Coverage Neglect in Homeowners Insurance*

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Abstract

Most homeowners lack sufficient insurance coverage to fully rebuild their homes after a total loss. Using contract-level data from Colorado, we document substantial variation in underinsurance across insurers that cannot be explained by policyholder characteristics. Instead, insurer attributes, such as local market experience and reputation, predict more complete coverage. These differences affect disaster recovery: among households that lost homes in a major wildfire, those insured by low-coverage firms were less likely to rebuild and more likely to sell. To understand why consumers do not fully insure, we estimate a discrete choice model of insurance demand. The results indicate that many homeowners could obtain greater coverage at no additional cost by switching insurers. This pattern suggests that consumers pay limited attention to differences in quoted coverage limits, a phenomenon we call *coverage neglect*. In partial equilibrium, we estimate that coverage neglect reduces consumer surplus by $290 per year—equivalent to about 10% of average annual premiums.

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1 INTRODUCTION

Three-quarters of American homeowners report feeling confident that their property insurance would fully cover the cost of rebuilding their homes after a disaster (Swartz and Howard, 2020; Metlife, 2010). Yet for decades, survivors of catastrophic natural disasters have found that their coverage falls short, often expressing confusion about how their policy limits were determined (Klein, 2018; Hassani, 2013). As climate change intensifies, so does the importance of understanding the factors contributing to the long-standing phenomenon of underinsurance. Despite its practical significance, the economics literature offers little systematic evidence on the issue. This paper addresses that gap by asking why homeowners are underinsured against total losses and by examining how both household and insurer characteristics explain variation in relative coverage limits.

In this paper, we analyze new data that are uniquely suited to quantify the drivers and consequences of underinsurance. Drawing on administrative records from the Colorado Division of Insurance (DOI), we observe the homeowners insurance contracts of 4,859 policyholders affected by the Marshall Fire—a major, unexpected wildfire that swept through suburban neighborhoods between Denver and Boulder, Colorado, on December 30, 2021. These data cover 24 insurers operating in the same market. For each insurance contract in our estimation sample, we observe the insurer's identity, standardized premiums derived from regulatory rate filings, and detailed policy characteristics, including coverage limits and provisions for extended or full replacement cost coverage.[1] We also observe changes in coverage amounts between the original policy inception and the last renewal before the fire. We merge the insurance contract data with policyholder credit characteristics as well as with property data, including sales and the physical characteristics used to estimate home replacement costs. We supplement our policy data with post-disaster surveys from United Policyholders tracking underinsurance in the Marshall Fire and across ten other wildfires dating back to 2007.

Our data confirm the widespread and severe underinsurance reported after the Marshall

[1] Per agreement with the Colorado DOI and IRB, the paper does not disclose information that could identify any insurer or homeowner. When combining insurance data with other sensitive datasets, such as credit bureau records, we first convert insured addresses into unique property codes and perform all merges on secure servers without personally identifiable information.

Fire. We estimate that 74% of policyholders were underinsured and that 36% were severely underinsured, with dwelling limits below three-quarters of their home's replacement cost. As shown in Figure 1, these estimates closely align with the underinsurance rates observed in United Policyholders' post-disaster surveys.

We evaluate several commonly cited explanations for underinsurance and find that none fully account for the patterns in our data. First, a sharp post-fire rise in rebuilding costs does not explain the observed coverage gaps: coverage limits were insufficient to rebuild according to our pre-fire replacement cost estimates. Underinsurance is also unrelated to policy age. Older and newer policies exhibit similar shortfalls, indicating that failure to update coverage over time is not the primary driver. Likewise, policyholders with large mortgage balances are no more or less underinsured than those with lower mortgage balances, suggesting that leveraged homeowners do not strategically underinsure and that lenders do not prevent underinsurance by monitoring coverage limits. Finally, although higher-income households are less likely to be underinsured than lower-income households, a majority of higher-income households remain underinsured–suggesting a limited role for a financial constraints mechanism in our setting.

The strongest predictor of underinsurance is a policyholder's insurer. Underinsurance varies markedly across insurers, and these differences persist even after controlling for policyholder characteristics. We find little evidence of systematic selection into insurers with different average coverage ratios based on policyholder income or credit score. Nor do we find that insurers ration coverage: nearly 95% of policyholders in our sample hold extended replacement cost policies, which require coverage at least equal to the insurer's estimated replacement cost, and there is no evidence of adverse selection in coverage limit choices. Instead, insurers with longer local market experience, larger market shares, and higher quality ratings provide fuller coverage. These patterns suggest that reputational capital and soft information play a central role in shaping variation in relative coverage limits across firms.

Our analysis of United Policyholders' survey data from 1,700 survivors of the Marshall Fire and three other wildfires reinforces the central role of insurers in shaping underinsurance. Mirroring our results from administrative policy data from the Marshall Fire, the survey responses indicate substantial within-disaster heterogeneity in underinsurance across the insurers in the survey data. Consistent with the idea that inadequate coverage carries reputational costs, underinsured

respondents are nine percentage points more likely to report having filed a complaint with their state division of insurance.

Using our linked administrative insurance policy data and Boulder County permit records, we find that Marshall Fire survivors with lower coverage ratios experienced substantially worse post-fire outcomes. Exploiting insurer-level variation in coverage ratios, we show that underinsured survivors whose homes were destroyed were less likely to rebuild within two years and more likely to sell their properties without rebuilding. This finding is important in light of prior evidence that post-disaster rebuilding creates positive spillovers and that population re-sorting after disasters has lasting effects on local recovery (Deryugina, Kawano, and Levitt, 2018; Deryugina, Molitor, et al., 2018; Fu and Gregory, 2019; Issler, Stanton, Vergara-Alert, and Wallace, 2020; Zivin, Liao, and Panassie, 2023).

Our evidence is consistent with reports that insurers employ heterogeneous models to recommend coverage limits (Klein, 2023). Although buyers ultimately choose their coverage limit, our results suggest that consumers anchor on insurers' recommended coverage limits. To explain this phenomenon, we propose a "coverage neglect" hypothesis in which consumers fail to account for differences in recommended coverage when comparing quotes across insurers. This hypothesis is rooted in the qualitative conclusions of legal scholars, who argue that consumers select coverage limits equal to the home reconstruction cost estimates provided by insurance agents even though these estimates commonly understate costs (United Policyholders, 2024; Phillips, 2022; Klein, 2023, 2025).[2]

As an alternative to coverage neglect, some policyholders might deliberately choose lower coverage limits. Incomplete insurance can arise rationally if, for example, homeowners face liquidity constraints, anticipate post-disaster aid, or plan to move rather than rebuild (Ericson and Sydnor, 2018; Billings, Gallagher, and Ricketts, 2022; Sastry, Scharlemann, Sen, and Tenekedjieva, 2024). Similarly, canonical insurance models predict that some households, particularly wealthy ones, may optimally self-insure (Mossin, 1968; Lewis, 1989; Koijen, Van Nieuwerburgh, and Yogo, 2016), though recent work shows that wealthier households buy more insurance on average (Grop-

[2] In 2010, the California Department of Insurance investigated underinsurance and concluded: "The examined insurers each state that it is the responsibility of the policyholder to select appropriate coverage limits. However, the examinations revealed that regardless of the insurers' stated positions, the policyholder is relying upon the insurers' estimate (as calculated using the insurer's replacement cost estimation tool) to select Coverage A limits in a significant number of cases." See Klein (2025).

per and Kuhnen, 2023).

To distinguish deliberate underinsurance from coverage neglect, we test whether consumers account for differences in coverage limits when comparing quotes across insurers. Using policy-level data across insurers, we estimate an insurance contract choice model to test whether consumers shop on coverage-adjusted premiums, or simply compare quoted premiums without adjusting for coverage differences. To do this, our discrete choice model includes both quoted and coverage-adjusted premiums, inferred from standardized insurance rate filings. Under the null hypothesis of deliberate underinsurance, only the coverage-adjusted premium should predict insurer choice, as policyholders would seek the lowest-cost provider for their desired coverage. Following the demand estimation literature, we include insurer fixed effects to capture unobserved differences in quality—such as brand reputation, trust, and customer service—that we allow to vary by flexible functions of homeowner and property characteristics.

Our estimates indicate that policyholders respond strongly to quoted premiums but *not* to coverage-adjusted premiums. Homeowner inattention to coverage differences when comparing quotes is consistent with the coverage neglect hypothesis. This inattention is not meaningfully correlated with income or mortgage status, suggesting that socioeconomic factors and lender monitoring have little impact on the degree of coverage neglect. Instead, the most price-sensitive consumers (those who switch insurers at least once after purchasing their home) exhibit the greatest coverage neglect, focusing narrowly on quoted premiums when shopping for new policies.

To quantify the cost of coverage neglect, we estimate how consumer welfare would change if policyholders instead shopped on coverage-adjusted premiums. Under conservative assumptions in partial equilibrium, we estimate that the average homeowner welfare would improve by $290 per year—roughly 10% of average annual premiums. We also find that a policy requiring full replacement cost coverage would reduce welfare losses from coverage neglect by standardizing coverage across insurers. Although the general equilibrium effects of such policies are uncertain, our model suggests that enabling consumers to compare coverage-adjusted prices would shift demand toward higher-quality insurers that tend to write fuller coverage.

In principle, coverage neglect creates an incentive for insurers to attract customers by quoting policies with understated coverage (i.e., through estimation tools or inputs that yield low replacement cost figures). However, we find that insurers with stronger ties to the local area

and higher quality scores are less prone to underinsure, suggesting that reputation concerns help prevent a full race to the bottom. Indeed, the survey data show that insurers face reputational consequences for revealed underinsurance following disasters. Future research might examine the supply-side responses, in terms of insurance product offerings and marketing, to the demand-side biases documented in this paper.

2 Literature

This paper contributes to a growing literature on homeowners insurance, climate risk, and real estate markets. Existing studies have focused on the pricing of homeowners insurance (Oh, Sen, and Tenekedjieva, 2022; Eastman and Kim, 2023; Boomhower, Fowlie, and Plantinga, 2023; Boomhower, Fowlie, Gellman, and Plantinga, 2024), interactions with the mortgage market (Issler et al., 2020; Biswas, Hossain, and Zink, 2023; Sastry, 2025; Sastry, Sen, and Tenekedjieva, 2023; Ge, Johnson, and Tzur-Ilan, 2024), and the capitalization of premiums into home prices (Ge, Lam, and Lewis, 2022; Eastman, Kim, and Zhou, 2024).[3] Our paper complements this emerging research by studying micro-level determinants of the quantity of coverage selected and the role underinsurance plays in disaster recovery.

As such, our paper also relates to studies of how households prepare for and recover from disasters. This body of work includes research on expectations, shifting beliefs, and mitigation efforts (Painter, 2020; Jha, Liu, and Manela, 2021; Bernstein, Billings, Gustafson, and Lewis, 2022; Goldsmith-Pinkham, Gustafson, Lewis, and Schwert, 2023; Du and Karolyi, 2023) as well as studies of inequalities in the resources available to households to recover from natural disasters (Billings et al., 2022; Collier and Ellis, 2024; Collier, Hartley, Keys, and Ng, 2024; Begley, Gurun, Purnanandam, and Weagley, 2024; Aron-Dine, 2024).[4] We show that private insurance markets play an important role in influencing homeowners' choice of how much coverage they need to withstand disaster, and that errors in these estimates have real consequences for recovery.

[3] A related literature studies the broader capitalization of climate risks into real estate markets (Bernstein, Gustafson, and Lewis, 2019; Keys and Mulder, 2020; Bakkensen and Barrage, 2022; Baldauf, Garlappi, and Yannelis, 2020; Giglio, Maggiori, Rao, Stroebel, and Weber, 2021; Murfin and Spiegel, 2020).

[4] Also related is Cookson, Gallagher, and Mulder (2023), which examines post-disaster crowdfunding also in the context of the Marshall Fire. That paper focuses on the use of GoFundMe as a form of informal social network insurance that can be tapped after disasters but disproportionately benefits high-income households. Both papers find that underinsurance, be it formal or informal, hinders recovery, thereby highlighting how lower-income households systematically lack the resources for post-disaster recovery.

Given our focus on the drivers of underinsurance in homeowners contracts, our paper is closely related to the contemporaneous work of Sastry et al. (2024). Their paper provides a complementary national picture of underinsurance, documenting levels of underinsurance nationally that closely resemble the levels we observe in our sample of Marshall Fire survivors. Sastry et al. (2024) also uncover important variation in consumers' price elasticities of demand according to financial constraints and climate risk exposure. We also find evidence that homeowners make discretionary adjustments to their coverage limits based, in part, on their income. Unlike Sastry et al. (2024), however, our paper explores the role of coverage neglect as a complimentary cause of underinsurance.

Relative to the approach in Sastry et al. (2024), our setting is narrower, but our data provide a more precise and detailed picture of each homeowner's insurance contract features. In particular, we uniquely observe extended replacement cost policies and use the observed insurance company chosen by each household to isolate the role of insurer heterogeneity in driving underinsurance. This feature of our data also allows us to construct a discrete choice model to test whether consumer demand-side frictions contribute to underinsurance. Our data also uniquely allow us to compare the insurance contracts of policyholders with and without mortgages, which we use to test predictions related to leverage and lender monitoring. Because we study insurance contracts in force at the time of a major wildfire and observe the precise property location of each policyholder, we can also link coverage to disaster recovery outcomes, pointing to real effects. In sum, our contribution is to provide a detailed study of the market forces that generate underinsurance and to document the real consequences in the specific context of a major disaster.

Our paper is also closely related to Collier and Ragin (2020), which studies the phenomenon of *over*insurance in the NFIP – a unique market where premiums are subsidized and the maximum coverage limit is capped at $250,000 for a single-family home. Despite the differences in settings, both papers find significant insurer-fixed effects, suggesting that insurance agents influence policyholders' selected coverage limits.

Our findings also relate to the literature on limited attention and neglected attributes in consumer choice. Consumers have been shown to be inattentive to key product attributes across a variety of domains, including mortgage, auto, and online retail markets (Agarwal, Song, and Yao, 2022; Lacetera, Pope, and Sydnor, 2012; Brown, Hossain, and Morgan, 2010; Ellison and Ellison,

2009). When consumers are faced with complicated financial products, inattention and shrouded attributes can lead to pricing and quality responses by suppliers (Hortaçsu and Syverson, 2004; Gabaix and Laibson, 2006; Roussanov, Ruan, and Wei, 2021; Célérier and Vallée, 2017; Guiso, Pozzi, Tsoy, Gambacorta, and Mistrulli, 2022; Oprea, 2024). For insurance in particular, our findings are consistent with a literature showing that consumers can make suboptimal decisions in the presence of "information frictions" in health (Sydnor, 2010; Abaluck and Gruber, 2011; Handel and Kolstad, 2015; Abito and Salant, 2019; Boyer, De Donder, Fluet, Leroux, and Michaud, 2020; Collier, Schwartz, Kunreuther, and Michel-Kerjan, 2022) and flood insurance (Mulder, 2024; Wagner, 2022; Weill, 2023).

Our paper provides several new insights on shrouded attributes and information frictions. We show that coverage neglect arises from an interaction between demand-side frictions (consumers relying on agents to estimate their replacement cost) and supply-side frictions (insurers that make poor replacement cost estimates). Furthermore, and consistent with the literature on shrouded attributes, our results suggest that the degree of underinsurance is to some extent strategically chosen by insurers based on their local knowledge and reputation. As a study of a disaster insurance market failure, we show that even in competitive private insurance markets where a large majority of homeowners buy coverage on the extensive margin, choice frictions can still lead to suboptimal coverage decisions and underinsurance with potentially large welfare consequences.

3 Data, Sample, and Measurement

3.1 Data

We investigate the determinants of underinsurance with a novel combination of data on insurance contracts, premiums, property characteristics, and household financial attributes. These data are detailed below.

Colorado Division of Insurance Policy Data

The Colorado Division of Insurance (DOI) collected data on individual homeowner's insurance contracts in the wake of the Marshall Fire. The data contain 4,859 policies from 24 insurers of

homeowners who filed claims linked to the Marshall Fire. These insurers collectively insure a large majority of the Colorado market. The data include 989 policies linked to homes that were completely destroyed. The remainder of policies are linked to claims for smaller damages.

