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COMPOUND CLIMATE SHOCKS AND INSURANCE WITHDRAWAL ACROSS VULNERABLE HOUSING MARKETS

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Abstract:

This analysis examines whether declared climate hazards are associated with residential insurer withdrawal, addressing international debates over exposure and insurance availability. California insurance records and OpenFEMA declarations form a balanced panel of 290 observations across 58 counties during 2016 to 2020. Ordinary least squares models include county and year fixed effects, with standard errors clustered by county. An additional fire incident corresponds to a withdrawal change of negative 0.008 percentage points, compared with positive 0.041 for a flood or severe storm incident. Annual hazard co-occurrence corresponds to negative 0.331 percentage points. All confidence intervals include zero, providing no statistical support for the proposed positive associations. This assessment persists under an alternative denominator and exclusion of the final year, although the fire coefficient changes direction. The findings qualify the assumption that recorded hazard exposure translates directly into insurance contraction without identifying causal transmission channels. The contribution separates administrative exposure measures from observed supply adjustment. Insurance monitoring should therefore integrate direct retention records, consistent classifications, and sensitivity checks rather than infer withdrawal from declarations alone.

Key Words: Climate Risk; Insurance Withdrawal; Compound Hazards; Residential Insurance; Panel Data

1. Introduction:

Residential exposure to climate hazards does not establish how insurers respond to that exposure. Insurance supports financial recovery, but its availability depends on decisions about accepting and retaining risks. Kousky (2019) distinguishes the contribution of insurance to recovery from its influence on risk reduction, showing why these outcomes require separate empirical evaluation. The same distinction applies to hazard occurrence and insurance withdrawal. For households and regulators, the relevant question is whether observed hazards are accompanied by reduced policy retention, rather than whether hazards occur in locations where insurance matters.

The expansion of housing into exposed landscapes makes this question consequential. Radeloff et al. (2018) report that the number of houses within the United States wild land urban interface increased from 30.8 million to 43.4 million between 1990 and 2010. However, residential exposure and realised losses are not interchangeable. Evidence from southern California shows that housing arrangement and location influence the likelihood of destruction during wildfire (Syphard et al., 2012). These findings establish the importance of geographic exposure while leaving a separate insurance question: whether the frequency of declared incidents is associated with insurers ending existing coverage.

Insurance supply theory provides a reason to expect a positive association, but not an automatic relationship. Kousky and Cooke (2012) explain how dependence among losses and extreme loss distributions can constrain catastrophe insurance provision. Additional hazard incidents may therefore encourage insurers to reassess geographically concentrated policies. Yet policy retention also operates within institutional arrangements that can reshape the response to risk. Thistlethwaite (2017) documents the expansion of flood insurance in Canada under institutional pressure, demonstrating that insurance provision need not follow physical exposure through a single adjustment process. These competing considerations make the relationship between declared hazards and observed withdrawal an empirical question.

The distinction between insurance demand and supply further sharpens that question. Gallagher (2014) finds that flood insurance purchasing increases after flooding and subsequently declines toward its earlier level. This evidence establishes a household response to hazard information, rather than an insurer response through cancellations or nonrenewals. Kunreuther (2015) likewise identifies interactions among consumers, insurers and regulators in managing catastrophic risks. Changes in insurance purchasing, coverage availability and insurer-initiated cessation consequently represent different outcomes. Treating them as equivalent can obscure whether an observed market adjustment originates in household choices or insurer retention decisions.

Exposure to multiple hazards introduces another distinction. Zscheischler et al. (2018) explain that compound impacts can arise from combinations of climatic drivers and hazards whose dependence matters for consequences. This framework motivates examining joint exposure alongside separate hazard counts. Annual

occurrence of fire and hydroclimatic incidents within the same county, however, does not establish that the events physically interact. The present analysis therefore treats co-occurrence as an observed measure of joint geographic exposure. Its role is to assess whether county years containing both hazard groups exhibit an additional withdrawal association beyond the separate incident counts.

The reviewed evidence establishes constraints on catastrophe insurability, household responses to flooding, institutional influences on coverage and the importance of interacting hazards. It does not establish whether county annual counts of declared fire incidents, declared flood and severe storm incidents, and their co-occurrence distinguish insurer-initiated withdrawal within a common empirical specification. This analysis addresses that bounded gap by separating hazard occurrence from the insurance outcome and evaluating the additional association represented by annual co-occurrence. The contribution concerns the empirical application of the proposed relationships, rather than the identification of a causal transmission mechanism.

This study aims to determine whether declared fire incidents, declared flood and severe storm incidents, and their annual co-occurrence are associated with residential insurer withdrawal in California. The research question is whether these three exposure measures are positively associated with insurer-initiated cancellations and nonrenewals relative to preceding insurance activity. The specific objectives are to estimate the separate fire and hydroclimatic associations, assess the additional co-occurrence association, and examine whether the assessments depend on denominator choice or the inclusion of 2020.

The analysis integrates California Department of Insurance County records with OpenFEMA disaster declarations. California provides a useful setting because county insurance records can be aligned with geographically identified incidents across the same residential market system. The baseline is a balanced panel of 290 observations across 58 counties during 2016-2020. Withdrawal is measured as insurer-initiated cancellations and nonrenewals divided by the preceding year's new and renewed voluntary policies. This ratio distinguishes policy cessation from household insurance purchasing and scales the outcome to preceding activity without treating it as an individual termination probability.

Ordinary least squares estimation includes county and year effects, with inference clustered by county. The design compares changes within counties after accounting for common annual shifts. It supports conditional associations rather than causal effects. A matched sample analysis substitutes current insurance activity as the denominator, while a separate sensitivity analysis excludes 2020 to assess the reporting discontinuity. These comparisons evaluate specific measurement and sample choices without adding unmeasured explanatory pathways.

