READ BUT NOT UNDERSTOOD? AN EMPIRICAL ANALYSIS OF CONSUMER COMPREHENSION IN HOMEOWNERS INSURANCE

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Modern contract law assumes that consumers meaningfully assent to the standard forms that govern their daily lives. However, this assumption is widely regarded as a legal fiction for two key reasons: first, most consumers do not read standard forms, and second, even those who do often struggle to fully comprehend their terms and implications. Although the lack of consumer reading has been well documented through empirical research, consumers' ability to comprehend standard-form contracts has received surprisingly little attention.

This Article addresses the latter issue by empirically examining whether providing excerpts from the dominant standard-form homeowners insurance policy improves consumer understanding of coverage. Through a series of survey-based experiments, we compare consumers' general beliefs about homeowners insurance with their beliefs after reading key policy excerpts. Our main finding is that providing policy language only moderately improved consumer understanding in some scenarios, while affirmatively decreasing accuracy in others. We interpret these results as suggesting that respondents often struggle with partial reading or misinterpreting policy provisions, especially when policy language grants broad coverage that is later restricted by specific exclusions in the same section—a common structural feature of insurance policies.

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These findings carry significant legal and regulatory implications. Even if most consumers do not read standard-form contracts, improving the readability and comprehensibility of standard-form terms can limit firms' discretion in disputes, enhance regulatory oversight of unfair provisions, and empower markets to penalize firms relying on excessively one-sided terms. This Article argues that addressing these challenges is essential to fostering fairer and more effective consumer protections.

INTRODUCTION

A foundational premise of modern contract law is that consumers meaningfully assent to the boilerplate agreements that shape their daily lives.[1] Yet this premise is widely rejected by academic commentators,

[1] See Restatement of Consumer Conts. § 2 (A.L.I. 2024); Margaret Jane Radin, Boilerplate: The Fine Print, Vanishing Rights, and the Rule of Law 12–13 (2013).

who routinely assail it as nothing more than a legal fiction.[2] These academic critiques typically rely on two key empirical observations. The first, which is often called the no-reading problem, is that most consumers accept the contracts they ostensibly agree to without attempting to read or understand them.[3] The second key observation is that even diligent consumers who attempt to parse standard-form contracts often struggle to grasp their full implications.[4] We label this second critique the no-understanding problem.

Although the no-reading and no-understanding problems are closely related, they can lead to different conclusions about what legal and regulatory rules should apply to consumer contracts. For instance, consumers who do not even attempt to read their contracts can plausibly be deemed to be making a personal, entirely rational, choice.[5] So framed, the judicial doctrine imposing a "duty to read" on consumers is coherent, albeit contestable.[6] However, deeming consumers to have assented to contracts that they are predominantly unable to comprehend is not just unreasonable, but arguably illogical.[7] If even diligent consumers cannot understand the standard forms to which they supposedly assent, then the contract law foundations of modern consumer law become easier to supplement, or perhaps even replace, with more proactive legal and regulatory interventions.[8]

[2] See, e.g., Radin, supra note 1, at 14; Imre Stephen Szalai, The Prevalence of Consumer Arbitration Agreements by America's Top Companies, 52 U.C. Davis L. Rev. Online 233, 236 (2019).

[3] See, e.g., Oren Bar-Gill, Seduction by Plastic, 98 Nw. U. L. Rev. 1373, 1376–77 (2004); Omri Ben-Shahar & Carl E. Schneider, More Than You Wanted to Know: The Failure of Mandated Disclosure 77 (2014); Melvin Aron Eisenberg, The Limits of Cognition and the Limits of Contract, 47 Stan. L. Rev. 211, 240–41 (1995).

[4] See, e.g., Tess Wilkinson-Ryan, A Psychological Account of Consent to Fine Print, 99 Iowa L. Rev. 1745, 1749 (2014); Melvin Aron Eisenberg, Comment, Text Anxiety, 59 S. Cal. L. Rev. 305, 309 (1986); Robert A. Hillman & Jeffrey J. Rachlinski, Standard-Form Contracting in the Electronic Age, 77 N.Y.U. L. Rev. 429, 436 (2002).

[5] See, e.g., Avery Katz, Your Terms or Mine? The Duty to Read the Fine Print in Contracts, 21 RAND J. Econ. 518, 519–20 (1990); Omri Ben-Shahar, The Myth of the 'Opportunity to Read' in Contract Law, 5 Eur. Rev. Cont. L. 1, 2 (2009).

[6] Charles L. Knapp, Is There a "Duty to Read"?, 66 Hastings L.J. 1083, 1085 (2015).

[7] See Uri Benoliel & Shmuel I. Becher, The Duty to Read the Unreadable, 60 B.C. L. Rev. 2255, 2277, 2282–83 (2019).

[8] See Daniel Schwarcz, A Products Liability Theory for the Judicial Regulation of Insurance Policies, 48 Wm. & Mary L. Rev. 1389, 1435–36 (2007); Oren Bar-Gill & Elizabeth Warren, Making Credit Safer, 157 U. Pa. L. Rev. 1, 98 (2008); CFPB Warns Against Deception in Contract Fine Print, Consumer Fin. Prot. Bureau (June 4, 2024, at 10:01 ET), https://www.consumerfinance.gov/about-us/newsroom/cfpb-warns-against-decep

More prosaically, the legal and regulatory measures available to counteract the no-reading and no-understanding problems in consumer contract law are often quite distinct. To encourage reading, regulations and judicial doctrines can push firms to draft shorter contracts,[9] highlight or capitalize key terms,[10] make contracts more accessible,[11] or require consumers to scroll through all terms or individually assent to specific terms before completing a transaction.[12] Conversely, improving consumer understanding of contracts requires a distinct set of potential legal and regulatory tools, including expanding and better enforcing quantitative and qualitative readability standards,[13] promoting technologies like "smart readers,"[14] requiring effective disclosures,[15] or

tion-in-contract-fine-print/ [https://perma.cc/24BT-BPAC]; Susan Block-Lieb & Edward J. Janger, Fit for Its Ordinary Purpose: Implied Warranties and Common Law Duties for Consumer Finance Contracts, 59 Hou. L. Rev. 551, 599 (2022).

[9] See Cynthia Adams, The Move Toward Using Plain Legal Language, 20 TYL, no. 4, Summer 2016, at 6, 6.

[10] See, e.g., Yonathan A. Arbel & Andrew Toler, ALL-CAPS, 17 J. Empirical Legal Stud. 862, 863 (2020); David A. Hoffman, Relational Contracts of Adhesion, 85 U. Chi. L. Rev. 1395, 1428–31 (2018) (detailing examples of Kickstarter's use of highlighting to improve comprehension).

[11] See Benedikt Schmitz & Charlotte Pavillon, Measuring Transparency in Consumer Contracts: The Usefulness of Readability Formulas Empirically Assessed, 9 J. Eur. Consumer & Mkt. L. 191, 191 (2020); George R. Milne & Mary J. Culnan, Strategies for Reducing Online Privacy Risks: Why Consumers Read (or Don't Read) Online Privacy Notices, 18 J. Interactive Mktg., no. 3, Summer 2004, at 15, 25.

[12] Cf. Jonathan A. Obar & Anne Oeldorf-Hirsch, The Biggest Lie on the Internet: Ignoring the Privacy Policies and Terms of Service Policies of Social Networking Services, 23 Info. Commc'n & Soc'y 128, 140 (2020) (finding that research participants merely scrolled to accept the terms and conditions).

[13] See John Aloysius Cogan Jr., Readability, Contracts of Recurring Use, and the Problem of Ex Post Judicial Governance of Health Insurance Policies, 15 Roger Williams U. L. Rev. 93, 100 (2010); Michael A. Blasie, The Rise of Plain Language Laws, 76 U. Mia. L. Rev. 447, 481–84 (2022) [hereinafter Blasie, Rise of Plain Language Laws]; Michael A. Blasie, Regulating Plain Language, 2023 Wis. L. Rev. 687, 708–11 [hereinafter Blasie, Regulating Plain Language]; see also Michelle Boardman, Insuring Understanding: The Tested Language Defense, 95 Iowa L. Rev. 1075, 1077 (2010) (proposing a "tested language defense" that would allow insurers to defend against ambiguity claims by demonstrating that their policy language was empirically tested for consumer comprehension).

[14] See Yonathan A. Arbel & Shmuel I. Becher, Contracts in the Age of Smart Readers, 90 Geo. Wash. L. Rev. 83, 115 (2022).

[15] Cf. Omri Ben-Shahar & Adam Chilton, Simplification of Privacy Disclosures: An Experimental Test, 45 J. Legal Stud. S41, S61 (2016) (employing a survey-based methodology and finding that simplifying disclosures according to "best practices" had no significant effect on consumer comprehension of contract terms).

using interpretive principles to incentivize firms to craft less ambiguous or technical terms.[16]

Despite the differing theoretical and practical implications of the no-reading and no-understanding critiques of modern consumer contract law, there exists surprisingly limited empirical evidence focused exclusively on the latter question of how well consumers can understand typical consumer contracts when they affirmatively attempt to do so.[17] What is more, the limited evidence that does exist suggests that the no-understanding critique may be overblown, at least when one focuses on the majority of consumers.[18] By contrast, an increasingly sizable literature confirms the widespread intuition that the no-reading problem is indeed real and pervasive.[19] Other important entries in the empirical literature document the combined effect of the no-reading and no-understanding problems, demonstrating that consumers often fail to appreciate the meaning of key terms like arbitration agreements and class action waivers contained within the broader contracts they receive.[20] But because these studies focus on consumer comprehension

[16] See Ian Ayres & Robert Gertner, Filling Gaps in Incomplete Contracts: An Economic Theory of Default Rules, 99 Yale L.J. 87, 120–21 n.147 (1989).

[17] See infra Part I; Lior Jacob Strahilevitz & Matthew B. Kugler, Is Privacy Policy Language Irrelevant to Consumers?, 45 J. Legal Stud. S69, S72–73 (2016); Uri Y. Hacohen, Amit Elazari & Talia Schwartz-Maor, A Penny for Their Creations—Apprising Users' Value of Copyrights in Their Social Media Content, 36 Berkeley Tech. L.J. 511, 531 (2021); Tess Wilkinson-Ryan, The Perverse Consequences of Disclosing Standard Terms, 103 Corn. L. Rev. 117, 120 (2017).

[18] See Omri Ben-Shahar & Lior Jacob Strahilevitz, Interpreting Contracts via Surveys and Experiments, 92 N.Y.U. L. Rev. 1753, 1787–88, 1791, 1794 (2017) (reporting the results of three surveys suggesting that, in the aggregate, surveyed consumers correctly alter their interpretation of contract terms that are redrafted to clarify the intended meaning).

[19] See Yannis Bakos, Florencia Marotta-Wurgler & David R. Trossen, Does Anyone Read the Fine Print? Consumer Attention to Standard-Form Contracts, 43 J. Legal Stud. 1, 3–4, 32 (2014); Jean R. Sternlight, Creeping Mandatory Arbitration: Is It Just?, 57 Stan. L. Rev. 1631, 1648 (2005); Ian Ayres & Alan Schwartz, The No-Reading Problem in Consumer Contract Law, 66 Stan. L. Rev. 545, 546 (2014); Florencia Marotta-Wurgler, Does Contract Disclosure Matter?, 168 J. Inst'l & Theoretical Econ. 94, 95–96 (2012).

[20] See Jeff Sovern, Elayne E. Greenberg, Paul F. Kirgis & Yuxiang Liu, "Whimsy Little Contracts" with Unexpected Consequences: An Empirical Analysis of Consumer Understanding of Arbitration Agreements, 75 Md. L. Rev. 1, 45–47 (2015) (reporting that a minority of respondents understood that mandatory arbitration provisions contained within broader contracts precluded them from litigating large disputes and that class action waivers prevented them from participating in a class action); Roseanna Sommers, What Do Consumers Understand About Predispute Arbitration Agreements? An Empirical Investigation, PLOS ONE, Feb. 23, 2024, at 1, 9–12, https://doi.org/10.1371/journal.pone.0296179 [https://perma.cc/986Y-ACWZ]; Arbel & Toler, supra note 10, at 866–67.

of entire contracts, they cannot clearly differentiate between the no-reading and no-understanding critiques of modern consumer contract law.

For these reasons, this Article empirically assesses how well typical consumers can understand key terms in one particularly important and pervasive type of standard-form consumer contract: homeowners insurance policies. Homeowners insurance provides a good setting to test consumer comprehension of contract language for several reasons.[21] First, a central goal of insurance law and regulation is to promote clear and comprehensible insurance policy language.[22] Toward this end, the primary rule of insurance law is that ambiguities are interpreted against the drafter,[23] and a substantial majority of states impose readability requirements on insurance policies.[24] Second, comprehensible policy language can play a potentially vital role in promoting fair insurance markets even if consumers do not read their policies at the time of purchase.[25] For instance, comprehensible insurance policy language can discourage insurers from unreasonably denying claims by empowering consumers, insurance agents, and lawyers to detect and challenge such coverage denials.[26] Finally, the terms and structure of homeowners insurance policies are somewhat standardized across the country,

[21] Because the terms of insurance policies, including homeowners insurance policies, are sometimes structured in ways that are unusual though perhaps not unique to the insurance context, our results may not be fully generalizable to all consumer contract settings.

[22] Kyle Logue, Daniel Schwarcz & Brenda J. Cude, The Value of Understandable Consumer Insurance Contracts, 8 Int'l Rev. Fin. Consumers, no. 1, June 2023, at 1, 2; Boardman, supra note 13, at 1077; Christopher C. French, Understanding Insurance Policies as Noncontracts: An Alternative Approach to Drafting and Construing These Unique Financial Instruments, 89 Temp. L. Rev. 535, 553 (2017); Kenneth S. Abraham & Daniel Schwarcz, Insurance Law and Regulation: Cases and Materials 151 (7th ed. 2020).

[23] See Kenneth S. Abraham, A Theory of Insurance Policy Interpretation, 95 Mich. L. Rev. 531, 531, 537–38 (1996); Michelle Boardman, Penalty Default Rules in Insurance Law, 40 Fla. St. U. L. Rev. 305, 327–28 (2013).

[24] See Cogan, supra note 13, at 120. To be sure, readability requirements often apply to consumer contracts other than insurance policies. See Blasie, Rise of Plain Language Laws, supra note 13, at 495; Blasie, Regulating Plain Language, supra note 13, at 703.

[25] See Daniel Schwarcz, Coverage Information in Insurance Law, 101 Minn. L. Rev. 1457, 1491 (2017).

[26] See Willem H. Van Boom, Pieter Desmet & Mark Van Dam, "If It's Easy to Read, It's Easy to Claim"—The Effect of the Readability of Insurance Contracts on Consumer Expectations and Conflict Behaviour, 39 J. Consumer Pol'y 187, 195 (2016); cf. Daniel Schwarcz, Transparently Opaque: Understanding the Lack of Transparency in Insurance Consumer Protection, 61 UCLA L. Rev. 394, 414 (2014) (explaining that opportunistic insurers may leverage complex coverage policies to delay payments or deny coverage).

allowing us to test widely used policy language, as well as less common variants.[27]

To better understand how well consumers can comprehend specific insurance policy terms, we designed and deployed a series of survey-based experiments, which we administered to approximately 2,500 current U.S. homeowners who were previously involved in the decision to purchase or renew a homeowners insurance policy. In these experiments, we asked a control group of respondents to evaluate the likelihood that a typical homeowners insurance policy would cover a variety of losses, described in vignettes we crafted for this research, without providing these respondents with relevant insurance policy language. We presented the same coverage vignettes to a treatment group of respondents whom we provided with relevant excerpts from the most common template for homeowners insurance policies in the United States: the 2010 ISO HO3 policy.[28] To determine whether the vignettes resulted in clear coverage or non-coverage, we relied on the combined expertise of the co-authors, which we confirmed by querying several advanced AI systems.[29] By comparing responses from participants who saw the operative policy language with those from participants who did not, we assessed how effectively that language fulfills its core function of giving policyholders meaningful notice of their coverage.[30]

We initially hypothesized that respondents provided with the relevant policy language would consistently offer more accurate answers about coverage than those without it. However, our results contradicted this hypothesis and diverged from prior literature, which suggested that consumers, on average, correctly interpret unambiguous contract

[27] See Daniel Schwarcz, Reevaluating Standardized Insurance Policies, 78 U. Chi. L. Rev. 1263, 1272–73 (2011).

[28] See generally Daniel Schwarcz, The Role of Courts in the Evolution of Standard Form Contracts: An Insurance Case Study, 46 BYU L. Rev. 471 (2021) (discussing the evolution of the ISO HO3 policy leading up to its 2010 revision).

[29] Notably, in one coverage scenario, an AI analysis produced by OpenAI o3 uncovered coverage issues that the human co-authors did not initially observe.

[30] In this Article, we focused our analysis on vignettes that resulted in unambiguous coverage determinations under the ISO HO3 policy. But we also tested consumers' responses to ambiguous, atypical, and potentially unenforceable policy language. Cf. Ben-Shahar & Strahilevitz, supra note 18, at 1783–87 (using a similar vignette-based survey methodology to test consumer interpretation of both ambiguous and unambiguous contract provisions). Here, too, our initial hypothesis—that providing ambiguous policy language would increase the likelihood that respondents would recognize that there was no clear answer to the coverage question—proved incorrect in at least some of the coverage vignettes we tested. We report and discuss these results in Appendix C.

language.[31] Across the seven coverage vignettes we tested, respondents in three vignettes were less accurate when provided with the policy language than those who were not. The reduction in accuracy was substantial—ranging from approximately 18 to 33 percentage points—and statistically significant at the 1% level. In a fourth vignette, there was no significant difference in accuracy between those who received the policy language and those who did not. Even in the remaining three vignettes, where respondents with access to the policy language performed better, the accuracy improvements were inconsistent across the vignettes and smaller than might be expected.[32]

The variation in our results appears to be best explained by the structure of the policy language provided to respondents, though this conclusion is speculative. Specifically, in cases where the policy language was associated with less accuracy in respondents' coverage assessments, the provisions were written in a way that could mislead readers who focused on only the first part of the excerpt. A careful reading of the initial portion often suggests one answer to the coverage question, whereas a thorough reading of the entire provision reveals the opposite answer to be correct.[33] This pattern suggests the existence of a type of problem not previously identified in the literature—a partial-reading or partial-understanding problem. Even more importantly, it

[31] Cf. Ben-Shahar & Strahilevitz, supra note 18, at 1801 (finding that survey respondents reliably shifted toward the intended interpretation when ambiguous contract language was revised for clarity).