The DOI required all insurers to report the contract information for all policies in which the homeowner submitted a claim for losses from the Marshall Fire. For each insurance contract, we observe the total coverage for the main dwelling ("coverage A"), including any guaranteed or extended replacement coverage provisions. We also observe coverage for other structures ("coverage B"), and contents ("coverage C"). We see these coverages both at the time of the loss and at policy inception. For each policy, the insurer name is listed, as is the policyholder's property address. To combine the insurance data with other sources while keeping personal identifying information secure, we first convert the address into a separate property code. Next, homeowner names and addresses are deleted and the data are transmitted to a separate server where only deidentified data are stored. We merge in the other datasets described below using the non-identifying property code.

The insurance data have limitations. First, insurers do not report deductible amounts, additional endorsements (e.g. for furs and jewelry), or their original quotes and estimates of the replacement costs of insured dwellings. The DOI requested that insurers report the premium charged for coverage A on each policy. However, only 9 out of the 24 insurers, representing about three-quarters of policies, populated this field. Others reported limitations in their ability to separate out the coverage A premium from the total premium charged. To overcome this limitation, we augment the DOI's insurance policy data with the other datasets described below.

Boulder County Housing Data

The Boulder County assessor's office provides property-level information on tax assessment values, property characteristics (e.g., square footage, bedrooms, finished basement), construction permitting, and property sales. We use these property attributes to estimate replacement costs and to model insurer coverage limits and premiums. In addition, we use post-fire rebuilding permits and home sales as outcome variables. We supplement the housing data with market valuation estimates (Zestimates) manually gathered for each home from Zillow.com at the time of the fire.

Quadrant Insurance Company Rate Filing Data

The pricing of homeowners insurance is heavily regulated by states. A company's quoted premiums must be calculated from rate manuals previously approved by each state's department of insurance. Quadrant Information Services ("Quadrant") is an insurance services company that reconstructs insurers' pricing formulas from those rate filings.[5] Using the 2021 Colorado rate filings, we obtain standardized premiums for each of the properties in our data at multiple levels of coverage A for each insurance company in the Colorado DOI data and available in Quadrant. Other coverages are held fixed (e.g. liability limits) or allowed to vary with coverage A according to company standards (e.g. contents coverage C is often 75% of coverage A).

Premiums are rated according to the following characteristics: zip code, credit score (as sourced from the credit bureau data described below), year built, basement type, building frame and walls, roof type, garage type, number of stories, and construction materials. For those insurers without available rate filings from Quadrant, we use their reported premiums in the Colorado DOI filings to infer their rate manuals following a procedure described in Appendix B. Validating this method, we observe a strong correlation between the premiums implied from Quadrant pricing formulas and the premiums reported by the same insurance companies to the Colorado DOI. We merge the Quadrant insurance premium data with our Colorado DOI insurance data before moving the joined, deidendified data to the separate server.

United Policyholder Survey Data

We use data from post-wildfire surveys conducted by United Policyholders (UP), a policyholder advocacy group active on issues surrounding homeowners insurance and disaster recovery (United Policyholders, 2020). After large disasters, UP distributes information and hosts workshops to help survivors navigate the claim filing process. Through this outreach, UP fields "roadmap to recovery" surveys that ask survivors about their experiences with their insurer, claim filing, disaster recovery, and – importantly for this study – whether they had enough coverage to pay for their damages.

From the aggregate summaries of their one year post-fire surveys, we track rates of under-

[5] Quadrant data are representative of publicly sourced data and should not be interpreted as bindable quotes.

insurance across wildfires between 2007–2021, including the Marshall Fire itself. We also obtain response-level data from four one year post-fire surveys: 2007 San Diego wildfires, 2017 North Bay Fires, 2018 Camp Fire, and the Marshall Fire itself. In total, these surveys contain responses from 1,696 homeowners who suffered a total loss. We focus on data from three survey questions that are asked across all four surveys: The respondent's insurer, whether the respondent was underinsured, and whether the respondent filed a complaint with their state division of insurance.

Insurer Characteristics

We obtain data on measures of insurer quality, financial health, and market share. Our proxy for insurer quality comes from the J.D. Power 2021 U.S. homeowners insurance survey, which surveys a representative sample of policyholders about perceptions of insurer quality along different dimensions and assigns an overall quality score out of 1,000.[6] Financial health is measured using 2021 AM Best credit ratings. Finally, we measure market share on the basis of gross premiums written according to the Colorado Division of Insurance 2021 Insurance Industry Statistical Report.

Credit Data

We use proprietary credit bureau data from Experian, which provide a comprehensive view of individuals' financial profiles, including roughly 2,000 attributes such as mortgage balances, credit scores, and debt composition.[7] The dataset covers adults residing within two miles of the Marshall Fire burn area. Credit bureau data of this type have been widely employed in similar studies (e.g., ???Cookson, Gilje, and Heimer 2022; ?).

To measure household income, we use Experian's Income Insight variable, which estimates individual earnings using a proprietary predictive model trained on matched credit records and Form 1040 income tax data (see Cookson et al., 2022). We use observations from December 31, 2021, the day after the fire, to capture financial conditions at the onset of recovery. For each address, we aggregate or average credit attributes across all adults residing at that property, yielding household-level measures of income, debt, and creditworthiness. This approach provides a consistent and high-resolution picture of financial capacity in the affected area, facilitating analysis of

[6] https://www.jdpower.com/business/press-releases/2021-us-home-insurance-study

[7] Consistent with the academic use of credit bureau data, we report only aggregate statistics—such as summary measures and regression coefficients—that cannot be linked to individual identities.

how pre-disaster financial characteristics shape insurance choice and post-disaster outcomes.

3.2 Estimation Sample

We restrict the data in the following ways to obtain a consistent and fully populated estimation sample. First, we restrict attention to policies on single-family, owner-occupied, detached homes, thus, dropping 1,207 policies. This restriction ensures that the policies in our estimation sample cover the same basic features. By contrast, insurance on attached homes often excludes the home's exterior, which is separately insured through homeowners associations. Next, we exclude 35 contracts that are associated with either boutique insurers (that tend to only insure very high-value and/or risk-prone structures) or insurers that wrote no new policies in the five years before the Marshall Fire (signaling that they no longer write new business in the area). In addition to these restrictions, 111 policyholders could not be linked to credit data and 91 policies are missing important housing attributes from the Boulder County and/or Zillow datasets. Finally, we exclude observations with missing premiums from both the DOI *and* Quadrant rate filings. This affects four insurers accounting for only 74 policyholders.

Our final estimation sample contains 14 out of the 28 insurers that responded to the DOI data call and 3,089 policies, representing over 90% of the single-family policies in the original DOI data. Quadrant has available rates for 10 of these 14 insurance companies in our estimation data, representing two-thirds of estimation sample policies. For the four insurers not in the Quadrant data, we use their reported premiums in the Colorado DOI filings to infer their rate manuals following a procedure described in Appendix B.

3.3 Measuring Insurance Coverage Ratios

We assess an insurance policy's coverage limit relative to the insured home's replacement cost as a "coverage ratio," defined as coverage A divided by the cost to rebuild the home. Coverage ratios below 1 indicate less coverage than the cost to rebuild and those at or above 1 indicate enough coverage to rebuild. Building costs increased dramatically around the time of the fire due to both pandemic-related inflation and the correlated wildfire losses. To separate temporary trends from the structural drivers of relative coverage limits, we calculate separate "pre-fire" and "post-fire" coverage ratios.

To calculate these two coverage ratios, we must first estimate pre- and post-fire replacement costs for each property. The base of our replacement cost estimates comes from RSMeans, a replacement cost estimation model commonly used in the construction industry (RSMeans, 2024). We use estimates from RSMeans of the cost of building a home during the first quarter of 2023 as a function of its quality, market, square footage, and other structural characteristics available in the county assessor data.[8] Although the first quarter of 2023 is more than a year after the Marshall Fire, very few of the survivors with total losses had rebuilt their home by then – only approximately 30% had filed rebuilding permits, which is one of the first steps in the reconstruction process. Thus, 2023 construction costs are what most survivors faced when rebuilding.

To estimate the cost of replacing a home, insurance companies often rely on third-party software (e.g., CoreLogic Marshall & Swift, 360Value, among others). Consumer advocates as well as the inventor of 360Value have raised concerns that these software products are used in a way that leads to understated replacement costs (Hassani, 2013; Phillips, 2022; Klein, 2023; Neilson and Munce, 2025). In addition, most contractors operate under a cost plus model, adding 15% to cost as their own profit. Therefore, when measuring underinsurance, we are careful to account for potential underestimation in construction cost software. We do this by gathering rebuilding cost quotes from the Colorado DOI, a subsample of Marshall Fire survivors, and the Homebuilders Association of Metro Denver. These sources suggest that the lower end of average rebuilding costs after the Marshall Fire was around $350 per square foot. Therefore, we use RSMeans to generate cross-sectional heterogeneity in build costs after the Marshall Fire according to housing characteristics. Then, we inflate the RSMeans construction cost estimate for each home such that the average across our estimation sample equals $350 per square foot. While this adjustment affects our descriptive estimates of the fraction of underinsured policyholders, it does not affect our main results since those depend on cross-sectional variation in coverage ratios.

The post-fire replacement costs generated by this estimation method reflect both the surge in demand for labor and materials after the fire and the coincident supply chain issues brought by the COVID-19 pandemic. These inflationary factors substantially increased rebuilding costs

[8]We estimate cost per square foot factors from RSMeans using each home's number of stories, frame type, roof type, whether it has a finished basement or attached garage, and the number of full and half bathrooms. We set the home construction quality to "luxury," following the recommendation of an RSMeans consultant informed on the prevailing construction standards in the Boulder market.

between 2021, when the policies in our sample were last renewed, and 2023, when most Marshall Fire survivors began rebuilding. To measure underinsurance at the time when policies were last renewed, we generate a separate pre-fire replacement cost estimate for each policy by deflating the post-fire replacement cost estimates by the time-series change in the RSMeans historic cost index for residential homes in the Denver-region between Q1 2021 and Q1 2023 (22%). This method assumes that RSMeans accurately describes the time series change in construction costs even if, in any given year, the average level may be understated.[9]

Next, we calculate from the DOI data each policy's pre- and post-fire insurance coverage (the numerators in the coverage ratios). First, it is helpful to understand dwelling coverage A in the context of homeowner's insurance policies. Agents quote coverage A based on their estimate of the cost of rebuilding a home at current construction costs. Thus, the pre-fire coverage ratio is the coverage A limit divided by the pre-fire replacement cost:

$$R_i^{Pre} = \frac{\text{Coverage A}_i}{\text{Replacement}_i^{Pre}},$$

where R_i^{Pre} measures policyholder i's coverage A relative to the cost of replacing their home at the time they last renewed their policy before the fire.

A common add-on to an insurance contract is extended replacement cost (ERC) coverage, which augments a policyholder's coverage A when post-disaster costs exceed expectations, leaving the policyholder underinsured. ERC coverage is typically sold as a percentage of a policyholder's coverage A (e.g. 25% of coverage A). To access ERC coverage after a loss, homeowners must prove that rebuilding costs truly exceed coverage A limits (i.e., hire an architect and contractor to design and price the destroyed home). Our measure of the post-fire coverage ratio adds ERC provisions to policyholders' coverage A limits because these contingency provisions are designed for precisely the kind of cost inflation that came after the Marshall Fire. Approximately 87% of the policyholders in our estimation sample have ERC coverage, averaging 28% of coverage A.[10]

We calculate the post-fire coverage ratio as the extended coverage A divided by our post-fire

[9] By indexing pre-fire replacement costs to the earliest possible date when the policies were last renewed before the fire, we generate conservative estimates of replacement costs and, in turn, of pre-fire underinsurance.

[10] An additional 6.9% of policyholders have guaranteed replacement cost coverage, which guarantees full coverage. For these policyholders, we set the post-fire coverage ratio equal to the maximum of coverage A divided by post-fire replacement costs and 1.

replacement cost estimate:

$$R_i^{Post} = \frac{(1 + ERC_rate_i) \times \text{Coverage A}_i}{\text{Replacement}_i^{Post}},$$

where ERC_rate_i is the ERC coverage rate for policyholder i.

3.4 Summary Statistics

Table 1 presents statistics for key variables in our estimation sample. We note several significant characteristics of the data. First, the policyholders in the estimation sample have generally high average household incomes ($197,000), credit scores (798), and home values ($977,500). Despite this fact, the typical policyholder is underinsured against a total loss both before and after the fire, a fact that we document in detail in the following section. There is substantial heterogeneity across policyholders. Policyholders at the 75th percentiles are fully insured (according to both pre-fire and post-fire coverage ratios). There is also significant variation in homeowners insurance premiums per dollar of coverage, with policyholders at the 75th percentile of *premiums per $100 of coverage* paying 75% more than those at the 25th percentile.

4 Results

This section describes the extent of underinsurance before detailing the policyholder and insurer factors that predict more or less underinsurance. This section also documents the consequences of underinsurance on rebuilding outcomes. The next section investigates the mechanisms.

4.1 The Prevalence of Underinsurance

Using pre-fire coverage ratios, we estimate that 74% of the homeowners in our sample were underinsured after the Marshall Fire. For 36% of homeowners, we estimate their dwelling coverage was less than three-quarters of their home's replacement cost. These findings suggest that underinsurance is widespread and severe in our setting.

Our estimates of underinsurance are consistent with survey data from United Policyholders, which finds that 80% of Marshall Fire survivors with total losses were underinsured. Further-

more, in the organization's earlier post-wildfire surveys, a majority of survivors report not having enough coverage to fund the repairs or replacement of their home (Figure 1).[11]

Figure 2 plots histograms of pre-fire (top) and post-fire (bottom) coverage ratios, where a coverage ratio less than 1 indicates underinsurance. Although the distributions of pre-fire and post-fire underinsurance differ, it is striking to note that the average coverage ratios are nearly identical across both measures at 87.4% pre-fire and 87.3% post-fire. A frequently cited cause of underinsurance is sudden increases in construction costs. In our data, this rise was largely absorbed by ERC provisions. Hence, policyholders were underinsured on average, not because of cost inflation, but because they purchased an insufficient amount of coverage A.

Next, we explore some of the policyholder characteristics that might explain the wide distribution of coverage ratios. Figure 3 plots the average pre-fire coverage ratio within bins of household income (top) and credit score (bottom). Consistent with recent research showing that richer households tend to buy more insurance (Armantier, Foncel, and Treich, 2023; Gropper and Kuhnen, 2023; Sastry et al., 2024), we see that underinsurance is negatively associated with income. However, there is a flat relationship between credit scores and underinsurance.

It is striking to note, however, that among households with incomes above the sample median of $180,000, 72% of policyholders were underinsured before the Marshall Fire. Thus, while our descriptive results suggest that policyholders likely make some discretionary adjustments to their coverage limits, the prevalence of underinsurance across the wealth distribution suggests that additional factors, besides ability-to-pay, drive underinsurance.

4.2 The Role of Insurance Companies

Although differences in policyholder characteristics account for some of the variation in coverage, substantial underinsurance remains even after controlling for these demand-side factors. To illustrate the potential role of supply-side influences, Figure 4 shows the distribution of average policyholder coverage ratios across insurers in our estimation sample. The variation is pronounced: two insurers exhibit average coverage ratios at or above one, while another has an average ratio covering less than 75% of policyholders' pre-fire replacement costs.

[11] United Policyholders only started reporting underinsurance specifically for policyholders with total losses with its Marshall Fire survey. Thus, the earlier surveys in Figure 1 include respondents with only partial losses who are less likely to be underinsured relative to their damages.

One potential explanation for the heterogeneity in Figure 4 is that certain insurers attract higher income policyholders who request higher coverage ratios. As an initial, visual test of this hypothesis, Figure 5 plots the average policyholder's household income (y-axis) against their insurer's pre-fire coverage ratio (x-axis) and shows no meaningful relationship.

More formally, we test whether the heterogeneity in insurer average coverage ratios persists conditional on the characteristics of the policyholder and their insured structure using the following specification:

$$R_{ij}^{Pre} = \alpha + \lambda X_i + \lambda_j + \epsilon_{ij}, \tag{1}$$

where R_{ij}^{Pre} is the pre-fire coverage ratio multiplied by 100 and λ_j represents insurance company fixed effects.[12] The vector X_i captures the characteristics of the policyholder, including income, credit score, mortgage status, house value, estimated replacement cost, policy inception year fixed effects, and quadratics of years since the home was purchased, age, and square footage of the home. To account for the possibility that variation in pre-fire coverage ratios reflects price sensitivity to heterogeneous premium rates or the purchase of ERC provisions, we further include as covariates an ERC indicator and the premium rate per $100 of coverage at the estimated replacement cost. If observable policyholder characteristics primarily drive the variation in underinsurance across insurers, we would expect the coefficients on the insurer fixed effects, λ_j, to be insignificantly different from each other conditional on this full set of covariates. Standard errors are clustered by insurance company.

