months
General disaster preparedness	0.477 (0.333)	0.355 (0.260)	1.153 (0.290)	1.191 (0.297)	0.037 (0.457)	-0.271 (0.330)
Risk and damage recognition	0.386 —	0.154 —	1.914 (0.712)	2.082 (0.611)	-0.663 (0.427)	-1.399 (0.273)
Building and infrastructure safety	-0.378 (0.408)	-0.285 (0.406)	-0.152 (0.570)	0.021 (0.635)	-0.654 (0.524)	-0.611 (0.499)
Evacuation and shelter life	0.439 —	0.662 —	1.728 (0.554)	1.779 (0.533)	-0.325 (0.258)	-0.068 (0.119)
Supplies, rescue, and disaster medicine	0.542 (0.291)	0.569 (0.274)	1.153 (0.747)	1.317 (0.718)	0.102 (0.041)	-0.115 —
Administrative plans and implementation	-0.130 (0.142)	-0.095 —	-0.128 (0.353)	-0.072 (0.363)	-0.025 (0.276)	-0.044 (0.178)
Preparedness, drills, and communication	0.315 (0.360)	0.418 (0.341)	1.546 (0.592)	1.700 (0.607)	-0.292 (0.446)	-0.409 (0.275)
Recovery and reconstruction	0.230 —	0.415 —	1.006 (0.295)	1.124 —	-0.182 —	-0.134 —
Lessons from past disasters	0.156 (0.212)	0.161 (0.233)	0.101 (0.344)	0.124 (0.345)	0.113 (0.253)	0.147 (0.304)
Any earthquake mention	1.722 (0.979)	2.392 (1.024)	4.151 (1.334)	4.601 (1.209)	0.345 (0.995)	0.889 (1.092)
Municipalities	604	603	205	205	399	398
Municipality-months	13,341	12,300	5,004	4,920	8,337	7,380

Notes: All columns show months 1–3 with national year-month fixed effects; measures, controls, and percentage-point units are as in Table 3. Parentheses report two-way CR1 standard errors clustered by prefecture and by year-month. 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. The regular-month roster contains the 205 municipalities observed in all 24 regular council months; its all-month column includes additional observed months. The remaining roster is its complement. Calendar restriction leaves 603 full-sample and 398 remaining-roster municipalities; missing months are not imputed. These descriptive comparisons neither identify a coverage effect nor formally test roster differences. Original earthquake mentions are shown separately from the overlapping topic measures.

The larger short-run point estimates among the 205 municipalities remain when the calendar is held fixed. Using the common regular-session months, the full roster gives coefficients of 0.662 points for evacuation and shelter life and 0.418 for residents' preparedness, compared with 1.779 and 1.700 in the 205-municipality sample. Among the remaining municipalities, the corresponding estimates are -0.068 and -0.409. Conversely, retaining the 205 municipalities but using all observed months gives 1.728 and 1.546. Calendar coverage alone therefore does not account for the larger point estimates in the consistently observed municipalities.

Geographical comparisons. We also distinguish exposure differences within prefectures from differences in average exposure across prefectures. Define B_p as the unweighted mean of E_m among municipalities in prefecture p within the full sample of 604 municipalities, and let $W_m = E_m - B_{p(m)}$. The mean is fixed across months and is retained when restricting the sample to the 205 municipalities observed in all 24 regular-session months. We estimate

$$Y_{mt} = \alpha_m + \lambda_t + \sum_h (\beta_h^B B_{p(m)} + \beta_h^W W_m) D_t^h + \sum_h \theta'_h Z_m D_t^h + \rho' L_{mt} \\ + B_{p(m)} a_{y(t)}^B + W_m a_{y(t)}^W + B_{p(m)} c_{r(t)}^B + W_m c_{r(t)}^W + \varepsilon_{mt}. \quad (1)$$

This specification allows both the event response and the year and calendar-month adjustments to differ for B_p and W_m . The outcome remains a municipal-council measure in both cases. Prefecture-by-year-month fixed effects would absorb the prefecture-mean interactions, so this decomposition uses municipality and year-month fixed effects; the within-prefecture comparison with prefecture-by-year-month fixed effects is retained as a sensitivity analysis.

The decomposition shows that the clearer short-run association in the consistently observed sample includes comparisons within prefectures. Table S9 reports the estimates. In the full sample, columns (1) and (2) associate a one-unit difference in prefecture-mean exposure with increases of 2.62 and 3.19 percentage points in the two subsequent windows, compared with 0.99 and -0.55 for within-prefecture exposure. The difference between the prefecture-mean and within-prefecture coefficients is statistically distinguishable in months four to six ($p = 0.018$), but not in months one to three ($p = 0.479$). In the sample observed in all regular-session months, column (3) shows positive short-run coefficients for both components: 4.32 for prefecture-mean exposure and 4.82 for within-prefecture exposure. These coefficients are not statistically distinguishable ($p = 0.851$). The short-run pattern in that sample therefore extends to differences among municipalities in the same prefecture.