[32] In particular, the percentage of respondents who provided accurate answers was higher by between roughly 13 percentage points on the low end and 34 percentage points on the high end across these four vignettes. In absolute terms, the percentage of respondents who received policy language and provided accurate answers to coverage questions ranged from roughly 20% to 72%. See infra Section IV.A, Figure 5.

[33] That is, for several of the coverage vignettes, it appears that the respondents may have read until they thought they understood the terms of the policy and then either stopped reading or stopped reading carefully. Determining which of those it was—partial reading or partial understanding—is not possible from our data, and further examination of that question would likely require qualitative research with consumers. One technique to further investigate the partial-reading or partial-understanding question is one-on-one cognitive interviews with consumers. In a cognitive interview, the interviewer gives the consumer the relevant document and asks the consumer to verbalize what they see and think as they interact with the document. See Gordon B. Willis & Anthony R. Artino Jr., What Do Our Respondents Think We're Asking? Using Cognitive Interviewing to Improve Medical Education Surveys, 5 J. Graduate Med. Educ. 353, 353 (2013). Another technique is eye tracking, in which technology allows the researcher to see the portions of a document that a consumer views online. Benjamin T. Carter & Steven G. Luke, Best Practices in Eye Tracking Research, 155 Int'l J. Psychophysiology 49, 50 (2020).

creates significant consumer protection concerns, as this contractual structure—in which broad coverage grants are later restricted by specific exclusions—is a pervasive structural feature of insurance policies.[34] Notably, we also found limited evidence that respondents' demographics or sophistication influenced the extent to which providing relevant policy language enhanced the accuracy of their coverage assessments.

Respondents who reviewed policy language reported greater confidence in their coverage assessments than those who did not. We found some evidence suggesting that highly confident respondents are more likely to provide accurate coverage assessments than their less confident peers.[35] But we found no statistically significant support to conclude that sophisticated consumers, higher-income consumers, or white consumers were more likely than their counterparts to provide accurate coverage assessments.[36] Nor did our results support a conclusion that seeing relevant policy language improved accuracy more for those in the selected subgroups than for their counterparts.[37] These results suggest that, contrary to the conventional wisdom, a broad swath of the population (and not just a "vulnerable" subgroup) has difficulty decoding the "plain meaning" of insurance policy language.[38]

We interpret our results, viewed as a whole, to have important theoretical and practical implications. On the theory side, they provide a novel reason to question the cornerstone of modern consumer law, that consumers have a "duty to read." Although it has long been evident that most consumers do not attempt to read standard consumer forms, our research further indicates that even when they do, they often fail to fully grasp the terms. More concretely, our findings cast doubt on techniques aimed at increasing contract readability—such as highlighting key terms—when these measures are not accompanied by efforts to improve consumer comprehension and engagement with all terms. Finally,

[34] See Abraham & Schwarcz, supra note 22, at 248, 529.

[35] But see Lawrence Solan, Terri Rosenblatt & Daniel Osherson, False Consensus Bias in Contract Interpretation, 108 Colum. L. Rev. 1268, 1285, 1290, 1292–93 (2008).

[36] See Yonathan A. Arbel, The Readability of Contracts: Big Data Analysis, 21 J. Empirical Legal Stud. 927, 964 (2024) (positing that studies of contract readability must account for "diverse cultural, cognitive, and educational backgrounds").

[37] Id.

[38] See, e.g., Alan Schwartz & Louis L. Wilde, Imperfect Information in Markets for Contract Terms: The Examples of Warranties and Security Interests, 69 Va. L. Rev. 1387, 1389 (1983).

because our results reveal that even consumers with higher levels of sophistication often struggle to understand complex commercial contract language, they challenge efforts to tailor legal or regulatory interventions based on perceived consumer sophistication.

Our analysis is organized into five parts. Part I reviews the existing literature, highlighting the surprising lack of empirical evidence on consumers' ability to understand the terms of standard-form contracts. In Part II, we outline our methodology testing this issue, and in Part III, we describe the data employed in our study. Part IV presents our results, and Part V explores their broader normative and practical implications.

I. SURPRISINGLY LIMITED EMPIRICAL EVIDENCE FOR THE NO-UNDERSTANDING CRITIQUE

An increasingly significant body of empirical research focuses on consumers' expectations and behaviors with respect to standard-form contracts. Numerous entries in this literature convincingly demonstrate that consumer assent to standard-form contracts is typically remarkably shallow. For instance, several studies empirically document the familiar reality that consumers routinely do not attempt to read the contract terms to which they ostensibly assent.[39] Other important entries in this literature document that consumers often have incorrect understandings of key terms contained within standard-form contracts, such as arbitration clauses and class action waivers.[40] Even so, consumers often feel morally bound to terms within standard-form contracts, irrespective of whether they view those terms as fair.[41] But despite an increasingly robust body of empirical contract literature, there exists surprisingly

[39] See Bakos et al., supra note 19; Marotta-Wurgler, supra note 19. One recent study found that changing a paragraph within an end-user license agreement to all-caps did not significantly improve respondents' ability to answer questions about that paragraph. Arbel & Toler, supra note 10, at 865. The most natural interpretation of this result is that all-caps does not impact the no-reading problem. In fact, statements in all-caps are less easily read than statements written in sentence style. Design for Readability, Harv. Univ.: Digit. Accessibility Servs., https://accessibility.huit.harvard.edu/design-readability [https://perma.cc/DB7L-Q6ZX] (last visited Jan. 24, 2026). However, Arbel and Toler's study has limited implications with respect to the no-understanding problem.

[40] See, e.g., Strahilevitz & Kugler, supra note 17, at S71.

[41] Wilkinson-Ryan, supra note 4, at 1760–61, 1764–65; Tess Wilkinson-Ryan & David A. Hoffman, The Common Sense of Contract Formation, 67 Stan. L. Rev. 1269, 1296–97 (2015).

limited research evaluating how well typical consumers can understand specific contract terms in isolation.

This Part explains that assessment of the literature. Section I.A begins by describing the most closely related evidence on point, which reports how consumers interpret and respond to specific contract terms with which they are presented. Although highly relevant to the no-understanding critique, most of this literature focuses on studying how consumers respond to potentially ambiguous or unenforceable contract terms. By contrast, limited work focuses specific attention on consumer understanding of the plain meaning of unambiguous contract terms. Section I.B then provides an overview of the increasingly expansive empirical literature examining related but importantly distinct issues, such as how well consumers understand entire contracts or tailored consumer disclosures, as well as what can be learned from objective readability assessments of consumer contracts and privacy policies.

A. Empirical Evidence Focused on Consumer Understanding of Specific Contract Terms

Although commentators frequently claim that most typical consumers would be incapable of understanding and appreciating the terms in standard-form contracts, there is surprisingly limited empirical evidence testing this proposition. In fact, our research has located only a small handful of studies since the modern internet age that directly tested how typical consumers (who are not students)[42] understand specific contract terms rather than the entirety of a consumer contract. Each of these studies focuses on testing consumer responses to ambiguous, unclear, or potentially unenforceable contract terms, rather than assessing consumers' capacity to correctly understand the unambiguous plain meaning of contract terms.[43]

[42] One article reports a survey of 177 college students regarding the meaning of several different remedy clauses in real estate developer form contracts. Debra Pogrund Stark, Jessica M. Choplin & Eileen Linnabery, Dysfunctional Contracts and the Laws and Practices That Enable Them: An Empirical Analysis, 46 Ind. L. Rev. 797, 806 (2013). According to the study, respondents' survey answers demonstrated "a widespread failure . . . to understand the impact of this type of clause on their rights after a breach." Id. at 799. We do not view this study to be terribly informative, however, given that the survey respondents were college students who could not be reasonably expected to be familiar with real estate developer form contracts.

[43] Another related, but ultimately distinct, recent study found that participants were less inclined to consider legal action when a $200 "processing fee" for parking violations was

The most directly related study we located is reported in a prominent recent article focused on testing consumer interpretations of potentially ambiguous contract terms, as part of the authors' broader proposal for a "survey interpretation method" to resolve consumer contract interpretation disputes.[44] Despite this focus, the article reports the results of three survey-based experiments testing consumer interpretations of ambiguous policy language and a simplified version of that same language—involving liability coverage under a homeowners insurance policy,[45] payment under a bonus agreement,[46] and coverage under a consumer-oriented aviation insurance policy.[47] In each of the three settings, providing survey respondents with simplified contract language altered respondents' survey answers in the predicted direction.[48] At the same time, the magnitude of the shift was often less than the authors anticipated, a result that they described as "admittedly disquieting" but ultimately dismissed because "the strong, statistically significant majority" supported an interpretation consistent with the plain meaning of the tested language.[49]

A second, closely related recent study used survey techniques to assess consumer reactions to potentially ambiguous privacy policies. In particular, the researchers found that consumers who were provided with key excerpts from different firms' privacy policies were generally equally likely to believe that they had consented to certain data collection practices irrespective of whether the excerpts they received

contained within a standard-form contract than when it was listed separately on a company's website. Wilkinson-Ryan, supra note 17, at 121–22. The article did not, however, focus on testing the capacity of consumers to understand potentially complex contract terms and apply that understanding as in the current study. Another study evaluated consumers' expectations of coverage based on simplified descriptions of insurance policy language crafted by the authors rather than the relevant policy language itself, finding that respondents significantly overestimated the likelihood that others would share their conclusions about coverage in each of the scenarios. Solan et al., supra note 35, at 1285.

[44] See Ben-Shahar & Strahilevitz, supra note 18, at 1784–88.

[45] Id. at 1787.

[46] Id. at 1791.

[47] Id. at 1793–95.

[48] Id. at 1785–95.

[49] Id. at 1791. Summarizing this evidence, the authors conclude that although "individual judgments and responses can be quirky and mystifying, . . . majoritarian judgments about contractual meaning are comprehensible," as "respondents are good at identifying ambiguity when it clearly exists, and that they shift in the right direction when the language is made clearer through experimental manipulations." Id. at 1801.

were deemed excessively vague by courts.[50] The implication is that consumers and judges may understand the same contract language differently.[51] But whether this was a result of consumers' failure to understand the plain meaning of the tested privacy policies or instead was attributable to varying conceptions of contested normative concepts like "assent" was not the focus of the article.

Another pair of highly relevant studies used corpus analysis and experiments to show that many contracts are difficult even for lawyers to understand due to excessively complex drafting, rather than inherently specialized legal concepts.[52] Using corpus analysis of approximately 10 million words from contracts compared to standard English genres, one study demonstrated that legal texts disproportionately contain low-frequency jargon, passive constructions, non-standard capitalization, and particularly challenging center-embedded clauses.[53] Further, the studies show that these features substantially reduce comprehension and recall among readers, even those with extensive experience reviewing contracts. Particularly relevant here are experimental results finding that lawyers often struggled to comprehend and recall actual terms in contracts, as compared to simplified versions of the same documents.[54] Additionally,

[50] Strahilevitz & Kugler, supra note 17, at S71. In the underlying experiment, researchers randomly assigned some respondents to receive explicit privacy policy language that courts had found sufficient to authorize contested data collection practices, and others to receive vague privacy policy language that courts had found insufficient to obtain consent. Id. at S74–75. Even though respondents often read these short excerpts closely, they generally believed that they consented to the challenged data collection practice irrespective of whether they received the explicit or vague policy language. Id. at S71.

[51] The results of the study could reflect differing views between consumers and judges about the nature of consent, or instead, it could reflect consumers' inability to appreciate the nuances within the tested language. See id. at S92. A similar point applies to a small portion of another article evaluating consumer understanding of specific license terms, part of a larger discussion of user-generated content licensing and user attitude toward those policies. See Hacohen et al., supra note 17, at 562–63 (finding widespread misunderstanding of legal terms like "moral rights" and "derivative works" in licensing agreements).

[52] Eric Martínez, Francis Mollica & Edward Gibson, Even Lawyers Do Not Like Legalese, 120 Proc. Nat'l Acad. Scis., no. 23, 2023, at 1, 2 [hereinafter Martínez et al., Even Lawyers Do Not Like Legalese], https://doi.org/10.1073/pnas.2302672120; see also Eric Martínez, Francis Mollica & Edward Gibson, Poor Writing, Not Specialized Concepts, Drives Processing Difficulty in Legal Language, Cognition, July 2022, at 1, 1–2 [hereinafter Martínez et al., Poor Writing], https://doi.org/10.1016/j.cognition.2022.105070 (finding that complex contract drafting made it difficult for laypersons to comprehend the terms).

[53] Martínez et al., Poor Writing, supra note 52, at 2–3.

[54] Martínez et al., Even Lawyers Do Not Like Legalese, supra note 52, at 2.

this study found that lawyers rated simplified contracts as equally enforceable and generally superior in quality, style-appropriateness, and client acceptance.[55]

The only studies we located that focused on evaluating the capacity of consumers to understand specific and *unambiguous* contract text are from the pre-internet era, when both consumer contracting practices and consumer survey methods were of course quite different than they are today. For instance, one study published in 1994 asked students and clerical staff at a university to answer questions about hypotheticals based on excerpts from several contracts, including a mortgage, an agreement for sale of property, a bank loan, and a renewal of a lease.[56] Even the highest-performing respondents only answered about two-thirds of the questions correctly for simplified contracts, and they achieved only about 50% accuracy with respect to questions regarding the original contract excerpts.[57] Another study dating from 1970 focused on how well tenants understood specific terms in their lease agreements, finding that about 70% of respondents "thought most of their lease terms were 'fairly easy to understand,'" even though only about 50% were able to answer simple questions about specific lease terms.[58] A third study asked undergraduate students to identify exculpatory clauses in two types of insurance contracts—a health club contract and an auto repair shop contract. For both types of contracts, about two-thirds of the participants correctly identified whether an exculpatory clause was present, and the majority understood the clause might prevent their recovery in a lawsuit.[59]

B. Empirical Evidence Related to Consumer Understanding of Contracts and Disclosures

An increasingly influential literature is focused on better understanding the content, implications, and practices of consumer

[55] Id.

[56] Michael E.J. Masson & Mary Anne Waldron, Comprehension of Legal Contracts by Non-Experts: Effectiveness of Plain Language Redrafting, 8 Applied Cognitive Psych. 67, 70–73 (1994).

[57] Id. at 76–77.

[58] Warren Mueller, Residential Tenants and Their Leases: An Empirical Study, 69 Mich. L. Rev. 247, 276 (1970).

[59] Dennis P. Stolle & Andrew J. Slain, Standard Form Contracts and Contract Schemas: A Preliminary Investigation of the Effects of Exculpatory Clauses on Consumers' Propensity to Sue, 15 Behav. Scis. & L. 83, 88–89 (1997).

contracting.[60] However, much of this literature illuminates issues related to, but distinct from, the no-understanding critique that is our focus.

First, numerous important entries in the literature evaluate consumer comprehension of contracts by asking study participants to answer questions after they are provided with the entirety of a consumer contract. These studies evaluate pre-dispute arbitration terms within broader contracts,[61] auto insurance policies,[62] travel insurance policies,[63] end-user license agreements,[64] refrigerator purchase contracts,[65] and many others.[66] In general, these studies find that consumers are typically unable to accurately answer basic questions about the contracts with which they are provided. Collectively, these studies provide invaluable

[60] See, e.g., Meirav Furth-Matzkin, Consumer Contracts in Action, 82 Mont. L. Rev. 97, 97 (2021); Oren Bar-Gill, The Behavioral Economics of Consumer Contracts, 92 Minn. L. Rev. 749, 749–50 (2008); Yonathan A. Arbel, Adminization: Gatekeeping Consumer Contracts, 71 Vand. L. Rev. 121, 124 (2018); Danielle D'Onfro, Error-Resilient Consumer Contracts, 71 Duke L.J. 541, 552–53 (2021).

[61] See Sommers, supra note 20, at 5, 9, 12 (providing respondents with a twenty-eight page Bank Deposit Account Agreement and instructing respondents to read it as they would "normally read contracts in [their] everyday li[ves]," and finding that less than 5% of respondents recalled that the contract mentioned anything about arbitration and less than 1% of respondents fully grasped the implications of the arbitration agreement); Sovern et al., supra note 20, at 4 (finding that the majority of survey respondents who were provided with a typical credit card contract containing an arbitration clause failed to recognize the existence of the arbitration clause, and even among those who did notice such a clause, the majority failed to appreciate that it would limit their ability to litigate a dispute in court).

[62] Van Boom et al., supra note 26, at 193–95 (finding that respondents who were provided with a relatively readable auto insurance policy that excluded coverage for "reckless driving" expected to receive a larger amount of compensation from their insurer as compared to the participants who received the less readable policy, but neither group was more likely than the other to contest an adverse coverage determination).

[63] Kathy Conklin, Richard Hyde & Fabio Parente, Assessing Plain and Intelligible Language in the Consumer Rights Act: A Role for Reading Scores?, 39 Legal Stud. 378, 389 (2019) (assessing whether readability scores assigned to insurance contracts were predictive of participants' understanding of the terms in their contract).

