Coverage Neglect in Homeowners Insurance*

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December 11, 2024

Abstract

Most homeowners do not have enough insurance coverage to rebuild their house after a total loss. Using contract-level data from 24 homeowners insurance companies in Colorado, we show wide differences in average underinsurance across insurers that persist conditional on policyholder characteristics. Underinsurance matters for disaster recovery. Across households that lost homes to a major wildfire, each 10 p.p. increase in underinsurance reduces the likelihood of filing a rebuilding permit within a year by 4 p.p.. To understand why consumers purchase underinsured policies, we build a discrete choice insurance demand model. The results suggest that policyholders treat insurers that write less coverage as if they set lower premiums, forgoing options to get more coverage at the same premium from other insurers – a pattern we call *coverage neglect*. Our findings suggest that coverage limits are either not salient to consumers or they are difficult to estimate without the input of insurance agents. Under a counterfactual without coverage neglect, consumer surplus increases by $290 per year, or 10% of annual premiums, on average.

Keywords: Disaster Insurance; Disaster Recovery; Information Frictions and Limited Attention; Insurance Demand

JEL Codes: G22, G41, G52, G53, Q54, R22

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*We thank NBER Household Finance Small Grants Program, CU Boulder's Center for Ethics and Social Responsibility and the Center for Research on Consumer Financial Decision Making for funding. The views expressed in this paper are solely those of the authors and do not reflect the views of any of the associated funders or data providers. We are grateful to the Colorado Department of Insurance for their feedback and assistance with policy data. For helpful comments, we thank Patty Cookson, Michael Gropper, Stephen Karolyi, Joan Schmit, and Justin Sydnor. In addition, this draft has benefited from feedback at workshops at Purdue, Wisconsin School of Business, and University of Colorado Boulder. Any errors or omissions are the responsibility of the authors. No statements here should be treated as legal advice. Quadrant Information Services own the copyright to their respective data, which we use with permission. This draft has been reviewed to ensure non-disclosure of personal and insurer information by the Colorado Division of Insurance and a major credit bureau

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1 INTRODUCTION

Three-quarters of American homeowners report being confident that their property insurance is sufficient to fully replace their house in the event of a disaster (Swartz and Howard, 2020; Metlife, 2010). However, for decades, survivors of catastrophic natural disasters have routinely reported that they lack the coverage needed to rebuild their homes and expressed confusion about how their policy limits were originally set (Klein, 2018; United Policyholders, 2020; Hassani, 2013). As climate change intensifies, so too does the importance of understanding the factors contributing to the longstanding problem of underinsurance. Nonetheless, the economics literature is mostly silent on the topic. This paper aims to fill this gap by asking why homeowners are underinsured against total losses and what characteristics of households and insurers lead to underinsurance.[1]

The ideal data set for this exercise would contain granular information on the menu of insurance contract characteristics (e.g., premiums, coverage limits, provisions for extended coverage), matched with policyholder characteristics (e.g., income, home value, credit score) and the insurance companies they select. An analysis of *under*insurance also depends on accurate estimates of rebuilding costs. Finally, to gauge the economic significance of underinsurance, it is important to know if disaster-stricken households with larger gaps in insurance coverage experience worse recovery outcomes. Existing insurance data sources fall short of this ideal, typically reporting only premiums at an aggregate level and lacking policy-level detail.

In this paper, we examine new data that closely approximate this ideal. From the Colorado Division of Insurance (DOI), we have homeowners insurance contract details for 4,859 policyholders affected by the Marshall Fire, a major, unexpected wildfire that burned through a suburban area between Denver and Boulder, Colorado on December 30, 2021. These data cover 24 different insurance companies operating in the same market. For each insurance contract in our estimation sample, we see the insurer's name, standardized premiums calculated from regulatory rate filings, and detailed characteristics of the insurance contract, including coverage limits and any provisions for extended or full replacement cost coverage.[2] Critical for tracking underinsurance

[1] We define underinsurance as households having less homeowners insurance coverage than it costs to rebuild their home. This can arise for rational reasons (e.g., self-insurance) or behavioral reasons (e.g., inattention or inertia).

[2] Our analysis does not disclose details that can be traced back to any particular insurer or homeowner. Further, when combining the insurance data with other sensitive datasets – i.e., credit bureau data – we first convert policyholder address to a unique property code before performing all merges on a server without personally identifying information.

over time, we see changes in coverage amounts between the original policy inception date and the last renewal date prior to the fire.

We merge these detailed insurance contract data with anonymized data on the property and household characteristics of policyholders. In particular, we estimate the replacement cost of the insured structure using detailed physical characteristics and a construction cost estimator. We recalibrate these cost estimates according to real-world construction quotes from a subsample of disaster survivors. Next, after converting the address to a property code and removing name and address from the above data, we merge to credit data from a major credit bureau, which include information on credit score, estimated household income, and mortgage and credit card balances. We also obtain deeds and permitting records, allowing us to track recoveries and understand the real consequences of underinsurance. All told, these data provide an unprecedented level of detail to study the drivers of underinsurance.

Our data confirm the widespread and severe underinsurance reported after the Marshall Fire. We estimate that 74% of policyholders were underinsured after the fire, and 36% were *severely* underinsured with dwelling coverage limits equal to less than three-quarters of their home's replacement cost. A sharp increase in rebuilding costs after the fire does not fully explain the extent of underinsurance. Moreover, older policies are no more underinsured than newer ones, suggesting that underinsurance is not driven by policyholders failing to update their coverage limits over time. Policyholders with high mortgage balances are also no more or less underinsured than those without mortgages, suggesting that leveraged homeowners do not strategically underinsure and mortgage lenders do not prevent underinsurance by monitoring coverage amounts. Although higher-income households were less likely to be underinsured than lower-income homeowners, a majority of higher-income households were also underinsured.

Instead, the most important predictor of a policyholder's underinsurance is their chosen insurance company. Not only is there substantial variation in underinsurance across insurers, but the potency of insurer choice in predicting underinsurance is robust to accounting for policyholder characteristics. We do not find meaningful selection into insurers with different average coverage ratios by policyholder income, credit score, or home values. At the same time, we find no evidence that insurers ration coverage due to adverse selection (i.e., riskier homeowners selecting higher coverage limits). Instead, we find insurers with more local representation and knowledge, as proxied by the number and age of their policies, are less likely to underinsure. Hence, reputational capital and soft information may drive a wedge in average coverage ratios across insurance companies.

These patterns are consistent with numerous reports that insurers employ heterogeneous models to estimate replacement costs, which form the suggested coverage limits in initial quotes. While buyers have a degree of control over their chosen coverage limit, our results suggest that insurers anchor consumers' estimates of their replacement cost needs. To explain this phenomenon, we propose a model of "coverage neglect" where consumers do not fully internalize the differences in recommended coverage as they compare quotes across insurers. This concept has some basis in the qualitative conclusions of policyholder advocacy groups, which have argued that the software insurers employ frequently underestimates home replacement costs, thereby allowing insurers to cater to premium-sensitive buyers who are inattentive to true replacement costs (United Policyholders, 2024; Phillips, 2022).

As an alternative to coverage neglect, one may instead believe that the variation in underinsurance reflects preference heterogeneity and that households choose to underinsure under a "full information" hypothesis. Indeed, it is not *ex ante* obvious that household welfare is lower because of underinsurance. Underinsurance can arise rationally if, for example, homeowners face liquidity constraints or anticipate post-disaster aid (Ericson and Sydnor, 2018; Billings, Gallagher, and Ricketts, 2022). 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 (Gropper and Kuhnen, 2023).

To compare the coverage neglect and full information hypotheses, we first turn to the post-disaster consequences of underinsurance. If households choose to underinsure with full information, relying on self-insurance or access to other funding sources (e.g., Federal disaster loans), underinsurance might not predict recovery outcomes. In contrast, if boundedly rational homeowners inadvertently underinsure due to not knowing their home's replacement cost (i.e., coverage neglect), we would expect underinsurance to be associated with worse recovery outcomes.

Consistent with the coverage neglect hypothesis, we find that more underinsured policyholders fare significantly worse after the fire. Applying a leave-one-out instrument that exploits variation across insurance companies in the tendency to underinsure, we find that underinsured wildfire survivors with destroyed homes are less likely to file rebuilding permits and more likely to sell their property. These results hold conditional on controls for the characteristics of the policyholder and their home. Our estimates signal that underinsurance reduced the number of rebuilding permits filed within one year of the fire by 25% and contributed to over half of the destroyed property sales in the 18 months after the fire. This is an important result, not only because it suggests that some homeowners underinsure inadvertently, but also because decisions to rebuild generate large positive externalities on disaster-affected communities (Fu and Gregory, 2019).

As a second test of the full information and coverage neglect hypotheses, we estimate a discrete choice model of insurance demand where homeowners select from contracts offered by each insurer. Our demand model nests the two hypotheses with both a "full information premium" and a "coverage neglect premium" for each buyer and insurer. The full information premium reflects the cost per dollar of coverage, holding fixed the buyer's coverage choice across insurers, whereas the coverage neglect premium is the headline premium at the average coverage for each insurer, which is lower if that insurer tends to offer lower coverage limits. If consumers rationally underinsure, only the full information premium should affect choice probabilities as buyers choose the coverage they want and shop for the best rate *ceteris paribus*. In contrast, with coverage neglect, insurers who tend to underinsure entice buyers who mistake less coverage for lower cost.

To calculate the quoted premiums across insurers, we use premium schedules gathered from rate filing data in conjunction with the observed premiums in our data. We estimate the coverage neglect premium from the predicted coverage purchased from each insurer according to homeowners' financial and property characteristics. We measure each homeowner's coverage for the full information premium directly from their actual coverage choice in the data. In line with the demand estimation literature, the choice model includes insurer fixed effects, which capture unobserved differences in average insurer (brand) quality that vary by flexible functions of homeowner and property characteristics.