The findings do not support the three proposed positive associations. This assessment persists across both sensitivity checks, although the wildfire coefficient changes direction when 2020 is excluded. The contribution is therefore twofold: the analysis provides evidence on the distinction between administrative hazard occurrence and observed insurer withdrawal, and it separates stability in hypothesis assessment from stability in coefficient direction. The results do not establish that hazards are irrelevant to insurance decisions. They show that these annual declaration measures provide limited evidence of greater withdrawal within the observed panel.

The broader relevance lies in how insurance systems are compared and monitored. Hazard records describe exposure, while cancellation and nonrenewal records describe a particular supply response. Their relationship requires direct evaluation rather than inference from one measure to the other. The measurement distinctions and sensitivity comparisons can inform research across insurance systems, while the estimated associations remain specific to California, the observation period and the constructed outcomes.

The remainder of the article presents the research hypotheses in Section 2, the data in Section 3, and the methods in Section 4. Section 5 presents the findings, Section 6 discusses their significance, and Section 7 sets out the conclusions and implications.

2. Theory, Conceptual Framework and Hypotheses Development:

This analysis draws on insurance supply theory and risk learning to explain the relationship between declared climate hazards and residential insurance withdrawal. Insurers connect household protection to their capacity to absorb uncertain losses. Hazard exposure can therefore influence assessments of future claims and decisions about retaining policies. Koijen and Yogo (2015) demonstrate that financial constraints and regulatory incentives affect insurance provision. Their findings support the general supply mechanism, although their evidence concerns life insurance rather than property insurance.

The conceptual framework links three observed exposure measures to insurer withdrawal: declared fire incidents, declared flood and severe storm incidents, and their occurrence within the same county and calendar year. Withdrawal is measured by insurer-initiated cancellations and nonrenewals divided by the previous year's voluntary new and renewed policies. This measure relates policy cessation to the preceding volume of insurance activity and supports comparisons across county years during 2016-2020.

Wildfire exposure represents the number of distinct declared fire incidents within a county year. Fire can threaten several insured properties within the same locality, concentrating potential losses geographically. Greater exposure may consequently encourage insurers to reassess policy retention.

Abatzoglou and Williams (2016) establish that increased fuel aridity contributes to forest fire activity across the western United States. Their findings explain an important physical mechanism underlying wildfire exposure. The insurance hypothesis extends this reasoning to the expected association between declared fire incidents and underwriting withdrawal.

However, incident counts do not necessarily correspond proportionately to residential losses. Syphard et al. (2012) show that housing arrangement and location influence destruction during wildfire. Declared fire incidents therefore represent exposure rather than claims severity. The expected association with withdrawal remains positive because additional incidents can increase the perceived risk of retaining geographically concentrated policies.

H1: A higher number of declared fire incidents is associated with a higher insurer withdrawal ratio.

Flood and severe storm exposure captures disruption through the combined count of distinct declared incidents in these categories. These events can reveal vulnerabilities in residential locations and encourage insurers to reconsider geographic exposure. The mechanism concerns reassessment following disruption rather than assuming that homeowners' insurance covers every component of flood damage.

Demand can respond differently from supply. Gallagher (2014) finds that flood insurance purchases increase after flooding and subsequently decline toward their earlier level. Greater perceived risk can therefore increase household demand for protection while insurers reconsider policy retention. Insurance purchasing and insurer withdrawal represent distinct responses to hazard information.

Risk reassessment also varies across locations. Kousky (2010) documents different property market responses across floodplain categories following a major flood. This evidence supports heterogeneous responses rather than uniform adjustment after every declaration. Nevertheless, the proposed supply mechanism predicts that greater declared flood and severe storm exposure is associated with stronger insurer withdrawal.

H2: A higher combined number of declared flood and severe storm incidents is associated with a higher insurer withdrawal ratio.

Multiple hazard exposure identifies county years containing both declared fire incidents and declared flood or severe storm incidents. Its theoretical contribution differs from incident frequency alone. Exposure to both categories broadens the range of hazards insurers must assess within the same geographic market.

Zscheischler and Seneviratne (2017) demonstrate that dependence between climatic drivers can alter the probability of compound extremes. Their findings motivate examining joint exposure alongside separate hazard counts. The constructed co-occurrence indicator captures the presence of both hazard groups within a county year.

Zscheischler et al. (2018) define compound events through combinations of processes contributing to an impact. Annual co-occurrence provides an empirical proxy for joint geographic exposure rather than direct evidence of physical interaction. The hypothesis therefore concerns an additional association with withdrawal after accounting for the separate incident counts.

H3: Annual co-occurrence of declared fire and flood or severe storm incidents is positively associated with the insurer withdrawal ratio, conditional on the separate incident counts.

The conceptual framework connects three observed predictors to one observed insurance outcome. Fire incident counts represent wildfire exposure, flood and severe storm incident counts represent hydroclimatic exposure, and the binary co-occurrence indicator represents multiple hazard exposure. These variables remain separate observed measures rather than indicators of an estimated latent construct. The hypotheses express theoretical expectations about associations that the available county data can test.

3. Data:

This analysis combines county insurance records with declared hazard incidents to construct an annual panel of California housing insurance markets. The data audit establishes source provenance, verifies sample eligibility and reproduces the measures used to test the three hypotheses.