[64] Ayres & Schwartz, supra note 19, at 600–01 (reporting results of a survey regarding different terms in Facebook's end-user license agreement for purposes of identifying what types of terms might helpfully be included in a proposed "warning box").

[65] Jeffrey Davis, Protecting Consumers from Overdisclosure and Gobbledygook: An Empirical Look at the Simplification of Consumer-Credit Contracts, 63 Va. L. Rev. 841, 878–79 (1977) (finding that respondents who received the entirety of a refrigerator purchase contract struggled to answer basic questions about its terms, including the cost of credit and the definition of default).

[66] See, e.g., Rishab Bailey, Smriti Parsheera, Faiza Rahman & Renuka Sane, Disclosures in Privacy Policies: Does "Notice and Consent" Work?, 33 Loy. Consumer L. Rev. 189, 196, 230 (2021) (finding that across five Indian online services, consumers demonstrated a "low understanding" of the language in the privacy policies).

evidence regarding the combined impact of the no-reading and no-understanding problems in contract law, which has important implications for both the normative foundations of modern contract law and optimal regulatory strategies.

At the same time, however, these studies are generally limited in their ability to disentangle the no-reading and no-understanding critiques of modern contract law because it is difficult to know whether respondents' partial understanding is due to their decision not to read the relevant text or to their inability to understand text that is specifically brought to their attention. Several of the studies, moreover, suggest that the former (no-reading) explanation is dominant, both because consumers in these studies often report failing to notice relevant terms[67] and because some of the studies suggest that the best way to improve consumer understanding is to shorten the length of the contract with which consumers are provided.[68]

Another relevant branch of empirical research focuses on objectively assessing the readability of various types of consumer contracts but not testing how well consumers can actually understand these contracts. For example, several studies used quantitative readability metrics such as the Flesch Reading Ease and Flesch-Kincaid Grade Level tests, applying them to consumer-facing contracts from retailers, digital platforms, software firms, banks, and credit card issuers.[69] The research generally has found that consumer contracts are typically written at a college level, which is significantly higher than the average American adult's

[67] See, e.g., Sommers, supra note 20, at 9.

[68] See, e.g., Alexander J. Wulf & Ognyan Seizov, How to Improve Consumers' Understanding of Online Legal Information: Insights from a Behavioral Experiment, 56 Eur. J.L. & Econ. 559, 560, 578–79 (2023) (testing the impact of different terms and conditions in pre- and post-purchase settings, and finding that consumer accuracy was greatest with respect to simplified contracts in the post-contract setting); Victoria C. Plaut & Robert P. Bartlett, III, Blind Consent? A Social Psychological Investigation of Non-Readership of Click-Through Agreements, 36 Law & Hum. Behav. 293, 304 (2012) (finding that shortening click-through agreements had the greatest effect on increased comprehension).

[69] See Michael L. Rustad, Why a New Deal Must Address the Readability of U.S. Consumer Contracts, 44 Cardozo L. Rev. 521, 524–25 (2022); Benoliel & Becher, supra note 7, at 2259, 2271–72; Michael L. Rustad & Thomas H. Koenig, Wolves of the World Wide Web: Reforming Social Networks' Contracting Practices, 49 Wake Forest L. Rev. 1431, 1459–61 (2014); Shmuel I. Becher & Uri Benoliel, Law in Books and Law in Action: The Readability of Privacy Policies and the GDPR, *in* Consumer Law and Economics 179, 193–94 (Klaus Mathis & Avishalom Tor eds., 2021).

reading level.[70] Although instructive, these results are constrained by the well-known limitations of quantitative readability scores, which use average sentence length and number of syllables per word to score the readability of text.[71] By contrast, these scores ignore the number of sentences used to convey an idea, the organization and formatting of the language, the use of technical words or jargon (which need not contain an unusually large number of syllables), the ordering of words and concepts, and the extent to which words are put together in logical and clear sentences.[72]

A third relevant, yet distinct, strand of literature empirically examines the effectiveness of summary consumer disclosures that are not themselves contracts.[73] However, these studies offer limited insights into consumers' ability to understand specific contract terms. This limitation arises because consumer disclosures are designed to simplify or omit complex details, focusing instead on conveying a limited set of key features about another document, business practice, or risk.[74] By contrast, consumer contracts generally cannot simplify or disregard complex details without risking alterations to the legal rights and obligations of the parties or introducing unnecessary ambiguities and uncertainties.[75] While these complexities suggest that consumers are likely to find contracts harder to understand than disclosures,

[70] Nat'l Ctr. for Educ. Stat., U.S. Dep't of Educ., Data Point: Adult Literacy in the United States (July 2019), https://nces.ed.gov/pubs2019/2019179/index.asp [https://perma.cc/5QLC-8VUM]. However, other work has described the claim that Americans cannot read beyond the eighth-grade level as an "unsubstantiated myth." See Arbel, supra note 36, at 927, 964–65.

[71] George R. Klare, Assessing Readability, 10 Reading Rsch. Q. 62, 69 (1974).

[72] See Janice (Ginny) Redish, Readability Formulas Have Even More Limitations Than Klare Discusses, 24 ACM J. Comput. Documentation 132, 133, 136 (2000).

[73] See, e.g., Ben-Shahar & Schneider, supra note 3, at ix; Lauren E. Willis, Against Financial-Literacy Education, 94 Iowa L. Rev. 197, 271 (2008); James M. Lacko & Janis K. Pappalardo, The Failure and Promise of Mandated Consumer Mortgage Disclosures: Evidence from Qualitative Interviews and a Controlled Experiment with Mortgage Borrowers, 100 Am. Econ. Rev. 516, 516 (2010); Ben-Shahar & Chilton, supra note 15, at S47; Roger A. Formisano, The NAIC Model Life Insurance Solicitation Regulation: Measuring the Consumer Impact in New Jersey, 48 J. Risk & Ins. 59, 77–79 (1981); Brenda J. Cude, Insurance Disclosures: An Effective Mechanism to Increase Consumers' Insurance Market Power?, 24 J. Ins. Reg., no. 2, Winter 2005, at 57, 69.

[74] Another reason companies provide such disclosures is to minimize their own risk of liability, regulatory intervention, or consumer complaints. See, e.g., Ben-Shahar & Schneider, supra note 3, at 12, 38.

[75] See Lori D. Johnson, Say the Magic Word: A Rhetorical Analysis of Contract Drafting Choices, 65 Syracuse L. Rev. 451, 452–54 (2015).

countervailing forces may improve contract clarity relative to the clarity of disclosures. For instance, firms often receive feedback from courts about the interpretation and clarity of their contract language, enabling them to revise ambiguous terms.[76] This iterative process, particularly for widely used contract language that is frequently litigated, has the potential to enhance the overall clarity of contracts.[77]

II. METHODOLOGY

In light of the surprisingly limited evidence in the literature about consumers' capacity to understand contract terms, we conducted a series of survey-based experiments designed to evaluate how well consumers understand homeowners insurance policy language.[78] What distinguishes our study from most of those described above is that, instead of giving the respondents the entire contract and hoping they would read all of the provisions (including the relevant ones), we gave them relatively short excerpts of text that contained only the key provisions relating to a specific coverage question with which they were presented. The idea was that, given the short and clearly relevant nature of the policy language provided, respondents could be counted on to read it. And, as we discuss further below, there is ample evidence that they did in fact read the supplied policy language—or at least parts of it. Section II.A describes our methodology in more detail and addresses some methodological objections and limitations. Section II.B then details the specific vignettes and policy language we used in our surveys.

A. Experimental Design

1. Methodological Overview

The specifics of our methodology are straightforward. We selected four types of risks typically associated with homeowners insurance policies—three property-related (earthquake, deck collapse, and fire)

[76] See Abraham & Schwarcz, supra note 22, at 45; Mark A. Geistfeld, Interpreting the Rules of Insurance Contract Interpretation, 68 Rutgers U. L. Rev. 371, 410 (2015).

[77] See Michelle E. Boardman, The Unpredictability of Insurance Interpretation, 82 Law & Contemp. Probs. 27, 28–29 (2019).

[78] We created the survey instrument, and all questions were original to the survey. Administration of the survey was deemed to be exempt from Institutional Review Board ("IRB") review by the University of Minnesota's IRB office.

and one personal liability. Next, we found the policy language in the 2010 ISO HO3 policy most relevant to each type of risk. Then, two of the co-authors (Schwarcz and Logue) crafted vignettes for each type of risk, including some in which Schwarcz and Logue determined that the policy language unambiguously indicated coverage ("clear coverage"), and some in which the language unambiguously indicated no coverage ("clear non-coverage"). They also crafted various vignettes in which it was unclear whether the policy would cover the event described in the vignette ("unclear coverage"). All co-authors then workshopped the vignettes to confirm that they were clearly written and resulted in the anticipated coverage results. To confirm that the vignettes produced the anticipated coverage results, the authors also evaluated coverage results using several advanced generative AI tools.[79]

We randomly assigned respondents to a treatment or a control group.[80] Respondents in the control group were given the following general instructions:

> In this part of the survey, we ask specific questions about what you think homeowners insurance does and doesn't cover. We aren't giving you any specific insurance policy language to read, but want you to answer based on your existing understanding of this type of insurance. That might come from what you know about your own policy, from conversations you've had with an insurance professional, or from any other source you have previously consulted. Please do not conduct any new research to answer these questions.

Respondents in both the treatment and the control groups were then presented with a subset of the vignettes. Both the specific vignettes they received and the order in which they received them were randomized, though every respondent received vignettes relating to one of the four sets of policy language that are described below.

[79] See Yonathan Arbel & David A. Hoffman, Generative Interpretation, 99 N.Y.U. L. Rev. 451, 487–89 (2024). In particular, we ran the vignette and policy language through several OpenAI ChatGPT models, including OpenAI o3, which was the most advanced "reasoning" model offered by OpenAI at the time. This analysis confirmed our assessments for all of the vignettes except one: the Deck Collapse Clear Non-Coverage Scenario One, which we further describe below. See infra Section II.B.

[80] Because we were using the data for multiple purposes, we assigned roughly 60% of the sample to the treatment group to ensure sufficient statistical power to answer different questions.

After each vignette, control group respondents were asked two questions. The first was, "Do you think a typical homeowners policy would cover [the loss described in the vignette]?" Responses were assessed on a 1–5 Likert scale anchored by "Definitely not covered" and "Definitely covered." To distinguish how definitive a respondent's coverage assessment was from how confident they were in that assessment, we also asked respondents a second question after each vignette: "How confident are you that your answer is correct?" Responses were again collected on a 1–5 Likert scale anchored by "Not confident at all" and "Extremely confident."

Treatment group respondents were presented with the same coverage vignettes as those in the control group, with the subset of vignettes and the order in which they received them again randomized. However, these treatment group respondents were provided with the following initial set of instructions:

> In this part of the survey we ask specific questions about what homeowners insurance does and doesn't cover. Our goal in this section is to learn how well consumers who take the time to read carefully can understand common insurance policy language. You'll be given a series of hypothetical events (an earthquake, a deck collapsing, etc.) that homeowners insurance might cover. For each event, you'll be given part of a real-world insurance policy. Your task is to determine if you think the policy would cover the loss described in the event. *Please assume there's no other language in the homeowners policy that would prevent the policy from covering the loss*.

For each vignette, treatment group respondents saw the specific insurance policy language relevant to that vignette. The formatting of the online survey allowed participants to simultaneously review the coverage vignette and the relevant policy language. The policy language itself was edited only minimally to ensure that all relevant text to answer the coverage question was included in the excerpt without creating numbering or lettering discrepancies.

In the final section of the survey, all respondents were asked basic questions about their experience with homeowners insurance. They also were asked to provide key demographic and economic information.[81]

Our primary hypothesis was that respondents who saw relevant insurance policy language would be more likely to accurately assess coverage than respondents who did not receive this policy language. We also hypothesized that providing policy language would increase respondents' reported confidence in their perceptions of coverage and that respondents who reported high levels of confidence in their coverage assessments would in fact be more accurate in those assessments, both in the control and treatment groups. Finally, drawing on a set of predictors commonly examined in the financial literacy literature, we hypothesized that respondents who were more sophisticated as it relates to insurance, those with higher incomes, and whites would provide more accurate coverage assessments than their counterparts.[82]

Because our methodology used a randomized controlled experiment, many of our conclusions are evident from summary statistics. However, to confirm statistical significance and control for unintended variations in groupings of respondents, we also tested the first two hypotheses by estimating a series of logistic regressions in which the dichotomous outcome (accurate perceptions of coverage or confidence in coverage assessments) was a function of having received policy language, controlling for past experiences with insurance and economic and demographic characteristics. Similarly, to test the final hypothesis, we estimated a series of logistic regressions in which accurate perceptions of coverage was a function of reporting high levels of confidence in

[81] With respect to demographic characteristics, we asked respondents about their age, gender, race or ethnicity, marital status, and geographic location. The economic characteristics were education, income, and employment status. To assess their experiences with homeowners insurance, we asked whether they had ever sold insurance, whether they had ever been the final decision-maker in purchasing or renewing a homeowners insurance policy, whether they had attempted to read their own homeowners insurance policy, whether they felt that they understood the terms of their own homeowners insurance policy, whether they had previously switched insurance carriers, and whether they currently held negative views of insurers. We also asked them about factors important to them when they buy homeowners insurance and their past claims experience.

[82] See, e.g., Annamaria Lusardi & Olivia S. Mitchell, Financial Literacy Around the World: An Overview, 10 J. Pension Econ. & Fin. 497, 498, 504, 506 (2011) (reporting that financial-knowledge scores vary with demographic and socioeconomic characteristics, including income, education, and, in U.S. samples, race/ethnicity).

coverage assessments, along with the same experiential, demographic, and economic controls used in previous analyses.

2. Methodological Objections and Limitations

One concern about our experimental design is the heightened cognitive load experienced by the treatment group relative to the control group. Although we asked both the control and treatment groups to respond to the same number of insurance coverage vignettes, we asked only respondents in the treatment group to carefully read four policy excerpts of a combined length of 756 words. We did not ask the control group to read any additional materials. Thus, it is possible that the reliability of treatment group responses declined faster from the first-encountered vignette to the last-encountered vignette relative to control group responses.[83]

We are confident, however, that cognitive load does not drive our study's results for two reasons. First, the control group and treatment group surveys were identical up until the first vignette, so there was no difference in cognitive load at the outset of the experiment. Second, our survey instrument was programmed to randomize the order in which the vignettes were delivered to respondents in both the treatment and control groups.[84] The randomization allows us to test whether the increased cognitive load on the treatment group explains our results by focusing on the first vignettes encountered by all participants. The results of this robustness test, which are reported in the Appendix, mirror those of our main analyses.[85]

[83] The most reliable way to counter this potential effect would have been to ask control group respondents to read unrelated excerpts of similar length and difficulty in between vignettes.

[84] Of our analysis sample of 2,440 respondents, 801 individuals first received one of the earthquake vignettes, 822 individuals first received one of the deck collapse vignettes, and 817 individuals first received one of the slip-and-fall vignettes. The electrical fire vignette was not part of the randomization algorithm, so all respondents received that vignette fourth.

[85] See infra Appendix B, Table A13. In unreported analyses, we also compared outcomes from the first-encountered vignette for treatment-group respondents and the third-encountered vignette for control-group respondents (at which point control-group respondents had experienced a comparable or perhaps higher cognitive load than treatment-group respondents) and found that the results in those analyses are also consistent with our main results.

B. Coverage Vignettes and Policy Language

We applied the methodology described above to seven different coverage vignettes (two earthquake, two slip-and-fall, two deck collapse, and one electrical fire) using four policy language excerpts from the ISO HO3 policy that applied to the chosen vignettes. In each of the vignettes, the policy language supplied to the treatment group unambiguously resolved the coverage question based on the co-authors' analysis, as confirmed by subsequent AI analysis. As the Appendix describes, we also tested several vignettes in which the policy language provided to the treatment group did not clearly and unambiguously resolve the coverage question.[86]

1. Earthquake Policy Language: Clear Coverage Vignette and Clear Non-Coverage Vignette

The first pair of vignettes, described in Figure 1, focused on a loss involving an earthquake. In the clear non-coverage vignette in this pair, there was a direct loss to a home caused by an earthquake (a loss that the policy language unambiguously excluded). In the clear coverage vignette, the home was damaged by a fire that was triggered by the earthquake (a loss that the policy language unambiguously covered).[87] For both vignettes, the provided policy language focused on the earthquake exclusion in the ISO HO3 policy. The descriptions of the loss scenario used in these two vignettes, as well as the governing language from the ISO HO3 policy that the treatment group was given, are shown in Figure 1.

[86] See infra Appendix C.

[87] The loss is "clearly covered" because it is a "loss by fire" that results directly from the earthquake. To see how this works, note the structure of the provision. It first grants coverage ("We insure against direct physical loss to covered property."). Then, it excludes from coverage those losses resulting from certain causes ("We do not insure for loss caused directly or indirectly by any of the following" causes); and it lists "Earth Movement," which includes "Earthquake," as one of those causes. See infra Figure 1. Finally, it carves out of the earth movement exclusion—and therefore provides coverage for—any "direct loss by fire . . . resulting from any of the above," including earth movement. The back-and-forth structure, while not necessarily easy to follow, produces a clear result in this case. It should also be noted that this back-and-forth structure is common in insurance policies. See Abraham & Schwarcz, supra note 22, at 248, 529. Whether it is common in other types of consumer contracts is unclear.