Via this discrete choice model, we estimate that policyholders are more likely to purchase insurance with a low headline premium, while at the same time being somewhat *less* likely to purchase insurance with a low cost per dollar of coverage. This evidence is consistent with coverage neglect and inconsistent with the full information hypothesis. There is little heterogeneity in our estimates across incomes or mortgage statuses, suggesting that socioeconomic factors and lender monitoring have little impact on policyholder coverage decisions. To quantify the cost of coverage neglect, we estimate how consumer welfare would change if policyholders chose their insurer based on coverage-adjusted premiums. Under conservative assumptions, we estimate that the average homeowner would benefit by $290 per year, representing 10% of average annual premiums, under a full information counterfactual. Although it is difficult to forecast the impact of any specific market intervention, we conclude that alleviating information frictions that prevent policyholders from comparing quotes on a coverage-adjusted basis would drive buyers towards higher-quality insurers that are less likely to underinsure.

Our results suggest that insurance companies can guide buyers' coverage choices. When comparing quotes, homeowners do not fully internalize how suggested coverage limits vary across insurers. These findings may be surprising given that coverage limits are prominently displayed on homeowners insurance contracts and adjustable by the policyholder. However, consumers may find it hard to understand different coverage limit provisions (e.g., extended replacement policies) as well as the probabilistic consequences of selecting a low limit. Moreover, consumers may find it difficult to compare quotes across insurers. In other finance contexts, consumers struggle to compare prices across contracts that vary on multiple dimensions (Campbell, 2016). Similarly, impediments to estimating a home's true replacement cost may reduce the salience of coverage limits to policy shoppers. There are no free, independent estimators available to homeowners seeking to validate the level of coverage necessary to rebuild their homes (Fuqua, 2024). Documenting the underlying causes of and potential remedies for coverage neglect is an important avenue for future research.

2 Literature

Most directly, this paper contributes to literature on the impact of climate risk on real estate markets. A growing literature examines whether future climate risks are capitalized into real estate prices (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). More recent papers evaluate the impact of disaster risk and state insurance regulations on the homeowners insurance market (Issler, Stanton, Vergara-Alert, and Wallace, 2020; Sastry, Sen, and Tenekedjieva, 2023; Eastman and Kim, 2023; Keys and Mulder, 2024; Boomhower, Fowlie, and Plantinga, 2023; Boomhower, Fowlie, Gellman, and Plantinga, 2024). These papers find sharp increases in premiums rates in disaster-prone regions and attempts by well-capitalized insurers to either pull out of these markets or spread the risk (and rate increases) to less risky areas. Other recent work documents the pass-through of rising premiums into home prices (Eastman, Kim, and Zhou, 2024) and mortgage performance (Ge, Johnson, and Tzur-Ilan, 2024). Our paper complements this emerging research by studying, at a micro-level, the determinants of the quantity of coverage provided and the role underinsurance plays in disaster recovery.

As such, our paper also relates to studies of the *ex ante* decisions that households and firms faced when managing climate risk. 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; Mulder, 2024) as well as studies of inequalities in the resources available to households to recover from natural disasters (Billings et al., 2022; Begley, Gurun, Purnanandam, and Weagley, 2024).[3] Notably, Sastry (2022) find that lenders either offload flood risk to the government through the National Flood Insurance Program (NFIP) or ration credit in risky, underinsured areas. While our focus is on homeowners insurance and does not explore mortgage credit supply, it is interesting to note that we find little role for mortgage lenders (or related leverage) in driving the coverage limits in homeowners insurance policies. Our paper also relates 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 important differences in settings, both papers find significant insurer-fixed effects, suggesting that insurance agents influence policyholders' selected coverage limits.

Given our focus on the drivers of underinsurance in homeowners contracts, our paper is, perhaps, most closely related to the contemporaneous work of Sastry, Scharlemann, Sen, and Tenekedjieva (2024). Their paper provides a complementary national picture of underinsurance.

[3]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.

The authors estimate underinsurance by inferring premiums from mortgage escrow payments (net of taxes) and comparing inferred premiums to the cost of full coverage based on the average premiums in insurer rate filings. Relative to this approach, our data provide a more direct and granular picture of each homeowner's insurance contract features, premium paid, and coverages purchased. Importantly, we uniquely observe the insurance provider chosen by the household. As a result, we can precisely estimate the role of insurer heterogeneity in driving underinsurance and construct a discrete choice model to test whether consumer demand-side frictions contribute to underinsurance. Because we study insurance contracts in force as of a major wildfire, 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 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; Roussanov, Ruan, and Wei, 2021; Célérier and Vallée, 2017; Guiso, Pozzi, Tsoy, Gambacorta, and Mistrulli, 2022; Oprea, 2024). Like aspects of these markets, we present empirical evidence suggesting that homeowners neglect the total loss coverage amount, which might lead consumers to accept the coverage suggestions of insurance agents without knowing the methodology or doing an independent assessment, as described in the qualitative interviews in Hassani (2013). More generally, our findings relate to recent attempts to understand consumer inattention to first order financial product attributes (Kulkarni, Truffa, and Iberti, 2021; Berwart, Higgins, Kulkarni, and Truffa, 2024) and to the literature on search frictions (McDevitt, 2014; Hastings, Hortaçsu, and Syverson, 2017; Hortaçsu, Madanizadeh, and Puller, 2017). Our results suggest that coverage neglect by homeowners can cause underinsurance to persist in competitive markets.

Finally, this paper advances research on behavioral biases and information frictions in insurance markets (Sydnor, 2010; Handel and Kolstad, 2015; Abito and Salant, 2019; Boyer, De Donder, Fluet, Leroux, and Michaud, 2020; Collier, Schwartz, Kunreuther, and Michel-Kerjan, 2022). Most similar to our paper, Abaluck and Gruber (2011) identifies demand patterns that are inconsistent with rational, full-information decision making from a model of Medicare plan choice. In the context of disaster insurance, the literature focuses on applying these models to the take-up of public flood insurance (Mulder, 2024; Wagner, 2022; Weill, 2023). In this paper, we provide evidence of substantial choice frictions affecting private homeowners insurance coverage decisions. Unlike in the public flood insurance setting, where the NFIP is the dominant provider, suboptimal coverage choices in the homeowners insurance market stem from the interaction between consumer inattention and heterogeneity in coverage offerings across insurers. Thus, even in competitive disaster insurance markets where a large majority of homeowners buy coverage, choice frictions can still lead to suboptimal coverage decisions 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 Department of Insurance Policy Data

The Colorado Department 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, other structures ("coverage B"), and contents ("coverage C") 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 not observing personally identifying information, we first convert the address into a separate property code. At this point, homeowner names and addresses are deleted before we transmit the data to a separate server where the deidentified data are stored. On this server, we merge on non-identifying property code with the other datasets described below.

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 in some analyses. We supplement the housing data with market valuation estimates (Zestimates) manually gathered for each home from Zillow.com at the time of the fire. As with the insurance data, we convert the address to a property code before transmitting the data to the separate server to merge in the deidentified data environment.

Quadrant Insurance Company Rate Filing Data

Homeowners insurance pricing is heavily regulated by states. A company's quoted premiums must be calculated from rate manuals previously approved by each states' department of insurance. Quadrant Information Services ("Quadrant") is an insurance services company that reconstructs insurers' pricing formulas from those rate filings.[4] 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 cover-

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

ages 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 10% 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 the 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 Section 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 the insurance data before moving the data to the server with deidentified data.

Credit Data

We also obtain credit bureau data for adults residing within two miles of the Marshall Fire. These data include approximately 2,000 credit attributes, including mortgage balances and credit scores.[5] To measure household income, we use the credit bureau's estimated income model. The credit bureau estimates individual incomes from a proprietary predictive model trained on the credit data matched to Form 1040 income tax data (see Cookson, Gilje, and Heimer 2022 for an example of research using similar data). We use data from December 31, 2021, the day after the fire. We sum or average credit attributes, as appropriate, across adults at the same address, which we take to be a measure of household credit characteristics.

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, for example, often excludes the home's exterior, which is separately insured by homeowners associations. Next, we

[5] Per the academic use of the credit bureau data, we present only aggregate statistics of credit data (e.g., summary statistics and regression coefficients) that do not identify the individuals in our sample.

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 that are 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 Section B.

3.3 Measuring Underinsurance

We classify a home as underinsured according to its "coverage ratio," defined as coverage A divided by the cost to rebuild the home. Coverage ratios below 1 indicate underinsurance and those above 1 indicate overinsurance. Building costs increased dramatically around the time of the fire due to both pandemic-related inflation and the correlated losses caused by the wildfire. To separate temporary trends from the structural causes of underinsurance, 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.[6] Although the first quarter of 2023 is more than a year after the Marshall Fire,

[6]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.

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 frequently rely on third-party software (e.g., RSMeans, CoreLogic Marshall & Swift, 360Value, among others). Consumer advocates in multiple states have raised concerns that the overly simplistic use of these software products may lead insurance agents to understate replacement costs (Hassani, 2013; Phillips, 2022). Therefore, when measuring underinsurance, we are careful to account for potential underestimation in construction cost software. We do this by, first, gathering rebuilding cost quotes from the Colorado DOI, Marshall Fire survivors, and the Homebuilders Association of Metro Denver. Altogether, these sources suggest that average rebuilding costs after the Marshall Fire were around $350 per square foot. Therefore, we use RSMeans to generate cross-sectional heterogeneity in build costs after the Marshall Fire according to variation in home characteristics. However, 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 estimate of the fraction of underinsured policyholders, it does not affect our estimation results since these 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 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 policies were being 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 relies on the assumption that RSMeans accurately describes the change in construction costs even if, in any given year, the average level may be off. By indexing pre-fire replacement costs to the earliest possible date when the policies in force at the time of the fire were last renewed, we generate the smallest replacement cost, which gives the most conservative estimate of pre-fire underinsurance.