3.1 Data Sources, Population, and Sample Construction:

The target population comprises California county housing insurance markets. The accessible frame includes all 58 counties, with annual hazard observations from January 1, 2010, to December 31, 2020, and insurance observations from January 1, 2015, to December 31, 2020. County insurance counts come from the California Department of Insurance report on new, renewed and nonrenewed homeowners' policies, using its internally consistent 2020 reporting vintage. The extracted insurance file contains 348 county observations and six separate statewide observations. Statewide totals serve as reconciliation benchmarks and are excluded from county estimation (California Department of Insurance, 2020b).

The insurance collection covers companies writing at least USD 5 million in the relevant insurance lines and represents approximately 98 percent of the homeowner's insurance market. Its coverage includes specified homeowners, dwelling fire, mobile home and residential landlord policies. Insurer initiated cessations combine cancellations and nonrenewals. These observations measure annual policy activity rather than unique insured households or policies outstanding on a particular date (California Department of Insurance, 2020a).

Hazard records come from Disaster Declarations Summaries, version 2, published through OpenFEMA. The extraction contains 496 California declaration records with incident start dates within 2010-2020. The analytical selection retains county records classified as Fire, Flood or Severe Storm. Major disaster, emergency and fire management declarations remain eligible, with overlapping declarations consolidated through the incident identifier (Federal Emergency Management Agency, n.d.).

The workbook also contains statewide annual temperature and precipitation series from Climate at a Glance, using California identifier 4-and-twelve-month periods ending in December. Temperature is a period average, whereas precipitation is a period total (National Centers for Environmental Information, n.d.). Statewide wildfire counts and burned acres come from United States Forest Service historical tables (U.S. Forest Service, Pacific Southwest Region, n.d.-a, n.d.-b). These series provide descriptive context and are kept separate from the county predictors.

The source snapshot was accessed on October 24, 2026. This access date and the insurance report vintage differ from the observation period. OpenFEMA and federal climate records are publicly accessible government data. The insurance reports are publicly downloadable; no separate reuse licence was identified in the supplied documentation.

The selection includes every county in the accessible frame rather than drawing a random sample. The integrated hazard panel contains 638 observations. Insurance availability reduces the usable period to 348 observations during 2015-2020. Constructing withdrawal relative to the preceding year's insurance activity removes the 58 observations from 2015, producing a balanced baseline panel of 290 observations across 58 counties and five years.

Table 1: Sample construction

Stage	County Units	Observations	Implemented Rule
Eligible county calendar	58	638	All counties during 2010-2020
Insurance unavailable during 2010-2014	58 affected	290 excluded	No insurance values imputed
Integrated insurance and hazard panel	58	348	Complete annual coverage during 2015-2020
Preceding insurance denominator unavailable in 2015	58 affected	58 excluded	Previous calendar year required
Baseline estimation sample	58	290	Balanced panel during 2016-2020
Alternative current denominator model	58	290	Same observations as the baseline
Reporting change sensitivity sample	58	232	Excludes 2020

The baseline retains all county units. Its principal selection restriction concerns time coverage rather than the removal of counties.

3.2 Data Integration, Cleaning, and Quality Assurance:

Integration uses the five-digit county Federal Information Processing Standards identifier and calendar year. County identifiers remain text to preserve their leading zeros. Insurance observations are unique by county and year, while hazard records are aggregated before integration. The resulting relationship is one county year to one county year. This structure follows the separation of variables and observational units described by Wickham (2014) and the spreadsheet organisation principles of Broman and Woo (2018).

The hazard audit removes 11 records without a county allocation and 127 county records outside the selected hazard categories. The remaining 358 declaration records contain 11 overlaps when identified by county, incident identifier and hazard group. Consolidation produces 347 distinct county hazard incidents. These incidents occur across 178 county years in the complete hazard panel. All 638 constructed fire and hydroclimatic counts were independently checked against the retained incident records.

The insurance source contains 354 unique area and year observations, comprising 348 county observations and six statewide benchmarks. The audit identifies no duplicate county year keys or duplicate FEMA record identifiers. Aggregation introduces no additional county year observations. Source records, constructed variables and analytical outputs remain separate, preserving the distinction between original information and processing decisions recommended by Sandve et al. (2013).

Incident year follows the incident start date rather than the declaration date. Each incident contributes to its starting calendar year. The annual insurance counts already follow calendar years, so integration requires no fiscal year conversion. A zero-hazard count means that no qualifying declaration was identified for that county year. It does not establish the absence of every physical hazard.

Insurance values during 2010-2014 remain unavailable. Five FAIR Plan new policy cells contain source dashes: Colusa in 2015 and Glenn during 2015-2018. These entries remain unknown rather than being replaced with zero. They do not affect the baseline because FAIR Plan counts are not required to construct its outcome or predictors. All four baseline analytical variables are complete for the 290 retained observations.

The quality checks verify nonnegative counts, positive insurance denominators, county year uniqueness, hazard aggregation and agreement between stored and reconstructed withdrawal ratios. Published statewide insurance counts slightly exceed the sums of named counties. Across 2015-2020, the differences range from 3 to 12 new voluntary policies, 119 to 629 renewed voluntary policies and 7 to 107 insurer-initiated cessations annually. These differences remain unallocated; county counts are not rescaled.

No observations were trimmed, winsorised, interpolated or removed because they were extreme. The analysis preserves the supplied historical snapshot and records processing decisions in executable code and audit outputs, consistent with Wilson et al. (2017). Independent reconstruction of the measures supports computational reproducibility in the sense described by Peng (2011).