Figure 1. Instruction and Policy Language Distributed in Earthquake Clear Coverage and Clear Non-Coverage Vignettes

Variations in Earthquake Coverage Scenario	Control Group (No policy language)	Treatment Group (ISO HO3 Policy Language)
Clear Non-Coverage Scenario: A magnitude 6.0 earthquake strikes near your home. The shaking from the earthquake causes severe damage to your home's foundation. Major repairs are required.	Respondents instructed to answer coverage scenario based on their existing understanding of what a typical homeowners policy would cover, without looking at any specific insurance policy language or conducting any research.	We insure against direct physical loss to covered property. We do not insure for loss excluded under the Exclusions Section. **Section I -- Exclusions** A. We do not insure for loss caused directly or indirectly by any of the following. Such loss is excluded regardless of any other cause or event contributing concurrently or in any sequence to the loss. These exclusions apply whether or not the loss event results in widespread damage or affects a substantial area. 1. Earth Movement Earth Movement means: a. Earthquake, including land shock waves or tremors before, during or after volcanic eruption; b. Landslide, mudslide or mudflow; c. Subsidence or sinkhole; or d. Any other earth movement including earth sinking, rising or shifting. This Exclusion A.1 applies regardless of whether any of the above, in A.1.a. through A.1.d., is caused by an act of nature or is otherwise caused. However, direct loss by fire, explosion or theft resulting from any of the above, in A.1.a through A.1.d., is covered.
Clear Coverage Scenario: A magnitude 6.0 earthquake strikes near your home. The shaking from the earthquake knocks down an electrical pole in front of your home, which triggers a fire. The fire spreads to your home. Major repairs are required.		

2. Slip-and-Fall Liability Policy Language: Clear Coverage Vignette and Clear Non-Coverage Vignette

In the second pair of vignettes, a homeowner is sued by an individual who sustained injuries in a slip-and-fall accident that occurred on the homeowner's front walkway. For both vignettes, the supplied policy language from the ISO HO3 policy excluded coverage for liability arising from business conducted at one's home. In the clear coverage vignette, the accident takes place during a small social gathering of friends whom the homeowner hosts. Such an accident falls under the general liability coverage provided in the policy. For the clear non-coverage vignette, the accident victim is a customer of the homeowner's small business, which they operate from their home. The policy language unambiguously states that this liability is excluded from coverage. Figure 2 provides the language used for these paired vignettes as well as the governing language from the ISO HO3 policy provided to the treatment group.

Figure 2. Instruction and Policy Language Distributed in Slip-and-Fall Liability Clear Coverage and Clear Non-Coverage Vignettes

Variations in Slip-and-Fall Coverage Scenario	Control Group (No policy language)	Treatment Group (ISO HO3 Policy Language)
Clear Non-Coverage Scenario: Your primary income comes from a small bakery that you run out of your home. Customers place orders online and come to your front door to pick up the baked goods. The morning after a snowstorm, you shoveled your front walkway. But, you didn't shovel the steps leading up from the sidewalk to your front walkway. One of your customers slips on these steps, suffers a broken leg and concussion, and sues you for negligence.	Respondents instructed to answer coverage scenario based on their existing understanding of what a typical homeowners policy would cover, without looking at any specific insurance policy language or conducting any research.	If a claim is made or a suit is brought against an insured for damages because of bodily injury or property damage caused by an occurrence to which this coverage applies, we will pay up to our limit of liability for the damages for which an insured is legally liable. **Exclusions** Liability Coverage does not apply to the following: (1) "Business" a. Bodily injury or property damage arising out of or in connection with a "business" conducted from an insured location or engaged in by an insured, whether or not the "business" is owned or operated by an insured or employs an insured. This Exclusion applies but is not limited to an act or omission, regardless of its nature or circumstance, involving a service or duty rendered, promised, owed, or implied to be provided because of the nature of the "business". **DEFINITIONS** "Business" means: a. A trade, profession or occupation engaged in on a full-time, part-time or occasional basis; or b. Any other activity engaged in for money or other compensation, except the following: (1) One or more activities, not described in (2) through (4) below, for which no insured receives more than $2,000 in total compensation for the 12 months before the beginning of the policy period; (2) Volunteer activities for which no money is received other than payment for expenses incurred to perform the activity; (3) Providing home day case services for which no compensation is received, other than the mutual exchange of such services; or (4) The rendering of home day care services to a relative of an insured.
Clear Coverage Scenario: It snows the night before you are scheduled to host a small social gathering of friends and family at your home. Although you shoveled your front walkway, you didn't shovel the steps leading up from the sidewalk to your front walkway. One of your guests slips on these steps, suffers a broken leg and concussion, and sues you for negligence.		

3. Deck Collapse Policy Language: Two Clear Non-Coverage Vignettes

The third pair of coverage vignettes we tested involved the collapse of a deck due to a termite infestation in its structural support beams. For both vignettes, the policy language involved the collapse coverage in the ISO HO3 policy. In the first vignette, the loss was not covered because the damage to the deck was not itself the "direct result of the collapse of a building or any part of a building," under clause (b) of the policy excerpt. This conclusion follows from the fact that the policy distinguishes a deck from a building, and the direct cause of the deck collapse was damage to its independent support beams rather than damage to the building itself.[88] In the second vignette, the loss is not

[88] We initially had identified this coverage vignette as a clear coverage scenario, reasoning that the cause of the damage was covered under clause (a)(2) as "[i]nsect or vermin damage, to a building or any part of a building, that is hidden from view" and that was not known to the insured prior to the collapse. However, we reversed this determination after several advanced AI reasoning models noted that the loss would not, in fact, be covered due to clause (b) because the damage to the deck was not, in fact, a direct result of the collapse of a

covered both for this reason, and also for a second reason: the homeowner first learned of the termite infestation before the collapse and then ignored warnings to replace the damaged beams, meaning that the presence of insect damage was "known to an 'insured' prior to collapse" under clause (a)(2), which specifies the relevant potentially covered cause of a deck collapse. Figure 3 contains the details of the two coverage vignettes as well as the policy language from the ISO HO3 pertaining to this issue, which was provided to the treatment group.

Figure 3. Instruction and Policy Language Distributed in Deck Collapse Clear Non-Coverage Vignettes

Variations in Deck Collapse Coverage Scenario	Control Group (No policy language)	Treatment Group (ISO HO3 Policy Language)
Clear Non-Coverage Scenario One: Two large wooden beams support your home's deck. Termites have burrowed into the beams causing serious structural damage, but you're totally unaware of that fact because the support beams are not visible from the outside. As a result of the termite infestation, the deck collapses suddenly and is destroyed.	Respondents instructed to answer coverage scenario based on their existing understanding of what a typical homeowners policy would cover, without looking at any specific insurance policy language or conducting any research.	We do not insure for loss involving collapse, except as provided in the Collapse Coverage. **Collapse** a. We insure for direct physical loss to covered property involving abrupt collapse of a building or any part of a building if such collapse was caused by one or more of the following: (1) Decay, of a building or any part of a building, that is hidden from view, unless the presence of such decay is known to an "insured" prior to collapse; (2) Insect or vermin damage, to a building or any part of a building, that is hidden from view, unless the presence of such damage is known to an "insured" prior to collapse; (3) Weight of contents, equipment, animals or people; (4) Weight of rain which collects on a roof; or (5) Use of defective material or methods in construction, remodeling or renovation if the collapse occurs during the course of the construction, remodeling or renovation. b. Loss to an awning, fence, patio, deck, pavement, swimming pool, underground pipe, flue, drain, cesspool, septic tank, foundation, retaining wall, bulkhead, pier, wharf or dock is not included under a.(1) through (5) above, unless the loss is a direct result of the collapse of a building or any part of a building. c. This coverage does not increase the limit of liability that applies to the damaged covered property.
Clear Non-Coverage Scenario Two: Two large wooden beams support your home's deck. Termites have burrowed into the beams causing serious structural damage. You discover the termites and promptly consult a structural engineer, who tells you of the damage and says you must replace the beams right away or the deck likely will collapse. You ignore the engineer's advice and do nothing. As a result of the termite infestation, the deck collapses suddenly and is destroyed.		

building or any part of a building. In other words, even the co-authors with expertise in insurance policy interpretation fell prey to the very same mistake that many of our survey respondents experienced: we read through the relevant policy text until we reached a coverage conclusion and then failed to fully internalize the meaning of later text that was inconsistent with this initial determination.

4. Electrical Fire Policy Language: Clear Coverage Vignette

For the final policy language excerpt we tested, we used only a clear coverage vignette and did not test a clear non-coverage vignette involving the ISO HO3 policy language.[89] The policy language focused on the exclusion for the insured's neglect to minimize damage after a loss has occurred. The loss in the vignette involved a fire caused by a faulty electrical switch, which the insured had earlier decided not to use after observing the switch produce sparks. The vignette and the relevant policy language are described in Figure 4. Although the policy states that losses arising out of neglect are not covered, the term "neglect" is defined to apply only to the failure to take reasonable steps to "save and preserve property at and after the time of a loss." As such, the loss described in the vignette is unambiguously covered.

[89] We initially planned to test a clear non-coverage vignette by using nonstandard policy language that would have excluded the described loss. However, during data analysis, we realized that the appropriate interpretation of this clear non-coverage vignette was complicated by the fact that we had specifically instructed the control group to supply answers based on "a typical homeowners insurance policy," and the policy language we used for the clear non-coverage vignette was decidedly atypical. It is therefore difficult to interpret the gap between the control and the treatment group as we did for the ISO HO3 policy language. For that reason, we report the results for this vignette in Appendix C, which describes the impact of providing policy language where the existence of coverage is potentially ambiguous. For completeness, however, we note that providing the nonstandard policy language whose plain meaning appears to exclude coverage for the electrical fire did indeed have a large and statistically significant impact on respondents, who thought the loss would not be covered.

Figure 4. Instruction and Policy Language Distributed in Electrical Fire Clear Coverage Vignette

Coverage Scenario	Control Group (No policy language)	Treatment Group (ISO HO3 Policy Language)
Clear Coverage Scenario: An electrical switch in your home's guest room starts to spark when you turn on the light. Instead of repairing the switch, you simply decide not to use the room. Two months later, however, you forget about the malfunctioning light switch, go into the room, and flip the switch on. The resulting sparks trigger a fire that burns down your home.	Respondents instructed to answer coverage scenario based on their existing understanding of what a typical homeowners policy would cover, without looking at any specific insurance policy language or conducting any research.	We insure against direct physical loss to covered property. We do not insure for loss excluded under the Exclusions Section. **SECTION I -- EXCLUSIONS** We do not insure for loss caused directly or indirectly by any of the following. Such loss is excluded regardless of any other cause or event contributing concurrently or in any sequence to the loss. These exclusions apply whether or not the loss event results in widespread damage or affects a substantial area. 1. Neglect Neglect means neglect of an insured to use all reasonable means to save and preserve property at and after the time of a loss.

III. DATA

We recruited all survey respondents through Dynata, a large survey platform that maintains geographically and demographically representative panels of willing survey participants. Dynata takes a variety of measures to help ensure data quality, including vetting participants before they are allowed to join panels, monitoring their behavior across different projects, and evaluating their behavior within individual surveys.[90]

To participate in our surveys, respondents had to meet two conditions: (1) they had to be at least twenty-one years old, and (2) they had to have previously been involved in the decision to purchase or renew a homeowners insurance policy for a home in the United States that they owned or in which they otherwise had a financial interest.[91] In total, we collected survey responses from 2,500 respondents who met these criteria. We asked Dynata to collect responses from participants

[90] A Shared Vision for Data Quality, Dynata (July 7, 2023), https://www.dynata.com/a-shared-vision-of-data-quality/ [https://perma.cc/J6ZZ-6D6X].

[91] For purposes of this screening question, we indicated that a renters or condo policy is not considered a homeowners policy.

who were diverse and well distributed across the demographic and economic variables we chose.[92]

To ensure the participants were attempting to read questions carefully, we removed from the dataset responses from any respondents who failed an attention-check question embedded within the surveys (twenty-one respondents)[93] as well as any incomplete responses (thirty-nine respondents), resulting in our final analysis sample of 2,440 individuals. Table 1 reports a breakdown of demographic and economic characteristics for our analysis sample and for the treatment and control subsamples.

[92] Partway through data collection, we asked Dynata to oversample female, working age, minority respondents to ensure balance in the demographic characteristics.

[93] This question asked: "Which of the following have you done in the last two years? (check all that apply) (1) Filed a homeowners insurance claim for damage caused by a lightning strike to your home; (2) Switched homeowners insurance carriers more than five times in a single year; (3) Switched homeowners insurance carriers at least once; (4) Filed complaints against a homeowners insurer with multiple regulators from different states; (5) Asked your homeowners insurer to defend you in a lawsuit alleging liability for an oil spill; (6) Purchased a homeowners insurance policy with no coverage limit; (7) None of the above." We excluded from our sample any respondents who provided an answer other than (3) or (7), as all of the remaining options were either impossible or unlikely. See, e.g., Press Release, Triple-I: Lightning Caused $1.04B in US Homeowners Claim Payouts in 2024; Frequency Drops 21.5% Year-Over-Year, Ins. Info. Inst. (June 19, 2025), https://www.iii.org/press-release/triple-i-lightning-caused-104b-in-us-homeowners-claim-payouts-in-2024-frequency-drops-215-year-over-year-061925 [https://perma.cc/ZMN5-CFZB] (demonstrating a significant decline in insurance claims for lightning strikes).

Table 1. Sample Descriptive Statistics

		No Policy Language (Control Group) *N = 974*		Policy Language (Treatment Group) *N = 1,466*		Total Sample *N = 2,440*	
		N	*%*	*N*	*%*	*N*	*%*
Gender	Male	451	46.3	743	50.7	1194	48.9
	Female	522	53.6	717	48.9	1239	50.8
	Other	1	0.1	6	0.4	7	0.3
Age	21–34	122	12.5	209	14.3	331	13.6
	35–44	184	18.9	304	20.7	488	20.0
	45–64	406	41.7	567	38.7	973	39.9
	65 or greater	262	26.9	386	26.3	648	26.6
Race/ Ethnicity	White	666	68.4	978	66.7	1644	67.4
	Black	118	12.1	206	14.1	324	13.3
	Asian	110	11.3	145	9.9	255	10.5
	Hispanic	80	8.2	141	9.6	221	9.1
	Other	22	2.3	37	2.5	59	2.4
	White (without multiracial)	647	66.4	946	64.5	1593	65.3
	Nonwhite (with multiracial)	327	33.6	520	35.5	847	34.7
Household Income	$49,999 or less	296	30.4	366	25.0	662	27.1
	$50,000–$149,999	537	55.1	897	61.2	1434	58.8
	$150,000 or greater	141	14.5	203	13.8	344	14.1
Employment Situation	Self-employed	69	7.1	122	8.3	191	7.8
	Working full-time	391	40.1	653	44.5	1044	42.8
	Working part-time	83	8.5	102	7.0	185	7.6
	Temporarily unemployed	53	5.4	61	4.2	114	4.7
	Not working, not seeking work	80	8.2	113	7.7	193	7.9
	Retired	298	30.6	415	28.3	713	29.2
Education	Less than Bachelor's Degree	510	52.4	772	52.7	1282	52.5
	Bachelor's Degree	307	31.5	423	28.9	730	29.9
	Advanced Degree	157	16.1	271	18.5	428	17.5
Marital Status	Married	583	59.9	895	61.1	1478	60.6
	Divorced/Separated	113	11.6	159	10.8	272	11.1
	Widowed	57	5.9	86	5.9	143	5.9
	Never Married	187	19.2	269	18.3	456	18.7
	Other	34	3.5	57	3.9	91	3.7
Region	West	297	30.5	390	26.6	687	28.2
	Midwest	207	21.3	357	24.4	564	23.1
	South	305	31.3	462	31.5	767	31.4
	Northeast	165	16.9	256	17.5	421	17.3

There was a good distribution of respondents in both the treatment and control groups across most demographic and economic

characteristics. Both groups were nearly equally distributed between males and females. The majority of those in both groups were younger than sixty-five years old, as were the majority (67%) of U.S. homeowners in 2020.[94] About two-thirds (65% and 66%, respectively) of the treatment and control groups were non-Hispanic whites;[95] 73% of U.S. homeowners in 2020 were non-Hispanic whites.[96] The majority in both groups were married, but nearly 20% were never married. All of the U.S. regions were represented in the sample, with the smallest proportion (17.3%) from the Northeast.

The sample was well educated. Nearly half (48%) had completed at least a bachelor's degree; 18% had at least one advanced degree. By comparison, 40% of U.S. homeowners in 2020 had a bachelor's degree, and 70% had at least some college education.[97] Nearly two-thirds (63%) of the sample were currently in the labor market (including self-employed individuals, those employed full-time or part-time, and those who were temporarily unemployed and seeking work). More than half of the sample (59%) had annual incomes between $50,000 and $150,000, but 28% had incomes at the lower end of the distribution (below $50,000). The remainder (roughly 14%) reported incomes above $150,000. The median household income of U.S. homeowners in 2020 was $81,400.[98]

We asked respondents about a variety of experiential factors relating to homeowners insurance. A small but nontrivial portion of the sample, 7% (6% in the control group and 8% in the treatment group), currently or previously sold homeowners insurance professionally. More than three-quarters (77% in the control group and 78% in the treatment

[94] U.S. Census Bureau, Homeownership by Age of Householder, Tableau Public (Sep. 28, 2023), https://public.tableau.com/app/profile/us.census.bureau/viz/HomeownershipbyRaceandEthnicityofHouseholder/state_age_db [https://perma.cc/W887-LHQP].

[95] Respondents were allowed to select any combination of race/ethnicity in the survey. Table 1 reports these non-mutually exclusive response frequencies for individual races and ethnicities. For our analysis, however, we identified non-Hispanic white people as those who only identified themselves as white; any respondent who reported a multiracial identity was considered non-white.