Table S10 replaces the year-month effects with prefecture-by-year-month fixed effects. These absorb the prefecture-mean interactions and all common changes within each prefecture and month. This narrower comparison gives an aggregate-month estimate of 0.83

Table S9: Geographical decomposition of earthquake-mention responses

	604 municipalities		205 regular-month municipalities	
	(1)	(2)	(3)	(4)
Months after event	1–3	4–6	1–3	4–6
Prefecture mean B	2.62 (1.66)	3.19*** (0.98)	4.32* (2.51)	4.53* (2.22)
Within-prefecture W	0.99 (1.38)	-0.55 (0.97)	4.82*** (0.85)	1.01 (1.44)
$B = W$: p -value	0.479	0.018	0.851	0.272
Controls	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes
Year and seasonal exposure terms	B, W	B, W	B, W	B, W
Observations	13,341	13,341	4,920	4,920

Notes: The outcome, samples, controls, event windows, and equal municipality-month weights follow Table 2. B is the fixed unweighted mean of municipal exposure among the 604 municipalities in each prefecture, and $W = E - B$; these means are retained in the 205-municipality sample. Two regressions each supply two post-event columns and include event-month terms. Event responses and year and calendar-month adjustments are estimated separately for B and W . Coefficients and standard errors are percentage points for a unit exposure difference. Parentheses report two-way CR1 standard errors clustered by prefecture and year-month; stars denote unadjusted t -test p -values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The $B = W$ row tests equality using the joint covariance. Prefecture-by-year-month fixed effects would absorb the B interactions; Table S10 reports that narrower comparison. These comparisons are exploratory.

Table S10: Earthquake mentions with prefecture-by-year-month fixed effects

Months after event	Event month	1–3	4–6
Within-prefecture exposure W	-2.71 (1.93)	0.83 (1.48)	-0.45 (1.04)
Two-way p -value	0.167	0.577	0.669
Prefecture-only p -value	0.208	0.618	0.757
Municipalities	604	604	604
Observations	13,341	13,341	13,341
Municipality FE	Yes	Yes	Yes
Prefecture-by-year-month FE	Yes	Yes	Yes
Controls and exposure time terms	Yes	Yes	Yes

Notes: The specification uses the same literal-mention outcome, 604-municipality sample, controls, equal municipality-month weights, and event windows as Table 2. Prefecture-by-year-month fixed effects absorb all prefecture-level monthly shocks and the prefecture-mean exposure interactions. The reported coefficients are on W , with W -specific year and calendar-month adjustments. Coefficients and standard errors are percentage points per unit exposure. Parentheses report CR1 two-way standard errors clustered by prefecture and year-month. Both reported p -values are unadjusted t tests; the second row of p -values clusters only by prefecture. The estimate for months 1–3 is 0.83 points (two-way $p = 0.577$).

percentage points for months one to three in the full sample, which is not statistically distinguishable from zero. The table retains the event-month estimate and both two-way and prefecture-only inference.

For individual topics, the corresponding comparison is

$$\begin{aligned}
Y_{mt}^{(k)} = & \alpha_m^{(k)} + \lambda_{p(m)t}^{(k)} + \sum_h \beta_h^{W,(k)} W_m D_t^h + \sum_h \theta_h^{(k)'} Z_m D_t^h + \rho^{(k)'} L_{mt} \\
& + W_m a_{y(t)}^{(k)} + W_m c_{r(t)}^{(k)} + \varepsilon_{mt}^{(k)}.
\end{aligned} \tag{2}$$

Here $W_m = E_m - B_{p(m)}$ uses the same fixed prefecture means defined above. All eight attribute-by-window controls and the three local-shaking indicators are retained, and the year and calendar-month interactions use W_m . Because prefecture-by-year-month fixed effects absorb $B_{p(m)}$ interacted with any event window, year, or calendar month, replacing W_m by E_m throughout gives the same event-response coefficients. Tables S11 and S12 retain the full and 205-municipality rosters under both calendar choices, using the same observations as their pooled counterparts. The changed comparison absorbs responses shared across a prefecture; subtracting it from the pooled estimate does not identify a prefectural transmission effect.