3.3 Variable Measurement, Construction, and Validation:

The dependent variable measures insurer withdrawal relative to preceding insurance activity. Equation 1 constructs annual voluntary activity.

$$P_{it} = A_{it} + R_{it}$$

Here, P_{it} denotes voluntary policy activity in county i during year t, A_{it} denotes new voluntary policies and R_{it} denotes renewed voluntary policies. Equation 2 defines the withdrawal ratio.

$$W_{it} = \frac{N_{it}}{P_{it}^{prior}}$$

Here, W_{it} denotes the insurer withdrawal ratio, N_{it} denotes insurer-initiated cancellations and nonrenewals, and P_{it}^{prior} denotes voluntary policy activity in the preceding calendar year.

The denominator provides a comparable scale across counties with different volumes of insurance activity. The ratio remains a descriptive withdrawal measure rather than the probability that a particular household loses coverage. This distinction matters because numerator and denominator choices can change the interpretation of normalised observations, as Curran-Everett (2013) explains.

Wildfire exposure is the annual count of distinct declared fire incidents. Hydroclimatic exposure is the annual count of distinct incidents classified as Flood or Severe Storm, consolidated within that combined group. Neither count measures insured losses or physical severity. Housing configuration can influence losses conditional on fire occurrence, reinforcing the distinction between hazard incidents and property damage established by Syphard et al. (2012). Equation 3 defines multiple hazard exposure.

$$C_{it} = \begin{cases} 1, & F_{it} > 0 \text{ and } H_{it} > 0 \\ 0, & \text{otherwise} \end{cases}$$

Here, C_{it} denotes annual co-occurrence, F_{it} denotes declared fire incidents and H_{it} denotes declared flood and severe storm incidents.

The indicator captures joint geographic exposure within a calendar year. Leonard et al. (2014) explain that compound impacts depend on the combination, timing and dependence of contributing processes. Zscheischler et al. (2018) similarly distinguish interacting hazards from isolated drivers. Consequently, the constructed indicator represents annual co-occurrence without asserting simultaneous events or physical amplification.

The three predictors enter separately. Total climate incidents are excluded because that total equals the sum of the fire and hydroclimatic counts. The analysis applies no latent variable estimation, normalisation or weighting to these observed predictors. Statewide weather series remain contextual because they provide no independent county variation within a year. This separation of geographic and temporal scales follows the measurement concerns discussed by Hsiang (2016).

Table 2: Variable dictionary

Variable	Analytical Role	Definition and Construction	Source And Identifier	Unit and Frequency	Transformation And Direction	Missing Value Rule	Coverage and Limitation
Insurer cessations, N	Outcome numerator	Reported insurer-initiated cancellations and nonrenewals	Insurance source, Insurer nonrenewed	Policies annually	Higher values indicate more cessations	No replacement of unavailable values	2015-2020; reasons are not exclusively climatic
New policies, A	Denominator component	Reported new voluntary policies	Insurance source, Voluntary new	Policies annually	Untransformed count	No imputation	2015-2020
Renewed policies, R	Denominator component	Reported renewed voluntary policies	Insurance source, Voluntary renewed	Policies annually	Untransformed count	No imputation	2015-2020
Prior voluntary activity, P^{prior}	Outcome denominator	Previous calendar year's new plus renewed voluntary policies	Calculated from insurance counts	Policies annually	Positive scaling denominator	First insurance year unavailable	Supports outcomes during 2016-2020
Withdrawal ratio, W	Primary outcome	Insurer cessations divided by prior	County panel, Insurer / prior	Ratio annually	Higher values indicate greater	Requires numerator and	290 baseline observations

Variable	Analytical Role	Definition and Construction	Source And Identifier	Unit and Frequency	Transformation And Direction	Missing Value Rule	Coverage and Limitation
		voluntary activity	voluntary		withdrawal intensity	positive denominator	
Current denominator ratio	Alternative outcome	Insurer cessations divided by current voluntary activity	County panel, Insurer / current voluntary	Ratio annually	Higher values indicate greater withdrawal intensity	Same completeness requirements	Evaluated on the same 290 observations
Fire incidents, F	H1 predictor	Distinct county fire incidents	OpenFEMA version 2, incidentId and incidentType	Incidents annually	Untransformed count	Zero for no qualifying record	2010-2020; declaration based
Hydroclimatic incidents, H	H2 predictor	Distinct Flood and Severe Storm incidents pooled within county year	OpenFEMA version 2, incidentId and incidentType	Incidents annually	Untransformed count	Zero for no qualifying record	2010-2020; combines different hazards
Annual co-occurrence, C	H3 predictor	One when both hazard counts exceed zero	Calculated from Fand H	Binary annually	One indicates both groups present	Complete wherever both counts are observed	Annual joint exposure rather than physical interaction
County and year indicators	Adjustment terms	Categorical indicators for county and calendar year	County FIPS and Year	Categories annually	Absorb county constants and common annual shifts	Complete in baseline	Do not remove all changing confounders

Table 3: Descriptive statistics for the baseline sample

Variable	Valid Observations	Mean	SD	Median	Minimum	Maximum
Withdrawal ratio	290	0.027281	0.016023	0.023848	0.009887	0.143171
Current denominator ratio	290	0.026999	0.015378	0.023720	0.009920	0.135641
Fire incidents	290	0.527586	1.022717	0	0	5
Hydroclimatic incidents	290	0.441379	0.839145	0	0	3
Annual co-occurrence	290	0.062069	0.241698	0	0	1
Insurer cessations	290	3,209.362	5,824.507	1,255	11	38,438
Current voluntary activity	290	147,545.772	266,786.742	54,405	622	1,747,204

The statistics give equal weight to each county year. The average withdrawal ratio is approximately 2.73 percent. Annual co-occurrence appears in 18 baseline observations, comprising 15 in 2017 and three in 2019. This concentration determines the available information for evaluating H3.