[96] Homeownership by Race and Ethnicity of Householder, U.S. Census Bureau (Sep. 28, 2023), https://www.census.gov/library/visualizations/interactive/homeownership-by-race-and-ethnicity-of-householder.html [https://perma.cc/5AW6-D4A2].

[97] Alexandra Ciuntu, Homeownership by Education: Degree-Holding Owners Surge as Those Without High School Drop 30%, Point2Homes (Apr. 6, 2022), https://www.point2homes.com/news/us-real-estate-news/homeownership-by-education-us.html [https://perma.cc/689R-AWD3] (summarizing U.S. Census data on U.S. homeownership by education).

[98] Id.

group) were identified as the final decision-maker in their household's decision to purchase or renew a homeowners insurance policy.[99] The overwhelming majority (85% of the total sample, 84% of the control group, and 87% of the treatment group) claimed to have either briefly looked over or attempted to closely read the most relevant terms of any of their own homeowners insurance policies over the past few years. Notably, however, a much smaller but surprisingly large percentage (39%) claimed to have attempted to closely read the most relevant terms of their homeowners insurance policy. About one-fifth (21% for both control and treatment groups) said they had switched homeowners insurance carriers at least once. Finally, the majority (86% for both control and treatment groups) reported having positive or optimistic views about their own homeowners insurance company. The remaining 14% believed their own homeowners insurance company's approach to paying a claim is to work hard to find a reason to reject the claim.

IV. RESULTS

This Part reports the results of our experiments. Our key finding, discussed in Section IV.A, is that while providing relevant policy language improved the accuracy of coverage assessments in some scenarios, it decreased accuracy or had no effect in others. We interpret these results to suggest that many respondents struggled with partial reading or partial understanding of complex provisions, often misinterpreting critical details that could reverse an initial impression. Sections IV.B and IV.C report that, consistent with our expectations, providing policy language does indeed increase respondents' confidence in their coverage assessments. Further, in both the control and treatment groups, respondents who were more confident in their coverage assessments were also more likely to be accurate, but we found no evidence that receiving policy language affected the likelihood of confident respondents' accuracy. Finally, Section IV.D shows that insurance sophistication, income, and race did not affect the accuracy of respondents' understanding of homeowners insurance policy language.

[99] The remaining 22% of the sample were also involved in the decision to purchase or renew their homeowners insurance policy, reporting that they helped with the process but were not the final decision-maker.

A. The Impact on Accuracy of Providing Policy Language

In total, our results were only partially consistent with our hypothesis that providing relevant insurance policy language that unambiguously resolved coverage questions would increase the accuracy of respondents' coverage assessments. Figure 5 reports the rate of accurate responses by respondents in the treatment and control groups for each of the seven vignettes.[100]

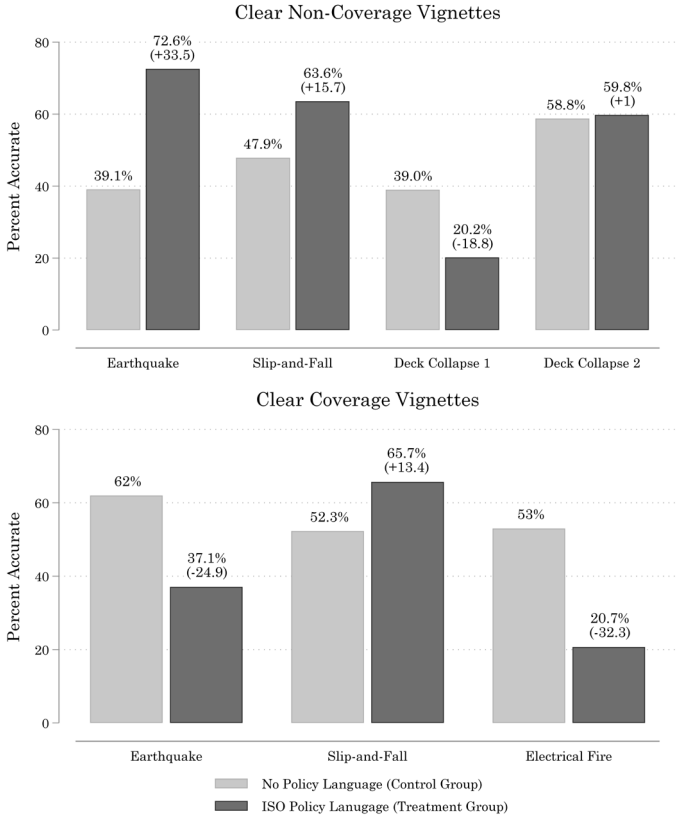
As is evident in Figure 5, the results in three of the seven vignettes we tested were consistent with our initial hypothesis, as a larger proportion of respondents who received policy language (treatment group) accurately assessed coverage relative to the respondents who did not receive such language (control group). These were the earthquake non-coverage vignette and the two slip-and-fall vignettes. The magnitude of this effect varied substantially across the three vignettes, however. The increase in the accuracy of respondents' coverage assessments ranged from roughly 13 percentage points (in the slip-and-fall clear coverage vignette) to roughly 34 percentage points in the earthquake clear non-coverage vignette. Notably, in each of these three vignettes, the absolute percentage of respondents who accurately assessed coverage when provided with the applicable policy language hovered between approximately 64% and 73%, meaning that even in the scenarios where respondents best grasped the implications of the policy language, more than a quarter still misinterpreted it.

Figure 5 also shows that the results in four of the seven vignettes we tested were inconsistent with our hypothesis, meaning that providing respondents with the key policy language pertaining to a vignette failed to improve the accuracy of their coverage assessment. Most starkly, in three of the vignettes (the first deck collapse clear non-coverage,

[100] For the purposes of this analysis, we grouped together in the "accurate" category those respondents whose answers in the clear coverage vignettes were either "definitely covered" or "probably covered," and we did the same with answers that were either "definitely not covered" or "probably not covered" in the clear non-coverage vignettes. Strictly speaking, there is no "accurate" or "inaccurate" answer to the coverage vignettes in the absence of governing policy language. We nonetheless labeled the answers supplied by respondents in the control group as "accurate" or "inaccurate" because we asked the respondents in the control group to answer the questions we provided to them based on what a "typical homeowners policy would cover." The ISO HO3 policy is the definition of a typical homeowners insurance policy, even though past research has demonstrated that specific insurers' homeowners policies vary in the language they use. See Schwarcz, supra note 27, at 1273–74.

earthquake clear coverage, and electrical fire clear coverage), the treatment group's assessments of coverage were *less* accurate than the control group's assessments. For these vignettes, providing the relevant and unambiguous policy language affirmatively decreased the accuracy of respondents' coverage assessments. Nor were these effects small or insubstantial: in the earthquake clear coverage vignette, supplying policy language decreased the accuracy of respondents' coverage assessments by 25 percentage points, and in the electrical fire clear coverage vignette, doing so decreased accuracy by 32 percentage points. In a fourth vignette—the second clear non-coverage deck collapse—providing the relevant policy language had no meaningful impact on the accuracy of respondents' coverage assessments.

Figure 5. Accuracy of Respondents' Assessments of Insurance Coverage



Collectively, these results confirm that respondents did at least partially read the policy language given to them in at least six of the seven vignettes: otherwise, we would expect that providing policy language would have had little to no effect on respondents' coverage assessments, a result we found in only one of our vignettes. Moreover, given the similar structure of the vignettes and the policy language supplied, we are confident that most respondents in fact read the provided policy language in the one vignette where doing so produced no statistically significant impact on their coverage assessments.

To test for the significance of these differences and account for observed differences between the groups, we used logistic regression analysis, controlling for respondents' experiential, demographic, and economic characteristics.[101] The dependent variable for this analysis was a dichotomous variable denoting the accuracy of the coverage assessments, equal to one if the respondents' perception of coverage was accurate and zero otherwise.[102] The primary independent variable of interest was a dichotomous variable indicating whether respondents received policy language.[103] The coefficients produced by that analysis

[101] The experiential variables were dichotomous to indicate whether respondents reported any of the following: selling insurance professionally, being the main decision-maker in their household's homeowners insurance purchasing decision, reading their own homeowners insurance policy, understanding what would and would not be covered under their own homeowners insurance policy, switching insurance carriers at least once, and having negative views of insurers' willingness to cover a hypothetical claim. (We intended to ask whether respondents had prior experience filing a homeowners insurance claim, but the question was only presented to the control group, so we omitted it from our analysis.) Three demographic control variables also were dichotomous: gender, race (white vs. non-white), and marital status. Age was a categorical variable, with ages thirty-four to sixty-four as the omitted category. Region also was a categorical variable, with West being the omitted category. Among the economic factors, income was a categorical variable, with $50,000 to $150,000 the omitted category—as was education, with bachelor's degrees the omitted category. Employment was a dichotomous variable, with one indicating not currently in the labor force.

[102] Again, for our main analyses, we grouped together in the "accurate" category those respondents whose answers were either "definitely covered" or "probably covered" for clear coverage vignettes, and either "definitely not covered" or "probably not covered" for clear non-coverage vignettes. In Appendix B, we demonstrate that our results are, for the most part, not impacted by defining the only correct answers in each pair of vignettes to be "definitely" covered or not covered, rather than also treating answers with "probably" as correct in each setting.

[103] We ran separate regressions for each coverage context (e.g., we analyzed responses from both earthquake damage vignettes in the same regression) and included a dichotomous variable denoting the non-coverage vignette (the second non-coverage vignette in the deck-collapse regressions). The aim was to capture the differences in respondents' accuracy

are reported in the Appendix.[104] We then used postestimation analysis of these regression outputs to calculate the predicted probability that respondents in the treatment and control groups had accurate perceptions of insurance coverage even after accounting for observable differences across these groups, reported in Table 2.[105]

between clear coverage and clear non-coverage vignettes in the same coverage context (and the difference between the first and second clear non-coverage vignettes in the deck-collapse context). We then interacted the variable denoting receipt of policy language with the variable denoting respondents who were given the non-coverage vignette to capture whether the effect of receiving policy language differed between clear coverage and clear non-coverage vignettes. We used this identification strategy to test whether respondents systematically lean toward coverage or non-coverage in their perceptions of coverage, regardless of the specific vignettes and policy language they were given. However, we found no clear evidence that such a phenomenon occurs in our data. We also controlled for the respondents' demographic, economic, and experiential characteristics in these regressions; see supra note 101 for details.

[104] See infra Appendix A, Table A3.

[105] The predicted probabilities are predictive margins across two dimensions: vignette type (i.e., clear coverage versus clear non-coverage) and study subsample (i.e., control group versus treatment group). In other words, holding all other variables constant at their mean values, we estimated the likelihood of accuracy of insurance coverage perceptions for each respondent and reported the average likelihood for four groups: recipients of clear non-coverage vignettes in the control and treatment groups, respectively, and recipients of clear coverage vignettes in the control and treatment groups, respectively. For each vignette, we then calculated the average marginal effect of receiving policy language and reported it, along with the corresponding standard errors and markers of statistical significance, in Table 2. We used Stata's "margins" postestimation command to calculate predictive average marginal effects for each coverage vignette. See Richard Williams, Using the Margins Command to Estimate and Interpret Adjusted Predictions and Marginal Effects, 12 Stata J. 308, 319–20, 322 (2012). This is best practice when interpreting results involving interaction terms in nonlinear models. See Chunrong Ai & Edward C. Norton, Interaction Terms in Logit and Probit Models, 80 Econ. Letters 123, 123–24 (2003).

Table 2. Predicted Probability of Accurate Coverage Assessments

Vignette	Predicted Probability		Average Marginal Effect
	No Policy Language	Policy Language	
Earthquake Damage			
Clear Non-Coverage	0.392	0.724	0.302** (0.025)
Clear Coverage	0.622	0.370	-0.241** (0.031)
Slip-and-Fall Liability			
Clear Non-Coverage	0.483	0.637	0.151** (0.033)
Clear Coverage	0.517	0.657	0.137** (0.033)
Deck Collapse			
Clear Non-Coverage 1	0.391	0.201	-0.180** (0.029)
Clear Non-Coverage 2	0.589	0.597	0.008 (0.035)
Electrical Fire Damage			
Clear Coverage	0.531	0.206	-0.325** (0.023)

Note: The table reports average predicted probabilities that respondents accurately assessed insurance coverage, conditioned on treatment, as well as the average marginal effect of treatment. All numbers were calculated with the logit regression coefficients in Columns (3), (6), (9), and (10) in Table A3 in Appendix A, using Stata's postestimation "margins" command. Standard errors are reported in parentheses. The symbols * and ** represent significance at the 5% and 1% levels, respectively.

Not surprisingly, given the number of respondents we surveyed and the randomized character of their assignments to the control and treatment groups, this analysis produces the same basic results as those reported in Figure 5: providing policy language improved the predicted accuracy of respondents' answers for only three vignettes, decreased respondents' predicted accuracy in three vignettes, and had no significant impact on accuracy in one of the vignettes. This analysis does confirm, however, that in all six of the vignettes where providing policy language impacted coverage assessments, this result was statistically significant at the 1% level.

For the three scenarios in which providing respondents with the actual policy language caused them to arrive at less accurate conclusions, the most plausible explanation involves a "partial-reading" or "partial-understanding" problem. In particular, we hypothesize that respondents read the supplied language carefully until they discovered an initial indication about the coverage question, and then either stopped reading or read less carefully any subsequent text.[106] Take the earthquake clear coverage vignette, for example: many respondents likely focused on the initial portion of the policy provision—specifically, the exclusions section's introduction and the definition of "[e]arth [m]ovement," which explicitly states that direct losses caused by earth movement are not covered. Based on this, they may have concluded their loss was excluded and either stopped reading or continued with less attention, missing the crucial "direct loss" exception in the final line of the provision.

A similar pattern explains our results in the electrical fire clear coverage and the first deck collapse vignettes. Focusing on the former, respondents may have read carefully until encountering the term "neglect." At that point, they likely applied their own understanding—associating neglect with failing to take reasonable precautions—and concluded there was no coverage. However, respondents may have missed the critical clarification at the end of the policy language: that neglect only precludes coverage if it occurs at or after the time of the loss.

A partial-reading / -understanding explanation also helps explain why supplying the relevant policy language did not negatively affect

[106] Given that the treatment groups in both earthquake vignettes were given exactly the same policy language, this variation in result cannot be explained by differences in the readability or length of the policy language.

understanding in the other vignettes. For example, respondents in the slip-and-fall scenarios may have provided more accurate answers when presented with policy language because the policy provisions we supplied did not include exceptions or caveats. The policy language provided to the treatment group in these vignettes made it clear early on that losses arising from a "'business' conducted from an insured location or engaged in by an insured" were not covered. Crucially, nothing in the language that followed reversed or contradicted that initial conclusion. Even the definition of the term "business," which appeared later in the provision, contained no unexpected or counterintuitive elements from the perspective of the average reader. As a result, if a respondent carefully read only the first third of the policy language before letting their attention wane, their coverage guess would still likely remain accurate. This was true for both the clear coverage and clear non-coverage versions of the slip-and-fall vignette. A similar dynamic explains our results for the earthquake clear non-coverage vignette: respondents who received the policy language likely read carefully up to the initial exclusionary language. If they stopped reading at that point or merely skimmed the remainder of the policy language, their guesses about coverage would not have been significantly impacted.

It is unclear whether our results are generalizable to other forms of consumer contracts. Insurance policies are commonly written to include broad grants of coverage, followed by subsequent specific exclusions, then followed by later exceptions to those exclusions.[107] This sort of contractual structure, which is common in insurance policies, creates conditions that can create or worsen the partial-reading problem. If other consumer contracts do not use this sort of back-and-forth structure, the results from this section may not be generalizable to that setting.

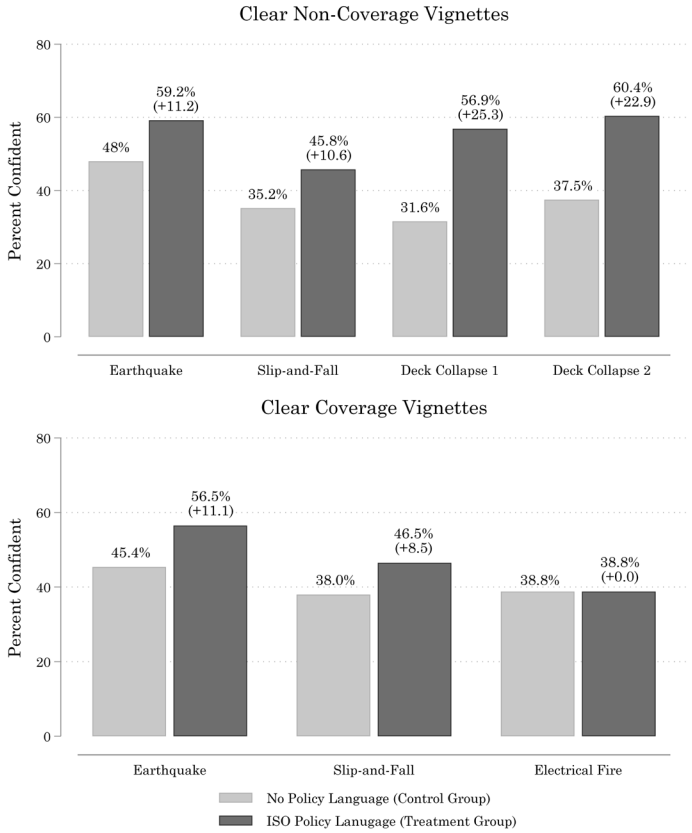
B. The Impact on Confidence in Coverage Assessments of Providing Policy Language

Although our results were partly inconsistent with our primary hypothesis—that providing respondents with relevant insurance policy language would improve the accuracy of their coverage assessments—they were quite consistent with our second hypothesis that respondents who received relevant policy language would have greater confidence in

[107] See Abraham & Schwarcz, supra note 22, at 46, 529.

the accuracy of their coverage assessments. Figure 6 reports the relevant responses for each of the seven vignettes.[108]

Figure 6. Perceptions of Confidence in Accuracy of Coverage Assessments



As shown in Figure 6, providing respondents with relevant policy language increased their confidence in coverage assessments in six of the seven vignettes we tested. This effect ranged from roughly 25 percentage points on the high end, to 9 percentage points on the low end. The one exception involved the electrical fire vignette, where there

[108] As above, we included in the "Confident" category respondents who reported being "very confident" or "extremely confident" that their perceptions of coverage accuracy were correct, though our results are similar if we focus solely on respondents who reported being "extremely confident" in their coverage assessments.

was no difference in confidence between the treatment and control groups. Further analysis, which is reported in the Appendix, confirmed that these effects were statistically significant after accounting for observed differences between the groups.[109] Following a similar process to that which is described above, Table 3 reports the statistical significance of variations in the predicted probabilities, after controlling for observed differences, that respondents in the treatment and control groups reported confidence in their coverage assessments.