Comparisons within the same prefecture and month also retain positive short-run point estimates in the 205-municipality sample. The evacuation coefficient becomes 2.924 points, although its two-way clustered variance estimate is negative and a confidence interval is

Table S11: Within-prefecture topic responses: months 1–3

Topic	Full sample		Regular-month roster	
	All months	Same 24 months	All months	Same 24 months
General disaster preparedness	0.101 (0.181)	0.053 (0.244)	1.124 (0.346)	1.083 (0.382)
Risk and damage recognition	-0.210 (0.623)	-0.293 (0.636)	1.471 (1.842)	1.652 (1.815)
Building and infrastructure safety	-0.607 (0.595)	-0.289 (0.687)	-0.460 —	-0.301 —
Evacuation and shelter life	0.081 (0.367)	0.289 (0.378)	2.904 —	2.924 —
Supplies, rescue, and disaster medicine	0.694 (0.436)	0.769 (0.445)	1.919 (1.614)	2.039 (1.641)
Administrative plans and implementation	-0.250 —	-0.393 —	0.319 —	0.220 —
Preparedness, drills, and communication	0.346 (0.572)	0.345 (0.479)	2.277 (1.401)	2.302 (1.414)
Recovery and reconstruction	0.312 —	0.697 —	1.332 (0.352)	1.400 (0.345)
Lessons from past disasters	-0.015 (0.235)	-0.125 (0.179)	-0.454 (0.469)	-0.502 (0.437)
Any earthquake mention	0.833 (1.481)	1.149 (1.492)	3.435 (2.266)	3.791 (1.974)
Municipalities	604	603	205	205
Municipality-months	13,341	12,300	5,004	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. No significance stars or multiple-testing adjustment are used. These specifications replace national year-month fixed effects with prefecture-by-year-month fixed effects, using the same observations as their calendar/roster counterparts. Exposure is centered on the fixed original prefecture mean, and the remaining controls are retained. They identify differences within the same prefecture and month and absorb responses shared within a prefecture; pooled-minus-within differences are not a causal decomposition. Missing months are not imputed.

Table S12: Within-prefecture topic responses: months 4–6

Topic	Full sample		Regular-month roster	
	All months	Same 24 months	All months	Same 24 months
General disaster preparedness	-0.289 (0.185)	-0.389 (0.235)	-0.298 (0.509)	-0.327 (0.529)
Risk and damage recognition	-0.918 (0.735)	-1.102 (0.671)	0.095 (1.156)	0.223 (1.133)
Building and infrastructure safety	-0.418 (0.413)	-0.330 (0.457)	-0.822 —	-0.684 —
Evacuation and shelter life	-0.922 —	-1.089 —	0.771 (1.239)	0.797 (1.226)
Supplies, rescue, and disaster medicine	0.406 (0.384)	0.369 (0.355)	1.965 (1.065)	2.064 (1.101)
Administrative plans and implementation	-0.268 (0.164)	-0.349 (0.096)	0.460 (0.951)	0.397 (0.947)
Preparedness, drills, and communication	0.130 (0.440)	-0.170 (0.347)	1.077 (1.724)	1.103 (1.734)
Recovery and reconstruction	0.096 —	0.344 —	0.688 —	0.715 —
Lessons from past disasters	-0.226 (0.272)	-0.351 (0.263)	-1.437 (0.896)	-1.469 (0.897)
Any earthquake mention	-0.449 (1.042)	-0.290 (1.129)	0.679 (1.709)	0.918 (1.549)
Municipalities	604	603	205	205
Municipality-months	13,341	12,300	5,004	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. No significance stars or multiple-testing adjustment are used. These specifications replace national year-month fixed effects with prefecture-by-year-month fixed effects, using the same observations as their calendar/roster counterparts. Exposure is centered on the fixed original prefecture mean, and the remaining controls are retained. They identify differences within the same prefecture and month and absorb responses shared within a prefecture; pooled-minus-within differences are not a causal decomposition. Missing months are not imputed.

undefined. The coefficient for residents' preparedness is 2.302, with an interval of [-0.622, 5.226], and that for risk and damage recognition is 1.652, with an interval of [-2.102, 5.407]. In the full sample, the corresponding evacuation and residents' preparedness coefficients are 0.081 and 0.346. Tables S11 and S12 report all topics. The positive regular-month point estimates are therefore not eliminated by removing prefecture-wide monthly changes, but their uncertainty remains substantial.

A stricter topic definition. Tables S13 and S14 require the earthquake reference and topic to occur in the same sentence. The outcomes retain the same questioner-month denominator and differ only in the accepted textual evidence. Both fixed-effect specifications are reported for the full and regular-month samples, with every category retained, including small and imprecise estimates. The event-month interaction remains in each regression; the tables display the subsequent one-to-three- and four-to-six-month responses.

Requiring the earthquake reference and the topic to occur in the same sentence provides a further check on the measured content of discussion. In the regular-month sample with prefecture-by-year-month fixed effects, the short-run evacuation coefficient is 2.003 points, with a two-way clustered interval of [0.998, 3.008]. In the full sample, the same-sentence supplies, rescue, and disaster-medicine coefficient is 0.819, with an interval of [0.337, 1.302]. Prefecture-only clustering gives p -values of 0.252 and 0.152, respectively. The tables report the complete classification comparisons; Table S15 reports the alternative inference.