3.4 Analytical Framework, Identification, and Reproducibility:

The analysis estimates conditional associations between declared hazard exposure and insurer withdrawal. It does not identify causal effects because declarations are not randomly assigned and the available records do not isolate changes in insurer behaviour attributable exclusively to hazards. This interpretation follows the emphasis on research design articulated by Angrist and Pischke (2010). Equation 4 presents the implemented baseline specification.

$$W_{it} = \alpha_i + \lambda_t + \beta_F F_{it} + \beta_H H_{it} + \beta_C C_{it} + \varepsilon_{it}$$

Here, W_{it} is the withdrawal ratio; α_i represents county effects; λ_t represents year effects; F_{it}, H_{it} and C_{it} are the three observed predictors; β_F, β_H and β_C are their conditional association coefficients; and ε_{it} is the residual.

The model uses ordinary least squares with county and year indicators. Each county year receives equal weight. County effects absorb characteristics that remain constant within a county, while year effects absorb common annual shifts. The design matrix contains 65 linearly independent parameters across 290 observations. Fixed effects do not automatically address dynamic relationships or changing confounders, as Imai and Kim (2019) demonstrate.

The inference procedure clusters errors by county across 58 clusters and applies the CR1 finite sample correction. Confidence intervals and two-sided tests use 57 degrees of freedom. This permits heteroskedasticity and correlation within counties, following Cameron and Miller (2015). A coefficient multiplied by 100 expresses its association with withdrawal in percentage points. Statistical significance is evaluated separately from the magnitude and uncertainty of that association.

County clustering assumes independence across county clusters. Shared insurers and geographically extensive hazards may violate that assumption. Cameron et al. (2011) establish that dependence can occur across more than one grouping dimension. The implemented inference therefore supports county clustered uncertainty without claiming to resolve every spatial or insurer level dependence pattern.

Two supplementary checks were implemented during the data audit. The first replaces the preceding activity denominator with current voluntary activity while retaining the same 290 observations. The second excludes 2020 and re-estimates the baseline on 232 observations. This exclusion addresses the reporting clarification that classified nonpayment related cessations as consumer initiated rather than insurer initiated. Both checks retain all 58 counties.

These checks assess denominator choice and the reporting discontinuity. They do not establish immunity to omitted variable bias, whose interpretation requires separate assumptions about unobserved confounding (Cinelli & Hazlett, 2020). The baseline and supplementary specifications are documented without asserting preregistration.

The reproducibility package preserves the supplied workbook, source checksum, construction code, audited panel, descriptive statistics and all fitted specifications. The code reconstructs hazard counts and withdrawal ratios before estimation. The resulting definitions, calendar alignment and sample restrictions are consistent across the baseline and supplementary analyses.

4. Methods:

4.1 Research Design and Identification Strategy:

This analysis uses a balanced panel of California counties to estimate associations between declared climate hazards and insurer withdrawal. The unit of analysis is the county year. The baseline estimation covers 58 counties over 2016 to 2020, yielding 290 observations. The outcome measures insurer-initiated cancellations and nonrenewal relative to the preceding year's voluntary policy activity. The explanatory variables capture declared fire incidents, declared hydrological incidents, and their occurrence within the same county and calendar year.

The design exploits changes in declared hazard exposure within counties over time. County fixed effects absorb persistent differences in geography, insurance market composition, and other time invariant characteristics. Year fixed effects absorb statewide changes shared across counties. Consequently, the coefficients describe associations between deviations in county exposure and deviations in insurer withdrawal after accounting for these two sources of variation.

The estimand is a conditional association rather than a causal effect. Disaster declarations reflect administrative recognition of events and do not provide random exposure assignment. Moreover, hazards and insurer decisions are aggregated within the same calendar year, preventing identification of their precise temporal sequence. County and year fixed effects cannot eliminate confounding from changing local conditions or establish that exposure precedes withdrawal. Imai and Kim, 2019, explain why causal interpretations of longitudinal fixed effects models require assumptions beyond the inclusion of unit indicators.

The co-occurrence indicator identifies years containing both hazard categories. It does not establish that events overlap physically, interact environmentally, or jointly cause insurance decisions. This distinction matters because temporal and spatial aggregation can alter the interpretation of climate exposure measures, as discussed by Hsiang, 2016.

4.2 Model Specification and Measurement Alignment:

Equation 4 specifies insurer withdrawal as a linear function of fire incidents, hydrological incidents, their binary co-occurrence indicator, county fixed effects, and year fixed effects. The fire coefficient describes the adjusted association with one additional declared fire incident. The hydrological coefficient has the corresponding interpretation for a declared flood or severe storm incident. The co-occurrence coefficient estimates the additional association attached to the presence of both hazard categories, conditional on their incident counts. It therefore represents an additive departure associated with joint presence rather than a continuous interaction between incident counts.

Tables 1 to 3 document source coverage, sample construction, and variable definitions. The estimation sample contains all counties with the required outcome components and hazard measures. Constructing the preceding year's denominator removes the first insurance observation for each county, leaving five observations per county. No observations are removed because of missing baseline regressors.

The withdrawal measure remains a ratio during estimation. Reported coefficients, standard errors, and confidence limits are multiplied by 100 to express changes in percentage points. The denominator represents recorded voluntary policy activity rather than a cohort of individual contracts. Accordingly, the outcome measures withdrawal intensity relative to market activity and should not be interpreted as the probability that an individual household loses coverage. Because denominator choice can affect ratio interpretation, the sensitivity analysis also uses contemporaneous voluntary policy activity. Curran Everett, 2013, discusses the interpretive consequences of normalization through ratios.