Figure 6 plots the fitted insurer fixed effects $\widehat{\lambda_j}$ from estimating Equation 1 with (blue) and without (red) policyholder covariates, X_i.[13] Adding our rich set of policyholder characteristics does little to change the ordering or magnitude of coverage heterogeneity across insurers. Without covariates, moving a policyholder from the insurer with the second highest average coverage ratio to the one with the second lowest coverage ratio (which are both statistically significantly different from the omitted insurer) predicts a decrease in coverage of 27.4% of estimated replacement cost. With policyholder covariates, the same counterfactual predicts a decrease of 25.7%.

[12] For brevity, we focus on pre-fire coverage ratios throughout this section. Since ERC policies are nearly universal in our data and, as such, the two ratios are distributed similarly in Figure 2, all of our findings hold using post-fire coverage ratios.

[13] Appendix Table A1 shows the coefficient estimates from estimating Equation 1 with controls.

Why does average underinsurance vary systematically across insurers? One explanation is that insurers ration coverage to limit their exposure to climate risk. Our data does not support this explanation, however. Examining the distribution of coverage limits within each insurer, we see no evidence of bunching at any hypothetically binding maximum coverage limit. Moreover, our study area had a healthy homeowners insurance market with more than a dozen private insurers offering coverage before the Marshall Fire.[14] Finally, 94% of the policyholders in our sample had extended or guaranteed replacement cost coverage, which insurers would be unlikely to sell if they were trying to limit their exposure. In fact, as a condition of these endorsements, those policyholders would have been required to set their base coverage A limit to at least the replacement cost estimate quoted by their insurance agent.[15]

An alternative explanation, suggested in both the legal literature and news reports (Klein, 2023, 2025; Neilson and Munce, 2025), is that policyholders rely on coverage recommendations provided by insurance agents when shopping for homeowners insurance. Agents use software-based costs models to estimate rebuilding expenses, which directly inform the amount of Coverage A included in insurance quotes. Insurers differ in both the software employed and the housing characteristics incorporated into these models. As documented in Klein (2023), insurance agents' coverage recommendations often serve as anchors for homeowners when selecting coverage limits. In many cases, these insurer-provided recommendations may represent the only source of rebuilding cost information available to homeowners, as obtaining an independent appraisal is expensive. It is, therefore, plausible that homeowners anchor their replacement cost coverage to insurers' recommendations, such that differences in rebuilding cost estimates across companies translate into systematic variation in underinsurance across policyholders.

Although we cannot directly observe insurers' replacement cost estimates or the specific modeling approaches they employ, we conduct a suggestive test for differences along these dimensions by examining whether average underinsurance varies with insurer characteristics that proxy for experience and reputational capital in the local market. Analogous to the mortgage lending literature, which highlights the role of soft information in accurate loan underwriting

[14] As a sign of private insurers' willingness to write coverage at the time of the Marshall Fare in December 2021, Colorado only introduced a state-backed FAIR plan in 2025 (https://www.bankrate.com/insurance/homeowners-insurance/colorado-launches-fair-plan/.

[15] See the Colorado Revised Statues §10-4-110.8, which was in effect at the time of the Marshall Fire.

(e.g., Agarwal and Hauswald, 2010; Berger, Miller, Petersen, Rajan, and Stein, 2005), insurers with a larger local presence may invest more in tools, data, and training that enable more precise rebuilding cost estimates. Supporting this hypothesis, Boomhower et al. (2024) show that larger insurers in California tend to employ more sophisticated wildfire risk models when setting premiums. Moreover, consistent with research linking price transparency and firm reputation, insurers with stronger reputations for quality and greater market share may have stronger incentives to invest in accurate replacement cost modeling, given the higher reputational costs they would incur if their customers were systematically underinsured following a disaster (Seim, Vitorino, and Muir, 2017; Chiles, 2021).

To test the explanatory power of information and reputational capital in explaining heterogeneity across insurers in average underinsurance, we estimate:

$$R_{ij}^{Pre} = \beta_0 + \beta_1 X_i + \beta_2 Q_j + \epsilon_{ij}, \tag{2}$$

where R_{ij}^{Pre} is the pre-fire coverage ratio (multiplied by 100) of policyholder i contracted with insurer j. As in Equation 1, we include X_i to adjust for differences in policyholder characteristics across insurers. We proxy for insurer soft information and reputational capital in Q_j, which captures: (1) the insurer's Colorado market share in 2021, (2) the age of the insurer's oldest policy in our database, (3) the insurer's overall quality score from the J.D. Power 2021 U.S. homeowners insurance survey.

Table 2 shows the results of estimating Equation 2 without (Panel a) and with (Panel b) policyholder characteristic controls X_i. Starting with the bivariate relationships in Columns 1–3, we find that larger insurers, those with more experience, and those with higher quality scores all tend to write higher average coverage ratios. For example, we find that a one percentage point increase in an insurer's state market share is associated with a one percentage point higher average coverage ratio (column 1). These positive relationships hold in Column 4 when we test the joint relationship across all three of proxy variables and when we add policyholder characteristic controls (panel b). In sum, insurer-level heterogeneity in underinsurance appears to reflect differences in modeling sophistication and market experience, not coverage rationing or regulatory limits.

INSURERS, UNDERINSURANCE, AND REPUTATION: ADDITIONAL EVIDENCE FROM SURVEY DATA

We use data from United Policyholder post-fire surveys following the 2007 San Diego Fire, the 2017 North Bay Fire, the 2018 Camp Fire, and the 2021 Marshall Fire to provide more evidence on the relationships between insurers and underinsurance.

First, we show that the heterogeneity in relative coverage limits across insurers extends beyond the Marshall Fire. In Appendix Figure A1, we plot the percent of respondents who reported not having enough coverage to rebuild for each insurer in each fire relative to the overall rate of underinsurance in the survey. As in our policy data, the figure shows substantial differences in the extent of underinsurance across insurers within the same fire both in the Marshall Fire survey and across the other three surveys. The gap between the the insurers with the highest and lowest share of underinsured policyholders was over 60 percentage points in the Camp Fire, nearly 30 percentage points in the North Bay Fire, and over 20 percentage points in the San Diego Fire.

In interpreting Appendix Figure A1, we caution that, unlike in the administrative data, we cannot use respondent characteristics to test the extent to which this insurer heterogeneity can be explained by differences in policyholder characteristics. Although this limitation motivates our use of administrative data rather than survey data for our main analysis, these consistent findings across other fires support the external validity of our results.

A second advantage of the survey data is that it asks respondents whether they have filed a complain with their state division of insurance. We use this data to explore how underinsurance affects insurers' reputations with customers and regulators. We first consider a simple bivariate regression between a respondent filing a complaint and not having enough coverage to rebuild. The results, shown in column (1) of Table 3, show that underinsured policyholders are nine percentage points more likely to file a complaint. This estimate suggests a substantial increase given the mean rate of complaints in the data is 16%. The relationship between underinsurance and complaints remain similar when we include fire and insurer fixed effects (column 2) and exclude the Marshall Fire survey (column 3).

Although we still caution that our estimates might be biased by unobserved respondent characteristics, they suggest that underinsurance creates reputational and regulatory risk for insurers. Such an effect would explain why larger and more established insurers tend to have higher

coverage ratios (Table 2). Finally, the fact that respondents without enough coverage to rebuild are more likely to file complaints supports the qualitative evidence that policyholders typically do not realize that they are underinsured until they have a loss.

4.3 The Effect of Underinsurance on Rebuilding and Home Sales

This section provides evidence on the extent to which underinsurance affects post-disaster recovery. Identifying this relationship is empirically challenging for two main reasons. First, there is limited data linking property-level recovery outcomes to insurance coverage. Our unique dataset, which combines detailed insurance contract information with property-level observations, addresses this data limitation. Second, insurance coverage decisions are endogenously related to policyholders' access to other financial resources that can support recovery.

To address this endogeneity, we exploit cross-insurer variation in average coverage ratios to isolate differences in coverage that are plausibly exogenous to unobserved policyholder characteristics influencing both insurance choices and recovery outcomes. Specifically, we estimate the following specification:

$$Y_{ij} = \beta_0 + \beta_1 X_i + \beta_2 R_{ij}^{Pre} + \epsilon_{ij}, \tag{3}$$

where Y_{ij} is a measure of disaster recovery (rebuilding a destroyed home or selling a destroyed home without rebuilding) for total-loss policyholder i contracted with insurer j. R_{ij}^{Pre} is the associated pre-fire coverage ratio, and X_i is a vector of policyholder characteristics. The identification challenge is that unobserved characteristics in the error term ϵ_{ij} may affect both the choice of coverage (R_{ij}^{Pre}) and recovery outcomes (Y_{ij}), biasing the β_2 estimate.

To address this concern, we estimate a two-stage least squares instrumental variables model (2SLS IV), where we instrument for R_{ij}^{Pre} with insurer j's average coverage ratio across all other policyholders. The first stage of our estimating equation is:

$$R_{ij}^{Pre} = \lambda_0 + \lambda_1 X_i + \lambda_2 \overline{R}_{j,-i}^{Pre} + \mu_{ij}. \tag{4}$$

The excluded instrument, $\overline{R}_{j,-i}^{Pre}$, is the mean pre-fire coverage ratio for insurer j excluding policyholder i. Appendix Table A2 documents a strong first stage, with F-stats ranging from 32.66 to

144.26, depending on the mix of controls. We then use the fitted values $\widehat{R_{ij}^{Pre}}$ in the second stage regression shown in Equation 3.

Our identification assumption is that recovery outcomes are correlated with the choice of insurer only through differences in insurers' average coverage ratios. Two potential threats could violate this assumption. First, policyholders might systematically sort across insurers in ways that jointly influence coverage and recovery outcomes. Second, insurer characteristics other than average coverage ratios—such as customer service quality or claims-processing speed—could independently affect recovery.

We provide several pieces of evidence supporting the validity of our identification strategy. Regarding the first concern, our earlier analysis finds little systematic difference in observable policyholder characteristics across insurers with varying average coverage ratios. Moreover, our specification includes a rich set of policyholder controls to account for any residual sorting on observables. Lastly, we construct our instrument as a leave-one-out measure of the insurer's mean coverage ratio, following the "judge IV" literature, which helps ensure that policyholder i's coverage is not mechanically related to their own recovery outcome (Dobbie, Goldin, and Yang, 2018). Regarding the second concern, we demonstrate that our results remain robust when controlling for observable insurer characteristics that could plausibly influence recovery.

Table 4 presents the second-stage estimates. The first outcome examined is the speed of rebuilding under the premise that underinsurance may lead to liquidity constraints that delay reconstruction. We define Y_i as an indicator variable that equals 100 if a new home has been built on a lot where household i suffered a total loss by the end of 2022 (one year after the fire) in column (1), by 2023 in column (2), and by 2024 in column (3). Panel (a) includes the controls specified in Equations (3) and (4). We find positive and statistically significant effects of instrumented pre-fire insurance coverage on rebuilding speed, such that a policyholder with a 10 p.p. higher coverage ratio is approximately 3 p.p. (or 20% of the dependent variable mean) more likely to have rebuilt within a year of the fire. The positive effect of coverage on rebuilding persists into 2023 (column 2), but attenuates and becomes statistically insignificant by the end of 2024 (column 3). By that time, over 80% of destroyed homes had been rebuilt, suggesting that the primary effect of underinsurance is to stall recovery.

Columns 4 to 6 consider the effect of pre-fire underinsurance on whether the owner of a destroyed home sells the property. In our setting, home sales indicate that the homeowner was unable to afford rebuilding.[16] The dependent variable, Y_i, equals 100 if household i's insured property with a total loss had been sold without rebuilding by the end of 2022, 2023, or 2024. Having more coverage makes disaster survivors less likely to sell without rebuilding. One year after the fire, a 10 p.p. higher pre-fire coverage ratio results in a 1.9 p.p. (or 50% of the dependent variable mean) reduction in the likelihood of selling. In Appendix Table A3, we estimate similar effects when we use post-fire coverage ratios, which account for extended replacement cost policy provisions.

The treatment effects estimated in panel (a) could reflect not only differences in average coverage limits across insurers but also variation in claim-handling practices or service quality. For instance, insurers that recommend higher coverage levels may also process claims more efficiently, potentially confounding the effect of coverage on recovery. To assess this possibility, we augment Equations (3) and (4) with additional insurer-level controls, including the J.D. Power 2021 quality score, 2021 Colorado homeowners insurance market share, market tenure (measured by the age of the oldest policy in our estimation sample), 2021 AM Best financial strength rating (binned indicators), and the insurer's propensity to offer supplemental coverages—proxied by the median amount of contents ("Coverage C") and law and ordinance coverage (reported as percentage of Coverage A).

The results, presented in Panel (b) of Table 4, remain broadly consistent with our baseline estimates, though the effect of coverage on rebuilding by the end of 2022 becomes statistically insignificant. While we cannot fully exclude the possibility that unobserved insurer characteristics contribute to the observed relationships, the persistence of the main patterns after accounting for insurer quality and market presence supports the interpretation that variation in recovery outcomes across insurers is primarily driven by differences in coverage limits rather than other insurer attributes.

One may be concerned that homeowners who are more willing to move choose insurers that tend to write less coverage. If our estimates were affected by this form of endogeneity, we would

[16] Indeed, in our estimation sample, 11.1% of the owners of homes destroyed in the fire had sold their property by the end of 2023 versus only 6.7% of the owners of homes that were not destroyed.

expect low coverage ratios to predict home sales even for those homeowners who did not suffer a total loss. As a falsification test, we regress home sales on coverage ratios for homes that were not destroyed (but for which there is an associated insurance claim for fire-related damages). The results in Appendix Table A4 indicate null results. Hence, in the absence of a loss that would make coverage limits bind, homeowners with policies from insurers that tend to write less coverage are no more likely to move.

We conclude that underinsurance meaningfully hinders post-disaster recovery. Our estimates indicate that greater insurance coverage accelerates the rebuilding of destroyed homes and increases the likelihood that fire survivors remain (rather than sell their properties without rebuilding). Extrapolating from the 2SLS IV results in Panel (a), if all underinsured households in our sample had been fully insured (i.e., with a pre-fire coverage ratio of 1), the share of homeowners who rebuilt within one year of the fire would have risen from 14% to 19%, while the share who sold without rebuilding by the end of 2024 would have fallen from 12.8% to 8.2%.

5 Mechanisms

Although the previous section highlights the insurer's role in explaining variation in coverage limits, consumer behavior and strategic choices may also contribute to underinsurance. In this section, we test three leading explanations for why homeowners purchase insufficient coverage: (1) failure to update limits over time, (2) risk-shifting to lenders, and (3) adverse selection. Then, we introduce a new mechanism: coverage neglect.

5.1 Do Policyholders Fail to Update their Coverage?

One commonly advanced theory of underinsurance is that homeowners fail to update their coverage A over time to account for inflation or improvements to their homes. Under a "failure to update" mechanism, policies may have accurate coverage limits when they are first written but become increasingly underinsured over time. If this were the case, we would expect policies originated further in the past to be more underinsured.[17]

[17] A unique feature of our data is that we observe coverage A limits from when policies were first written in addition to as of the last renewal date before the fire. Coverage A at inception is missing for 34 policies, or approximately 1% of the estimation sample.

Our data reveal that the overwhelming majority of policyholders update their coverage over time, either by requesting more coverage or by accepting the updated coverage A suggestion of their insurer at annual renewal. In particular, only 4.9% of policies that were renewed at least once since origination had an unadjusted coverage A limit.

It is still possible, however, that these updates to coverage A are insufficient. To test the adequacy of coverage A adjustments, we regress the percentage increase in each policy's coverage A limit between the year it was first written and the year it was last renewed prior to the fire (2021) on the corresponding percentage increase in construction costs according to RSMeans. The data is restricted to policies that were renewed at least once. If coverage updates keep up with inflation, we would expect to see (at least) a 1% increase in limits for every 1% increase in costs.

According to the bivariate regression in Table 5, column (1), for every 1% increase in construction costs since policy inception, there is a 1.5% increase in coverage A limits, on average. This relationship persists when we add policyholder and structure characteristics in column (2) and insurer fixed effects in column (3). These estimates do not account for the possibility that policyholders may have improved their homes, making coverage adjustments insufficient. Therefore, in column (4) we drop policies where the square footage at loss is different from the square footage at policy inception—a proxy for having made home improvements. The construction cost growth coefficient changes little in this subgroup, signaling that a more detailed accounting for home improvements is unlikely to alter our broad conclusion: that coverage updates keep up with cost inflation, on average.