Table S13: Same-sentence topic responses: months 1–3

Topic	National year-month FE		Prefecture-by-year-month FE	
	Full sample	Regular-month	Full sample	Regular-month
General disaster preparedness	0.262 (0.223)	0.582 (0.248)	-0.004 (0.140)	0.599 (0.155)
Risk and damage recognition	-0.058 —	0.734 (0.400)	-0.390 (0.332)	0.562 (0.738)
Building and infrastructure safety	-0.281 (0.373)	0.441 (0.614)	-0.472 (0.441)	0.009 —
Evacuation and shelter life	0.006 (0.398)	1.375 (0.468)	-0.421 (0.636)	2.003 (0.486)
Supplies, rescue, and disaster medicine	0.783 (0.223)	0.760 (0.371)	0.819 (0.239)	0.956 (1.016)
Administrative plans and implementation	-0.314 (0.137)	0.088 (0.131)	-0.207 (0.053)	0.519 —
Preparedness, drills, and communication	0.329 (0.203)	1.110 (0.199)	0.609 (0.416)	1.839 —
Recovery and reconstruction	-0.001 —	0.329 —	-0.162 (0.215)	0.439 (0.575)
Lessons from past disasters	0.311 (0.218)	0.173 (0.375)	0.229 (0.244)	-0.279 (0.600)
Municipalities	604	205	604	205
Municipality-months	13,341	4,920	13,341	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. No significance stars or multiple-testing adjustment are used. This stricter measure requires the earthquake expression and topic to occur in the same sentence. The main contextual measure is reported in Table 3 and the corresponding within-prefecture tables. The full sample uses all observed months; the regular-month sample contains 205 municipalities in the same 24 regular council months. Sample membership, controls, weights, and event definitions are unchanged. A stricter topic rule is not a test of perfect classification.

Table S14: Same-sentence topic responses: months 4–6

Topic	National year-month FE		Prefecture-by-year-month FE	
	Full sample	Regular-month	Full sample	Regular-month
General disaster preparedness	0.094 (0.150)	-0.013 (0.171)	-0.139 (0.093)	-0.145 (0.194)
Risk and damage recognition	-0.462 (0.345)	-0.113 (0.648)	-0.858 (0.574)	-0.171 (0.775)
Building and infrastructure safety	-0.275 (0.340)	-0.204 (0.595)	-0.269 (0.412)	-0.583 (0.607)
Evacuation and shelter life	-0.119 (0.333)	0.620 (0.943)	-0.625 (0.549)	2.113 (1.266)
Supplies, rescue, and disaster medicine	0.119 (0.171)	0.292 (0.274)	-0.004 (0.326)	0.528 (0.946)
Administrative plans and implementation	-0.139 (0.076)	0.029 (0.327)	-0.025 —	0.368 (0.426)
Preparedness, drills, and communication	0.196 (0.252)	0.576 (0.476)	0.932 (0.418)	1.620 —
Recovery and reconstruction	0.371 (0.278)	0.478 —	-0.022 (0.154)	0.021 —
Lessons from past disasters	0.238 (0.316)	-0.438 (0.551)	-0.203 (0.269)	-1.415 (0.935)
Municipalities	604	205	604	205
Municipality-months	13,341	4,920	13,341	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. No significance stars or multiple-testing adjustment are used. This stricter measure requires the earthquake expression and topic to occur in the same sentence. The main contextual measure is reported in Table 3 and the corresponding within-prefecture tables. The full sample uses all observed months; the regular-month sample contains 205 municipalities in the same 24 regular council months. Sample membership, controls, weights, and event definitions are unchanged. A stricter topic rule is not a test of perfect classification.