The linear specification estimates an adjusted mean relationship on the observed ratio scale. It does not require the outcome to represent an individual binary event. No latent constructs, measurement factors, mediators, or moderators enter the implemented model. County characteristics that remain constant over the estimation period are absorbed by county fixed effects, while statewide annual measures are absorbed by year

fixed effects. A total incident count is excluded because it would duplicate the sum of the two included hazard counts.

4.3 Estimation and Statistical Inference:

Equation 4 is estimated using unweighted ordinary least squares with explicit county and year indicators. Equal observation weights define the target as an average county relationship rather than a relationship weighted toward larger insurance markets. The baseline design matrix contains 65 linearly independent columns, comprising an intercept, 57 county indicators, four-year indicators, and three hazard variables. Coefficients are obtained using the NumPy least squares routine with its rank tolerance set to None.

Inference uses standard errors clustered by county. This approach permits heteroskedasticity and unrestricted dependence among observations within the same county. Following the framework described by Cameron and Miller, 2015, the covariance estimator includes a finite sample correction that accounts for the number of clusters and estimated parameters.

$$\widehat{\mathrm{Var}}[\hat{\theta}] = \frac{G}{G-1}\frac{n-1}{n-K}[X'X]^{-1}\left[\sum_{i=1}^{G} X_i' \hat{u}_i \hat{u}_i' X_i\right][X'X]^{-1}.$$

In Equation 5, $\hat{\theta}$ contains all estimated coefficients, X is the complete design matrix, and X_i contains observations for county i. The vector $\hat{u}_i$contains that county's fitted residuals. The symbols G, n, and K denote the number of counties, observations, and estimated parameters, respectively. Their baseline values are 58, 290, and 65.

Individual coefficients are assessed using two-sided tests and 95 percent confidence intervals from a student distribution with 57 degrees of freedom. A directional hypothesis receives statistical support when its coefficient has the proposed sign and its confidence interval excludes zero. Otherwise, the evidence is treated as insufficient to support that directional relationship rather than as proof of no association. The joint contribution of the three hazard coefficients is assessed using a cluster robust Wald statistic divided by three and evaluated against an F distribution with three and 57 degrees of freedom.

County clustering does not accommodate every possible dependence structure. In particular, residual dependence across counties may arise through shared insurers or geographically connected events. The reported uncertainty therefore remains conditional on independence across county clusters after adjustment.

4.4 Diagnostics, Sensitivity and Replicability:

Processing proceeds from the audited county panel to outcome construction, hazard aggregation, estimation sample restriction, fixed effects estimation, and inference. Incident records are deduplicated by county, incident identifier, and hazard category before annual counts are constructed. The preceding year's denominator is then aligned within each county. No hazard predictors are lagged. No imputation, interpolation, trimming, or winsorization is applied to the estimation variables.

Specification checks assess whether the implemented model contains usable variation and whether overlap among the hazard predictors affects coefficient precision. The complete design matrix is checked for full column rank. Each hazard predictor is then residualized against county and year indicators. Variance inflation factors are calculated from these residualized predictors, ensuring that the assessment concerns the variation used to estimate the focal coefficients. The analysis also reports the additional proportion of outcome variation explained by the hazard variables beyond the fixed effects and their joint Wald test. These checks assess numerical identification and explanatory contribution rather than establish causal identification.

Table 6 presents two sensitivity specifications addressing distinct measurement and coverage concerns. The first replaces the preceding year's denominator with contemporaneous voluntary policy activity while retaining the same 290 observations. This comparison isolates the consequences of denominator timing. The second excludes the final observation year because of the identified classification change affecting nonpayment related records. It retains all 58 counties across 2016 to 2019, producing 232 observations. Differences in this specification reflect both the revised observation period and the associated sample composition. Both checks retain the same hazard definitions, fixed effect's structure, and county clustered inference. They are supplementary analyses rather than preregistered tests.

The computational implementation uses Python 3.12.14, NumPy 2.3.5, pandas 2.2.3, and SciPy 1.17.0. Estimation is deterministic and involves no random sampling, simulation, or iterative optimization. The replication package contains the source workbook, construction and estimation script, audited analytical panel, descriptive statistics, model outputs, diagnostic results, and execution instructions. A SHA256 checksum identifies the input workbook. This documentation links the source data to the reported estimates and follows the reproducibility principles articulated by Sandve et al., 2013.

5. Findings:

This analysis examines whether declared fire incidents, flood and severe storm incidents, and their annual co-occurrence are associated with insurer withdrawal. The findings establish the analytical sample; report adjusted estimates and assess their sensitivity to denominator choice and the reporting change in 2020. Each hypothesis is evaluated against the direction, magnitude and uncertainty of its estimated association.

The baseline contains 290 annual observations across all 58 California counties during 2016-2020. Each county contributes five observations. The outcome and three predictors are complete within this sample. Insurance observations from 2015 supply the preceding year's denominator but do not enter as outcome observations. Earlier years lack the insurance information required for estimation.

Insurer withdrawal is measured as insurer-initiated cancellations and nonrenewals divided by the preceding year's new and renewed voluntary policies. This measure captures withdrawal relative to insurance activity rather than the probability that an individual household loses coverage. Fire and hydroclimatic exposure are counts of distinct declared incidents. The co-occurrence indicator equals one when both hazard groups appear within the same county and calendar year.