As additional evidence, Figure 7 plots the share of policyholders that were underinsured for the Marshall Fire against the year their policy was first written. The figure shows that the oldest policies (originally written before 2000) are *less* likely to be underinsured than newer policies. In the decade leading up to the fire in 2021, the relationship between underinsurance and policy age is essentially flat. More formally, Appendix Table A5 regresses the pre-fire coverage ratio on the age of the policy. The estimates reveal a weak and positive relationship between the age of a policy and its coverage ratio – the opposite of what we would expect if policyholders failed to update coverage.

5.2 Moral Hazard: Do Policyholders Shift Risk onto Mortgage Lenders?

Mortgage contracts may interact with underinsurance through two contrasting avenues. First, since insured property is collateral for mortgage loans, lenders typically require it to be adequately insured.[18] Thus, if lenders monitor homeowners insurance coverage, policyholders with mortgages should be less underinsured than those without. On the other hand, if monitoring is weak, then homeowners with highly leveraged mortgages may have less incentive to fully insure their home. If a borrower defaults after a total loss, their loss is limited by their home equity plus any economic costs of mortgage default. Indeed, studies have found that leverage induces this type of moral hazard in flood insurance take-up, labor market participation, and investment in home renovations (Liao and Mulder, 2021; Melzer, 2017; Bernstein, 2021).

The regressions in Table 6 evaluate whether risk shifting incentives contribute to underinsurance by regressing pre-fire coverage ratios on mortgage indicators. Columns (1)–(3) test whether lender monitoring mitigates risk-shifting behavior. The bivariate regression in column (1) shows that, on average, policyholders with a mortgage have coverage ratios 2.5 percentage points lower than those without. This difference attenuates after controlling for policyholder and structure characteristics (column 2) and becomes statistically insignificant once insurer fixed effects are included (column 3). Thus, contrary to the positive association predicted under a lender monitoring mechanism, we observe an insignificant negative relationship.

In column (4), we replace the mortgage indicator with a series of loan-to-value (LTV) ratio bins (in ten-percentage-point increments), using homeowners without a mortgage (0% LTV) as the reference group. This specification tests whether higher LTV ratios are associated with greater underinsurance, consistent with a risk-shifting hypothesis. However, the results reveal no systematic relationship between LTV and coverage, providing little support for the risk-shifting channel.

A plausible explanation for why mortgage indicators explain little of the variation in underinsurance is that Fannie Mae's mortgage servicing guidelines state that the replacement cost estimates that inform both homeowners' coverage A selections and lenders' minimum coverage A requirements can come *from the insurance company*. And, as we have established, some insurers

[18] Per Fannie Mae's property insurance requirements for mortgages it purchases, structure coverage must equal 100% of the property's estimated replacement cost as of the policy effective date or the maximum of the unpaid loan balance and 80% of the estimated replacement cost (Mae, 2024).

tend to underinsure on average.

5.3 Are Coverage Limits Adversely Selected?

A canonical explanation of underinsurance in equilibrium is adverse selection (Rothschild and Stiglitz, 1978; Einav and Finkelstein, 2011). If policyholders know that they have higher *ex ante* risk of a total loss along some dimension that is not priced by insurers, this may induce an equilibrium where total loss coverage is priced above the willingness-to-pay of lower-risk consumers. Under adverse selection, we would expect *ex ante* riskier policyholders to have higher coverage ratios.

To test whether adverse selection can explain homeowner underinsurance, we first show that homes with wood frames had a much higher probability of being destroyed relative to homes with brick frames conditional on distance to the fire. In the first two columns of Table 7a, we regress an indicator variable that equals 100 if a home was destroyed on an indicator variable for whether the home had a wood frame. The sample is restricted to homes within the fire perimeter that belong to policyholders in our estimation data. The results of the bivariate regression in column (1) show that homes with wood frames were fifty percentage points more likely to completely burn. This coefficient changes little with the inclusion of controls for homes' structural characteristics in column (2). In columns (3) and (4), the dependent variable is set to the cost per $100 of coverage A (assuming full insurance). The estimates show that despite their higher risk, wood frame homes do not have higher insurance premiums. Together, these estimates identify wood frames as an "unused observable" that is predictive of claims but unpriced.

Next, in Table 7b, we ask whether riskier policyholders buy more coverage by regressing their pre-fire coverage ratio (multiplied by 100) on an indicator for whether their house has a wood frame. This method of evaluating adverse selection by testing for asymmetric information follows Finkelstein and Poterba (2014). Contrary to the significant positive relationship predicted by adverse selection, the relationship in column (1) is slightly negative and statistically insignificant. The null relationship persists in columns (2) and (3) with additional controls and insurer fixed effects. As a more general test of asymmetric information based on *ex post* risk, in column (4) we test whether homes that suffered a total loss carry more coverage. Although the coefficient suggests that those with a total loss had 2.4% higher coverage ratios relative to policies that filed minor claims, the magnitude is economically small and only statistically significant at the 10%

level. Overall, these results lend little support to the hypothesis that adverse selection explains underinsurance.

5.4 Proposed Mechanism: Coverage Neglect

Our results offer little support for theories that low relative coverage limits can be explained in our setting by policyholders' strategic behavior or failure to update coverage limits over time. Why then do policyholders select underinsured policies?

We propose a "coverage neglect" mechanism as one factor contributing to underinsurance. Under coverage neglect, homeowners set their dwelling coverage limits according to the replacement cost estimates suggested by insurance agents without realizing how widely suggested limits can vary across insurers. A coverage neglect mechanism is consistent with qualitative interviews after wildfires during which survivors commonly report assuming that insurance companies all offer the same complete coverage at varying premiums (Hassani, 2013; United Policyholders, 2020; Phillips, 2022; Fuqua, 2024). This section uses a discrete choice demand model to evaluate the coverage neglect hypothesis against one in which homeowners choose to underinsure independent of insurers' suggested coverage limits.

Theoretical Framework

Fixing notation, buyer i chooses a policy from insurer j. Policies are described by coverage limits, R_i, and premiums defined by insurer-buyer-specific price schedules $p_{ij}(R)$. A fully optimizing policyholder selects their optimal coverage for each insurer's price-coverage schedule, then chooses the optimal insurer given their preferences over coverage, insurers, and premiums.

Rather than solving this general insurance policy choice problem, we consider two heuristic simplifications which reduce the optimization to a discrete choice across insurers. The first heuristic—a feature of the "deliberate underinsurance" hypothesis—is that the policyholder's optimal coverage limit (R^*) is the same across insurers.[19] The buyer requests quotes, $p_{ij}(R_i^*)$, from each insurer at the desired R_i^*. Thus, the insurer choice problem involves price shopping across

[19]This is a simplification, but a realistic one: Given the difficulty of inferring each insurer's full rate schedule, comparing premiums across a fixed R^* is a reasonable approximation of how diligent homeowners would shop. Empirically supporting this assumption, the results in Figure 6 show that the costs per unit of coverage do not explain differences in average coverage ratios across insurers.

quotes at the same level of coverage. Given this choice problem, the buyer's utility, U^r, from choosing a policy from insurer j is:

$$U_{ij}^d = \sigma_j X_i + \zeta_j - \alpha^d \frac{p_{ij}(R_i^*)}{R_i^*} + \epsilon_{ij}. \tag{5}$$

This model allows buyers to have heterogeneous preferences across insurers that depend on insurer-specific constants, ζ_j, an insurer-specific interaction with their own characteristics, $\sigma_j X_i$, and an idiosyncratic taste term, ϵ_{ij}. Buyers receive disutility from premiums that are normalized as a rate per dollar of coverage.

The second heuristic simplification—a feature of the "coverage neglect" hypothesis—is that buyers receive quoted premiums from different insurers, but the buyer does not account for the differences in coverage limits, R_{ij}, across insurers. Rather than choosing a deliberate R^*, policyholders assume that insurer-quoted premiums are all for the same amount of coverage. Given this choice problem, the coverage neglect utility function is given by U^n:

$$U_{ij}^n = \sigma_j X_i + \zeta_j - \alpha^n \frac{p_{ij}(R_{ij})}{R_i^*} + \epsilon_{ij}. \tag{6}$$

Under coverage neglect, i compares prices set by varying R_{ij} across insurers as if all contracts covered the same R_i^*. Thus, a buyer's decision utility under U^n accounts for differences in premiums, but not coverage, across insurers. Such a model could reflect a variety of underlying factors. For example, consumers may not be able to form a clear estimate R^* on their own and must, instead, rely on agents and, specifically, on the estimates generated by agents' replacement cost models. Search costs may also prevent homeowners from gathering multiple quotes or adjusting those quotes to form apples-to-apples comparisons.

DISCRETE CHOICE MODEL

To test whether buyers choose to insure based on U^d or U^n, we estimate a discrete choice demand model that nests both utility functions. Consumers solve the following maximization problem:

$$argmax_j \; \sigma_j X_i + \zeta_j - \alpha^d \frac{p_{ij}(R_i^*)}{R_i^*} - \alpha^n \frac{p_{ij}(R_{ij})}{R_i^*} + \epsilon_{ij}. \tag{7}$$

Equation (7) includes two price terms: the coverage-adjusted premium, $p_{ij}(R_i^*)$, and the quoted premium, $p_{ij}(R_{ij})$, with both premiums normalized by R_i^*.

Our approach parallels the tests in Abaluck and Gruber (2011), who show that Medicare beneficiaries often select financially dominated plans because they place inconsistent value on premiums relative to out-of-pocket costs. Similarly, our model examines choice inconsistencies among homeowners, specifically whether they focus on headline premiums rather than coverage-adjusted premiums.[20] The deliberate underinsurance hypothesis predicts that $\alpha^n = 0$. Because each buyer makes her own decision about how much coverage she wants, an insurer's coverage recommendation and headline premium has no impact on her utility conditional on the insurer's rate. In contrast, under the coverage neglect hypothesis, $\alpha^d = 0$ because the buyer compares premiums across insurers at varying coverage limits. Thus, under coverage neglect, homeowners may choose an insurer that quotes a lower coverage limit even though they could have bought the same coverage for less from a different insurer.

We take Equation (7) to the data by estimating a multinomial discrete choice model with latent utility V_{ij} (McFadden, 1974):

$$V_{ij} = \sigma_j X_i + \zeta_j - \alpha^d \frac{p_{ij}(R_i^*)}{R_i^*} - \alpha^n \frac{p_{ij}(\widehat{R_{ij}})}{R_i^*} + \epsilon_{ij}, \tag{8}$$

where ϵ_{ij} follows a Type I extreme value distribution. In addition to insurer fixed effects, ζ_j, our specification allows for insurer brand quality to vary across consumers according to individual characteristics, X_i (i.e., income, credit score, log estimated replacement cost, log home value, mortgage status, and quadratics of home age, years since home purchase, years since policy purchase, and square footage). For each policyholder, we define R_i^* as the pre-fire coverage ratio observed in the estimation data.

We calculate the coverage-adjusted premium, $p_{ij}(R_i^*)$, based on quoted and reported premium schedules for each insurer. We estimate insurer-specific quoted coverage limits for each policyholder under coverage neglect (R_{ij}) by setting $\widehat{R_{ij}}$ equal to the fitted values from regressing

[20] As documented in the exchange between Ketcham, Kuminoff, and Powers (2016) and Abaluck and Gruber (2016), classifying dominated plan choices depends on how customers value different insurers and non-financial plan characteristics. In our own approach, we flexibly estimate heterogeneous homeowner preferences across insurers according to the $\sigma_j X_i + \zeta_j$ term in Equation (7).

R_{ij} on policyholder characteristics and insurer fixed effects.[21] At this predicted level of coverage, we calculate $p_{ij}(\widehat{R_{ij}})$ from the quoted and estimated premium schedules.

There are several identification concerns that must be addressed with this approach. First, observed premiums may reflect unobserved coverage choices—such as deductible levels or discounts from bundling home and auto policies—that could also be correlated with unobserved demand for specific insurers. This potential endogeneity motivates our use of Quadrant rate filings as the primary measure of price whenever available. The Quadrant data provide standardized premiums that hold constant coverage levels, contract features, and discounts across buyers.

A second concern is the potential correlation between prices and unobserved insurer quality. To address this, we include insurer fixed effects, ζ_j, which absorb any quality-related differences across insurers in the estimation of price terms. The price coefficients are thus identified from variation in relative premiums within insurers across different buyers. This variation arises from differential pricing of property attributes—such as the number of stories, siding material, and roof type—both across insurers and within insurers at different coverage limits. We argue that these pricing differences are uncorrelated with unobserved idiosyncratic policyholder preferences (ϵ_{ij}), conditional on the rich set of policyholder characteristics included in Equation (8).

Further supporting this identification assumption, property insurance premiums are highly regulated and approved at the state level. Each insurance company files rate manuals with the Colorado Division of Insurance (DOI) that specify exactly what premiums it will charge based on customer and policy characteristics. These rates must be supported by actuarial modeling that shows they are reasonable and justified by historical claims, risk models, and company expenses. This leaves no discretion for agents to target premiums to specific customers through ad-hoc discounts or bargaining.

To illustrate the identifying variation in our price parameters, we residualize both the headline and coverage-adjusted premiums for each buyer-insurer pair in our estimation sample. Figure 8 presents a binscatter of these residualized prices against the probability that a buyer chooses a given insurer. The figure reveals two key patterns. First, there is substantial price variation even after accounting for buyer and insurer fixed effects. Second, while the coverage-adjusted premium is largely unrelated to an insurer's choice probability, insurers offering lower headline premiums

[21] Appendix Section B describes the imputation of coverage limit quotes and price schedules in more detail.

are more likely to be chosen. This reduced-form evidence is consistent with the coverage neglect hypothesis, which we examine more formally in our discrete choice estimation.

Discrete Choice Estimation Results

Table 8 shows the results of estimating our discrete choice model in Equation (8). Column (1) assumes deliberate underinsurance, restricting $\alpha^n = 0$. The coefficient on the coverage-adjusted premium is statistically significant, indicating that buyers prefer insurers that offer lower rates. Column (2) assumes coverage neglect, restricting α^r to zero. The associated price parameter is statistically significant, consistent with buyers preferring lower quoted premiums. The coefficient on α^n suggests a price elasticity of 2.3, nearly triple the price elasticity of 0.8 given by α^d in column (1).

As additional evidence that buyers are more sensitive to quoted premiums than to coverage-adjusted premiums, in column (3), we run a horse race model between the rational underinsurance and coverage neglect hypotheses and show the results of estimating an unrestricted Equation (8). These results imply that buyers are more likely to choose an insurer that quotes a low premium irrespective of coverage, which is consistent with coverage neglect. Furthermore, the price coefficient on the coverage-adjusted premium is attenuated, negative, and statistically insignificant, which is inconsistent with the rational underinsurance hypothesis.

If the observed R_i^* in the data truly represents each buyer's revealed preference coverage choice, then buyers should prefer the insurer that offers that level of coverage at the lowest rate, *ceteris paribus*. Instead, the estimates suggest that buyers are "leaving money on the table" by choosing insurers that appear to be offering a lower premium, but in reality, are just offering less coverage.

Next, Table 9, we evaluate heterogeneity in the relative sensitivity to quoted versus coverage-adjusted premiums. The specification in column (1) allows the coefficients on the premiums to vary by whether the household is above the median income in the estimation sample. Our hypothesis is that higher-income consumers may be more sophisticated and less price sensitive, and thus respond less to quoted premiums and more to coverage-adjusted premiums. In column (2), we consider heterogeneity across consumers with and without mortgages under the hypothesis that lenders may monitor coverage limits and enforce replacement cost policies, leading to a higher

loading on the coverage-adjusted premium. Neither test reveals meaningful heterogeneity. We conclude that coverage neglect is widespread across the income distribution and largely unrelated to mortgage status.

Another potential mechanism driving coverage neglect involves search frictions. Homeowners may find it time consuming to gather multiple quotes and compare premiums on a coverage-adjusted basis. We test this mechanism by comparing the quoted and coverage-adjusted price elasticities of homeowners with different search frictions. To do this, we compare policyholders who keep the same insurer since purchasing their home to those who switch at least once ("shoppers"). Practically, we infer shopped policies as those in which the insurance policy origination date is at least one year later than the home purchase date. Shopped policies account for nearly half of our estimation sample. If a policy was shopped, it indicates a willingness and ability to compare insurance premiums, which is indicative of lower search frictions. If high search frictions (e.g., time constraints) drive coverage neglect, then we would expect coverage neglect to be attenuated among shoppers.