Table S15: Inference for topic responses: months 1–3

Topic	National year-month FE				Prefecture-by-year-month FE			
	Full		Regular-month		Full		Regular-month	
	Two-way	Prefecture	Two-way	Prefecture	Two-way	Prefecture	Two-way	Prefecture
<i>A. Main contextual rule</i>								
General disaster preparedness	0.160	0.156	< 0.001	0.017	0.582	0.793	0.009	0.116
Risk and damage recognition	–	0.438	0.002	0.054	0.738	0.759	0.372	0.501
Building and infrastructure safety	0.359	0.531	0.974	0.980	0.314	0.466	–	0.793
Evacuation and shelter life	–	0.426	0.003	0.122	0.827	0.916	–	0.106
Supplies, rescue, and disaster medicine	0.070	0.224	0.080	0.140	0.119	0.334	0.226	0.396
Administrative plans and implementation	0.367	0.731	0.843	0.899	–	0.621	–	0.813
Preparedness, drills, and communication	0.387	0.547	0.010	0.098	0.548	0.695	0.117	0.320
Recovery and reconstruction	–	0.630	–	0.192	–	0.682	< 0.001	0.438
Lessons from past disasters	0.464	0.525	0.723	0.787	0.949	0.962	0.262	0.498
<i>B. Same-sentence rule</i>								
General disaster preparedness	0.245	0.257	0.028	0.062	0.975	0.986	< 0.001	0.101
Risk and damage recognition	–	0.912	0.080	0.400	0.246	0.549	0.453	0.745
Building and infrastructure safety	0.455	0.613	0.479	0.545	0.290	0.530	–	0.993
Evacuation and shelter life	0.988	0.991	0.007	0.129	0.511	0.595	< 0.001	0.252
Supplies, rescue, and disaster medicine	0.001	0.021	0.052	0.237	0.001	0.152	0.357	0.625
Administrative plans and implementation	0.026	0.226	0.508	0.819	< 0.001	0.610	–	0.491
Preparedness, drills, and communication	0.113	0.403	< 0.001	0.075	0.151	0.359	–	0.253
Recovery and reconstruction	–	0.999	–	0.589	0.455	0.791	0.453	0.724
Lessons from past disasters	0.160	0.232	0.649	0.681	0.352	0.419	0.647	0.703

Notes: All entries are unadjusted p -values for the months 1–3 coefficient, with no significance-based selection. Two-way uses separate prefecture and year-month CR1 clusters; Prefecture uses prefecture-only CR1 clustering. Full denotes 604 municipalities and all observed months; Regular-month denotes 205 municipalities in 24 regular council months. Dashes preserve undefined two-way inference when estimated variance is negative. Each pair uses the same point estimate. Inference can differ across covariance assumptions; the smaller p -value is not selected as preferred evidence. Full results remain in the frozen snapshots.

Inference and interpretation. Table S15 compares two-way clustering by prefecture and year-month with clustering by prefecture alone for the short-run topic coefficients. Dashes preserve undefined two-way inference. The aggregate response is also sensitive to the clustering convention: the 205-municipality estimate for months four to six in Table 2 has a prefecture-only p -value of 0.110.

The significance stars in Table S9 use unadjusted tests. The reported sample and specification comparisons were selected during exploration; they are not a new confirmatory exercise. A significant prefecture-mean coefficient and an insignificant within-prefecture coefficient do not establish a difference between them: the decomposition table reports joint-covariance tests of equality.

A separate diagnostic using an indicator for an event in the next one to three months found a positive exposure interaction. That lead can overlap another event’s subsequent window and is not a clean untreated-period placebo, but it further limits an interpretation confined to responses caused by the newly occurring earthquake. A causal interpretation requires that the specified adjustments account for other changes in discussion correlated with historical exposure, including relevant regional changes. These conditional differences in public discussion do not identify a prefectural transmission channel, constituents’ beliefs, or a causal mediation effect on the policy outcomes.

Table S16: Descriptive Statistics for Municipal Preparedness

Variable	<i>N</i>	Mean	SD	Min	Max
<i>Panel A. Post-2018 preparedness</i>					
Tasks for outside personnel specified, 2025	1,104	0.709	0.454	0.000	1.000
Designated receiving workspace, 2025	1,104	0.619	0.486	0.000	1.000
Bedding for evacuees with limited mobility, 2025	1,104	0.781	0.414	0.000	1.000
Tasks for outside personnel specified, 2018	1,104	0.102	0.303	0.000	1.000
Change in tasks specified for outside personnel, 2018–2025	1,104	0.607	0.535	-1.000	1.000
Adoption by 2025 if absent in 2018	991	0.702	0.457	0.000	1.000
Mean 2019–2024 change from 2018	1,104	0.358	0.414	-1.000	1.000
<i>Panel B. Seismic-retrofit support</i>					
Access to seismic-retrofit support, 2010	1,104	0.639	0.481	0.000	1.000
Access to seismic-retrofit support, 2025	1,104	0.938	0.241	0.000	1.000
Change in retrofit support, 2010–2025	1,104	0.300	0.470	-1.000	1.000
Adoption by 2025 if absent in 2010	399	0.845	0.363	0.000	1.000

Notes: Statistics are unweighted and use the 1,104-municipality policy sample unless the row reports a smaller conditional-adoption sample. SD is the sample standard deviation. Policy levels and adoption outcomes are indicators, so their means are sample shares. Changes equal endline minus baseline and take values of -1 , 0 , or 1 . Conditional-adoption rows retain only municipalities without the policy at baseline. The 2019–2024 row averages the six annual differences from the municipality’s 2018 indicator for specifying tasks for outside personnel. There are no missing observations in the full-sample rows.

C Municipal Preparedness

C.1 Policy Records and Inference

Table S16 reports the levels, changes, and conditional adoption rates for the policy outcomes.