Table 4: Characteristics of the baseline analytical sample

Variable	Observations	Mean	SD	Median	Minimum	Maximum
Insurer withdrawal, percent	290	2.728	1.602	2.385	0.989	14.317
Declared fire incidents	290	0.528	1.023	0	0	5
Declared flood and severe storm incidents	290	0.441	0.839	0	0	3
Annual hazard co-occurrence	290	0.062	0.242	0	0	1
Insurer initiated cessations	290	3,209.36	5,824.51	1,255	11	38,438
Current voluntary policy activity	290	147,545.77	266,786.74	54,405	622	1,747,204

Withdrawal varies across county years, ranging from 0.989 to 14.317 percent. Hazard counts have medians of zero, indicating that exposure is concentrated rather than continuously observed across the panel. Annual co-occurrence appears in 18 observations, comprising 15 in 2017 and three in 2019. Its estimated association therefore rests on a smaller set of exposed observations than either separate hazard count.

The baseline model estimates the three hazard associations jointly through ordinary least squares with county and year effects. Standard errors are clustered across 58 counties, with a finite sample correction and 57 degrees of freedom for inference. This procedure accommodates heteroskedasticity and dependence within counties, following Cameron and Miller (2015). Hypothesis support requires a positive coefficient and a two-sided p value below 0.05, corresponding to a 95 percent confidence interval entirely above zero.

Table 5: Adjusted associations between declared hazards and insurer withdrawal

Predictor	Estimate	Standard Error	Lower Confidence Limit	Upper Confidence Limit	p Value	Hypothesis Assessment
Fire incidents	−0.0083	0.0421	−0.0927	0.0760	0.8440	H1 not supported
Flood and severe storm incidents	0.0407	0.1580	−0.2756	0.3569	0.7978	H2 not supported
Annual hazard co-occurrence	−0.3311	0.3852	−1.1024	0.4401	0.3936	H3 not supported

The wildfire coefficient is negative and close to zero. One additional declared fire incident is associated with a decrease of 0.0083 percentage points in withdrawal, but the confidence interval extends from a decrease of 0.0927 to an increase of 0.0760 percentage points. H1 therefore receives no empirical support. The estimate does not establish that wildfire exposure leaves insurance decisions unaffected. It concerns declared incident frequency, which does not capture residential destruction or insured losses. Syphard et al. (2012) demonstrate that housing arrangement and location influence property losses during wildfire, supporting this distinction between incident occurrence and its consequences.

The hydroclimatic coefficient has the hypothesised positive direction but remains imprecisely estimated. An additional declared flood or severe storm incident is associated with an increase of 0.0407 percentage points in withdrawal. Its confidence interval accommodates both negative and positive associations, so H2 is not supported. This outcome should not be interpreted as evidence against household responses to flood information. Gallagher (2014) identifies increased flood insurance purchasing after flooding, whereas the present outcome measures insurer-initiated policy cessation. Demand adjustment and supply withdrawal are distinct empirical relationships.

Annual co-occurrence has a negative coefficient of 0.3311 percentage points relative to the additive specification containing the separate hazard counts. The confidence interval ranges from −1.1024 to 0.4401 percentage points. H3 is therefore not supported, and the negative estimate does not establish a protective association. The indicator identifies annual joint geographic exposure without measuring physical interaction, event sequencing or combined losses. Zscheischler et al. (2018) emphasise that compound impacts arise through interacting processes, making that distinction material to interpretation.

The three hazard terms are also jointly nonsignificant. The county clustered F statistic is 0.640, with three numerator and 57 denominator degrees of freedom and a p value of 0.592. Together, the hazard terms

explain approximately 0.46 percent of the withdrawal variation remaining after county and year adjustment. The model consequently provides limited evidence that these contemporaneous declaration measures distinguish withdrawal within counties over the observed period.

Linear overlap among the predictors is modest. After removing county and year effects, variance inflation factors are 1.275 for fire incidents, 1.008 for hydroclimatic incidents and 1.276 for annual co-occurrence. These diagnostics provide no indication that strong linear collinearity drives the coefficient uncertainty. They do not overcome the limited occurrence and temporal concentration of joint exposure.

Two supplementary analyses examine whether the baseline assessment depends on outcome scaling or inclusion of the reporting change in 2020. The first replaces preceding insurance activity with current activity while retaining the same 290 observations. The second excludes 2020, leaving 232 observations across the same 58 counties.

Table 6: Sensitivity of hazard associations to denominator choice and sample period

Specification	Predictor	Observations	Estimate	Lower Confidence Limit	Upper Confidence Limit	p Value
Current activity denominator	Fire incidents	290	−0.0213	−0.1007	0.0581	0.5924
Current activity denominator	Hydroclimatic incidents	290	0.0414	−0.2532	0.3360	0.7793
Current activity denominator	Annual co-occurrence	290	−0.2973	−1.0302	0.4356	0.4200
Excluding 2020	Fire incidents	232	0.0798	−0.0968	0.2563	0.3693
Excluding 2020	Hydroclimatic incidents	232	0.0818	−0.2752	0.4389	0.6481
Excluding 2020	Annual co-occurrence	232	−0.5492	−1.3902	0.2918	0.1963

Changing the denominator leaves the substantive assessment unchanged. The wildfire and co-occurrence coefficients remain negative, while the hydroclimatic coefficient remains positive. Every confidence interval includes zero. The lack of support for the proposed positive associations therefore does not depend on choosing preceding rather than current insurance activity.

Excluding 2020 produces a more qualified pattern. The wildfire coefficient changes from negative to positive, the hydroclimatic coefficient increases, and the co-occurrence coefficient becomes more negative. None becomes statistically distinguishable from zero. The hypothesis assessments remain unchanged, but the wildfire sign reversal shows that its estimated direction is sensitive to the sample period. Robustness therefore concerns the absence of statistical support rather than stability of every coefficient.

County and year adjustment does not establish causal identification. The estimates may still reflect changing underwriting conditions, differences in the insurance consequences of declared incidents and dependencies extending across counties. Imai and Kim (2019) show why fixed effects alone do not resolve the assumptions required for causal interpretation. The reported coefficients accordingly remain conditional associations.