We find just the opposite in column (3) of Table 9, where we interact both the quoted premium and the coverage-adjusted premium with the shopper indicator variable. The coefficient on the quoted premium is *larger* for shoppers than non-shoppers, with no statistically significant difference in the coverage-adjusted premium coefficient between the two groups. While non-shoppers are relatively inelastic with respect to both premiums, shoppers are guided by quoted premiums when choosing an insurer. Implicitly, this finding indicates shoppers do not take advantage of potential opportunities to get the same coverage at a lower cost. The fact that policyholders who take the time to shop for new policies are the ones most prone to coverage neglect suggests that coverage neglect is unlikely to be driven by search costs.

The Cost of Coverage Neglect to Consumers

Our evidence of coverage neglect in homeowners insurance markets is indicative of two underlying frictions that could harm consumer welfare. First, homeowners may set their coverage limits according to estimates provided by insurance agents, driving underinsurance to the extent that

such estimates understate true rebuilding costs.[22] Second, policyholders may leave money on the table by not shopping for the best rate for their chosen level of coverage. In this section, we describe how the welfare effects of coverage neglect are relevant to policy.

Efforts are underway in several states to address underinsurance, typically by improving transparency around pricing and coverage needs or by encouraging greater coverage.[23] Against this backdrop, it is useful to characterize the partial equilibrium policy effects implied by our estimated demand model. Such analysis clarifies how coverage neglect reshapes the welfare calculus for homeowners-insurance reforms.

To construct welfare estimates, we posit that policyholders choose their insurer according to the coverage neglect utility function in Equation (6) and fail to account for differences in coverage limits across insurers when comparing quotes. But, policyholders' welfare-relevant normative utilities, U^*_{ij}, are affected by coverage limits:

$$U^*(p_{ij}(R_{ij}), R_{ij}) = \sigma_j X_i + \zeta_j - \alpha^n \frac{p_{ij}(R_{ij})}{R^*_i} + f_{ij}(\frac{R_{ij}}{R^*_i}) + \epsilon_{ij}, \tag{9}$$

where $0 < R_{ij}$ is the coverage limit recommended to buyer i by insurer j at rate $p_{ij}(R_{ij})$, and R^*_i is i's replacement cost.

Calculating counterfactual welfare requires taking a stance on how policyholders value coverage. Because the normative value of coverage is unknown, we bound its influence on welfare by making two conservative assumptions about how coverage enters U^*. First, we assume that policyholders get no additional welfare from having a coverage limit greater than their replacement cost ($R_{ij} > R^*_i$). For valuing coverage limits below the replacement cost ($R_{ij} < R^*_i$), we use the parameter ϕ to denote how much the homeowner would be willing to pay for additional coverage relative to its cost. For example, if $\phi > 1$, then the welfare value of full coverage is greater than its extra cost (as might be true, for example, if buyers are risk averse), whereas when $\phi < 1$, the value of full coverage is less than the cost (due to, for example, loading fees or moral hazard). Formally,

[22] This mechanism for underinsurance is reinforced by competition. In fact, catering to consumers who exhibit coverage neglect would naturally lead insurers to reduce their reconstruction cost estimates — or make subtle shifts to how they produce these estimates — in order to build market share.

[23] For example, the California Department of Insurance offers an insurance cost comparison tool (https://www.insurance.ca.gov/01-consumers/105-type/9-compare-prem/), and Colorado's 2023 House Bill 23-1174 requires insurers to offer ERC policies of at least 50% of Coverage A and mandates an annual public report by the Colorado Insurance Commissioner on reconstruction costs statewide.

$$U^*_{ij}(p_{ij}(R_{ij}), R_{ij}) = U^*_{ij}(\phi \times p_{ij}(R^*_i), R^*_i), \quad \text{if } R_{ij} < R^*_i.$$

The above equation implies that buyers have the same welfare buying coverage $R_{ij} < R^*_i$ at price $p_{ij}(R_{ij})$ and buying full coverage ($R_{ij} = R^*_i$) at price $\phi \times p_{ij}(R^*_i)$. Our welfare calculations follow derivations from Leggett (2002) on the value of information in discrete choice models, described in more detail in Appendix Section C.

We first consider a "transparency" counterfactual that eliminates coverage neglect by enabling consumers to compare premiums at the same coverage across all insurers. This counterfactual keeps coverage choices fixed by setting R^*_i equal to i's chosen coverage limit in the estimation data, and it ignores general equilibrium effects by holding fixed insurer pricing as well as the menu of insurers and contracts. Consumers maximize Equation (9) with $R_{ij} = R^*_i \ \forall j$ following our estimates from column 2 of Table 8. We set $\phi = 1$, conservatively assuming no surplus from increasing coverage limits. Thus, we focus on buyers' welfare gains from more efficiently comparing premiums across insurers.

The top panel of Figure 9 plots the distribution of welfare effects from the transparency counterfactual. The average welfare gain from eliminating coverage neglect, as indicated by the vertical line, is approximately $290 per year. Although removing coverage neglect enables homeowners to compare prices more accurately, average premiums are only slightly lower under the transparency counterfactual ($2,910 versus $2,870). Thus, most of the transparency welfare gains come from consumers selecting higher quality insurers—those for which consumers have a higher willingness-to-pay due according to the estimated insurer fixed effects and policyholder characteristic interactions in Equation (8).

While our stylized transparency counterfactual can eliminate the inefficient shopping caused by coverage neglect, "full coverage mandates" (FCM) would directly eliminate underinsurance by requiring every insurer to cover the full replacement cost of a property rather than a set coverage limit.[24] We test how a FCM would interact with coverage neglect. The overall welfare effects of a FCM policy are ambiguous. On the one hand, this policy might increase premiums for consumers,

[24]This is not an unrealistic exercise. Until the early 2000s, when the industry started to shift towards coverage A limits augmented by ERC endorsements, standard homeowners policies included guaranteed replacement cost coverage (Klein, 2023). By the time of the Marshall Fire, only 6.8% of the policyholders in our estimation data had guaranteed replacement cost provisions.

create more uncertainty for insurers, and encourage moral hazard in rebuilding. On the other hand, it might address the market friction caused by homeowners being largely unaware that they are underinsured while also reducing the fiscal cost of disaster aid programs that manage underinsured losses (Billings et al., 2022; Collier and Ellis, 2024; Collier et al., 2024).[25]

The goal of our FCM counterfactual is not to weigh in on these hypothetical outcomes or analyze the overall welfare effect of an FCM policy. Rather, it is to point out that an FCM policy would eliminate coverage neglect by allowing consumers to shop on uniform replacement cost limits across insurers. We calculate the partial equilibrium welfare effects of an FCM policy by setting R_i^* equal to the estimated pre-fire replacement cost of home i. Now, consumers maximize Equation (9) with $R_{ij} = R_i^* \ \forall j$. We show how the welfare effects of the FCM counterfactual vary at different levels of ϕ. If one believes that the benefits of a FCM are greater than its costs, then one would expect $\phi > 1$, whereas $\phi < 1$ when the policy's costs outweigh its benefits, independent of coverage neglect.

The bottom panel of Figure 9 plots the distribution of welfare effects from the FCM counterfactual when $\phi = 1$ (blue) and $\phi = 0.75$ (red). Because of our conservative assumptions about how policyholders value coverage, the welfare gains from transparency strictly dominate those from an FCM policy whenever $\phi \leq 1$. Nonetheless, by enabling consumers to compare apples-to-apples premium quotes, the average policyholder gains approximately \$105 in annual welfare at $\phi = 1$ and \$50 when $\phi = 0.75$; A FCM remains remains welfare improving as long as $\phi \geq 0.53$. If $\phi \geq 1.8$, then the welfare benefits of the FCM will be greater than those of the transparency counterfactual.

6 Conclusion

This paper studies the causes and consequences of underinsurance using detailed contract data from homeowners affected by a major wildfire in Colorado. We document widespread underinsurance both before and after the fire, and show that it delays rebuilding and increases the likelihood that survivors sell their homes.

We examine why underinsurance is so common. Our evidence rules out several standard explanations in our setting, such as construction cost inflation, infrequent policy updates, moral

[25]The Small Business Administration paid approximately \$100 million in disaster loans at subsidized interest rates to underinsured survivors of the Marshall Fire (Cookson et al., 2023).

hazard, and adverse selection. Although coverage ratios rise with income, higher-income households still tend to be underinsured. Instead, we find large differences in underinsurance across insurers that cannot be explained by the differential sorting of buyers, and remain after controlling for detailed policyholder and property characteristics. Insurers offering more complete coverage have more local experience and higher perceived brand quality, suggesting that reputation and soft information influence coverage limits.

These findings are consistent with qualitative reports that consumers rely on agents and their valuation tools to set coverage, and pay limited attention to coverage differences across quotes. To test for such "coverage neglect," we estimate a discrete choice model of homeowners' insurance demand. Buyers are highly responsive to headline premiums but largely unresponsive to coverage differences when comparing prices across insurers. A transparency counterfactual that enables consumers to compare premiums at the same level of coverage across all insurers increases average consumer surplus by at least $290 per year—about 10% of average annual premiums—mainly through reallocation toward higher-quality insurers. A full coverage mandate would similarly eliminate coverage neglect by standardizing limits across insurers.

Overall, our results show that limited attention to coverage A limits is a central friction in homeowners insurance markets. As climate-driven disasters intensify, leading to more total loss events, addressing this friction is critical for policies aimed at improving household resilience.

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7 Tables

Table 1: Summary Statistics

This table presents summary statistics using our estimation sample. Unless otherwise specified, variables are measured just before the Marshall Fire. Insurance contract data come from the Colorado Division of Insurance (DOI). Housing characteristics come from the Boulder County Assessor's Office. Personal credit characteristics are from a major credit bureau.

	mean	sd	p25	p75
Insurance and Rebuild Characteristics				
Structure Coverage ($100s)	5,253	2,481	3,680	6,194
Extended (ERC) Coverage (% of Structure Coverage)	0.266	0.167	0.200	0.250
Guaranteed Replacement Cost Coverage (0/1)	0.0680	0.252	0	0
Policy Age (Years)	9	8.676	2	14
Replacement Cost ($100s, Post-Fire)	7,571	1,811	6,432	8,557
Replacement Cost ($100s)	5,911	1,414	5,022	6,680
Coverage Ratio	0.874	0.268	0.693	0.988
Coverage Ratio (Post-Fire)	0.873	0.272	0.688	1
Premium per $100 of Coverage	0.577	0.210	0.423	0.704
Policyholder Characteristics				
Square Footage	2,305	888.9	1,698	2,802
Home Value ($1,000s)	978	355	758	1,083
Mortgage Balance ($1,000s)	437	375	222	536
Household Income ($1,000s)	197	101	120	263
Credit Score	798.3	40.76	785.5	824
Has Mortgage (0/1)	0.732	0.443	0	1
Tenure in Home (Years)	14.30	10.35	5	22
Home Age (Years)	31.59	12.32	26	34
New Insurer Since Buying Home (0/1)	0.463	0.499	0	1
Policyholder Sample Size:	3089			
Insurance Company Sample Size:	14			

Table 2: The Role of Local Knowledge and Reputation on Average Coverage Ratios Across Insurers

This table presents regression estimates where the dependent variable is the policyholder's chosen insurer's average pre-fire coverage ratio, and the explanatory variables are the chosen insurer's Colorado homeowners market share in 2021 (in percentage points), age of oldest policy (in decades) in the estimation sample, and overall quality rating from the J.D. Power 2021 U.S. Home Insurance Study (scaled to a score out of 100 points). Panel (a) includes no policyholder characteristics as controls, while panel (b) introduces: log income, credit score, log home value, mortgage status, log replacement value, premium per $100 of coverage at policyholder's estimated replacement cost, year of original policy purchase fixed effects, and quadratics of tenure in home, home age, and square footage. The standard errors, shown in parentheses, are clustered by insurer: *** p<0.01, ** p<0.05, * p<0.1

Dep. variable:	100 X Pre-Fire Coverage Ratio			
	(1)	(2)	(3)	(4)
CO Market Share (p.p.)	1.06***			0.90***
	(0.11)			(0.13)
Insurer Decades in Market		5.77**		2.59**
		(2.17)		(1.19)
Quality Rating			1.68*	0.82
			(0.79)	(0.48)
Observations	3,089	3,089	3,089	3,089
R-squared	0.094	0.051	0.022	0.114
Policyholder Characteristics	N	N	N	N

(a) No Policyholder Controls

Dep. variable:	100 X Pre-Fire Coverage Ratio			
	(1)	(2)	(3)	(4)
CO Market Share (p.p.)	0.80***			0.74***
	(0.12)			(0.11)
Insurer Decades in Market		3.67**		2.04
		(1.66)		(1.40)
Quality Rating			1.46***	0.87*
			(0.36)	(0.45)
Observations	3,089	3,089	3,089	3,089
R-squared	0.445	0.420	0.422	0.459
Policyholder Characteristics	Y	Y	Y	Y

(b) With Policyholder Controls

Table 3: Underinsurance and Reputational Risk

This table presents estimates on the relationship between filing a complaint with the division of insurance and being underinsured from the response-level United Policyholder survey data of policyholders with total losses following the 2007 San Diego Fire, 2017 North Bay Fire, 2018 Camp Fire, and 2021 Marshall Fire. Column (1) is a bivariate regression of whether a respondent reported filing a complaint with their division of insurance over whether they reported not having enough coverage to rebuild their home. Column (2) introduces fire-by-insurer fixed effects, and column (3) excludes respondents from the Marshall Fire survey. Insurers with fewer than 20 respondents are grouped together for calculating fixed effects. Dependent variables are multiplied by 100 for interpretability. Robust standard errors are shown in parenthesis: *** p<0.01, ** p<0.05, * p<0.1

Dep. variable: 100 X	Filed Complaint		
	(1)	(2)	(3)
Underinsured	9.459***	9.130***	7.848***
	(1.718)	(1.829)	(1.975)
Observations	1,696	1,696	1,358
R-squared	0.016	0.056	0.054
Fire-by-Insurer Fixed Effects	N	Y	Y

Table 4: The Role of Underinsurance on Policyholders' Rebuilding and Sales Decisions

This table presents second-stage 2SLS IV regression estimates where the outcome variables are indicators for whether the policyholder rebuilt their home (cols 1-3) or sold their home without rebuilding (cols 4-6) as of December 2022, 2023, and 2024. The top panel includes policyholder charateristics covariates (log income, standardized credit score, log home value, log replacement value, and quadratics of time since home purchase, home age, and square footage), and the bottom panel adds insurer characteristics (Colorado homeowners 2021 market share, age of oldest policy, J.D. Power 2021 U.S. Home Insurance Study quality rating, AM Best 2021 credit rating, and median contents coverage limit as a fraction of the coverage A limit). Dependent variables are multiplied by 100 for interpretability. We instrument for pre-fire and post-fire coverage ratios, respectively, with the corresponding LOO insurer averages. First stage regression shown in Appendix Table A2, with the first stage F-stat and dependent variable means are listed at the bottom of the table. The estimation sample is subset to owners of destroyed homes. Standard errors, shown in parentheses, are clustered by insurer: *** p<0.01, ** p<0.05, * p<0.1

Dep. variable: 100 X	Rebuilt By:			Sold Vacant Lot By:		
	2022	2023	2024	2022	2023	2024
	(1)	(2)	(3)	(4)	(5)	(6)
Coverage Ratio (Pre-Fire)	30.37**	36.37**	8.76	-19.33***	-28.59**	-27.54**
	(12.16)	(17.78)	(7.03)	(6.75)	(11.52)	(12.44)
Observations	736	736	736	736	736	736
F-Stat	144.26	144.26	144.26	144.26	144.26	144.26
Policyholder Characteristics	Y	Y	Y	Y	Y	Y
Insurer Characteristics	N	N	N	N	N	N
Dep. Var. Mean	14.27	66.85	80.98	3.80	9.10	12.77
Dep. variable: 100 X	Rebuilt By:			Sold Vacant Lot By:		
	2022	2023	2024	2022	2023	2024
	(1)	(2)	(3)	(4)	(5)	(6)
Coverage Ratio (Pre-Fire)	-2.85	82.02**	20.42	-18.86**	-53.88***	-65.31***
	(21.88)	(29.97)	(16.71)	(7.05)	(10.66)	(13.11)
Observations	736	736	736	736	736	736
F-Stat	32.66	32.66	32.66	32.66	32.66	32.66
Policyholder Characteristics	Y	Y	Y	Y	Y	Y
Insurer Characteristics	Y	Y	Y	Y	Y	Y
Dep. Var. Mean	14.27	66.85	80.98	3.80	9.10	12.77

Table 5: Do Coverage Limits Keep Pace with Construction Cost Inflation?