[109] Our identification strategy for this analysis was identical to the accuracy analysis, with the exception that the dependent variable in this case was a dichotomous variable for reported confidence, equal to one if respondents reported feeling "extremely confident" or "very confident" in the accuracy of their perceptions of insurance coverage in the respective coverage vignette, and equal to zero otherwise. (In Appendix B, we demonstrate that our results are not impacted by defining confident respondents in each pair of vignettes to be only those who declared themselves as "extremely confident" rather than also treating answers with "very confident" as confident in each setting.) See infra Appendix A, Table A4 for key regression coefficients.

Table 3. Predicted Probability of Confidence in Policy Coverage Assessments

Vignette	Predicted Probability		Average Marginal Effect
	No Policy Language	Policy Language	
Earthquake Damage			
Clear Non-Coverage	0.496	0.595	0.098**
			(0.033)
Clear Coverage	0.463	0.547	0.084*
			(0.033)
Slip-and-Fall Liability			
Clear Non-Coverage	0.357	0.457	0.100**
			(0.033)
Clear Coverage	0.390	0.456	0.065*
			(0.033)
Deck Collapse			
Clear Non-Coverage 1	0.323	0.572	0.241**
			(0.031)
Clear Non-Coverage 2	0.375	0.599	0.216**
			(0.031)
Electrical Fire Damage			
Clear Coverage	0.395	0.408	0.013
			(0.020)

Note: The table reports average predicted probabilities that respondents report confidence in their assessments of insurance coverage, conditioned on treatment status, as well as the average marginal effect of treatment. All numbers were calculated with the logit regression coefficients in Columns (3), (6), (9), and (10) in Table A4 in Appendix A, using Stata's postestimation "margins" command.

Standard errors are reported in parentheses. The symbols * and ** represent significance at the 5% and 1% levels, respectively.

One notable trend in these results is that respondents' increased confidence associated with receiving policy language was generally consistent across clear coverage and clear non-coverage vignettes. This result is most obvious in Table 3, which groups results by the policy language received rather than by whether the vignette involved clear coverage or non-coverage. In each grouping of vignettes in which respondents received the same policy language, the increase in confidence produced by supplying the policy language was similar in magnitude. This was true even though, as previously discussed with respect to respondents' accuracy, the effect of providing policy language on coverage assessment accuracy often differed significantly depending on which vignette respondents received. Most starkly, providing the earthquake coverage policy language produced radically different results for the clear non-coverage vignette (increasing accuracy by roughly 30 percentage points) and the clear coverage vignette (decreasing accuracy by roughly 24 percentage points). This suggests that providing contract language that tends to decrease consumers' understanding may also increase consumers' confidence in their mistaken interpretation, which is an unsettling conclusion.

C. The Relationship Between Confidence and Accuracy

Recall that our third hypothesis was that respondents who reported high levels of confidence in their coverage assessments would in fact be more accurate in those assessments. We hypothesized that this effect would apply to respondents in both the control and treatment groups.[110] Our results largely supported these hypotheses.

We estimated a series of logistic regressions, controlling for experiential, demographic, and economic factors, to identify whether respondents' confidence predicted a greater likelihood of accurate assessments of insurance coverage, and whether high-confidence respondents who received relevant policy language would be even more likely to accurately understand insurance coverage than their high-

[110] Previous research has demonstrated that lay individuals believe their interpretation of contract language is consistent with the interpretations of others due to false consensus bias. See Solan et al., supra note 35, at 1291.

confidence counterparts who received no policy language.[111] (Key coefficients are reported in the Appendix.)[112] As in our earlier analyses, we used the regression outputs to calculate the predicted probability that highly confident respondents in both the treatment and control groups accurately predicted insurance coverage in each of the vignettes. Table 4 reports those predicted probabilities, as well as the average marginal effects associated with the treatment (receiving policy language).

Table 4. Predicted Probability of Accurate Coverage Assessments by Confidence Level

Vignette	No Policy Language			Policy Language			Average Marginal Effects
	Predicted Probability		Average Marginal Effects	Predicted Probability		Average Marginal Effects	
	Not Confident	Confident		Not Confident	Confident		
Earthquake Damage							
Clear Non-Coverage	0.293	0.493	0.197** (0.050)	0.573	0.828	0.243** (0.036)	0.025 (0.053)
Clear Coverage	0.555	0.696	0.139** (0.052)	0.275	0.450	0.173** (0.042)	0.020 (0.059)
Slip-and-Fall Liability							
Clear Non-Coverage	0.459	0.522	0.062 (0.058)	0.560	0.729	0.169** (0.041)	0.098 (0.066)
Clear Coverage	0.383	0.730	0.326** (0.043)	0.514	0.831	0.315** (0.037)	-0.032 (0.062)
Deck Collapse							
Clear Non-Coverage 1	0.400	0.373	-0.027 (0.057)	0.231	0.179	-0.052 (0.038)	-0.021 (0.061)
Clear Non-Coverage 2	0.526	0.694	0.165** (0.051)	0.479	0.676	0.192** (0.042)	0.029 (0.067)
Electrical Fire Damage							
Clear Coverage	0.442	0.670	0.222** (0.030)	0.142	0.311	0.158** (0.035)	-0.011 (0.046)

Note: The table reports average predicted probabilities that respondents accurately assessed insurance coverage, conditioned on

[111] Here, our identification strategy used the same dichotomous variable as in Section II.A, where accuracy of respondents' coverage perceptions was the dependent variable. Three dichotomous variables denoting treatment (i.e., receiving policy language), confidence, and vignette type (clear coverage versus clear non-coverage vignettes for earthquake and slip-and-fall, and the first clear non-coverage versus the second clear non-coverage vignette for deck collapse), as well as those variables' interactions, were the independent variables. We also controlled for experiential, demographic, and economic factors as described in Section II.A.

[112] See infra Appendix A, Table A5.

confidence level and treatment, as well as the average marginal effect of high confidence in both treatment and control groups, and the average marginal effect of treatment on high-confidence respondents. All numbers were calculated with the logit regression coefficients in Table A5 in Appendix A, using Stata's postestimation "margins" command. Standard errors are reported in parentheses. The symbols * and ** represent significance at the 5% and 1% levels, respectively.

Several conclusions emerge from Table 4. First, confidence predicted accuracy in five of seven vignettes in the control group. The smallest significant effect was a roughly 14-percentage-point greater likelihood of accuracy in the earthquake clear coverage vignette. The largest significant effect was a roughly 33-percentage-point greater likelihood of accuracy in the slip-and-fall clear coverage vignette. The two vignettes in which there was no detectable difference in accuracy between highly confident respondents and all other control group respondents were the slip-and-fall clear non-coverage vignette and the first deck collapse vignette.

Similarly, confidence predicted accuracy in six of the seven vignettes in the treatment group, with the first deck collapse scenario being the exception. The smallest statistically significant effect was a difference of roughly 16 percentage points in the likelihood of accuracy in the electrical fire clear coverage vignette. The largest effect was a difference of roughly 32 percentage points in the likelihood of accuracy in the slip-and-fall clear coverage vignette.

However, there was no detectable difference between the average predicted probability of accuracy of highly confident respondents in the control group and that of highly confident respondents in the treatment group. That is, the average marginal effect in the rightmost column of Table 4, which denotes the marginal impact of introducing policy language on the accuracy probability of highly confident respondents, was not statistically significant for any of the vignettes. In other words, we found no statistically significant evidence that the link between respondents' confidence and accuracy was strengthened (or was weakened for that matter) when they were given relevant policy language.

D. Results Among Sub-Populations of Respondents

Recall that we also hypothesized—consistent with prior findings in the financial literacy literature—that, for both the control and treatment groups, the accuracy of respondents' coverage assessments would vary by their level of insurance sophistication, income, and race. Contrary to our expectations, with the exception of one vignette, none of these factors significantly predicted improvements in coverage accuracy for the respondents in the treatment group, who were provided with policy language. Furthermore, in the control group—where respondents did not receive any policy language—we found that income and sophistication weakly correlated with the accuracy of coverage assessments in certain vignettes, while race had no correlation.

1. Influence of Insurance Sophistication on Accuracy of Coverage Assessments

We predicted that sophisticated insurance purchasers would better understand complex insurance policy language than the average consumer. This prediction aligned with a key rationale for limiting consumer protection laws to individual consumers while excluding sophisticated commercial parties—the belief that consumers require special legal safeguards, whereas sophisticated parties do not, in part because they are more adept at interpreting complex commercial contracts.[113] This proposition, however, has not previously been tested in insurance law.[114]

[113] Some courts apply different doctrinal tools in disputes involving sophisticated and unsophisticated policyholders. See Jeffrey W. Stempel, Reassessing the "Sophisticated" Policyholder Defense in Insurance Coverage Litigation, 42 Drake L. Rev. 807, 834–43 (1993); Hazel Glenn Beh, Reassessing the Sophisticated Insured Exception, 39 Tort Trial & Ins. Prac. L.J. 85, 86 (2003); Jeffrey E. Thomas, Insurance Law Between Business Law and Consumer Law, 58 Am. J. Compar. L. 353, 363 (2010); see also George A. Akerlof & Robert J. Shiller, Phishing for Phools: The Economics of Manipulation & Deception 166–69 (2015) (arguing that market incentives systematically encourage businesses to exploit consumers' cognitive limitations, particularly through complex financial products); Wendy E. Wagner, Rethinking Legal Requirements: A Case Study of Incomprehensible Consumer Contracts in the United States, *in* Research Handbook on Contract Design 114, 116 (Marcelo Corrales Compagnucci, Helena Haapio & Mark Fenwick eds., 2022) (observing that consumer contracts in the United States are routinely drafted in language that is effectively incomprehensible to the consumers they bind).

[114] Although the Federal Trade Commission has long used a "reasonable man" standard to decide which members of the general public to protect, see Ivan L. Preston, Reasonable Consumer or Ignorant Consumer? How the FTC Decides, 8 J. Consumer Affs. 131, 140–42

Contrary to our expectations, we found no statistically significant difference between the performance of sophisticated respondents and that of all other respondents, among both the groups who received the policy language and those who did not, with the exception of the slip-and-fall clear coverage vignette. Further, we found no statistical difference in accuracy between sophisticated respondents in the control group and their treatment group counterparts across all coverage vignettes. We initially defined sophisticated respondents as those who both (a) had a bachelor's degree or higher and (b) met all of the following conditions: (1) reported having read the most relevant terms of their own homeowners policy, (2) reported having understood those terms "very well" or "completely," and (3) reported having "mostly" or "completely" understood what their homeowners policy does and does not cover. We also tested several alternative definitions of consumer sophistication, which produced similar results and are reported in the Appendix.[115] In total, 170 respondents (roughly 7% of the sample) met our primary definition of consumer sophistication; seventy-three of those respondents were in the control group and ninety-seven in the treatment group.

After identifying the subpopulation of sophisticated respondents, we estimated a series of logistic regressions that aimed to capture whether these sophisticated respondents behaved differently from their

(1974), and the "reasonable investor" standard is used to determine materiality in securities law, see Alexandra Qingning Li, The Unreasonableness of Reasonable: Rethinking the Reasonable Investor Standard, 117 Nw. U. L. Rev. 1707, 1713 (2023), this approach is less well represented in insurance-related studies. One study assumed insurance literacy was a measure of sophistication and measured it using a ten-question quiz comprised of questions from existing insurance exams. The study found that insurance literacy was overall low for all consumers. The characteristics synonymous with commercial sophistication—i.e., experience with insurance, expressing an interest in saving and investing, and subscriptions to financial publication(s)—were correlated with slightly higher insurance literacy scores, and the differences were statistically significant. Sharon Tennyson, Consumers' Insurance Literacy: Evidence from Survey Data, 20 Fin. Servs. Rev. 165, 169–73 (2011). Other research on consumers' sophistication and homeowners insurance has generally focused on the economic inefficiency of consumers' ill-calibrated, risk-averse preferences, but not specifically on whether consumers understand the policies themselves or whether their preferences were inefficient. See Justin Sydnor, (Over)insuring Modest Risks, 2 Am. Econ. J., no. 4, Oct. 2010, at 177, 182–85 (finding that high-deductible, low-rate homeowners insurance policyholders are effectively subsidized by a much larger share of risk-averse consumers who opt for low-deductible, high-rate policies); Levon Barseghyan, Francesca Molinari, Ted O'Donoghue & Joshua C. Teitelbaum, The Nature of Risk Preferences: Evidence from Insurance Choices, 103 Am. Econ. Rev. 2499, 2526 (2013).

[115] See infra Appendix B, Table A10 and surrounding text.

unsophisticated counterparts, controlling for experiential, demographic, and economic factors as in previous analyses.[116] As before, we used these regression outputs[117] to calculate the predicted probability that sophisticated and unsophisticated respondents in the treatment and control groups accurately predicted insurance coverage in each of the coverage vignettes. Table 5 reports those predicted probabilities, as well as the average marginal effects associated with both sophistication level and the treatment (receiving policy language) itself.

Table 5. Predicted Probability of Accurate Coverage Assessments by Sophistication Level

Vignette	No Policy Language			Policy Language			Average Marginal Effects
	Predicted Probability		Average Marginal Effects	Predicted Probability		Average Marginal Effects	
	Not Sophisticated	Sophisticated		Not Sophisticated	Sophisticated		
Earthquake Damage							
Clear Non-Coverage	0.392	0.370	-0.023 (0.105)	0.719	0.786	0.072 (0.089)	0.057 (0.078)
Clear Coverage	0.613	0.709	0.101 (0.105)	0.366	0.441	0.073 (0.078)	-0.024 (0.112)
Slip-and-Fall Liability							
Clear Non-Coverage	0.480	0.489	0.009 (0.120)	0.647	0.499	-0.141 (0.083)	-0.154 (0.149)
Clear Coverage	0.495	0.759	0.285* (0.111)	0.655	0.684	0.030 (0.089)	-0.231 (0.124)
Deck Collapse							
Clear Non-Coverage 1	0.395	0.330	-0.067 (0.098)	0.207	0.124	-0.098 (0.088)	-0.014 (0.108)
Clear Non-Coverage 2	0.597	0.504	-0.091 (0.092)	0.599	0.577	-0.022 (0.084)	0.070 (0.126)
Electrical Fire Damage							
Clear Coverage	0.522	0.643	0.123 (0.064)	0.198	0.304	0.093 (0.061)	0.008 (0.081)

Note: The table reports average predicted probabilities that respondents accurately assessed insurance coverage, conditioned on sophistication level and treatment, as well as the average marginal effect of sophistication in both treatment and control groups, and the

[116] The identification strategy for this analysis is quite similar to that of the analysis in Section IV.C, except that we replaced the indicator for high confidence with an indicator for sophisticated respondents. We also removed any experiential controls used in our definition for sophisticated respondents. See infra Appendix A, Table A6 for key coefficients from these regressions.

[117] See infra Appendix A, Table A6.

average marginal effect of treatment on sophisticated respondents. All numbers were calculated with the logit regression coefficients in Table A6 in Appendix A, using Stata's postestimation "margins" command. Standard errors are reported in parentheses. The symbols * and ** represent significance at the 5% and 1% levels, respectively.

Sophistication was a significant predictor of accuracy in only one coverage vignette—the slip-and-fall clear coverage vignette in the control group. In this vignette, sophisticated consumers were more likely to accurately predict coverage than their unsophisticated counterparts, by a margin of 29 percentage points.

However, what stands out most in these results is that there were no statistically significant differences between the likelihood of accuracy of sophisticated respondents and that of unsophisticated respondents in any of the remaining vignettes—for either the control group or the treatment group. Further, none of the average marginal effects in the rightmost column of Table 5 are statistically significant, indicating that the accuracy of the sophisticated respondents' coverage assessments was not affected by the introduction of relevant policy language. Thus, we cannot say with any confidence that sophisticated consumers, as we defined them (either here, in our primary results, or using alternative definitions discussed in the Appendix[118]), are any better at making insurance coverage predictions than unsophisticated consumers.

2. Influence of Income on Accuracy of Coverage Assessments

We also predicted that higher-income respondents would, on average, be more likely to accurately assess insurance coverage than their lower-income counterparts. We defined higher-income respondents as those who reported annual gross household incomes of $150,000 or greater.[119] Once again, we also tested several alternative definitions of household income, which produced similar results and are reported in the Appendix.[120] Following a similar statistical approach to that described earlier, we calculated the predicted probability that income influenced

[118] See infra Appendix B, Table A10 and surrounding text.

[119] Using our primary definition of high-income respondents, we had roughly equal distributions of higher-income respondents in both the control and treatment groups (roughly 15% and 14%, respectively).