Next, we calculate each policy's pre- and post-fire insurance coverage, i.e. the numerators of our coverage ratios. First, it is helpful to understand dwelling coverage A in the context of homeowner's insurance policies. The software used by insurers to help policyholders set their coverage A limits is supposed to reflect 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:

$$R_i^{Pre} = \frac{\text{Coverage A}_i}{Replacement_i^{Pre}}.$$

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 standard line item in an insurance contract is extended replacement cost coverage, which augments a policyholder's coverage A when inflation would otherwise cause the policyholder to be underinsured. Extended replacement cost coverage is typically sold as a percentage of a policyholder's coverage A (e.g. 25%). Unlike coverage A, which pays out immediately after a verified total loss, homeowners must prove that rebuilding costs truly exceed coverage A limits to access their extended coverage (i.e., hire an architect and contractor to design and price the destroyed home). Our measure of the post-fire coverage ratio adds extended replacement cost 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 extended replacement cost coverage, and the average percentage is 28% of coverage A.[7]

We calculate the post-fire coverage ratio as the extended dwelling A coverage divided by the post-fire replacement cost estimates:

$$R_i^{Post} = \frac{(1 + ext\_rate_i) \times \text{Coverage A}_i}{Replacement_i^{Post}}$$

where ext_rate_i is the extended replacement cost coverage rate for policyholder i.

[7] An additional 6.9% of policyholders have guaranteed replacement cost coverage which guarantees full coverage of replacement costs. We set the post-fire coverage ratio of guaranteed replacement cost holders to the maximum of their coverage A divided by post-fire replacement costs and 1.

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 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. The existence of wide variation in premiums per dollar of coverage across policyholders helps motivate the insurance choice model described later in this paper.

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, in terms of 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.

Figure 1 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 offset by extended replacement cost provisions. Hence, policyholders were underinsured on average, not because of cost inflation, but because they bought too little coverage.

Next, we explore some of the policyholder characteristics that might explain the wide distribution of coverage ratios. Figure 2 plots the average pre-fire coverage ratio by bins of household income (top), pre-fire home value (middle), 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), we see that coverage ratios are positively associated with both incomes and home values. However, there is a flat relationship between credit scores and underinsurance.

Despite the positive relationship between underinsurance and wealth measures, it is striking to note that even among households with incomes above the sample median of $180,000, 72% of policyholders were underinsured before the Marshall Fire. 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

While policyholder characteristics explain some of the variation in coverage, a large amount of underinsurance persists even after accounting for these demand-side factors. As a first indication of the importance of supply-side factors, Figure 3 plots the distribution of average policyholder coverage ratios across the insurers in our estimation sample. There is significant variation in average coverage by insurer, with two insurers having an average coverage ratio at or above one and another insurer having an average coverage ratio that reflects less than 75% of policyholders' pre-fire replacement costs.

Next, we test whether the heterogeneity in the average insurer coverage ratios in Figure 3 persists conditional on the characteristics of the policyholder (including their insured structure). To do so, we estimate Equation 1 which models the coverage ratio of policyholder i as a function of policyholder characteristics X_i and insurance company fixed effects λ_j:

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

where R_{ij}^{Pre} is the pre-fire coverage ratio multiplied by 100. The vector X_i captures the characteristics of the policyholder (income, credit score, mortgage status, housing value, estimated replacement cost, and quadratics of years since the home was purchased, the age of the home, and the home's square footage). We also consider the hypothesis that variation in pre-fire coverage ratios may reflect price sensitivity to heterogeneous premium rates or the purchase of extended coverage provisions. Thus, we also include as covariates whether the policyholder purchased extended coverage and the premium rate per $100 of coverage. Standard errors are clustered by insurance company. In this specification, if observable policyholder characteristics drive variation in underinsurance across insurers, we would expect the coefficients on the insurer fixed effects, λ_j, to be insignificantly different from each other.

Figure 4 plots the fitted insurer fixed effects $\widehat{\lambda_j}$ from estimating Equation 1 first without policyholder covariates, X_i, (red) and then including these covariates (blue).[8] 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 their estimated replacement cost. Adding policyholder covariates, the same counterfactual predicts a decrease of 25.7%.

A potential explanation for the heterogeneity in average coverage ratios across insurers is that policyholders sort across insurers in a way that correlates with policyholders' propensities to underinsure. We test for this form of selection using the following equation:

$$\overline{R}_{j,-i}^{Pre} = \beta_0 + \beta_1 X_i + \epsilon_{ij}. \tag{2}$$

In equation 2, $\overline{R}_{j,-i}^{Pre}$ is the average pre-fire coverage ratio of policyholders with insurer j after excluding policyholder i and multiplying by 100. We call this the leave-one-out (LOO) average coverage ratio.[9] If heterogeneity across insurers is explained by differential sorting of policyholders, then we would expect policyholder characteristics to explain a large share of the variation in

[8] Appendix Table A1 shows the coefficient estimates from estimating equation 1 with controls.

[9] For simplicity, we focus on the pre-fire ratio in equation 2. Our results are similar when we employ the post-fire ratio instead, consistent with the fact that the two ratios are distributed similarly 1.

the LOO average coverage ratio.

The results of estimating Equation 2 are shown in Table 2. The full set of covariates (column 3) explain only 3% of the variation in average underinsurance. This result signals that heterogeneity in average underinsurance across insurers is not explained by a clientele effect in which the policyholders associated with less insurance coverage (e.g., lower incomes) tend to select into certain insurance companies.

A plausible explanation for the importance of insurance company in predicting the degree of underinsurance across policyholders is that policyholders receive coverage recommendations from insurance agents when shopping for insurance. Agents use software-based costs models to estimate rebuilding costs at the time of insurance quote and companies vary, not just in the type of software used, but in the housing inputs included in cost models. Insurance agent recommendations may serve as important anchors for homeowners determining their coverage limits. In fact, insurance company recommendations may be the sole source of rebuild cost information used by homeowners since the main alternative source – hiring an appraiser – is a significant expense. It is, therefore, likely that homeowners anchor their replacement cost coverage to the recommendations provided by insurance companies such that variation in rebuilding cost estimates across companies translates into variation in underinsurance across policyholders.

Although data limitations prevent us from offering direct evidence on systematic differences in the modeling approaches of different insurers, as a suggestive test, we evaluate whether average underinsurance varies by insurer experience in the local market. Similar to the mortgage lending literature, which documents an important role for soft information in accurate loan underwriting (see e.g. Agarwal and Hauswald (2010) or Berger, Miller, Petersen, Rajan, and Stein (2005)), insurers with better local information may select more relevant inputs when estimating rebuilding costs. Supporting this hypothesis, Boomhower et al. (2024) find that larger insurers in California tend to use more sophisticated wildfire risk models when setting premiums. Insurers with more contracts in the local market may also have more local agents and associated reputational capital on the line. We proxy for insurer soft information and reputational concerns with (1) the number of policies in the estimation data and (2) the number of years since we first observe the insurer writing contracts in the estimation sample.

The results in Table 3 (column 1) show that each additional 100 policyholders is associated with a 3.6 percentage point higher insurer average coverage ratio. And, the estimates in column (2) indicate that each additional decade in the local market is linked to a 5.8 percentage point higher insurer average coverage ratio. Adding as covariates the full suite of policyholder characteristics has little effect on these relationships (columns 3 and 4).

It is striking to compare the explanatory power of the local knowledge proxies with the policyholder characteristics. As evidenced by the R^2 statistics in columns (1) and (3) of Table 3, the number of policyholders and age of the oldest policy explain 39.2% and 31.3% of the variation in insurer average coverage ratios, respectively, and jointly explain nearly half the variation as shown in column (4). By comparison, the full suite of policyholder characteristics explain only 3.1% of the variation in insurer average coverage ratios according to Table 2. Thus, the characteristics of the insurers themselves, rather than their policyholders, best explain the heterogeneity in average underinsurance across insurers.

4.3 The Effect of Underinsurance on Rebuilding and Home Sales

Our results so far show that insurance companies play an important role in explaining variation in underinsurance. Therefore, in this section, we use cross-insurer variation in underinsurance to isolate variation in coverage that is plausibly unrelated to unobserved policyholder characteristics – since such characteristics could be endogenous to both underinsurance and disaster recovery outcomes. In this two-stage least squares instrumental variables model (2SLS IV), our identification assumption is that recovery outcomes are uncorrelated with the choice of insurer except through differences in their average coverage ratios. Supporting this assumption, our previous analysis shows little systematic difference in observable policyholder characteristics across insurers. However, we cannot separate the influence of the propensity to underinsure from other unobserved insurer characteristics that may influence recovery (e.g., quality of customer service or speed of paying out claims). Still, our estimates provide a plausible baseline for the impact of underinsurance on recovery outcomes for two reasons. First, while insurers may vary along unobserved dimensions, differences in coverage are likely to play a first-order role in rebuilding. Second, estimating the bundled "insurer effect" (including unobserved quality factors that correlate with coverage) still offers a valuable insight into how insurer choice affects disaster recovery.

The first stage of our estimating equation is:

$$R^{Pre}_{ij} = \lambda_0 + \lambda_1 X_i + \lambda_2 \overline{R}^{Pre}_{j,-i} + \mu_{ij}, \quad (3)$$

where R^{Pre}_i is the pre-fire coverage ratio, and X_i are policyholder and associated housing characteristics. We use $\overline{R}^{Pre}_{j,-i}$, the mean pre-fire coverage ratio for insurer j excluding policyholder i, as our excluded instrument.

The second stage is:

$$Y_{ij} = \beta_0 + \beta_1 X_i + \beta_2 \widehat{R^{Pre}_{ij}} + \epsilon_{ij}, \quad (4)$$

where Y_i is a measure of disaster recovery and $\widehat{R^{Pre}_{ij}}$ is the fitted value from equation 3. Standard errors are clustered by insurer. Our sample includes the 736 policyholder households in our estimation sample that suffered total losses, although $\overline{R}^{Pre}_{j,-i}$ is calculated from the full estimation sample. We analogously estimate equations 3 and 4 substituting R^{Pre}_{ij} and $\overline{R}^{Pre}_{j,-i}$ with their equivalent post-fire coverage ratios.