This table presents OLS regression estimates from a regression of percent changes in coverage A limits (dependent variable) on percent changes in construction costs between the year each policy was first written and 2021. The sample includes policies that have been renewed at least once. Column (1) presents the bivariate relationship, whereas column (2) adds controls and column (3) adds insurer fixed effects. To test the impact of home improvements, Column (4) excludes policies where the insured square footage changed between policy inception and the time of the loss. Robust standard errors are in parentheses: *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	Dep variable: % Increase in Coverage A Limit			
	(1)	(2)	(3)	(4)
% Increase in Construction Costs	1.51***	1.47***	1.26***	1.28***
	(0.10)	(0.10)	(0.10)	(0.14)
Log Income		2.25	2.96	0.60
		(3.32)	(3.19)	(3.52)
Credit Score (z)		1.01	1.36	1.75*
		(1.06)	(0.99)	(0.98)
Log Home Value		8.85	11.49*	24.72***
		(6.78)	(6.61)	(7.68)
Has Mortgage = 1		-2.70	-2.45	0.10
		(3.49)	(3.22)	(3.13)
Log Replacement Cost		9.38	18.25	26.59
		(23.52)	(23.02)	(24.56)
Purchase Age (Decades)		-26.48**	-25.55*	-29.47*
		(13.37)	(13.22)	(15.28)
Purchase Age^2		8.32**	8.91**	10.15**
		(4.06)	(4.04)	(4.72)
Home Age (Decades)		-5.44	-5.04	-9.50*
		(4.98)	(4.91)	(5.20)
Home Age^2		0.77	0.69	1.11*
		(0.58)	(0.57)	(0.62)
Square Footage (1000s)		-22.50*	-22.21**	-35.00***
		(11.55)	(11.06)	(11.42)
Square Footage^2		3.23**	2.21*	3.15***
		(1.33)	(1.23)	(1.20)
Observations	2,705	2,705	2,705	2,309
R-squared	0.452	0.470	0.522	0.504
Insurer FE	N	N	Y	Y

Table 6: The Role of Mortgages and Leverage on Underinsurance

This table presents OLS regression estimates from regressions of policyholders' pre-fire coverage ratios multiplied by 100 (dependent variable) on mortgage measures. In Columns 1-3, the key explanatory variable is an indicator variable for having an outstanding mortgage. Column 4 replaces this indicator with bins of the mortgage loan-to-value ratio and sets the omitted category to policyholders without a mortgage. Specified columns include policyholder covariates (log income, standardized credit score, log home value, log replacement value, and quadratics of time since home purchase, home age, and square footage) and insurer fixed effects. Robust standard errors are in parentheses: *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	Dep. variable: 100 X Coverage Ratio			
	(1)	(2)	(3)	(4)
Has Mortgage = 1	-2.46**	-2.04*	-1.26	
	(1.13)	(1.22)	(1.13)	
Loan-to-Value (%)				
0-10				1.87
				(2.14)
10-20				-0.94
				(1.56)
20-30				-2.53*
				(1.41)
30-40				-3.75***
				(1.37)
40-50				-0.56
				(1.53)
50-60				-1.11
				(1.61)
60-70				-0.04
				(1.95)
70-80				1.16
				(2.26)
80-90				1.93
				(3.46)
90-100				4.37*
				(2.34)
Observations	3,089	3,089	3,089	3,089
R-squared	0.002	0.340	0.472	0.476
Insurer FE	N	N	Y	Y
Policyholder Characteristics	N	Y	Y	Y

Table 7: Adverse Selection: Do Owners of Riskier Homes Buy More Coverage?

Panel (a) presents regressions of whether a home experienced a total loss (columns 1 and 2, multiplied by 100 for interpretability) and the premium per $100 of coverage at full coverage (columns 3 and 4) over an indicator variable for whether the home was constructed with a wood frame as opposed to a brick frame. Column (2) introduces structure characteristic controls, and column (4) introduces policyholder and structure characteristic controls as well as insurer fixed effects. Panel (b) presents regressions of the pre-fire coverage ratio multiplied by 100 for interpretability over an indicator variable for whether the home was constructed with a wood frame as opposed to a brick frame. In column (4), rather than regressing coverage ratios over the wood frame indicator, an indicator for whether a home had a total loss is the primary independent variable. Column (2) adds structure and policyholder characteristic controls, while columns (3) and (4) add insurer fixed effects. The estimation sample is subset to homes that were inside the fire perimeter. Heteroskedasticity robust standard errors are shown in parentheses. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

Dep. variable:	100 X Total Loss		Premium per $100 Coverage	
	(1)	(2)	(3)	(4)
Wood Frame	50.03***	54.85***	0.005	0.001
	(3.49)	(3.10)	(0.016)	(0.005)
Observations	1,108	1,108	1,108	1,108
R-squared	0.151	0.213	0.000	0.936
Insurer FE	N	N	N	Y
Home Characteristics	N	Y	N	Y
Policyholder Characteristics	N	N	N	Y

(a) Wood frames, risk, and premiums

	Dep. variable: 100 X Coverage Ratio			
	(1)	(2)	(3)	(4)
Wood Frame	-3.34	-0.63	-1.47	
	(2.54)	(2.04)	(1.86)	
Total Loss				2.35*
				(1.35)
Observations	1,108	1,108	1,108	1,108
R-squared	0.002	0.376	0.495	0.496
Insurer FE	N	N	Y	Y
Policyholder Characteristics	N	Y	Y	Y

(b) Wood frames and underinsurance

Table 8: Insurer Discrete Choice: Do Buyers Shop Based on Coverage-adjusted Premiums?

This table presents estimates of parameters from a multinomial choice model of the following latent utility function given in Equation (8):

$$V_{ij} = \sigma_j X_i + \zeta_j - \alpha^r \frac{p_{ij}(R_i^*)}{R_i^*} - \alpha^n \frac{p_{ij}(\widehat{R_{ij}})}{R_i^*} + \epsilon_{ij}.$$

Premiums are calculated according to insurer rate schedules $p_{ij}(R)$, with the coverage-adjusted premium set to the policyholder's observed coverage choice, R_i^*, and the quoted premium to the predicted insurer coverage, R_{ij}. Premiums are normalized as the cost per \$10,000 of coverage at R_i^*. All specifications include insurer fixed effects and policyholder characteristics (log income, credit score, mortgage status, log home value, log replacement cost, and quadratics of home age, square footage, and years since home purchase) interacted with insurer dummies to flexibly account for different consumer preferences for specific insurers. Robust standard errors are in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	(1)	(2)	(3)
Coverage-Adjusted Premium (α^r)	1.674***		-1.150
	(0.354)		(0.802)
Quoted Premium (α^n)		4.461***	4.901***
		(0.801)	(1.061)
Insurer Fixed Effects	Y	Y	Y
Insurer × Policyholder Characteristics	Y	Y	Y
N	43246	43246	43246

Table 9: Insurer Discrete Choice: Heterogeneity by Income, Mortgage Status, and Search Costs

Similar to Table 8, this table presents estimates of parameters from a multinomial choice model of the following latent utility function given in Equation (8), but in addition, we consider heterogeneity in price sensitivity by *High Income*, an indicator for above median income (column 1), *Mortgage*, which indicates whether the household has a mortgage (column 2), and *Shopped*, which equals one when a homeowner's policy start date is later than the home purchase date, indicating that they changed homeowners insurance policies since buying their home (column 3). Premiums are calculated according to insurer rate schedules $p_{ij}(R)$, with the quoted premium set to the policyholder's observed coverage choice, R_i^*, and the coverage neglect premium to the predicted insurer coverage, R_{ij}. Premiums are normalized as the cost per \$10,000 of coverage at R_i^*. All specifications include insurer fixed effects and policyholder characteristics interacted with insurer dummies to flexibly account for different consumer preferences for specific insurers. Robust standard errors are in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	(1)	(2)	(3)
Coverage-Adjusted Premium (α^r)	-1.411*	-0.235	-0.109
	(0.751)	(1.001)	(1.084)
Quoted Premium (α^n)	5.097***	3.516***	2.589*
	(0.782)	(0.905)	(1.395)
High Income X α^r	0.580		
	(1.433)		
High Income X α^n	-0.400		
	(1.360)		
Mortgage X α^r		-1.446	
		(1.492)	
Mortgage X α^n		2.142	
		(1.639)	
Shopped X α^r			-1.467
			(1.085)
Shopped X α^n			3.606***
			(1.036)
N	43246	43246	43246
Insurer Fixed Effects	Y	Y	Y
Insurer × Policyholder Characteristics	Y	Y	Y

8 FIGURES

Figure 1: Underinsurance Across Major Wildfires (2007–2021)

This figure plots the share of policyholders who report not having enough coverage to repair or rebuild their home in "Roadmap to Recovery" surveys fielded by United Policyholders. The underinsurance shares are as reported by United Policyholders in each one year follow-up survey (https://uphelp.org/media/surveys/). The underinsurance share for the Marshall Fire is only reported for respondents with total losses, whereas the other surveys include respondents with partial damages (who are, therefore, less likely to be underinsured relative to their damages).

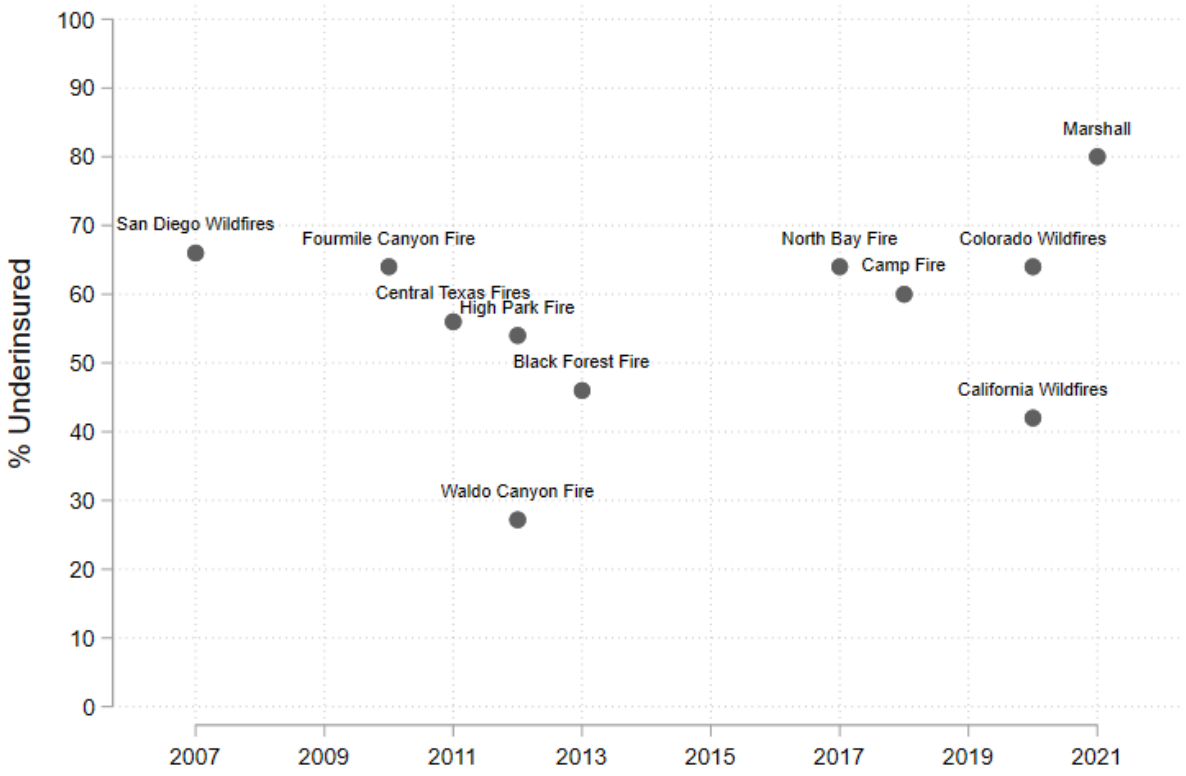
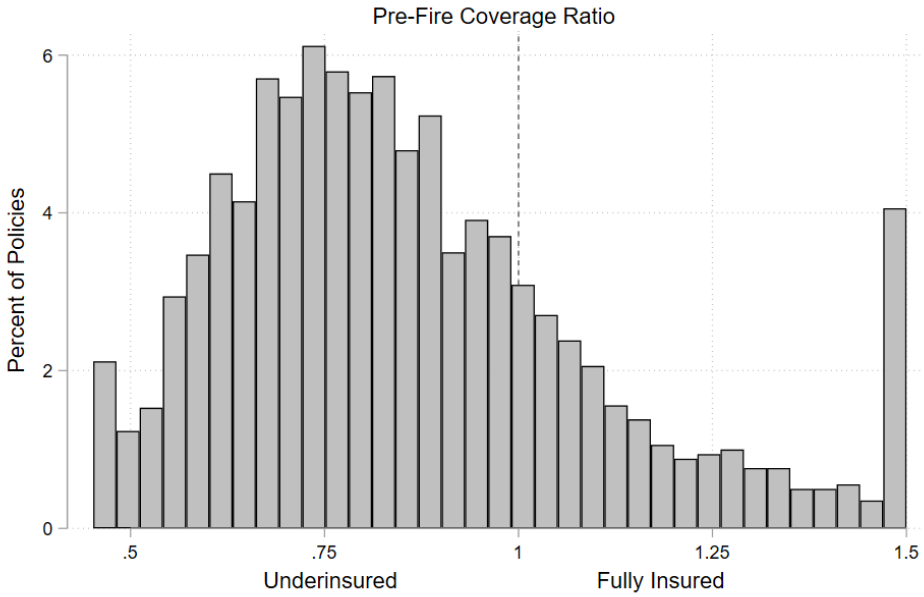


Figure 2: The Distribution of Underinsurance Pre- and Post-Fire

The top panel shows the distribution of pre-fire coverage ratios, calculated as policyholders' coverage A limits divided by pre-fire replacement costs (indexed to Q1 2021). The bottom panel plots the same distribution using post-fire replacement costs (captured as of Q1 2023) and adding any ERC provisions to the numerator. We make two adjustments to improve the comparative visibility of these graphs. First, we group coverage ratios above 1.5 together at 1.5. Second, the bottom panel excludes the 6.9% of policyholders with guaranteed replacement cost coverage who would otherwise cluster at a coverage ratio of 1.