[120] See infra Appendix B, Table A11 and surrounding text.

the accuracy of coverage assessments in the treatment and control groups across the seven vignettes.[121]

Table 6, which reports the predicted probabilities emerging from this analysis, shows that the evidence did not support our hypothesis: higher-income respondents were generally no more accurate than their lower-income counterparts. In the control group, we found no evidence that the higher-income respondents were more likely to be accurate in six of the seven clear coverage vignettes.[122] In the treatment group, we found no evidence in any of the seven vignettes that higher-income respondents were more likely to be accurate than their lower-income counterparts when they received relevant policy language. Further, we found no statistical evidence that higher-income respondents who received policy language were more likely to assess coverage correctly than higher-income respondents who did not receive policy language (the average marginal effects of receiving policy language, located in the rightmost column of Table 6, were not statistically significant for any coverage vignette).

[121] The identification strategy for this analysis is quite similar to that of the analysis in Section IV.C, except that we replaced the indicator for high confidence with an indicator for high income. We also removed the income controls used in prior analyses. See infra Appendix A, Table A7 for key coefficients from logistic regressions used to calculate these predicted probabilities.

[122] In the earthquake clear non-coverage vignette, we found a statistically significant difference of roughly 16 percentage points between higher-income respondents and their lower-income counterparts. See infra Appendix A, Table A7.

Table 6. Predicted Probability of Accurate Coverage Assessments by Income

Vignette	No Policy Language			Policy Language			Average Marginal Effects
	Predicted Probability		Average Marginal Effects	Predicted Probability		Average Marginal Effects	
	Not High Income	High Income		Not High Income	High Income		
Earthquake Damage							
Clear Non-Coverage	0.368	0.533	0.158* (0.074)	0.714	0.786	0.076 (0.062)	-0.080 (0.077)
Clear Coverage	0.617	0.651	0.035 (0.080)	0.373	0.359	-0.014 (0.063)	-0.042 (0.083)
Slip-and-Fall Liability							
Clear Non-Coverage	0.471	0.556	0.085 (0.083)	0.622	0.739	0.124 (0.067)	0.028 (0.091)
Clear Coverage	0.498	0.625	0.128 (0.079)	0.665	0.614	-0.049 (0.060)	-0.173 (0.097)
Deck Collapse							
Clear Non-Coverage 1	0.410	0.229	-0.197** (0.095)	0.201	0.201	0.001 (0.059)	0.169 (0.093)
Clear Non-Coverage 2	0.581	0.631	0.050 (0.072)	0.582	0.681	0.102 (0.064)	0.049 (0.089)
Electrical Fire Damage							
Clear Coverage	0.531	0.518	-0.013 (0.047)	0.210	0.196	-0.014 (0.057)	-0.004 (0.069)

Note: The table reports average predicted probabilities that respondents accurately assessed insurance coverage, conditioned on household income and treatment, as well as the average marginal effect of high income in both treatment and control groups, and the average marginal effect of treatment on high-income respondents. All numbers were calculated with the logit regression coefficients in Table A7 in Appendix A, using Stata's postestimation "margins" command. Standard errors are reported in parentheses. The symbols * and ** represent significance at the 5% and 1% levels, respectively.

3. Influence of Race on Accuracy of Coverage Assessments

In addition to insurance sophistication and income, we tested whether the respondents' race predicted accuracy of coverage assessments in the treatment and control groups. Both groups had roughly equal distributions of non-white respondents (roughly 34% and 36%, respectively), including those who reported multiple racial or ethnic

identities. As above, we estimated a series of logistic regressions, controlling for potentially confounding experiential, other demographic, and economic characteristics[123] and used these regression outputs to calculate the predicted probability that race influenced the accuracy of coverage assessments in the treatment and control groups across the seven vignettes.[124] Table 7 reports these predicted probabilities.

Table 7. Predicted Probability of Accurate Coverage Assessments by Race

Vignette	No Policy Language			Policy Language			Average Marginal Effects
	Predicted Probability		Average Marginal Effects	Predicted Probability		Average Marginal Effects	
	White	Nonwhite		White	Nonwhite		
Earthquake Damage							
Clear Non-Coverage	0.418	0.337	-0.082 (0.058)	0.738	0.700	-0.037 (0.042)	0.037 (0.052)
Clear Coverage	0.633	0.599	-0.034 (0.058)	0.343	0.421	0.076 (0.046)	0.098 (0.067)
Slip-and-Fall Liability							
Clear Non-Coverage	0.473	0.504	0.031 (0.059)	0.650	0.614	-0.036 (0.046)	-0.064 (0.071)
Clear Coverage	0.551	0.449	-0.101 (0.058)	0.700	0.583	-0.112** (0.041)	-0.012 (0.070)
Deck Collapse							
Clear Non-Coverage 1	0.393	0.389	-0.004 (0.058)	0.215	0.173	-0.043 (0.040)	-0.033 (0.060)
Clear Non-Coverage 2	0.623	0.523	-0.098 (0.057)	0.611	0.571	-0.040 (0.048)	0.060 (0.075)
Electrical Fire Damage							
Clear Coverage	0.522	0.547	0.025 (0.035)	0.212	0.196	-0.016 (0.040)	-0.038 (0.048)

Note: The table reports average predicted probabilities that respondents accurately assessed insurance coverage, conditioned on race and treatment, as well as the average marginal effect of race in both the treatment and control groups, and the average marginal effect of treatment on non-white respondents. All numbers were calculated with the logit regression coefficients in Table A8 in Appendix A,

[123] See Appendix A, Table A8.

[124] The identification strategy for this analysis is quite similar to that of the analysis in Section IV.C, except that we replaced the indicator for high confidence with an indicator for non-white respondents. We also removed the race/ethnicity control used in prior analyses. Key coefficients from these regressions are reported in Table A8 in Appendix A.

using Stata's postestimation "margins" command. Standard errors are reported in parentheses. The symbols * and ** represent significance at the 5% and 1% levels, respectively.

The data reported in Table 7 suggest there is, at most, only a limited relationship between respondents' race and the accuracy of their coverage assessments. The data revealed no statistically significant relationship between accuracy and race in the control group; in none of the seven vignettes did respondents' race predict accuracy with any statistical significance. The data point to largely the same conclusion for the treatment group (which received policy language): in six of the seven vignettes, race did not predict accuracy of coverage assessments.[125] In addition, the results in the rightmost column of Table 7, which reports the average marginal effects of the treatment on accuracy, indicate no significant difference in the effect on coverage assessment accuracy of receiving policy language by race.

* * *

To summarize, we found some evidence that reading relevant policy language was associated with improved accuracy in homeowners insurance coverage assessments, although the impact was inconsistent across vignettes and smaller than expected. More disturbing was the evidence in some vignettes that reading relevant policy language was associated with less accurate coverage assessments. Also concerning was evidence that providing relevant policy language was positively associated with confidence in coverage assessments, even when that assessment was wrong. Our analyses indicate that sophistication as an insurance consumer, income, and race did not explain variations in the accuracy of coverage assessments.

V. IMPLICATIONS

Our results provide novel support for the critique that modern contract law fails to ensure consumers' capacity to understand their contractual rights and responsibilities. This critique matters because comprehensible contract terms are essential for consumer protection even if, as compelling evidence in fact shows, the vast majority of

[125] In the slip-and-fall clear coverage vignette, non-white respondents were 11 percentage points less likely to be accurate than their white counterparts. See supra Table 7.

consumers do not read their contracts when they ostensibly assent to them.[126] Section V.A explains this oft-overlooked point. It highlights how comprehensible contracts can help inform consumers of their rights after contract formation if and when disputes arise. It also argues that comprehensible contracts heighten reputational risks for firms that include unfair terms in their contracts. More foundationally, it argues that consumers' opportunity to review contract terms prior to assent critically depends on those terms being comprehensible, meaning that blanket assent to consumer contracts should not extend to contract language that large swaths of consumers cannot understand even when they attempt to do so.

Section V.B examines strategies to address incomprehensible consumer contract terms. It suggests that courts could consider survey-based evidence to determine if the contract terms a firm seeks to enforce are understandable among a significant percentage of consumers, rooting this inquiry in the "reasonable expectations" doctrine of consumer contract law.[127] Section V.B also discusses options to require or incentivize firms to provide tools to help consumers understand contract terms, such as AI-based smart readers.

A. The Benefits of Understandable Consumer Contract Language

Understandable contract language is a key consumer protection, even though most consumers choose not to read their contracts when they ostensibly assent to them.[128] Subsection V.A.1 highlights several practical consumer protection benefits of comprehensible contracts. Subsection V.A.2 addresses the theoretical implications of contract terms that are incomprehensible to most consumers, arguing that consumer assent to such terms is highly questionable under prevailing contract law rules and theories.

[126] See supra Part I.

[127] See Restatement of Consumer Conts. § 4 cmt. d (A.L.I. 2024); David A. Hoffman, Consumers' Unreasonable Textual Expectations, 15 Harv. Bus. L. Rev., no. 2, 2025, at 43, 49.

[128] See sources cited supra note 3.

1. The Practical Consumer Protection Benefits of Comprehensible Contracts

Few typical consumers read contracts when they ostensibly agree to them.[129] However, clear and comprehensible contracts nonetheless benefit consumers in at least three concrete ways.[130] First, comprehensible contracts help consumers identify and navigate legitimate disputes with businesses. During potential disputes, consumers have a compelling reason to understand their contractual rights and obligations.[131] Consumers are therefore more likely to read their contracts carefully when a potential dispute arises than at the time of contract formation.[132] This is particularly true when firms highlight specific contract language in response to consumer complaints or inquiries, as is sometimes legally required.[133]

Consumers who read the governing, clearly-defined contract terms during disputes are more likely to receive fair treatment and avoid unproductive efforts if they correctly understand that contract language.[134] Most importantly, these consumers are more likely to be able to accurately assess whether a firm has honored its contractual commitments.[135] If so, then they can avoid prolonged and unproductive disputes.[136] And if not, then they can threaten remedial action unless the

[129] Bakos et al., supra note 19, at 3; Ayres & Schwartz, supra note 19, at 546–47; Marotta-Wurgler, supra note 19, at 95–96; Shmuel I. Becher & Esther Unger-Aviram, The Law of Standard Form Contracts: Misguided Intuitions and Suggestions for Reconstruction, 8 DePaul Bus. & Com. L.J. 199, 206 (2010).

[130] See Logue et al., supra note 22.

[131] See Schwarcz, supra note 26; Boardman, supra note 13, at 1077; Van Boom et al., supra note 26.

[132] See Wulf & Seizov, supra note 68, at 559, 573 (finding that in post-contract scenarios in which the consumer has a dispute with a business, "consumers do in fact read, retain and understand more when the attempt has been made to optimize disclosures"); Shmuel I. Becher & Tal Z. Zarsky, E-Contract Doctrine 2.0: Standard Form Contracting in the Age of Online User Participation, 14 Mich. Telecomms. & Tech. L. Rev. 303, 315 (2008).

[133] See Schwarcz, supra note 25, at 1496 (noting that state laws typically require insurers to quote the relevant policy language when denying a claim); see also Meirav Furth-Matzkin, On the Unexpected Use of Unenforceable Contract Terms: Evidence from the Residential Rental Market, 9 J. Legal Analysis 1, 39, 44 (2017) (recommending regulators require landlords emphasize important rights by using a "salient format").

[134] Van Boom et al., supra note 26, at 194–95.

[135] Id.

[136] An objection is that consumers may be more likely to accept unfair practices that are specified in contract terms because individuals believe these terms have legal and moral force. See Wilkinson-Ryan, supra note 4, at 1760–61; Meirav Furth-Matzkin & Roseanna Sommers, Consumer Psychology and the Problem of Fine-Print Fraud, 72 Stan. L. Rev. 503,

firm changes course. Although such remedies might include legal or regulatory actions in extreme cases, they more frequently will involve reputation-based sanctions, such as leaving negative reviews online or posting on social media.[137] Conversely, consumers who do not understand a firm's contractual commitments may be poorly positioned to assess whether their grievance is legitimate and less likely to succeed in pursuing remedies from a firm. Consequently, many consumers who cannot make sense of a firm's contract language may feel too intimidated to challenge objectionable actions.

A second benefit of understandable contract language is that it increases the negative reputational consequences for firms that draft unfair terms. While most consumers may not read their contracts at the time of assent, some consumer markets are monitored by sophisticated consumers, journalists, regulators, academics, and market intermediaries.[138] These actors can help deter firms from drafting unfair terms and compel them to reverse course when they do.[139] Comprehensible contract language supports these market-based constraints on unfair terms in two ways. First, it helps market watchdogs identify unfair contract language. Although watchdogs tend to be more sophisticated than average consumers, their focus on market-wide dynamics rather than individual firms makes it harder for them to spot unfair or atypical terms when the language is confusing. Second, clear contract language makes it easier for watchdogs to communicate their

524–26 (2020). If so, then there is an argument that some consumers may be more likely to challenge firm practices that they perceive to be unfair when they cannot understand the governing contract language as compared to when they can. This problem is compelling with respect to unenforceable contract terms, which present unique problems that warrant distinct treatment. See Daniel Wilf-Townsend, Deterring Unenforceable Terms, 111 Va. L. Rev. 943, 996 (2025); Meirav Furth-Matzkin, The Harmful Effects of Unenforceable Contract Terms: Experimental Evidence, 70 Ala. L. Rev. 1031, 1055–56 (2019). But most contract terms are indeed enforceable.

[137] All such remedies depend critically on consumers being able to explain how a firm has violated its contractual commitments. See Jens Dammann, Electronic Word of Mouth and Consumer Protection: A Legal and Economic Analysis, 94 S. Cal. L. Rev. 423, 434 (2021).

[138] See Alan Schwartz & Louis L. Wilde, Intervening in Markets on the Basis of Imperfect Information: A Legal and Economic Analysis, 127 U. Pa. L. Rev. 630, 638 (1979); Robert A. Hillman, Online Boilerplate: Would Mandatory Website Disclosure of E-Standard Terms Backfire?, 104 Mich. L. Rev. 837, 853 (2006).

[139] Cf. Bakos et al., supra note 19, at 19 (suggesting that the informed minority argument is unlikely to work in settings like software licenses, where evidence suggests that one-tenth of one percent of consumers read the terms).

concerns about specific firms' contract language to the public.[140] Instead of needing to explain both the meaning of a firm's provisions and why that meaning is unreasonable, clear language lets watchdogs address the issue directly, avoiding accusations of misinterpretation.

A final, perhaps more aspirational, potential benefit of comprehensible contract language is that it may, over time, increase the percentage of consumers who choose to read contracts. Many consumers avoid reading terms and conditions because doing so is difficult and time-consuming.[141] Reducing the effort required to read contracts therefore has the potential to increase the percentage of consumers who read, especially if this improvement becomes widely known.[142] While the majority of consumers are unlikely to read their contracts under any conditions, even small increases in the percentage of reading consumers could influence firms' incentives in drafting their contracts.[143]

2. Assent-Based Implications of the No-Reading Critique

In addition to the practical benefits of comprehensible contract language, evidence supporting the no-understanding critique has important theoretical implications for consumer law. A core principle of contract law is that consumers are bound to terms and conditions of which they have actual or constructive knowledge when they indicate assent.[144] A corollary, often labeled as the "duty to read," is that the enforceability of a contract provision is not impacted by whether a consumer actually reads it at the time of assent.[145] As long as a

[140] Schwarcz, supra note 25, at 1491–94.

[141] See, e.g., Eric A. Posner, *ProCD v Zeidenberg* and Cognitive Overload in Contractual Bargaining, 77 U. Chi. L. Rev. 1181, 1185–87 (2010); Zev J. Eigen, Experimental Evidence of the Relationship Between Reading the Fine Print and Performance of Form-Contract Terms, 168 J. Inst'l & Theoretical Econ. 124, 134 (2012); Hillman & Rachlinski, supra note 4.

[142] See, e.g., Van Boom et al., supra note 26, at 191, 193–94 (testing consumers' self-reported comprehension of two substantively equivalent insurance contract versions of different reading difficulty and finding "that participants who received the 'easy' version found it easier to understand . . . than participants who received the difficult version"); see also Ayres & Schwartz, supra note 19, at 552–53 (proposing a system of "term-substantiation" in which unexpected, unfavorable contract terms are highlighted in contracts to increase reading).

[143] See Ayres & Schwartz, supra note 19, at 575.

[144] See Restatement (Second) of Conts. § 211 (A.L.I. 1981); see, e.g., Specht v. Netscape Commc'ns Corp., 306 F.3d 17, 20 (2d Cir. 2002).

[145] Wilkinson-Ryan, supra note 4, at 1753; Knapp, supra note 6, at 1085; Rustad & Koenig, supra note 69, at 1451.

consumer manifests assent to a proposed agreement, the agreement includes all terms a reasonable consumer would have understood to be part of the deal had they read it.[146]

Justifications for these principles vary, but most depend on the assumption that consumers have a realistic opportunity to understand the terms of a proposed deal at the time it is offered. Once provided with this opportunity, consumer acceptance of a deal includes "blanket assent" to the unread terms because the consumer had the opportunity to understand them.[147] This opportunity allows courts to interpret assent to encompass unread terms, as long as they do not undermine the basic spirit of the agreement as reflected in the "dickered terms."[148] In economic terms, the decision not to read can be understood as a voluntary assumption of risk by the consumer.[149] Just as tort law treats a knowing assumption of risk as a legitimate defense to negligence, contract law treats assent to unread terms as a presumptively enforceable reflection of consumer preferences.