We estimate a strong first stage, shown in Appendix Table A2, with F-stats of 122 and 43 when we instrument for pre-fire and post-fire coverage ratios, respectively, with the corresponding LOO insurer averages. Conditional on our controls, a 10 percentage point increase in the average pre-fire coverage ratio of other policyholders with the same insurer predicts a 7.7 percentage point higher pre-fire coverage ratio.

Moving to the second-stage regression, the first outcome we consider is the speed of rebuilding since underinsurance can cause liquidity issues that delay reconstruction. In panel (a) of Table 4, we define Y_i as an indicator variable that equals 100 if household i has filed for a rebuilding permit by December 2022 (one year after the fire) in column (1) and/or by October 2023 in column (2). Using the policyholder's instrumented pre-fire coverage ratio, we see positive and statistically significant effects of insurance coverage on rebuilding permits in December 2022, such that a policyholder with a 10 p.p. higher coverage ratio is approximately 4 p.p. (or 20% of the dependent variable mean) more likely to have filed a rebuilding permit. In column (2), we see a noisier and slightly attenuated estimate for filing rebuilding permits by October 2023 as more homeowners begin reconstruction. In columns (3) and (4), we use the policyholder's instrumented post-fire coverage ratio as the independent variable and find the relationship between rebuilding speed and insurance coverage to be even stronger – which may reflect the importance of extended replacement policies in funding rebuilding efforts. In particular, each 10 p.p. increase in a policyholder's post-fire coverage ratio increases their probability of filing rebuilding permits by 5.7 and 6.3 p.p. by December 2022 and October 2023, respectively.

Next, in Panel (b) of Table 4, we consider the effect of underinsurance on whether the owner of a destroyed home sells the property. Home sales may indicate that the homeowner was unable to afford rebuilding.[10] The dependent variable, Y_i, equals 100 if household i's insured home had been sold by December 2022 (columns 1 and 3) and/or by October 2023 (column 2 and 4). Having more coverage makes disaster survivors less likely to sell. One year after the fire, a 10 p.p. higher pre-fire coverage ratio results in a 1.8 p.p. (or 44% of the dependent variable mean) reduction in the likelihood of selling. As in panel (a), we see an even larger treatment effect in column (3) when the explanatory variable is the policyholder's instrumented post-fire coverage ratio. The coefficients on coverage remains statistically significant and grow in magnitude for home sales out to October 2023 in columns (2) and (4).

One may be concerned that the choice of insurer is correlated with unobservable policyholder preferences for remaining in the local area. Homeowners who are more willing to move may sort into insurers that tend to write less coverage. If our estimates were affected by this form of endogeneity, we would expect low coverage ratios to predict home sales even for those homeowners who did not suffer a total loss. Thus, 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 A3 indicate null results using both pre- (column 1) and post-fire (column 2) coverage ratios. Hence, in the absence of a total loss that would make coverage limits bind, homeowners with policies from insurers that tend to write less coverage are no more likely to move.

Our estimates show that underinsurance is an economically meaningful impediment to disaster recovery. Extrapolating from our 2SLS IV results, if all the underinsured homeowners in our sample had been fully insured (i.e., with a pre-fire coverage ratio of 1), then 25.4% of homeowners would have filed for a rebuilding permit by December 2022 instead of 18.8% and only 5.4% of

[10] Indeed, in our estimation sample, 9.7% of the owners of homes destroyed in the fire had sold their property as of October 2023 versus only 5.9% of the owners of homes that were not destroyed. Among those destroyed homes that sold, over 75% did not have a rebuilding permit.

homeowners would have sold their homes as of October 2023 instead of 9.7%.

5 Mechanisms

Although the previous section establishes the importance of insurer heterogeneity in explaining variation in coverage limits, these findings do not preclude consumer behavioral or strategic factors from affecting demand and, in turn, contributing to underinsurance. In this section, we first test three prominent hypotheses for why buyers may purchase underinsured policies: failure to update coverage limits over time, shifting of risk onto lenders, and adverse selection. Then, we propose and evaluate 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 (Biswas, Biswas, and Zink, 2023). Under a "failure to update" mechanism, policies may have accurate coverage limits when they are first written, but become increasingly underinsured over time. Under this mechanism, we would expect policies that were originated longer ago to be more underinsured. A unique feature of our data is that we observe policy coverage A limits from when policies were first written in addition to as of the last renewal date prior to the fire.[11]

Our data reveals 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 limits are insufficient to keep up with rising construction costs. 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 percentage increase in construction costs over the corresponding interval 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)

[11] Coverage A at inception is missing for 34 policies, or approximately 1% of the estimation sample.

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 as we add policyholder and structure characteristics in column (2). Conditional on insurer fixed effects in column (3), there is a 1.26% increase in coverage limits for for each 1% increase in costs. 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 5 plots the share of policyholders that are underinsured according to their pre-fire coverage ratio (as of the last renewal before the 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, Table 6 regresses the pre-fire coverage ratio multiplied by 100 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. We conclude that, in contrast to a failure to update mechanism, homeowners policies do not tend to become more undeirnsured over time.

5.2 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. [12] 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,

[12] 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).

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 7 evaluate whether risk shifting incentives explain the underinsurance in our data by regressing pre-fire coverage ratios, multiplied by 100, on mortgage indicators. First, we test whether lender monitoring might prevent risk shifting in Columns (1)–(3). The bivariate regression in column (1) reveals that policyholders with a mortgage have a 2.5 percentage point lower coverage ratio than those without, on average. This relationship weakens as we introduce policyholder and structure characteristics (column 2) and becomes statistically insignificant with insurer fixed effects (column 3). Hence, rather than the positive relationship predicted by a lender monitoring mechanism, we instead observe an insignificant negative relationship. Next, in column (4), explanatory variables are a series of indicators for policyholders' loan-to-value (LTV) ratios in ten percentage point bins. This specification tests whether homeowners with higher LTVs are more likely to underinsure per a risk-shifting channel. Contrary to a risk-shifting mechanism, there is no clear relationship between LTV and coverage.

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 tend to underinsure on average.

5.3 Are Coverage Limits Adversely Selected?

A canonical explanation for suboptimal provision of insurance 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 8a, 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. 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" by insurers that is predictive of claims but unpriced.

Next, in Table 8b, 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. This null relationship between *ex ante* risk and coverage 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, 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 underinsurance can be explained 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 in which insurance buyers rely on the replacement cost estimates suggested by insurance companies to set their dwelling coverage limits only to learn after a total loss that the suggested limit was insufficient to rebuild. Furthermore, buyers may not realize how widely these suggested limits vary across insurers on initial quotes. 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). Alternatively, underinsurance may reflect a revealed preference on the part of fully informed policyholders. Policyholders know the coverage limit they want, and choose the insurer who offers it at the best rate.

This section tests the coverage neglect hypothesis, where policyholders are inattentive to differences in coverage limit suggestions across insurers, against the "full information" hypothesis, where policyholders consciously choose to underinsure. To do so, we estimate a discrete choice insurance demand model that nests the coverage neglect hypothesis and the full information hypothesis.

THEORETICAL FRAMEWORK

We begin by describing buyer insurance choices in the full information case. Buyers i have some desired coverage ratio R_i^*. We do not need to specify exactly how R_i^* is set, simply noting it as i's revealed coverage preference. Buyer i chooses among insurers $j = 1, ..., J$ from whom to buy R_i^*, noting that each insurer offers a buyer-specific premium from a menu $p_{ij}(R_i)$. The buyer's utility from their insurance decision with full information, U^F, is given by:

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

Buyers have heterogeneous preferences across insurers that depend on insurer-specific constants ζ_j, their characteristics X_i, and an idiosyncratic taste term ϵ_{ij}. Buyers receive disutility from premiums that are normalized as a rate per dollar of coverage, and utility from their coverage purchased R_i^*.

Next, we contrast the full information utility function with the coverage neglect utility function. In the coverage neglect case, buyers have potential coverage ratios R_{ij} that vary across insurers. As with R_i^*, we can be flexible about how exactly R_{ij} is set. Two factors distinguish the coverage neglect coverage ratio R_{ij} from the full information coverage ratio R_i^*:

1. R_{ij} varies across insurers for the same buyer
2. Differences in R_{ij} across insurers do not enter the buyer's utility function

Thus, the coverage neglect utility function U^N is a slight alteration of U^F,

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

Under coverage neglect, buyers compare prices across insurers that are set according to R_{ij} as if all the contracts covered R_i^*. Thus, while buyers account for the differences in premiums in U^N, they do not account for differences in coverage, evaluating their choices as if each insurer were offering the same coverage. This is a stylized assumption that can reflect a variety of underlying factors. For example, consumers may not respond to coverage differences because it is difficult to understand the consequences of a lower coverage limit or because agents steer consumers to choose policies based on premiums and deductibles rather than on coverage amounts.

Discrete Choice Model

To test whether buyers choose to insure based on U^F or U^N, we estimate a discrete choice demand model that nests both utility functions:

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

Equation (7) includes two price terms: the "full information" premium $p_{ij}(R_i^*)$ at R_i^* and the "coverage neglect" premium $p_{ij}(R_{ij})$ at the insurer-specific coverage ratio R_{ij}, with both premiums normalized by R_i^*.[13] The full information hypothesis makes a precise prediction that $\alpha^N = 0$. If the buyer has made her own decision about the coverage she wants to buy, then the cost of the insurer's recommended coverage would have no impact on her utility conditional on the insurer's rate. In contrast, under the coverage neglect hypothesis, $\alpha^F = 0$, because buyers do not actively

[13] Because the perceived amount of coverage R_i^* does not vary across insurers under either hypothesis, the βR_i^* term drops out of the discrete choice model.

choose their coverage and instead compare premiums across insurers as if they were all for the same amount of coverage. We estimate a discrete choice model that includes both price terms, which essentially performs a horse race between these alternative prices via consumers' revealed preferences. Importantly, under coverage neglect, consumers do not recognize when $R_{ij} < R_i^*$, leading them to mistake less coverage for a lower price. Hence, when comparing the loadings on the full information and coverage neglect premiums, we are essentially asking whether consumers buy the coverage we observe in the data at the best possible price (full information) or are influenced by the differences in headline premium (coverage neglect).