6. Discussion:

Hazard occurrence and insurance withdrawal are distinct empirical outcomes. Table 5 shows no support for the proposed positive associations between declared hazards and insurer withdrawal under Equation 4. The wildfire estimate is close to zero, the hydroclimatic estimate is positive but imprecise, and the co-occurrence estimate is negative with uncertainty spanning both directions. The contribution is therefore a measured boundary on the assumed translation from hazard exposure into insurance contraction. Kousky (2019) distinguishes insurance coverage, recovery and risk reduction as separate relationships. This analysis extends that distinction to declared incidents and observed policy cessation: evidence of exposure cannot substitute for evidence of withdrawal. The findings refine the empirical application of insurance supply theory without implying that climate hazards are financially unimportant.

The denominator comparison clarifies how outcome construction affects this interpretation. Equation 2 measures cessations relative to preceding insurance activity, whereas Table 6 substitutes current activity on an identical sample. The hypothesis assessments remain unchanged, indicating that the baseline assessment does not depend on that denominator choice. This stability concerns measurement rather than a demonstrated behavioural transmission channel. Kunreuther (2015) explains how risk information, insurance incentives and institutional arrangements can shape decisions, but the present estimates do not identify which of those processes governs policy retention. The theoretical advance is to distinguish a plausible supply mechanism from its empirical identification. A hazard coefficient in Equation 4 cannot, by itself, establish the pathway through which insurers adjust coverage.

The evidence also constrains claims about pathway dominance. The three hazard terms jointly explain approximately 0.46 percent of withdrawal variation remaining after county and year adjustment, while the joint test provides no statistical support for their combined contribution. This percentage describes explanatory variation rather than a decomposition of causal effects. Table 5 consequently provides no defensible basis for ranking wildfire, hydroclimatic exposure and co-occurrence as dominant mechanisms. Their coefficients also represent different comparisons, since incident counts and a binary co-occurrence indicator have different scales. Wasserstein et al. (2019) emphasise that statistical thresholds do not establish scientific importance or the absence of an association. The contribution here is a calibrated assessment of the proposed framework: its positive relationships receive no support, while the confidence intervals preserve uncertainty about substantively relevant associations.

The reporting sensitivity reveals why institutional comparability matters when interpreting insurance responses. In Table 6, excluding 2020 changes the wildfire coefficient from negative to positive, although neither estimate provides statistical support for H1. The assessment is stable, but the estimated direction is not. This distinction prevents robustness from being overstated as uniform coefficient stability. Thistlethwaite (2017) documents how institutional pressures accompanied the expansion of flood insurance in Canada, illustrating that insurance provision can respond to forces beyond physical exposure alone. That evidence does not explain the California coefficient change, but it shows why institutional settings require explicit treatment in comparative models. Equation 4 adjusts for common annual shifts; the sensitivity analysis demonstrates the continuing importance of consistent classifications and observation periods.

The international relevance lies in separating different links between hazards, losses and insurance decisions. Hudson et al. (2014) identify reductions in flood damage associated with household mitigation in Germany, whereas Table 5 evaluates insurer withdrawal following declared incidents. These findings address different outcomes and should not be treated as contradictory evidence about a single risk response. Their comparison instead shows why global insurance research needs harmonised definitions before transferring conclusions across countries. Decision makers should monitor actual cancellations and nonrenewals alongside hazard records, preserve consistent classifications and distinguish policy counts from withdrawal ratios. Equations 2 and 4 provide a transparent structure for those comparisons, while Table 6 shows why alternative denominators and sample periods should remain visible. Future comparative analyses can test whether the same associations persist across insurance systems using longer overlapping records and equivalent measures. The resulting theoretical extension is precise: administrative hazard exposure and observed insurance supply adjustment require separate empirical evaluation.

7. Conclusion and Implications:

Climate exposure and insurance withdrawal require separate evidence. This analysis finds no statistically supported positive association between the examined hazard patterns and insurers ending coverage, including when different hazards occur within the same annual period. The contribution is an empirical boundary on the assumption that greater recorded exposure necessarily translates into insurance contraction. Stable hypothesis assessments alongside sensitivity in coefficient direction distinguish uncertainty about an association from evidence of its absence. This finding qualifies the application of insurance supply theory without establishing a causal mechanism. Its international relevance lies in a transferable analytical distinction: records describing hazardous events cannot substitute for records describing insurance decisions. Managers should therefore evaluate policy retention directly alongside exposure information when reviewing geographic portfolios. Regulators should maintain consistent classifications of insurer and consumer decisions, preserve comparable reporting periods, and examine whether conclusions depend on measurement choices. Operationally, these priorities support auditable records, transparent denominators and documented sensitivity analysis. Such practices can improve the evidence used to identify coverage pressures and direct regulatory attention. For households and communities, the practical value is more accurate diagnosis of insurance availability concerns. The evidence supports disciplined monitoring rather than automatic conclusions about market withdrawal, protection or resilience based solely on hazard occurrence.

The short annual panel creates an opportunity to examine adjustment over longer periods and across renewal cycles. Declaration counts can be complemented with property damage, insured losses and coverage terms to distinguish event occurrence from financial consequences. Annual co-occurrence can be refined through event timing and geographic overlap. Records identifying individual insurers could clarify dependencies across county boundaries, while comparable international datasets could test transferability across regulatory systems. Designs exploiting plausibly external changes would strengthen causal identification. These extensions would assess whether the observed uncertainty reflects measurement, timing or differences in institutional responses, while preserving transparent reporting of unsupported relationships.

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