Policy records across years. The records show whether municipalities had specified tasks for outside disaster-response personnel in 2018, 2025, and the intermediate years. The survey placed this item in a business-continuity block through 2020 and in a receiving-plan appendix from 2021 onward. The supplementary annual checks below retain a separate comparison using later waves in the 2021–2024 question context; the 2018 baseline still comes from the earlier context.

For the consecutive-survey comparisons in Appendix C.3, baseline dates for the task item are June 1 of 2019–2023, followed by June 1 surveys through 2023 and April 1 in 2024. Workspace uses the same dates for baselines 2021–2023. The final interval is ten months and the earlier intervals are twelve. Bedding uses December 31 baselines in 2021–2023 and the corresponding next-year dates. These dates also determine the prior-discussion windows specified with that analysis.

Workspace items retain the same activity-base and workspace response field in the 2021–2024 receiving-plan appendices. For bedding, the item is the standing stock of bedding

for people with limited mobility in questions 5, 20, 21, and 25 of the 2021–2024 municipal questionnaires. Question-number and punctuation changes are reconciled using the original questionnaires and response fields. The 20 unpublished designated-city responses in 2021 and three officially excluded municipalities in 2023 remain missing. Selected and valid unselected binary response cells are coded one and zero, respectively; parsing failures are not coded zero. The source audit records the original files, raw cells, value states, extraction code, and hashes.

Dependence through historical holdings. For the historical-exposure policy regressions, a municipality’s modal alternating domain is the holder with the largest W_{mj} defined in Appendix A.2. Kurashiki is assigned to Okayama, whose weight is 45.5 of the total 88.5, or 51.4 percent. This assignment changes the calculation of uncertainty, not Kurashiki’s continuous exposure of 0.288. Across the 1,104 municipalities in the three headline comparisons, the 179 mapped domains yield 163 modal-domain clusters: a domain can contribute holdings without being the largest holder anywhere.

The cluster-robust variance estimator permits arbitrary residual dependence among municipalities sharing a modal domain while treating score contributions from different modal-domain clusters as independent. It does not capture every overlapping connection through smaller holdings. The supplementary label-permutation analysis reassigns the fixed schedule labels at the domain level and reconstructs municipal exposure from the complete municipality-by-domain weight matrix. Appendix C.2 reports both raw and studentized permutation inference. The separate discussion-to-policy regressions in Appendix C.3 use prefecture clustering, as specified there.

C.2 Supplementary Annual Updating

The paper’s timing result compares whether municipalities had specified tasks for outside personnel in 2018 and 2025. Intermediate records from 2019–2024 provide a supplementary view of updating during the period of national receiving-support promotion. Averaging each municipality’s six annual changes from its 2018 level yields an estimated exposure difference of 8.52 percentage points, with a cluster-robust standard error of 3.14 points. The corresponding cluster p -value is 0.007.

The annual-level ANCOVA gives a positive estimate of 5.62 percentage points. Its cluster p -value is 0.047, but its studentized label-permutation p -value is 0.083. The intermediate-year exercise therefore supports the direction and timing of the 2018–2025 comparison, but it does not provide a separate rejection at the 5-percent level under the more demanding permutation statistic.

Table S17: Supplementary Annual Updating: Inference and Sensitivity*Panel A. Annual estimates and inference*

Estimand	Effect (pp)	SE (pp)	Cluster p	Raw perm. p	Studentized perm. p
Mean 2019–2024 change from 2018	8.52	3.14	0.007	0.032	0.022
Mean 2019–2024 level, ANCOVA	5.62	2.82	0.047	0.091	0.083

Panel B. Sensitivity of the supplementary annual evidence

Check	Effect or range (pp)	Cluster p	Repetitions
Historical design controls	8.80	0.005	1
Historical controls plus municipality type	8.92	0.004	1
Municipal capacity and recent-earthquake controls	9.04	0.004	1
Later waves in the 2021–2024 standalone-question context; 2018 baseline context differs	11.31	0.002	1
Transition among 2018 non-adopters	5.14	0.082	1
Kokudaka-weighted historical exposure	9.01	0.005	1
Leave one prefecture out	7.43–9.78	–	44
Leave one region out	7.66–9.32	–	9
Village-row/kokudaka and native/common-sample specifications	8.52–9.24	–	8

Notes: Panel A averages the 2019–2024 changes from the 2018 municipal value or compares later levels conditional on the 2018 value (ANCOVA). Cluster-robust inference uses the modal alternating domain. Label-permutation inference reassigns the fixed schedule labels; its p -values are unadjusted. Panel B varies the annual window, construction, covariates, exposure weighting, and sample rules. These checks supplement the main 2018–2025 comparison and are not separate replications. Repetitions count the reported alternative specifications or leave-one-out fits.