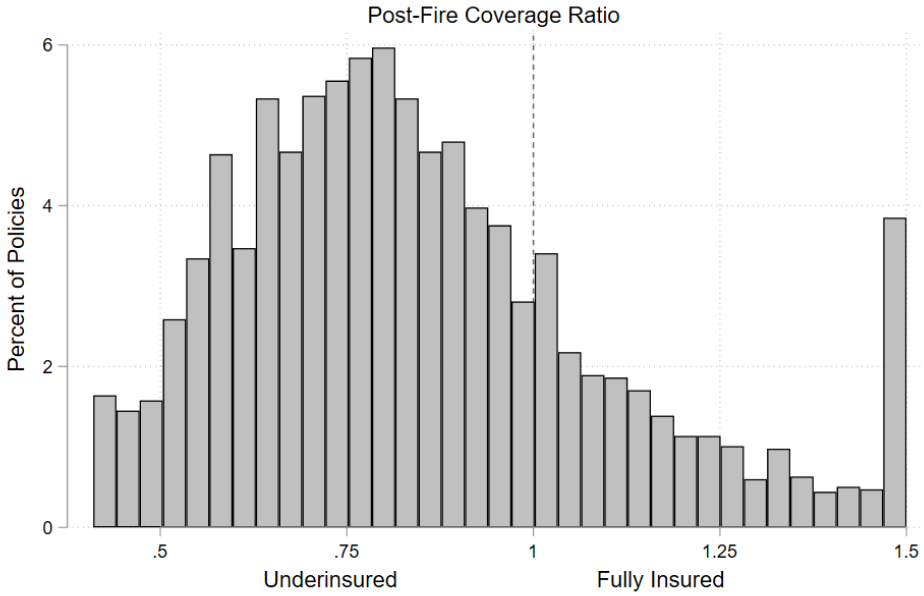
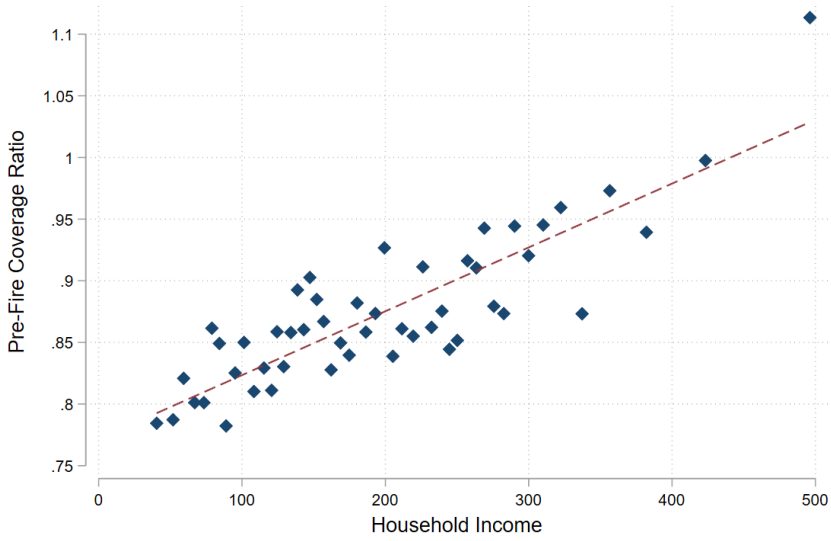
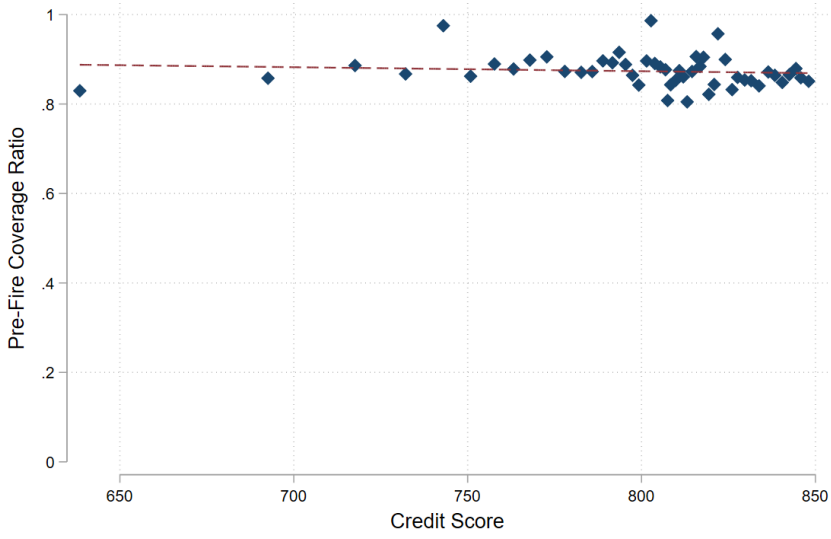


Figure 3: Underinsured Share by Policyholder Characteristics

This figure plots the share of policyholders who are underinsured according to their pre-fire coverage ratio, binned according to 50 quantiles of household income (top) and credit score (bottom).



(a)



(b)

Figure 4: Heterogeneity in Average Coverage Ratios Across Insurers

This figure plots the number of insurers in our estimation sample by bins of insurer average pre-fire coverage ratio.

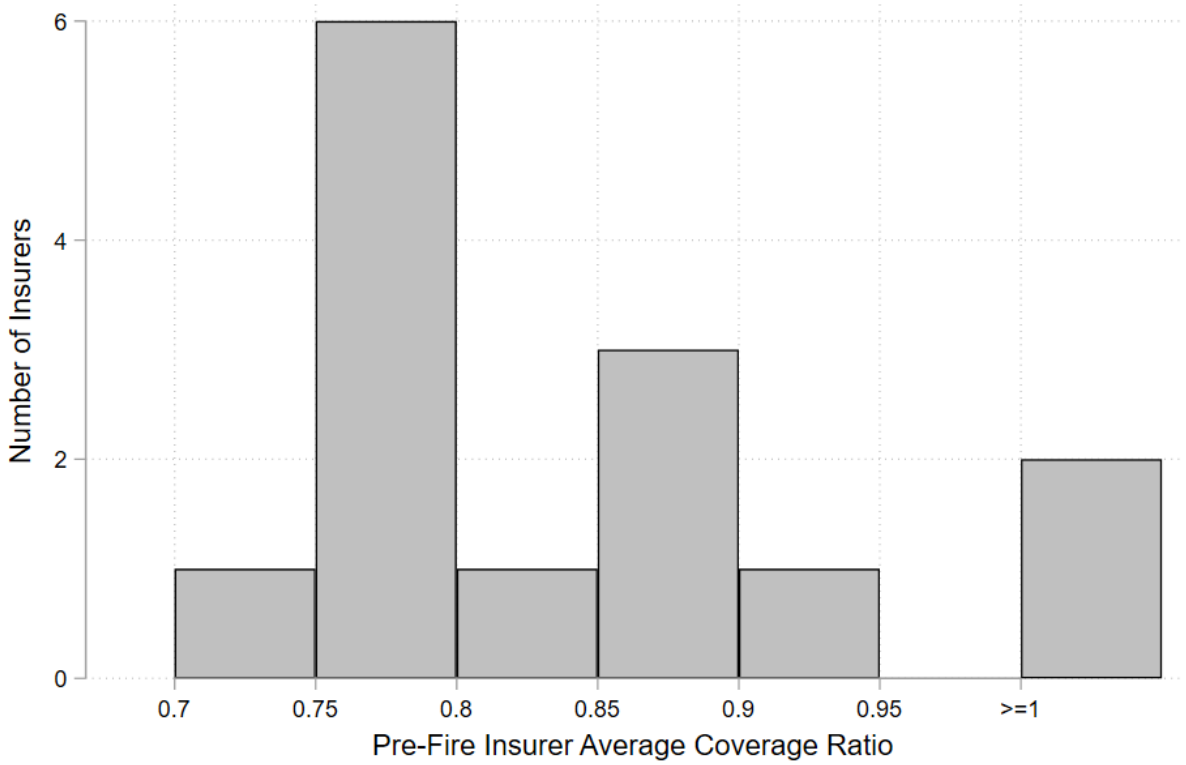


Figure 5: Relationship Between Average Policyholder Income and Average Coverage Ratio

This figure plots the average policyholder household income (y-axis) and pre-fire coverage ratio (x-axis) for the 14 insurers in our estimation sample.

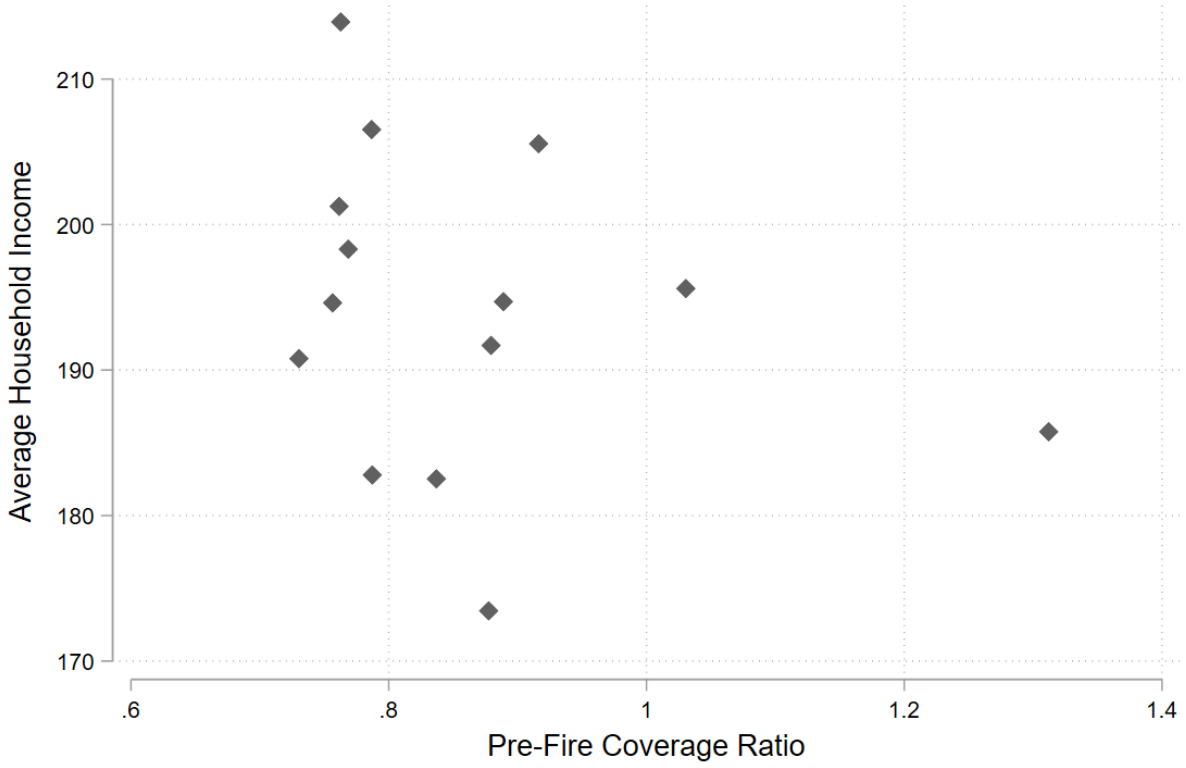


Figure 6: The Effect of Insurer Fixed Effects on Policyholder Coverage Ratios

This figure plots the coefficients on the insurer fixed effects when the dependent variable is each policyholder's pre-fire coverage ratio multiplied by 100. These coefficients are estimated without controls for policyholder characteristics (red) and with such controls (blue). The fixed effect for insurer 8 is omitted (with a coefficient set to zero). Lines represent the 95% confidence interval around the estimate; confidence intervals for "No Controls" specification are included but so small as to be negligible.

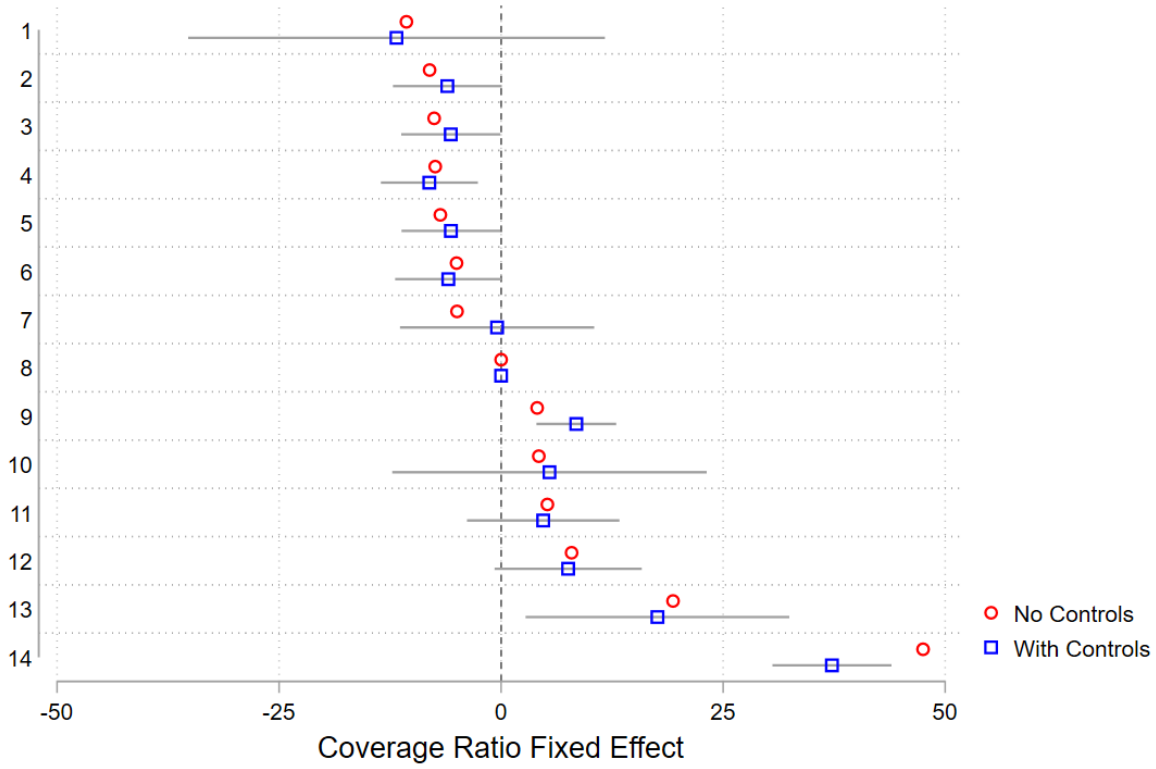


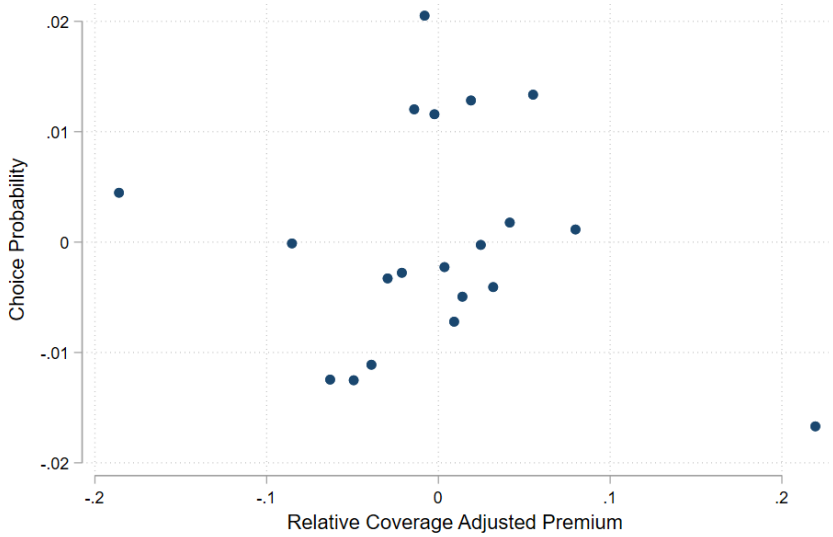
Figure 7: Underinsured Share by Age of the Policy

This figure plots the share of policyholders who are underinsured (according to their pre-fire coverage ratio) by the year they first originated the homeowners insurance policy they had at the time of the fire. For visibility, we pool policyholders who bought on or before 1990 into the 1990 bin.

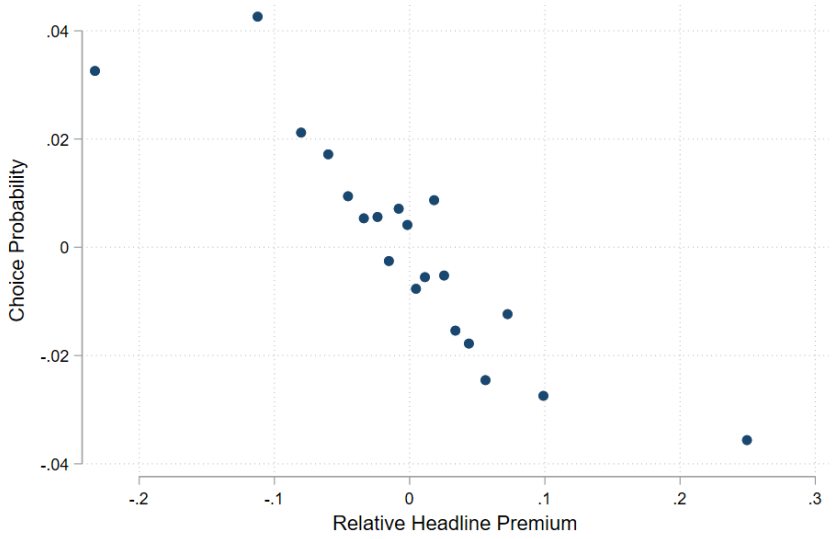


Figure 8: Buyers Sensitive to Relative Headline Premiums Not Coverage-Adjusted Premiums

This figure shows the relationship between relative choice probabilities (y-axis) and relative prices on the y-axis. The top panel shows the relative coverage-adjusted premium on the x-axis, which is defined as the insurer's premium at the buyer's chosen coverage limit from the data ($p_{ij}(R_i^*)$). The bottom panel shows the relative headline premium on the x-axis, which is defined as the insurer's premium at their predicted quoted coverage limit ($p_{ij}(\widehat{R_{ij}})$). Both premiums are normalized by the policyholder's purchased coverage in \$100s. The relative choice probabilities and premiums are taken as the residuals from regressions on insurer and buyer fixed effects in the discrete choice estimation data.