Turning to the empirical approach, for each policyholder, we define R_i^* as the observed pre-fire coverage ratio in the estimation data. We calculate the premium $p_{ij}(R_i^*)$ based on quoted and reported premium schedules for each insurer.[14] For consumers who neglect coverage, and thus, choose an insurer based on $p_{ij}(R_{ij})$, we need to approximate the insurer-specific coverage limit for each policyholder, R_{ij}. To do this, we calculate $\widehat{R_{ij}}$ as the fitted values from regressing R_{ij} on policyholder characteristics and insurer fixed effects as in Equation (1). At this predicted level of coverage, we calculate $p_{ij}(\widehat{R_{ij}})$, again using the quoted and estimated premium schedules.

Our specification employs both prices in a standard multinomial discrete choice model with latent utility V_{ij}:

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

where ϵ_{ij} follows a Type I extreme value distribution. With this structure and the optimization problem above, the discrete choice model can be estimated using a multinomial logit model (McFadden, 1972). In addition to insurer fixed effects ζ_j, our specification allows for insurer brand values to vary across consumers according to individual characteristics (specifically, income, credit score, estimated replacement cost, home value, mortgage status, home age, and years since home purchase), captured in the $\sigma_j X_i$ terms.

The inclusion of insurer fixed effects ζ_j ensures that the α^F and α^N terms are not driven by correlation between prices and unobserved insurer quality. Instead, we identify the price parameters from variation in relative premiums within insurers across different buyers. Because we

[14] See Appendix Section B for more details.

can either observe or estimate the full rate schedules for each potential insurer-by-policyholder pair in our data, we can exploit substantial differences in the cost per unit of coverage driven by idiosyncrasies in insurers' pricing schedules. Premiums can vary widely across insurers, even for the same coverage on the same home. Indeed, there is wide variation in relative premium rates according to policyholders' rating characteristics.

To understand the source of variation in the coverage neglect premium $p_{ij}(\widehat{R_{ij}})$, note that $\widehat{R_{ij}}$ varies within buyer across insurers. The differences in $\widehat{R_{ij}}$ that relate to wide differences in average coverage ratios across insurers are absorbed by the insurer fixed effects in Equation 8. Thus, variation in the coverage neglect premium is driven by heterogeneity in the "steepness" of premium rates with respect to coverage.

A remaining identification concern is that within insurer variation in rates could be correlated with the idiosyncratic preference term ϵ_{ij}. Supporting our identification strategy, property insurance premiums are highly regulated and approved at the state level. While insurers can set specific premiums across the state based on characteristics such as a home's age or zipcode, they cannot target premiums to specific buyers through targeted discounts or bargaining. This makes it unlikely that premium variation is correlated with unobservable demand shocks conditional on our covariates X_i.

One may also be concerned that variation in observed premiums is driven by other coverage choices, such as deductibles or discounts from bundling home and auto coverage, that might be correlated with unobserved insurer demand. This possible source of endogeneity motivates our decision to use Quadrant rate filings as our main measure of price. The Quadrant rates create standardized premiums that hold fixed other coverages, contract characteristics, and discounts across buyers. Since these prices do not contain bundling, the identifying variation in premiums is driven by idiosyncratic differences in how insurers price certain property characteristics at different amounts of coverage.

Discrete Choice Estimation Results

Table 9 shows the results of estimating Equation 8. In column (1), we estimate the model assuming that the full information hypothesis is true and restrict $\alpha^N = 0$. We observe a statistically significant coefficient on the price with the expected sign, indicating that buyers tend to prefer insurers who offer lower premium rates. The coefficient results in an average price elasticity of 0.6, suggesting relatively price inelastic demand. In column (2), we assume the coverage neglect hypothesis and restrict α^F to be zero. We once again observe a statistically significant α parameter, again consistent with buyers preferring lower total premiums under coverage neglect. The coefficient on α^N suggests a higher price elasticity of 2.3, more than triple the price elasticity given by α^F in column (1).

Comparing the results between columns (1) and (2) of Table 9 already suggest that buyers are more sensitive to their coverage neglect premium than their full information premium. In column (3), we run a horse race model between the full information 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 when the headline premium is low, consistent with coverage neglect. Further, inconsistent with the full information hypothesis, buyers are somewhat *less* likely to choose an insurer whose cost per dollar of coverage is low.

The results in column (3) demonstrate choice inconsistencies under the full information hypothesis. If the observed R_i^* in the data represented each buyer's revealed preference coverage choice, then buyers should prefer the insurer that offers that level of coverage at the lowest premium 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 are in reality just offering less coverage. In contrast, the fact that variation in the headline premium predicts insurer choice suggests that an insurer's predicted coverage $\widehat{R_{ij}}$ determines the relative price that is salient to buyers.

We test for heterogeneity in the sensitivity to premiums in Table 10. In column (1), we allow the coefficients on the premium rate and the total premium to vary by whether the household is above the median income in the estimation sample. Higher income consumers may be more sophisticated and less price sensitive, and thus respond less to total premiums and more to premium rates (per dollar of coverage). In column (2), we consider heterogeneity across consumers with and without mortgages under the hypothesis that policyholders with mortgages may be more liquidity constrained and thus respond more to total premiums rather than to premium rates. Neither test reveals meaningful heterogeneity, suggesting that coverage neglect is widespread.

Many studies of insurance and other consumer financial products document substantial inertia effects, where policyholders face large costs of switching products after an initial decision (see e.g. Handel (2013) or Andersen, Campbell, Nielsen, and Ramadorai (2020)). Such inertia can attenuate discrete choice parameters if policyholders are reluctant to re-shop for policies even when prices, coverage, and/or insurer reputation changes. To test the sensitivity of our estimates to inertia, we re-estimate our discrete choice model in column (3) after dropping policyholders who first purchased their coverage more than five years prior to the fire. We continue to estimate strong sensitivity to coverage neglect premiums and find a null coefficient on the full information premium, rejecting the full information hypothesis.

The Cost of Coverage Neglect to Consumers

We use our multinomial choice model's estimates to infer a rational policyholder's willingness-to-pay to transparently view and compare rates across insurers. This counterfactual exercise allows us to place a dollar value on the deadweight loss imposed by coverage neglect, thus informing potential reforms to combat underinsurance, such as publicly available replacement cost estimates or a platform for comparing insurance quotes with uniform coverage limits.

We posit that policyholders choose their insurer according to U^N in Equation 6 and fail to account for differences in coverage limits across insurers. Thus, policyholders' experienced utility is not equal to Equation 6, but is instead:

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

where cross-insurer variation in quoted coverage relative to full information coverage, $R_{ij} - R^*_i$, affects consumer welfare according to the function f_{ij}.

We consider a counterfactual that eliminates coverage neglect such that policyholders choose their insurer by maximizing U^F rather than U^N. This counterfactual ignores general equilibrium effects, holds insurer pricing fixed, and assumes that consumers act rationally (as in the full information scenario). In principle, removing coverage neglect improves consumer welfare via two channels. First, consumers no longer mistake less coverage for a lower price and, instead, shop for the best rates per unit of coverage R^*_i. Second, consumers no longer accidentally buy too little or too much coverage.

Calculating these welfare effects requires taking a normative stance on how policyholders should value coverage. Because we cannot estimate the normative value of coverage in our setting, we make two assumptions about how coverage enters U^*:

$$U^*_{ij}(p_{ij}(R_{ij}), R_{ij}) = U^*(p_{ij}(R^*_i), R^*_i) \text{ if } R^*_i > R_{ij}$$

and

$$U^*_{ij}(p_{ij}(R_{ij}), R_{ij}) = U^*(p_{ij}(R_{ij}), R^*_i) \text{ if } R^*_i \leq R_{ij}$$

.

The first assumption implies that consumers are normatively indifferent between purchasing coverage R_{ij} at cost $p_{ij}(R_{ij})$ and purchasing coverage R^*_i at cost $p_{ij}(R^*_i)$ whenever $R_{ij} < R^*_i$. This condition conservatively assumes away any surplus policyholders might gain from buying more coverage. The second assumption says that consumers place no value on coverage above their revealed preference coverage choice R^*_i.

Let $U^*_{ij}(p_{ij}(R_{ij}), R_{ij}) = v^N_{ij}$ (utility of buyer i with insurer j under coverage neglect) and $U^*_{ij}(p_{ij}(R^*_i), R^*_i) = v^F_{ij}$ (utility under full information). Following derivations in Leggett (2002) on the value of information in discrete choice models, policyholder i's willingness-to-pay for removing coverage neglect can be expressed as:

$$WTP_i = \frac{1}{\alpha^N}(ln(\sum_j e^{v^F_{ij}}) - (ln(\sum_j e^{v^N_{ij}}) + \sum_j \pi^N_{ij}(v^F_{ij} - v^N_{ij}))), \tag{10}$$

where α^N is the estimated coverage neglect premium coefficient from column (2) of Table 9. The log summation terms are the usual expression for expected utility in multinomial discrete choice models, and π^N_{ij} is the probability that policyholder i chooses insurer j under the coverage neglect estimates.

The intuition of Equation 10 is to take the difference between expected utility under the information provision counterfactual and the coverage neglect baseline. The adjustment term $\pi^N_{ij}(v^F_{ij} - v^N_{ij})$ corrects the expected welfare term for the policyholder's misperception of their utility under coverage neglect.

Figure 6 plots the distribution of WTP_i from Equation 10. The average gain from eliminating coverage neglect, as indicated by the vertical line, is approximately \$290 per year. This average heavily influenced by policyholders in the right tail, where coverage neglect substantially affects their predicted choice of insurer.