Panel B of Table S17 shows that the positive sign is stable across the reported construction, control, exposure-weighting, and leave-one-out checks. These rows assess whether the supplementary pattern depends on one implementation choice; they are not separate replications of the main 2018–2025 result.

C.3 Prior Discussion and Subsequent Preparedness

A supplementary analysis asks whether municipalities with more prior discussion of a preparation are more likely to report it in place at the next survey. We examine whether municipalities have specified tasks for outside disaster-response personnel, designated workspace for those personnel, and stocked bedding for evacuees with limited mobility. These comparisons link discussion and subsequent preparedness in the same municipality, conditional on historical exposure and other municipal attributes.

Measuring policy discussion. We use the same eligible general-question corpus described in Appendix B.1 to identify remarks about receiving outside disaster-response personnel and about beds in evacuation shelters. A fixed vocabulary screen selects 4,305 candidate text windows from the 146,168 accepted question blocks. Each window retains the relevant councillor turn, nearby sentences, source identifiers, and normalized character offsets.

We distinguish broad discussion of these topics from narrower discussion of particular preparations. Broad receiving discussion covers plans and arrangements for receiving outside personnel; narrower indicators identify discussion of defining or assigning their duties and of workspaces or activity bases for them. Broad shelter-bed discussion covers bed provision and use in disaster or shelter settings, including procurement agreements, while the narrower indicator requires an explicit connection between beds, functional mobility limitations, and physical standing stocks. Fixed lexical and context rules separately identify explicit requests or proposals for action on each topic. These measures do not require an earthquake keyword. Administrative replies are not independent speech observations, and a statement about an existing policy does not itself establish the policy outcome.

The recorded classifications can be traced to source sentences and rule identifiers. All 4,385 fixed classification inputs, including 80 separately sampled screen-negative audit windows, reproduce their saved classifications exactly; the audit windows do not enter the main speech counts. Independent AI-assisted readings of a fresh 40-window candidate sample yielded 9 true positives, 1 false positive, and 2 false negatives for broad receiving discussion, and 13, 1, and 1, respectively, for bed discussion. Request detection missed 2 of 3 reference-positive receiving requests and 2 of 5 bed requests. This stratified, small comparison is

not human gold-standard validation or a corpus-wide accuracy estimate. A zero means no discussion detected by the fixed rules, not verified absence of discussion.

Survey intervals and prior discussion. We retain municipality–survey intervals in which the municipality reports the policy absent at the baseline reference date and its next response is observed. For policy k , the prior discussion share D_{ms}^k pools councillor-months with detected topic discussion over the twelve months before baseline survey s , divided by all observed general-question councillor-months in that window. We count each observed councillor at most once per municipality and month for each topic; the same councillor can contribute in different months. The measure is therefore neither an annual unique-person share nor an unweighted average of monthly percentages. A separate regression replaces discussion with the share making requests or proposals on the topic, using the same denominator.

The main comparison requires at least three observed general-question months in the prior year and complete topic classifications in every observed month; an alternative requires one observed month. Missing coverage is not coded as no discussion. We use consecutive surveys of tasks for outside personnel from 2019 through 2024 and corresponding workspace and bedding items from 2021 through 2024, aligned by their reference dates in Appendix C.1. A return from a previous positive response is allowed, so the outcome is not necessarily first adoption.

Receiving-policy speech windows run from June of the preceding year through May of the baseline year. Bedding uses speech in the baseline calendar year. No accepted question-block dates fall on December 31, so observed calendar-year speech coincides with the strict twelve-month window ending before the bedding reference date.

Repeated survey responses can reverse. In the main bedding sample, 183 of 351 intervals have an earlier positive response observed since 2021 before the current negative response. The outcome consequently includes returns to a positive response as well as newly observed implementation. The frozen interval data, classification provenance, and complete estimates are retained, and a separate numerical implementation reproduces the coefficients and prefecture-clustered inference.

Specification and results. Table S18 reports all fourteen specifications: four for tasks for outside personnel, six for workspace, and four for bedding. The six emphasized comparisons use broad receiving discussion for tasks and workspace and broad shelter-bed discussion for bedding, with separate discussion and request predictors. Broad receiving discussion for workspace was specified as a secondary comparison before estimation; the table also retains

the narrower workspace-specific models. We estimate

$$P_{m,s+1}^k = a_k + \beta_k D_{ms}^k + \gamma_k E_m + \delta'_k Z_m + \mu_{p(m)}^k + \tau_{y(s)}^k + u_{ms}^k, \quad P_{ms}^k = 0, \quad (3)$$

where $P_{m,s+1}^k$ indicates a positive policy response in the next survey. We include historical exposure, the eight fixed municipal attributes, and prefecture and baseline-year fixed effects. Both coverage rules retain the original standardization of the attributes without rescaling them to the policy subsamples. Each interval receives equal weight. Inference clusters by prefecture, using the CR1 adjustment and t critical values with the number of prefectures minus one degrees of freedom.