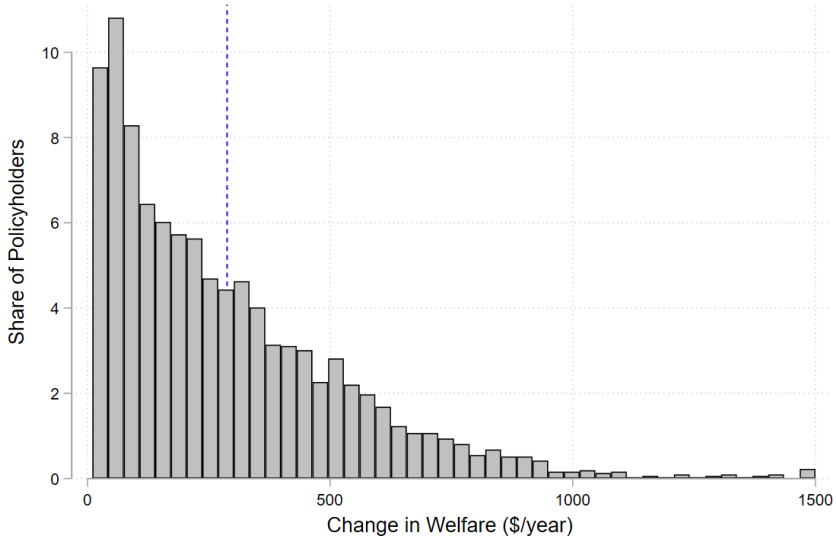
(a)



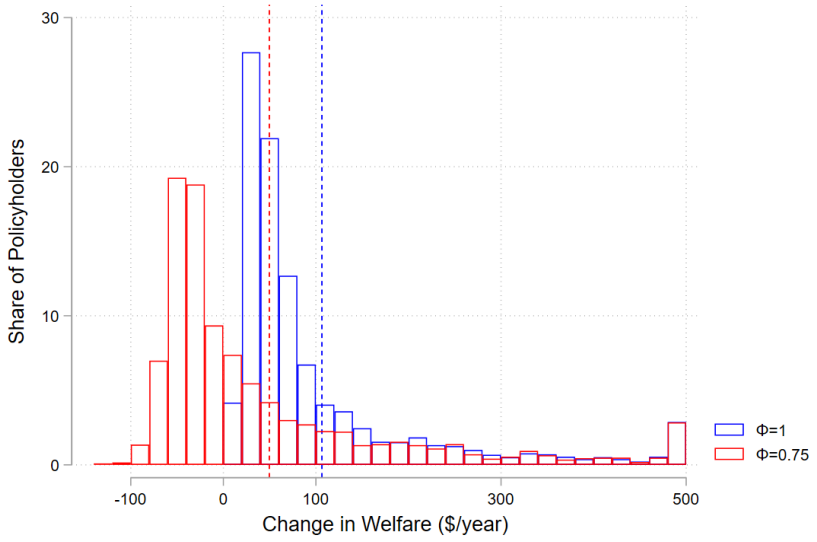
(b)

Figure 9: Consumer Welfare Effect of Removing Coverage Neglect.

This figure plots the distribution of partial equilibrium consumer welfare effects under the transparency (top panel) and full coverage mandate (FCM, bottom panel) counterfactuals relative to the baseline with coverage neglect. For the FCM counterfactual, we show separate welfare estimates for the cases where policyholders value extra coverage at its marginal cost ($\phi = 1$, in red) and at 75% of its marginal cost ($\phi = 0.75$, in blue). The vertical lines represent the average welfare gains under each counterfactual. For visibility, values above \$1,500 (\$500) are grouped together at \$1,500 (\$500) in the transparency (FCM) counterfactual.



(a)



(b)

A Appendix Tables and Figures

Table A1: Coefficients on Covariates from Equation (1)

This table presents the results from regressing policyholders' pre-fire coverage ratios (multiplied by 100 for interpretability) on insurer fixed effects without any additional covariates (column 1) and with policyholder covariates (column 2). The insurer fixed effect coefficients are plotted in Figure 6. Standard errors, shown in parentheses, are clustered by insurer: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	Dep. variable: 100 X Coverage Ratio	
	(1)	(2)
Premium per $100 at Full Coverage		-3.56
		(10.94)
Log Income		5.26***
		(0.89)
Credit Score (z)		-0.73
		(0.49)
Log Home Value		55.22***
		(10.20)
Has Mortgage (0/1) = 1		-1.23
		(1.33)
Log Replacement Cost		-75.93***
		(5.83)
Purchase Age (Decades)		2.25
		(1.69)
Purchase Age^2		0.19
		(0.33)
Home Age (Decades)		-9.82***
		(2.22)
Home Age^2		0.68***
		(0.20)
Square Footage (1000s)		10.52*
		(5.56)
Square Footage^2		-0.06
		(0.50)
Observations	3,089	3,089
R-squared	0.164	0.472

Table A2: First Stage Estimation of Policyholder Coverage Ratios

This table presents the first stage estimation results from regressing policyholders' pre-fire coverage ratios on their insurer's corresponding leave-one-out (LOO) average coverage ratios conditional on policyholder characteristics (column 1) and additional insurer characteristics (column 2). These variables represent the endogenous outcome variables and the instruments, respectively, in our 2SLS IV. The estimation sample is subset to owners of destroyed homes. Standard errors, shown in parentheses, are clustered by insurer: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Dep. variable:	Pre-Fire Coverage Ratio (1)	Pre-Fire Coverage Ratio (2)
Av. Coverage Ratio (LOO)	0.722***	0.828***
	(0.060)	(0.145)
Log Income	0.096***	0.097***
	(0.024)	(0.024)
Log Home Value	0.469***	0.467***
	(0.101)	(0.102)
Log Replacement Cost	-0.993***	-0.956***
	(0.147)	(0.160)
Credit Score (z)	-0.000	-0.002
	(0.007)	(0.007)
Mortgage	-0.035	-0.038
	(0.029)	(0.028)
Purchase Age (Decades)	0.045**	0.050**
	(0.018)	(0.017)
Purchase Age^2	-0.008	-0.008
	(0.006)	(0.006)
Home Age (Decades)	-0.071***	-0.071***
	(0.023)	(0.022)
Home Age^2	0.005**	0.005**
	(0.002)	(0.002)
Square Footage (1000s)	0.239***	0.223***
	(0.064)	(0.056)
Square Footage^2	-0.011*	-0.010**
	(0.006)	(0.005)
Policy Age (Decades)	0.001	0.000
	(0.005)	(0.006)
Policy Age^2	0.000	0.000
	(0.000)	(0.000)
Quality Rating		0.001*
		(0.001)
CO Market Share (p.p.)		0.004
		(0.002)
Insurer Decades in Market		-0.016
		(0.026)
Other Coverages (% of Cov A)		0.039
		(0.069)
AMBest A+		-0.001
		(0.024)
AMBest A++		-0.103
		(0.064)
Observations	736	736
F-Stat	144.26	32.66
R-squared	0.464	0.473

Figure A1: Relative Underinsurance by Insurer Across Wildfires

This figure plots the share of underinsured policyholders by insurer for each fire from the response-level United Policyholders survey data. The y-axis plots the share of policyholders who report not having enough coverage to rebuild their home for each insurer relative to the overall rate of reported underinsurance in that survey. For each fire, insurers with fewer than 20 respondents are aggregated together and shown by the markers in blue.

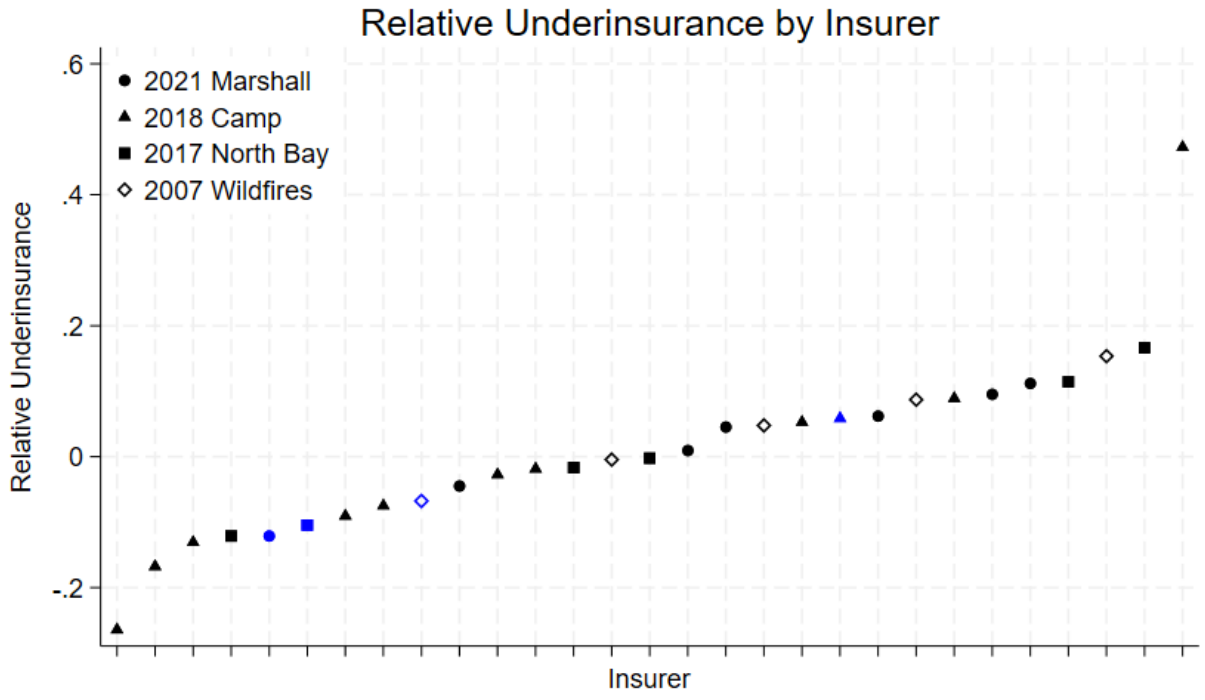


Table A3: Underinsurance and Recovery: Robustness with Post-Fire Coverage Ratios

This table presents second-stage 2SLS IV regression estimates where the outcome variables are indicators for whether the policyholder filed a rebuilding permit (panel a) or sold their home (panel b) before December 2022 (cols 1 and 3) and October 2023 (cols 2 and 4). Dependent variables are multiplied by 100 for interpretability. We instrument for pre-fire and post-fire coverage ratios, respectively, with the corresponding LOO insurer averages. The predicted values from this first stage regression (shown in Appendix Table A2) form the key explanatory variables. The first stage F-stat and dependent variable means are listed at the bottom of the table. The estimation sample is subset to owners of destroyed homes. Standard errors, shown in parentheses, are clustered by insurer: *** p<0.01, ** p<0.05, * p<0.1

Dep. variable: 100 X	Rebuilt By:			Sold Vacant Lot By:		
	2022	2023	2024	2022	2023	2024
	(1)	(2)	(3)	(4)	(5)	(6)
Coverage Ratio (Post-Fire)	29.89***	64.17***	10.99	-27.98***	-51.24***	-50.37***
	(10.73)	(12.88)	(8.87)	(6.23)	(6.65)	(10.93)
Observations	736	736	736	736	736	736
F-Stat	38.59	38.59	38.59	38.59	38.59	38.59
Policyholder Characteristics	Y	Y	Y	Y	Y	Y
Insurer Characteristics	N	N	N	N	N	N
Dep. Var. Mean	14.27	66.85	80.98	3.80	9.10	12.77

Table A4: Test of Whether Choice of Insurer is Correlated with Unobserved Propensity to Move

This table presents the results of a falsification test that asks whether homeowners with a higher propensity to move, irrespective of fire losses, sort into insurers that tend to write less coverage. We regress a policyholder home sale indicator variable on their coverage ratio, restricting the sample to homes that were not destroyed by the fire. The dependent variable captures whether the policyholder sold their home before December 2022 (multiplied by 100 for interpretability). In column 1, the primary independent variable is the policyholder's instrumented pre-fire coverage ratio (the fitted values from the first-stage regression in Appendix Table A2), while column 2 uses the instrumented post-fire coverage ratio. Standard errors, shown in parentheses, are clustered by insurer: *** p<0.01, ** p<0.05, * p<0.1.

Dep. variable: 100 X	Sold Home By:		
	2022	2023	2024
	(1)	(2)	(3)
Coverage Ratio (Pre-Fire)	3.17	-0.48	2.25
	(3.91)	(6.64)	(5.92)
Observations	2,353	2,353	2,353

Table A5: The Relationship between Policy Age and Underinsurance

This table presents OLS estimates from a regression of pre-fire coverage ratios (multiplied by 100 for interpretability) on policy age (measured as years since policy inception). Specified regressions control for policyholder characteristics that may correlate with both policy age and coverage amounts. Column (3) adds insurer fixed effects. Robust standard errors are in parentheses: *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	Dep variable: 100 X Coverage Ratio		
	(1)	(2)	(3)
Policy Age	0.67***	0.71***	0.31***
	(0.06)	(0.06)	(0.06)
Log Income		4.92***	4.94***
		(0.97)	(0.92)
Credit Score (z)		-1.00**	-0.70*
		(0.43)	(0.41)
Log Home Value		54.08***	54.48***
		(4.04)	(4.06)
Has Mortgage = 1		-1.59	-1.10
		(1.18)	(1.12)
Log Replacement Cost		-75.78***	-75.62***
		(7.55)	(6.98)
Purchase Age (Decades)		-0.85	0.20
		(1.45)	(1.31)
Purchase Age^2		0.38	0.37
		(0.44)	(0.39)
Home Age (Decades)		-9.47***	-9.66***
		(1.38)	(1.34)
Home Age^2		0.70***	0.68***
		(0.13)	(0.12)
Square Footage (1000s)		9.79**	11.62***
		(3.98)	(3.68)
Square Footage^2		0.36	-0.18
		(0.53)	(0.49)
Observations	3,088	3,088	3,088
R-squared	0.047	0.375	0.478
Insurer FE	N	N	Y

B Measuring Premiums and Quoted Coverage Limits

As described in Section 3, we estimate the premium schedule for four insurers who do not appear in the Quadrant data but reported their premiums in the Colorado Division of Insurance data call. We estimate the premium schedule to be:

$$p_{ij} = \lambda_j + \gamma^j(Cov_i + Cov_i^2) + \alpha^j X_i + \epsilon_{ij}. \tag{A1}$$

The dependent variable is the annual premium. We include insurer fixed effects and insurer-specific coefficients on a quadratic of the amount of coverage purchased, and property and household characteristics (log zestimate, standardized credit score, log income, extended replacement cost coverage provisions as a percentage of coverage A, building condition rating fixed effects (excellent, good, fair, poor), single versus double story home indicator, asphalt roof indicator, masonry frame indicator,and quadratics of years since policy purchase, home age, and square footage). We use the leave-one-out fitted values (i.e., we estimate Equation A1 excluding the focal policyholder i) as the estimated premium for a given policy i with insurer j across different coverage limits Cov_{ij}.

To validate our approach, we use three insurers who both have their rate filings in Quadrant and reported their premiums in the data call. We use this overlapping sample to assess the quality of our premium estimates. The quadrant premiums and our estimated premium schedules all have correlations coefficients between 0.7 and 0.8 for each of the three insurers.

We also impute the quoted coverage limits homeowners would receive from each insurer. To do so, we estimate:

$$R_{ij} = \lambda_j + \beta X_i + \epsilon_{ij}, \tag{A2}$$

where X_i are property and policyholder characteristics: log zestimate, standardized credit score, log income, building condition rating fixed effects (excellent, good, fair, poor), and quadratics of home age and square footage. As with our fitted premium estimates from Equation A1, we use leave-one-out fitted values from the estimated Equation A2.

C Welfare Calculations Following Leggett (2002)

The formulas for the welfare effects of our counterfactuals as follows. Let v_{ij}^n be the utility of buyer i with insurer j under coverage neglect and let v_{ij}^d be utility under deliberate underinsurance. Let π_{ij}^n be the probability that i chooses j under coverage neglect and let α^n be the estimated coverage neglect premium coefficient from column (2) of Table 8. Following derivations in Leggett (2002) on the value of information in discrete choice models, policyholder i's willingness-to-pay for the transparency counterfactual is:

$$WTP_i = \frac{1}{\alpha^n}(ln(\sum_j e^{v_{ij}^d}) - (ln(\sum_j e^{v_{ij}^n}) + \sum_j \pi_{ij}^n(v_{ij}^d - v_{ij}^n))). \tag{A3}$$

The intuition of Equation A3 is to take the difference between expected utility under the transparency counterfactual and the coverage neglect baseline while adding the adjustment term $\pi_{ij}^n(v_{ij}^r - v_{ij}^n)$ to correct for the policyholder's misperception of their normative utility under coverage neglect.