Figure 7 illustrates how this intervention – leading homeowners to select the full information counterfactual over the coverage neglect baseline – could reshape the homeowners insurance market. The figure plots the average change in the probability of choosing a given insurer (y-axis) as a function of the insurer's predicted average coverage ratio (x-axis), which is the insurer average of $\widehat{R_{ij}}$ grouped into 20 bins. The dotted line is the line of best fit. Full information tilts consumer choice away from insurers with lower average coverage ratios, on average, and toward insurers where policyholders are more likely to be fully insured.

Although removing coverage neglect would enable insurance buyers to compare prices more accurately, average premiums are only slightly lower under the full information counterfactual (\$2,910 versus \$2,870). Thus, most of the welfare gains from information come, not from finding lower premiums, but instead from choosing higher-quality insurers that appear expensive under coverage neglect.

This counterfactual assumes no welfare effects from changes in coverage decisions under full information. We argue that this assumption makes these welfare estimates a conservative lower bound on the true gains from removing coverage neglect. In the case of flood protection, underinsurance, driven by information frictions, has been shown to have large welfare consequences (Mulder, 2024; Wagner, 2022). In addition to such private "internalities," our findings on the consequences of underinsurance suggest that full coverage induces positive rebuilding externalities, a finding echoed in Fu and Gregory (2019) and Issler et al. (2020).

6 Conclusion

This paper studies the causes and consequences of underinsurance using detailed insurance contract data from homeowners affected by the Marshall wildfire in Colorado. We find widespread underinsurance both before and after the fire, including at policy inception. Our analysis rejects commonly proposed mechanisms – that underinsurance can be explained solely by financially constrained lower income buyers, sudden increases in building costs post-fire, adverse selection in homeowners markets, or a failure to update coverage limits over time.

Instead, we find large differences in average underinsurance across insurers. These differences cannot be explained by differential sorting of buyers and are robust to including a rich set of controls for the characteristics of the policyholder and their insured structure. Using this heterogeneity across insurers as an externally-determined source of variation in coverage, we find that underinsurance significantly delays rebuilding and makes fire survivors more likely to sell their homes.

These findings are consistent with reports that consumers rely on insurers' models to set their coverage limits and have limited awareness of differences in coverage when comparing homeowners quotes. To test for such "coverage neglect", we build a discrete choice model of homeowners insurance demand. Our estimates show that buyers are attentive to differences in premiums across insurers but not differences in associated coverage. We estimate that consumer welfare would improve by an average of $290 per year, or 10% of average annual premiums, under a counterfactual that removes coverage neglect. Our results highlight the importance of information frictions facing homeowners as they navigate growing climate disaster risks.

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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 prior to 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 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
Policyholder Sample Size:	3089			
Insurance Company Sample Size:	14			

Table 2: Test of Policyholder Selection Across Insurers

This table presents regression estimates where the dependent variable is the policyholder's chosen insurer's leave-one-out (LOO) average pre-fire coverage ratio (excluding the focal policy holder i and multiplied by 100 for interpretability, $100 * R_{j(-i)}$), and the explanatory variables capture policyholder i characteristics, including the characteristics of their insured structure. The standard errors, shown in parentheses, are clustered by insurer: *** p<0.01, ** p<0.05, * p<0.1

Dependent: 100 X Insurer LOO Average Coverage Ratio	(1)	(2)	(3)
Log Income	-0.18	-0.43	0.53**
	(0.23)	(0.32)	(0.20)
Credit Score (z)		-0.33	-0.26
		(0.21)	(0.19)
Log Home Value		1.86	0.92
		(1.44)	(1.21)
Has Mortgage (0/1) = 1			-0.86**
			(0.35)
Log Replacement Cost			0.15
			(6.06)
Purchase Age (Decades)			1.98*
			(1.11)
Purchase Age^2			-0.28
			(0.26)
Home Age (Decades)			0.08
			(0.35)
Home Age^2			0.03
			(0.03)
Square Footage (1000s)			-4.08
			(3.37)
Square Footage^2			0.86*
			(0.44)
Observations	3,089	3,089	3,089
R-squared	0.000	0.003	0.031

Table 3: The Role of Insurers' Local Knowledge on Average Coverage Ratios Across Insurers

This table presents regression estimates where the dependent variable is the policyholder's chosen insurer's leave-one-out (LOO) average pre-fire coverage ratio (excluding the focal policy holder i and multiplied by 100 for interpretability, $100 * R_{j(-i)}$), and the explanatory variables are the chosen insurer's number of policyholders (in 100s) and age of oldest policy (in decades) in the estimation sample. Columns (2), (4), and (5) introduce the following policyholder characteristics as controls: log income, credit score, log home value, mortgage status, log replacement value, 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 Insurer LOO Average Coverage Ratio				
	(1)	(2)	(3)	(4)	(5)
Insurer Policyholders (100s)	3.56***	3.54***			2.61*
	(1.10)	(1.06)			(1.30)
Insurer Decades in Market			5.77**	5.70**	2.88*
			(2.17)	(2.15)	(1.55)
	(4.03)	(70.33)	(5.93)	(69.24)	(69.52)
Observations	3,089	3,089	3,089	3,089	3,089
R-squared	0.392	0.416	0.313	0.330	0.466
Policyholder Characteristics	N	Y	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 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 Rebuilding Permit By:			
	Dec '22	Oct '23	Dec '22	Oct '23
	(1)	(2)	(3)	(4)
Coverage Ratio (Pre-Fire)	39.71***	33.87*		
	(8.67)	(19.26)		
Coverage Ratio (Post-Fire)			57.15***	62.80**
			(9.03)	(25.66)
Observations	736	736	736	736
F-Stat	121.89	121.89	121.89	121.89
Policyholder Characteristics	Y	Y	Y	Y
Dep. Var. Mean	18.75	63.59	18.75	63.59

(a) Rebuilding Permits

Dep. variable:	100 X Sold Home By:			
	Dec '22	Oct '23	Dec '22	Oct '23
	(1)	(2)	(3)	(4)
Coverage Ratio (Pre-Fire)	-17.91***	-25.66***		
	(5.82)	(7.91)		
Coverage Ratio (Post-Fire)			-24.65***	-41.25***
			(6.80)	(7.97)
Observations	736	736	736	736
F-Stat	43.47	43.47	43.47	43.47
Policyholder Characteristics	Y	Y	Y	Y
Dep. Var. Mean	4.08	9.65	4.08	9.65

(b) Home Sales

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 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

Table 7: 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 8: 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 parenthesis. *, ** 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 9: Insurer Discrete Choice: Are Choices Sensitive to Per-Coverage Prices or Headline Premium?

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

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

Premiums are calculated according to insurer rate schedules $p_{ij}(R)$, with the full information 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 (log income, credit score, mortgage status, log home value, log replacement cost, home age, 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)
Full Information Premium (α^F)	1.216***		-1.628**
	(0.326)		(0.796)
Coverage Neglect Premium (α^N)		4.401***	5.000***
		(0.829)	(1.083)
Insurer Fixed Effects	Y	Y	Y
Insurer × Policyholder Characteristics	Y	Y	Y
N	43246	43246	43246

Table 10: Insurer Discrete Choice: Heterogeneity by Income, Mortgage Status, and Inertia

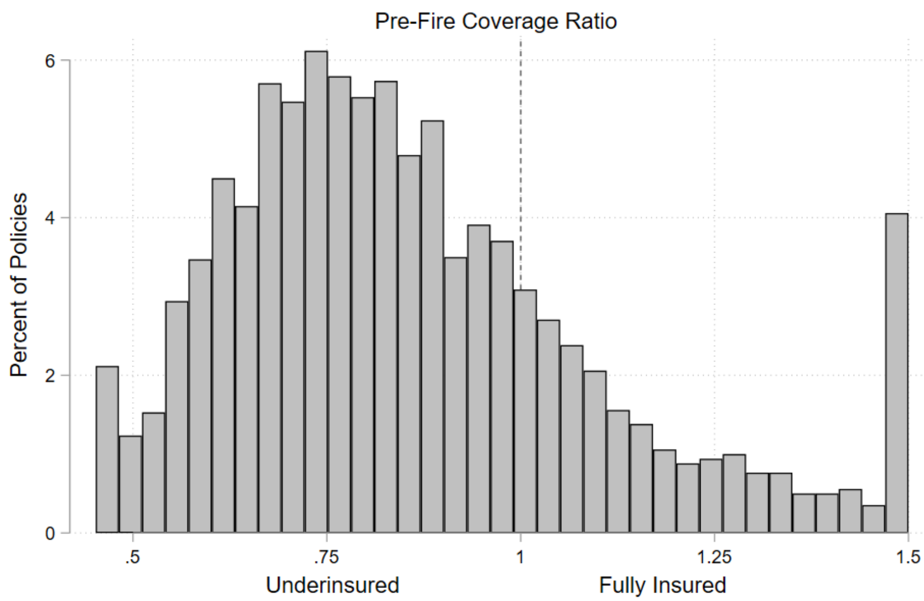
As in Table 9, this table presents estimates of parameters from a multinomial choice model of the following latent utility function given in equation (8), but in addition, we consider heterogeneity in price sensitivity by *High Income*, an indicator for above median income (column 1), and *Mortgage*, which indicates whether the household has a mortgage (column 2). Column 3 considers the subsample of households that purchased their policy in 2017 or later. Recent purchases are, by definition, less subject to concerns that policy choices are driven by inertia. Premiums are calculated according to insurer rate schedules $p_{ij}(R)$, with the full information 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)
Full Information Premium (α^F)	-1.968***	-0.820	-0.679
	(0.761)	(1.023)	(0.925)
Coverage Neglect Premium (α^N)	5.233***	3.759**	4.310***
	(0.771)	(0.900)	(0.887)
High Income X Full Info Premium	0.761		
	(1.439)		
High Income X Neglect Premium	-0.472		
	(1.385)		
Mortgage X Full Info Premium		-1.269	
		(1.524)	
Mortgage X Neglect Premium		1.920	
		(1.687)	
N	43246	43246	19656
Insurer Fixed Effects	Y	Y	Y
Insurer $\times$ Policyholder Characteristics	Y	Y	Y

8 Figures

Figure 1: 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 extended coverage A 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.