Prior receiving-support discussion is positively associated with subsequent reports of specifying tasks and designating workspace for outside personnel. A one-percentage-point higher prior discussion share is associated with a 2.54-percentage-point higher probability of a positive next-survey response for tasks for outside personnel and a 4.32-point difference for workspace. The respective 95% confidence intervals are [0.20, 4.89] and [0.56, 8.07]. The request-share estimates are also positive, with wider intervals that include zero. Bedding does not show the same consistent pattern: the discussion coefficient is -2.11, while the request coefficient is 5.11, and both intervals include zero.

Each exercise fixed its specification set before reading its coefficients, within the broader exploratory research. In the primary workspace-specific measure, only 1 eligible interval has detected discussion and none has a detected request. The mechanically computed negative discussion coefficient therefore does not provide credible evidence of a general negative relationship, and the request coefficient is unidentified. For bedding, the broad-topic measure is used because the strict limited-mobility standing-stock topic has insufficient detections and poor reference-sample sensitivity.

The receiving-support associations provide supplementary evidence that attention expressed in council discussion is related to later concrete preparedness. They do not isolate the policy consequences of the post-earthquake speech response: the topic measures differ from earthquake mentions, and these regressions do not use earthquake-event timing. Administrative preparation already under way or unobserved local demand could generate both discussion and a later positive report. The evidence thus supports a connection between discussion and preparedness in receiving support while leaving the causal contribution of speech unresolved.

Table S18: Prior council discussion and subsequent preparedness: all specifications

Predictor	Months	<i>N</i>	Nonzero	Coef.	95% CI
<i>A. Tasks for outside personnel specified</i>					
Marked rows: 466 municipalities, 44 prefectures; 2019–2023 baselines.					
Discussion [†]	≥ 3	1,493	164	2.54	[0.20, 4.89]
Requests [†]	≥ 3	1,493	42	4.03	[-0.87, 8.92]
Discussion	≥ 1	1,565	168	1.86	[-0.57, 4.29]
Requests	≥ 1	1,565	42	3.98	[-0.91, 8.87]
<i>B. Receiving workspace</i>					
Marked rows: 348 municipalities, 43 prefectures; 2021–2023 baselines.					
Workspace discussion ^a	≥ 3	859	1	-13.78	[-19.34, -8.22]
Workspace requests	≥ 3	859	0	–	–
Workspace discussion ^a	≥ 1	896	1	-13.69	[-18.93, -8.44]
Workspace requests	≥ 1	896	0	–	–
Broad receiving discussion ^{†,b}	≥ 3	856	60	4.32	[0.56, 8.07]
Broad receiving requests ^{†,b}	≥ 3	856	17	2.79	[-0.98, 6.57]
<i>C. Bedding for limited mobility</i>					
Marked rows: 228 municipalities, 44 prefectures; 2021–2023 baselines.					
Discussion [†]	≥ 3	351	59	-2.11	[-7.41, 3.19]
Requests [†]	≥ 3	351	9	5.11	[-11.15, 21.37]
Discussion	≥ 1	370	61	-2.17	[-7.80, 3.47]
Requests	≥ 1	370	10	5.23	[-2.30, 12.77]

Notes: All fourteen fixed specifications are reported. [†] marks the six comparisons discussed in the text, not statistical significance. The outcome is a positive next-survey report among municipalities reporting the policy absent at baseline; it need not be first adoption. Discussion and explicit requests enter separate regressions. Their shares pool detected questioner–months over all observed general-questioner–months in the twelve months before baseline. Tasks and broad workspace measures concern receiving outside assistance; bedding measures concern shelter beds. Months is the minimum observed-month count, *N* counts municipality–survey intervals, and Nonzero counts intervals with a positive predictor. Missing coverage and unresolved classifications are excluded, not coded zero. Coefficients are outcome percentage points per one-percentage-point increase in the predictor. All models include historical exposure, eight municipal attributes, prefecture and baseline-year fixed effects, equal interval weights, and no municipality fixed effects. Confidence intervals use prefecture-clustered CR1 standard errors and t_{G-1} critical values. Receiving surveys use June 1 through 2023 and April 1 in 2024 (last interval: ten months); bedding uses December 31 and twelve-month intervals. ^aWorkspace discussion occurs in only one interval and one prefecture; its coefficient and conventional interval do not provide reliable cluster-based evidence of a negative relationship. Workspace requests have no positive observations and are unidentified (dashes). ^bBroad receiving measures are prespecified secondary comparisons; workspace-specific models remain reported. These exploratory conditional associations are not causal effects; differences in dates, definitions, and samples preclude ranking policy effects.