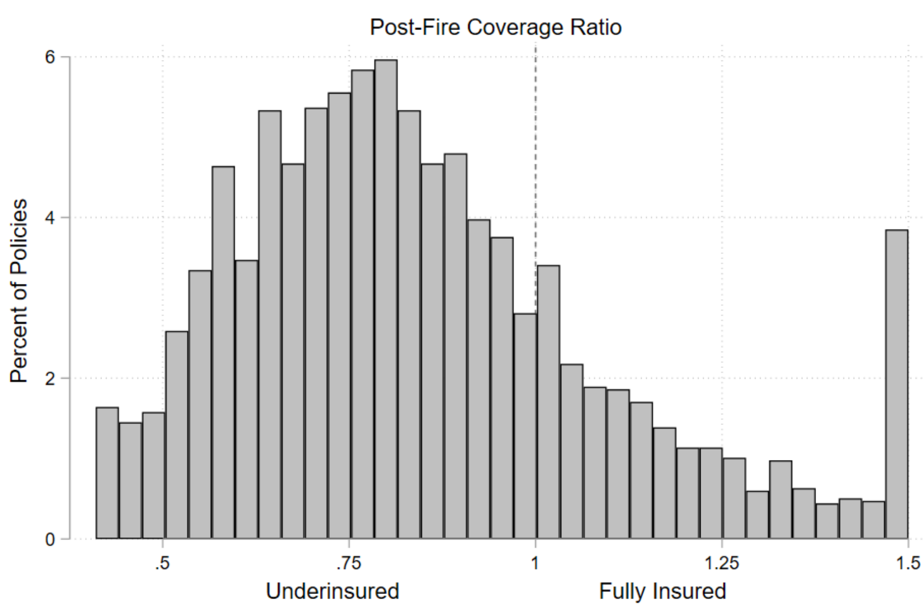
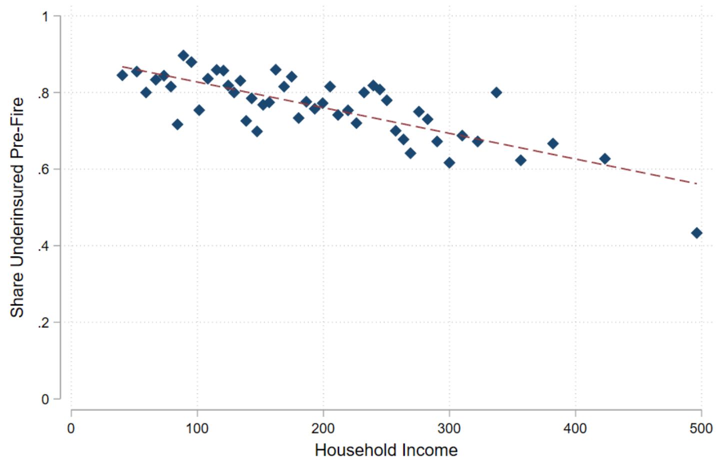
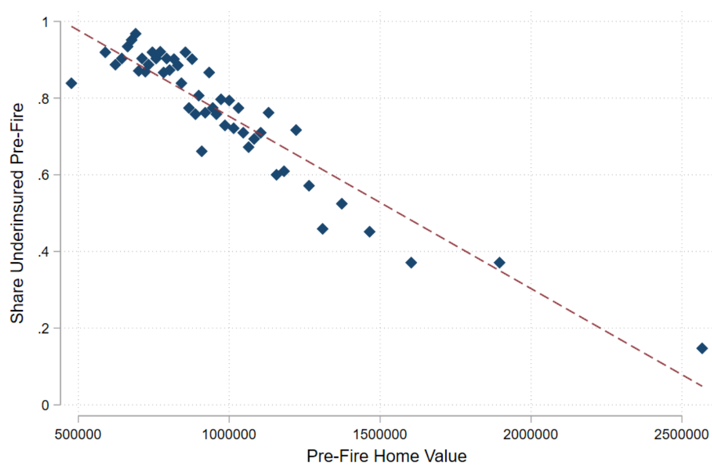


Figure 2: Underinsured Share by Policyholder Characteristics

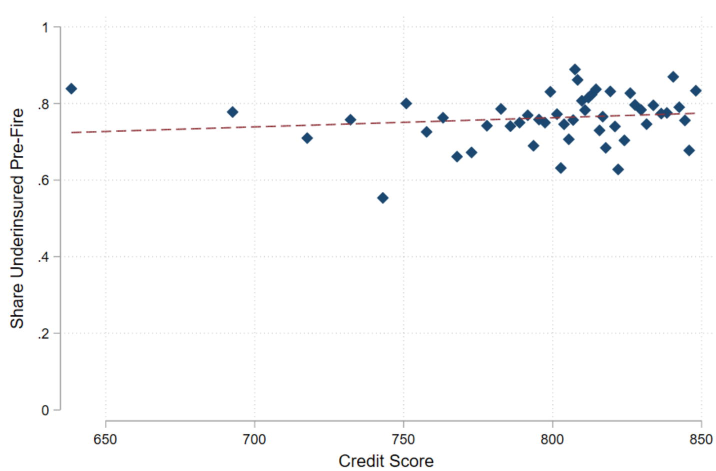
This figure plots the share of policyholders who are underinsuranced according to their pre-fire coverage ratio, binned according to 50 quantiles of household income (top), home value (middle), and credit score (bottom).



(a)



(b)



(c)

Figure 3: 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.

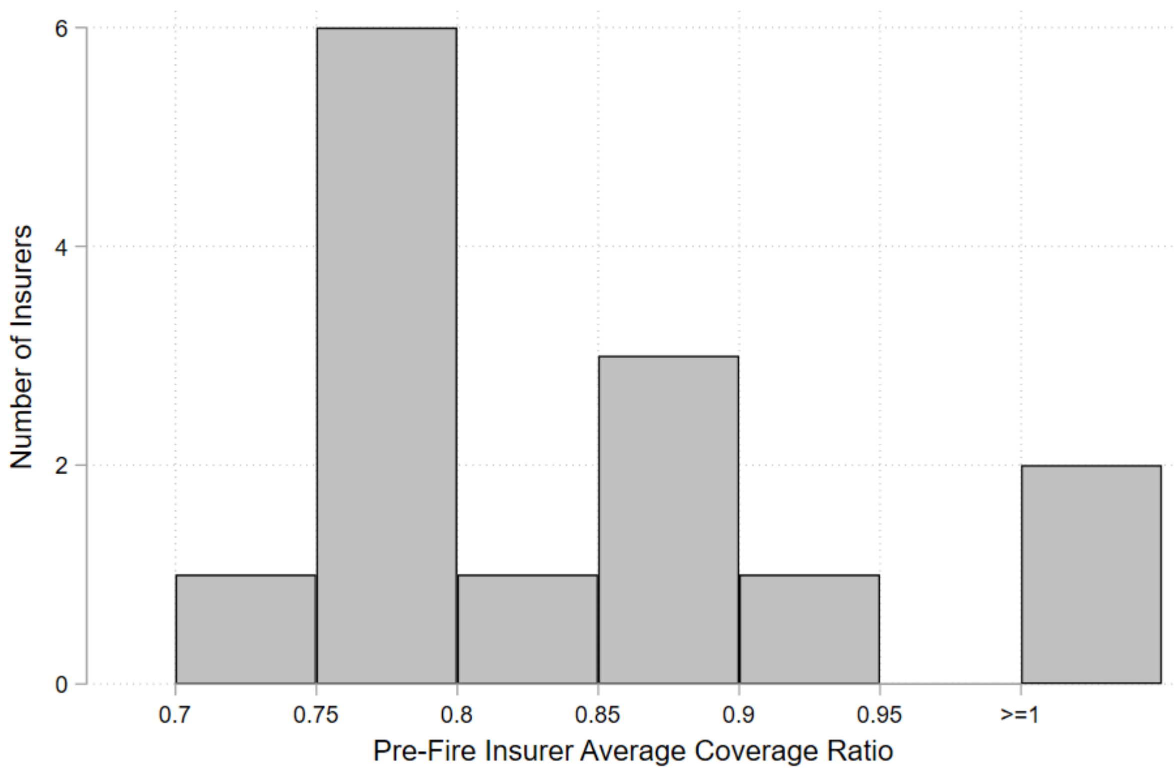


Figure 4: The Effect of Insurer Fixed Effects on Policyholder Coverage Ratios

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.

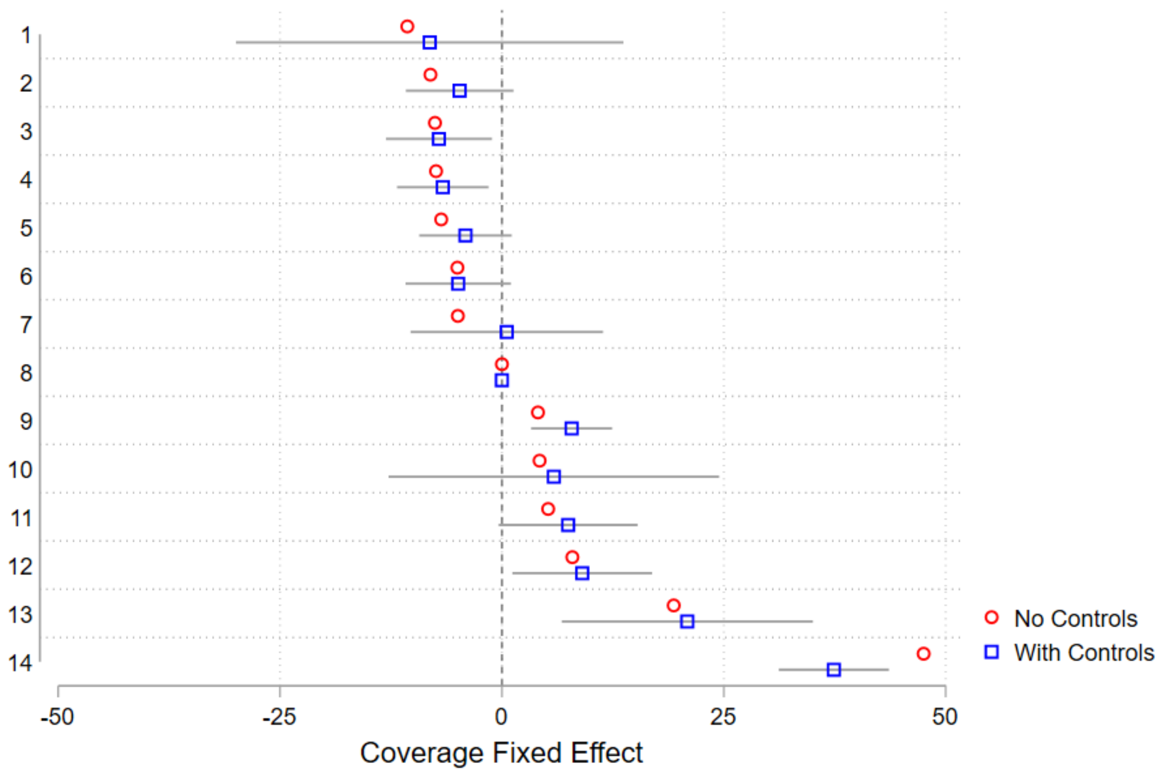


Figure 5: Underinsured Share by Age of the Policy

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 6: Consumer Welfare Effect of Removing Coverage Neglect.

This figure plots the distribution of welfare gains (compensating variation) under the full information coverage counterfactual relative to the baseline with coverage neglect. The vertical line represents the average welfare gain. For visibility, values above $1,500 are grouped together at $1,500.

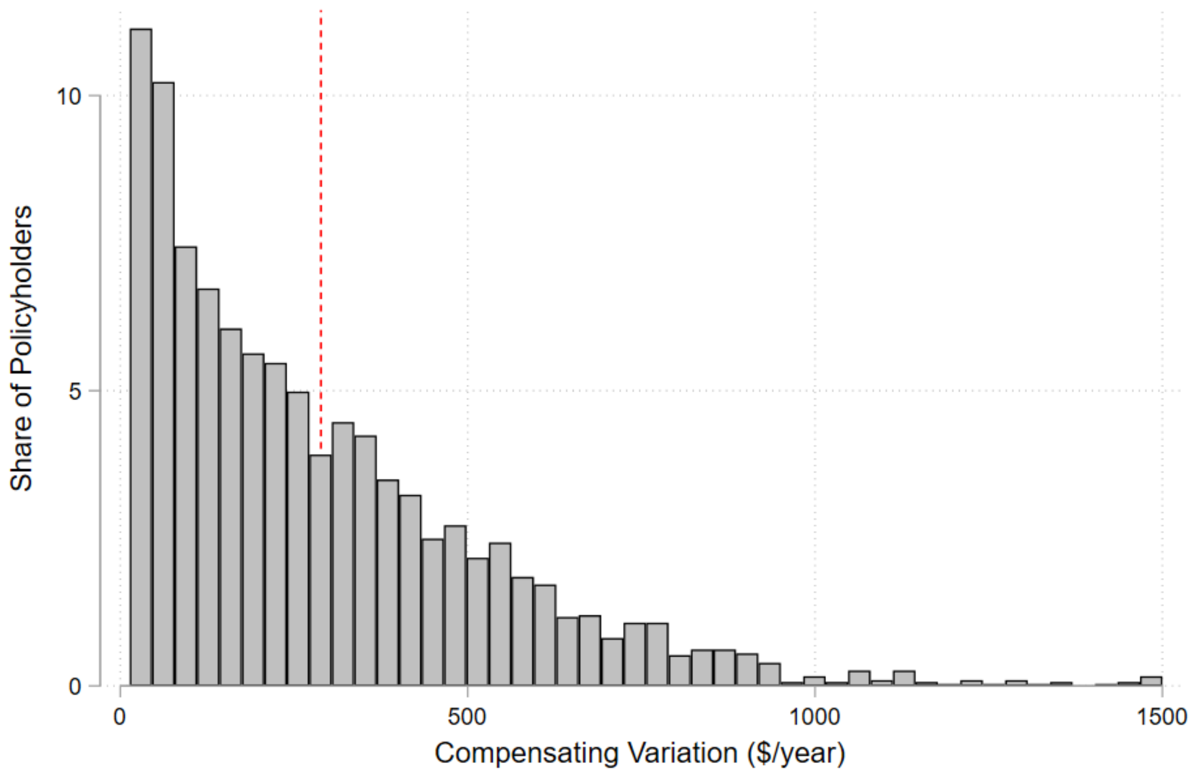
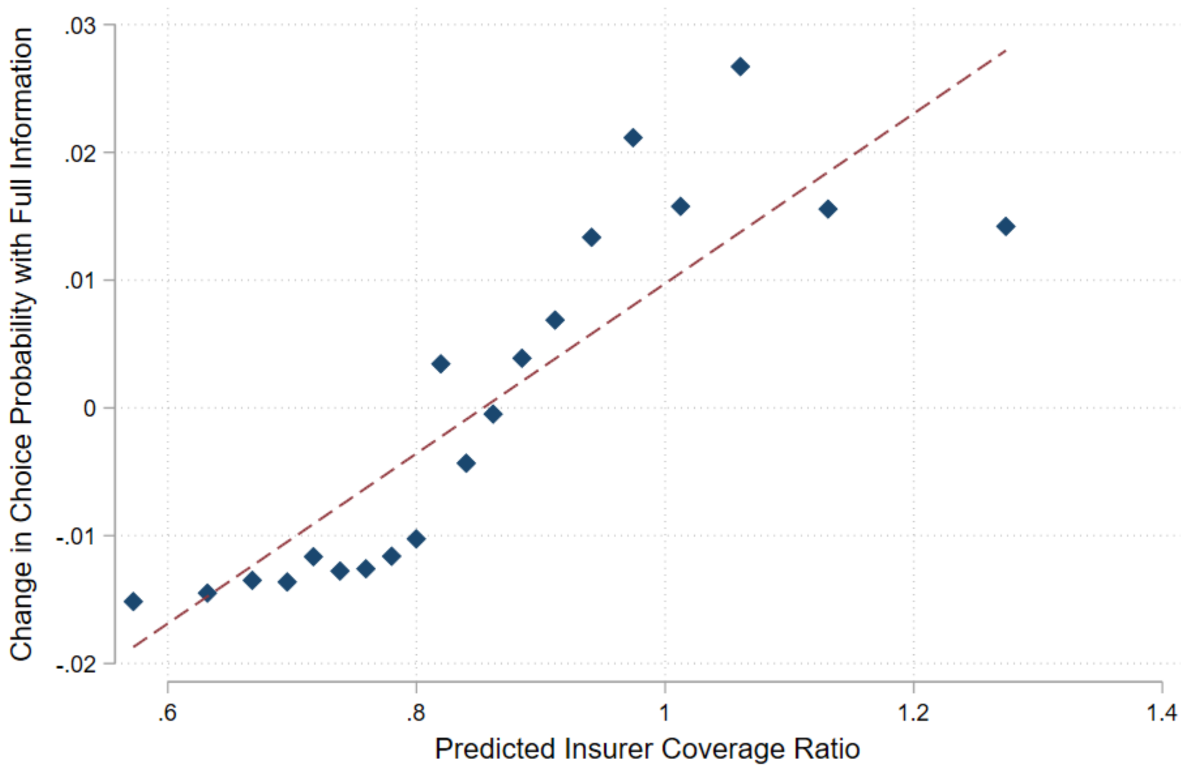


Figure 7: Intervention Impact on Insurer Choice Probability by Coverage Ratio

This figure highlights the potential impact of an intervention that leads homeowners to choose an insurer based on full information (as opposed to coverage neglect). The y-axis is the average change in the probability of a policyholder choosing a given insurer. The x-axis represents bins (twenty ventiles) of insurers' predicted pre-fire coverage ratios (i.e., the fitted values from regressing R_{ij} on policyholder characteristics and insurer fixed effects). The dotted line is the line of best fit.



A Appendix Tables

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 4. 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 (col. 1) and post-fire (col. 2) coverage ratios on their insurer's corresponding leave-one-out (LOO) average coverage ratios. 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	Post-Fire Coverage Ratio
	(1)	(2)
Av. Coverage Ratio (LOO)	0.767***	0.799***
	(0.069)	(0.121)
Log Income	0.100***	0.075***
	(0.023)	(0.024)
Log Home Value	0.475***	0.487***
	(0.104)	(0.098)
Log Replacement Cost	-0.998***	-0.919***
	(0.150)	(0.169)
Credit Score (z)	0.000	-0.003
	(0.007)	(0.007)
Has Mortgage	-0.039	-0.015
	(0.030)	(0.030)
Purchase Age (Decades)	0.056	0.034
	(0.032)	(0.036)
Purchase Age^2	-0.008	-0.004
	(0.008)	(0.009)
Home Age (Decades)	-0.073***	-0.066**
	(0.023)	(0.027)
Home Age^2	0.005**	0.004*
	(0.002)	(0.002)
Square Footage (1000s)	0.233***	0.198**
	(0.066)	(0.069)
Square Footage^2	-0.011*	-0.008
	(0.006)	(0.005)
Observations	736	736
F-Stat	121.89	43.47
R-squared	0.462	0.399

Table A3: 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.

	(1)	(2)
Dep. variable: 100 X	Sale (Dec. '22)	
Coverage Ratio (Pre-Fire)	3.12	
	(3.54)	
Coverage Ratio (Post-Fire)		-2.62
		(4.75)
Observations	2,353	2,353

B Measuring Premiums

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 Department 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, a quadratic of the amount of coverage purchased, and property and household characteristics (quadratic square footage, log zestimate, standardized credit score, log income, extended replacement cost coverage provisions, building condition rating, home age, and quadratic of policy years from inception). We use the leave-one-out fitted values from Equations A1 as the estimated premium for a given policy i at coverage Cov_i.

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