These conceptions of consumer assent to standard-form contracts become incoherent if consumers are unable to understand contract terms even when they attempt to read them carefully.[150] In such cases, the consumer is not put on reasonable notice of those terms because they are not given a reasonable opportunity to understand them by reading. Instead, incomprehensible contract terms are functionally equivalent to contract language that is not physically accessible to consumers at the time of agreement. Courts consistently find such physically inaccessible language unenforceable. Examples include browse-wrap terms hidden on websites and not prominently brought to consumers' attention[151] and noncontractual documents that are ostensibly incorporated by reference but not reasonably available to consumers at the time of assent.[152]

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[146] See Restatement (Second) of Conts. § 211 (A.L.I. 1981).

[147] See Karl N. Llewellyn, The Common Law Tradition: Deciding Appeals 370 (1960), for Karl Llewellyn's conception of "blanket assent."

[148] See, e.g., Robert A. Hillman & Maureen O'Rourke, Defending Disclosure in Software Licensing, 78 U. Chi. L. Rev. 95, 105 (2011) ("The *opportunity* to read a standard form is important in part because it substantiates assent to the form even if a party does not read it.").

[149] Ayres & Schwartz, supra note 19, at 549.

[150] See Benoliel & Becher, supra note 7, at 2289.

[151] See, e.g., Norcia v. Samsung Telecomms. Am., LLC, 845 F.3d 1279, 1289 (9th Cir. 2017); Specht v. Netscape Commc'ns Corp., 306 F.3d 17, 20 (2d Cir. 2002).

[152] See, e.g., Walker v. BuildDirect.com Techs., Inc., 2015 OK 30, ¶¶ 1, 3, 6, 349 P.3d 549, 551–52; Timmerman v. Grain Exch., LLC, 915 N.E.2d 113, 119–20 (Ill. App. Ct.

B. Improving Consumer Understanding of Contract Terms

Given the practical and theoretical problems created when consumer contract language is incomprehensible to a typical consumer, lawmakers and courts should consider options to improve the comprehensibility of these documents.

1. Judicial Solutions

One promising option to limit the risk of incomprehensible contract language is for courts to refuse to enforce consumer contract terms when survey evidence demonstrates that the language is not understandable. Advocates advancing this claim could use a similar methodology to that employed in this Article: surveying typical consumers about their expectations regarding a firm's contractual obligations, both with and without access to the relevant contract language. As some have argued regarding survey-based approaches to contract interpretation, surveys are regularly used in other areas of law where consumer expectations matter (such as trademark), and they are increasingly available at relatively low cost.[153] Allowing litigants to advance this type of argument is thus reasonably feasible.

This approach aligns with the theoretical case for consumer assent to standard-form contracts.[154] It also has reasonable doctrinal foundations beyond the cases refusing to enforce contract terms that are not physically reasonably available to consumers at the time of assent.[155] Several courts have recognized that consumer assent does not extend to contract language that typical consumers cannot understand. For instance, in *Gaunt v. John Hancock Mutual Life Insurance Co.*,[156] the court refused to enforce the technical meaning of insurance policy language that it believed ordinary consumers would not understand.[157]

2009); Hyde v. Humana Ins. Co., 598 So. 2d 876, 879–80 (Ala. 1992); see also Amy B. Monahan & Daniel Schwarcz, Rules of Medical Necessity, 107 Iowa L. Rev. 423, 474 (2022) (describing cases in which courts rejected health insurers' attempts to incorporate rules of medical necessity by reference into plan documents on the ground that those rules were not made reasonably available to the insured at the time of contract formation).

[153] See Ben-Shahar & Strahilevitz, supra note 18, at 1766–67, 1805–06.

[154] See supra Subsection V.A.2.

[155] See supra Subsection V.A.2.

[156] 160 F.2d 599 (2d Cir. 1947).

[157] Id. at 601–02. The language stated that

> if the Company is satisfied that on the date of the completion of Part B of this application I was insurable * * [*] and if this application * * * is, prior to my death,

More recent decisions continue to endorse the proposition that consumer contract language should not be enforced if it is so confusing or complicated that typical consumers would be unable to understand its meaning, even if they attempted to do so.[158]

Admittedly, the logic that courts should not enforce contract language that typical consumers would not understand is often tied to the "reasonable expectations" doctrine, which many courts have rejected or largely abandoned.[159] Hostility to the doctrine is driven largely by its malleability and imprecision, which courts historically used to mandate coverage for losses excluded in clear policy language.[160] But a doctrine allowing consumer-litigants to show that the specific contract language

> approved by the Company at its Home Office, the insurance applied for shall be in force as of the date of completion of said Part B.

Id. at 599–600. According to the insurer, the plain meaning of this language was not, as the insured argued, that he was insured as of the completion of Part B of his application, but instead that his insurance would be retroactively treated as if it were in force as of the completion of Part B if and when his application was approved by the company's home office. The court admitted that "[a]n underwriter might so understand the phrase," but nonetheless rejected this interpretation because it is not what a typical consumer might understand after having read that phrase. Id. at 601–02.

[158] See, e.g., Zacarias v. Allstate Ins. Co., 775 A.2d 1262, 1268 (N.J. 2001) ("[I]n enforcing an insurance policy, courts will depart from the literal text and interpret it in accordance with the insured's understanding, even when that understanding contradicts the insurer's intent, if the text appears overly technical or contains hidden pitfalls, cannot be understood without employing subtle or legalistic distinctions, is obscured by fine print, or requires strenuous study to comprehend.").

[159] Susan M. Popik & Carol D. Quackenbos, Reasonable Expectations After Thirty Years: A Failed Doctrine, 5 Conn. Ins. L.J. 425, 430 n.18 (1998) ("Some courts, of course, have rejected the [reasonable expectations] doctrine altogether on various grounds, including that existing equitable doctrines provide sufficient protection or that there is insufficient justification to depart from the usual rules that apply to all contracts."); see, e.g., Const. State Ins. Co. v. Iso-Tex Inc., 61 F.3d 405, 410 n.4 (5th Cir. 1995) ("Texas law does not recognize coverage because of 'reasonable expectation' of the insured."); Nielsen v. O'Reilly, 848 P.2d 664, 667 (Utah 1992) ("This court, however, has never adopted any version of the [reasonable expectations] doctrine."); Findlay v. United Pac. Ins. Co., 917 P.2d 116, 121 (Wash. 1996) (en banc) ("The 'reasonable expectations' doctrine has never been adopted in Washington, and there is no reasonable expectation that no exemptions to coverage exist."); Deni Assocs. of Fla., Inc. v. State Farm Fire & Cas. Ins. Co., 711 So. 2d 1135, 1140 (Fla. 1998) ("We decline to adopt the doctrine of reasonable expectations. There is no need for it if the policy provisions are ambiguous because in Florida ambiguities are construed against the insurer. To apply the doctrine to an unambiguous provision would be to rewrite the contract and the basis upon which the premiums are charged." (footnote omitted)).

[160] See Schwarcz, supra note 8, at 1429–30; Kenneth S. Abraham, Distributing Risk: Insurance, Legal Theory, and Public Policy 101–32 (1986) (explaining how courts have often relied on the reasonable expectations doctrine to create insurance coverage even when policy language suggests otherwise).

relevant to their dispute with a firm is incomprehensible to consumers would be targeted and narrow. Moreover, it would focus on consumers' actual capacity to understand contract language, rather than judicial speculation; judges, as experts in contract interpretation, often fail to appreciate the limitations of nonexperts.[161] Because it is the understanding of typical consumers that matters, courts should consider relevant survey-based evidence to judge the comprehensibility of consumer contract terms.

Of course, there are reasonable objections to this approach. First, it would increase uncertainty for firms about whether their contract terms would be enforced, as survey-based experiments can be manipulated to produce desired results. Also, surveys are difficult to design, and it is increasingly difficult to recruit representative samples who are motivated to provide quality survey responses. However, firms could mitigate this risk by pre-testing contract language to ensure that typical consumers can understand it. Some firms already conduct similar tests for consumer disclosures.[162] A second objection is that this approach would make it harder to resolve disputes early in litigation without extensive discovery. While this concern has merit, it could be addressed by placing the burden of providing relevant survey evidence on consumer-litigants.

2. Regulatory and Legislative Solutions

Lawmakers and regulators should also consider options to improve the comprehensibility of consumer contracts. One promising approach is enhancing the availability and usage of AI-powered smart readers.[163] These tools use advanced generative AI models to simplify and summarize complex contractual texts. A key benefit of such tools is that they can personalize text to the reader's preferences and benchmark contracts by comparing them to others. Although generative AI tools are prone to make errors, this risk is significantly reduced when they are

[161] See Solan et al., supra note 35, at 1291–94 (finding via experiments that judges overestimate the degree to which laypeople agree with their decisions).

[162] See Ctr. for Drug Evaluation & Rsch., U.S. Food & Drug Admin., Guidance for Industry: Label Comprehension Studies for Nonprescription Drug Products 1 (2010) (noting that nonprescription drug sponsors are expected to conduct "label comprehension studies" that assess how well consumers understand proposed drug facts labeling).

[163] See Arbel & Becher, supra note 14, at 96, 104–06, 109.

used to summarize specific text.[164] Lawmakers and regulators could encourage the use of smart readers by (i) requiring consumer contracts to be publicly available, (ii) urging regulated firms to make generative AI tools available to consumers, and (iii) facilitating their use within popular consumer platforms like Amazon or Yelp, or by directly supplying these tools to potential consumers and market intermediaries.

A more traditional approach to improving consumer understanding of contract terms is imposing heightened quantitative readability standards on contracts. This approach has several potential benefits. First, quantitative readability score standards already apply to many consumer contracts.[165] Second, evidence shows that most consumer contracts are written at complexity levels beyond the capacity of ordinary consumers to understand.[166] However, there are significant limitations to using readability measures to improve the comprehensibility of consumer contracts. Most notably, evidence indicates that improvements in readability scores often do not translate into better consumer comprehension.[167] In addition, research has demonstrated that there is a significant need to improve the reliability of existing readability measures or create new ones.[168] Thus, small changes in readability requirements may not produce meaningful improvements. While drafting contracts to match consumers' educational attainment might enhance comprehension, it could do so at the expense of contract detail and specificity, which also provide important benefits.

We could also imagine general warnings at the beginning of policies or their declarations pages to help consumers understand policy terms. For example, homeowners insurance policies could have a prominent warning such as, "This policy does not cover flood damage. It does cover some water damage. To learn what is covered, see Section *XY* of

[164] See Manveer Singh Tamber et al., Benchmarking LLM Faithfulness in RAG with Evolving Leaderboards, *in* Proceedings of the 2025 Conference on Empirical Methods in Natural Language Processing: Industry Track 799, 799 (Saloni Potdar, Lina Rojas-Barahona & Sébastien Montella eds., 2025).

[165] See Blasie, Rise of Plain Language Laws, supra note 13, at 482–87; Blasie, Regulating Plain Language, supra note 13, at 705–06.

[166] See, e.g., Rustad, supra note 69, at 526, 534–42, 546–55 (summarizing past studies of readability of consumer contracts and conducting a new empirical study of the readability of top U.S. companies' consumer contracts); Benoliel & Becher, supra note 7, at 2277–78.

[167] Conklin et al., supra note 63, at 389, 393 (finding readability scores for travel insurance policies and consumer comprehension of these policies were not correlated, but suggesting that this result was likely driven by the high readability scores of all tested contracts).

[168] Arbel, supra note 36, at 963–64.

the policy." Although we have no evidence that such warnings would be effective, we could imagine requiring insurers to conduct empirical testing of such warnings like the testing we conduct here, and perhaps even linking the results of that testing to the availability of pro-policyholder interpretive canons.[169] More effective might be targeted warnings placed just before provisions that are found (through research such as ours) to be especially prone to produce misunderstandings owing to the partial-reading problem. For example, such a targeted warning might read: "Caution: You may not understand what this insurance does and doesn't cover unless you *read to the very end* of this section." Of course, while such a warning might result in more careful and complete reading of the provision at issue, it might also result in less careful reading of other provisions. That result, however, might well be an improvement over the current situation. While it may be reasonable to expect consumers to understand clearly worded language that they read carefully, they have limited attention. Directing them to spend their limited attention on the provisions that are likely to upset their prior expectations might improve consumer welfare. However, improving the readability of consumer contracts would not address other explanations for the no-reading problem, including the belief that companies will not respect the terms of contracts and the sheer number of contracts most individuals would be expected to read.[170]

CONCLUSION

This Article presents new empirical evidence that complicates long-standing assumptions about consumer comprehension of standard-form contracts, specifically homeowners insurance policies. While our findings confirm that access to contract language can sometimes enhance understanding, they also reveal a paradox: providing contract language may, in some cases, impair consumers' grasp of their contractual rights and obligations. One likely explanation is a previously underexplored phenomenon—the partial-reading or partial-understanding problem—where consumers either misinterpret terms or disengage prematurely.

[169] See Boardman, supra note 13, at 1077–78.

[170] See Arbel, supra note 36, at 928. One proposed solution to address the proliferation of contracts is to make low-stakes, written-form contracts unenforceable, thus reducing the number of contracts consumers are expected to read. See David A. Hoffman, Defeating the Empire of Forms, 109 Va. L. Rev. 1367, 1409–10 (2023).

Even when respondents showed improved accuracy in coverage assessments after reviewing policy language, the gains were modest, and respondents' confidence in their coverage assessments remained low, underscoring the broader challenge of translating legal text into meaningful notice. Moreover, exposure to policy language increased confidence in coverage assessments, even when those assessments were incorrect. Analyses across subpopulations suggested that these comprehension difficulties are not confined to specific groups but are widespread among consumers.

These findings underscore the need for legal and regulatory reforms aimed at enhancing the clarity and accessibility of consumer contracts, as comprehensible contract language is an essential consumer protection, even if most consumers do not read contracts at the time of supposed assent to their terms. To address these challenges, courts could refuse to enforce terms shown to be widely misunderstood, while regulators could promote AI-powered contract summarization tools, impose stricter readability standards, or require targeted warnings for particularly misleading provisions—ensuring that contracts are not just available, but truly accessible to consumers.

APPENDIX A. ADDITIONAL TABLES

Table A1. Accuracy of Respondents' Assessments of Insurance Coverage

Vignette	Coverage Context	No Policy Language (Control Group)			ISO Policy Language (Treatment Group)			Difference
		N	*Accurate*	*%*	*N*	*Accurate*	*%*	*PP*
Clear Non-Coverage	Earthquake	325	127	39.1	493	358	72.6	33.5
	Slip-and-Fall	332	159	47.9	489	311	63.6	15.7
	Deck Collapse 1	323	126	39.0	476	96	20.2	-18.8
	Deck Collapse 2	328	193	58.8	485	290	59.8	1.0
Clear Coverage	Earthquake	326	202	62.0	501	186	37.1	-24.9
	Slip-and-Fall	329	172	52.3	490	322	65.7	13.4
	Electrical Fire	974	516	53.0	479	99	20.7	-32.3

Note: Percentages denote the proportion of the respective survey subsample that accurately predicted policy coverage. For the clear coverage vignettes, responses predicting that repairs were either probably covered or definitely covered were accurate; for the clear non-coverage vignettes, responses predicting that repairs were either probably not covered or definitely not covered were considered accurate.

Table A2. Perceptions of Confidence in Accuracy of Coverage Assessments

Vignette	Coverage Context	No Policy Language (Control Group)			ISO Policy Language (Treatment Group)			Difference
		N	*Confident*	*%*	*N*	*Confident*	*%*	*PP*
Clear Non-Coverage	Earthquake	325	156	48.0	493	292	59.2	11.2
	Slip-and-Fall	332	117	35.2	489	224	45.8	10.6
	Deck Collapse 1	323	102	31.6	476	271	56.9	25.3
	Deck Collapse 2	328	123	37.5	485	293	60.4	22.9
Clear Coverage	Earthquake	326	148	45.4	501	283	56.5	11.1
	Slip-and-Fall	329	125	38.0	490	228	46.5	8.5
	Electrical Fire	974	378	38.8	479	186	38.8	0.0

Note: Confident responses are from respondents who declared they were either very confident or extremely confident that their assessments of coverage were correct.

Table A3. Logit Regression Analyses of Accuracy of Respondents' Assessments of Homeowners Insurance Coverage

	Earthquake Damage (Coverage & Non-coverage)			Slip-and-Fall Liability (Coverage & Non-coverage)			Deck Collapse (Non-coverage Only)			Electrical Fire (Coverage Only)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Received policy language	1.172	1.173	0.352**	1.887**	1.886**	1.842**	0.685**	0.659**	0.384**	0.214**
	(0.121)	(0.123)	(0.053)	(0.202)	(0.202)	(0.279)	(0.072)	(0.074)	(0.063)	(0.029)
Second vignette		1.651**	0.387**		0.892	0.868		3.975**	2.275**	
		(0.167)	(0.063)		(0.093)	(0.142)		(0.436)	(0.370)	
2nd vignette × Rec'd policy language			11.825**			1.049			2.691**	
			(2.538)			(0.223)			(0.594)	
Baseline odds	0.663	0.515*	1.088	0.705	0.751	0.762	0.783	0.342**	0.454**	0.571
	(0.168)	(0.134)	(0.307)	(0.190)	(0.207)	(0.217)	(0.203)	(0.095)	(0.128)	(0.163)
No. of observations	1,644	1,644	1,644	1,640	1,640	1,640	1,611	1,611	1,611	1,453
Pseudo R-squared	0.0116	0.0225	0.0836	0.0465	0.0470	0.0470	0.0201	0.0967	0.106	0.108

Note: The table reports results from logit regressions in which the dichotomous outcome variable is equal to one if the respondent's understanding of insurance coverage is accurate and equal to zero otherwise. Estimates are shown as odds ratios (i.e., exponentiated coefficients of the logistic regression). Heteroskedasticity-robust standard errors are reported in parentheses. The reported pseudo R-squared is McFadden's R-squared. The analyses controlled for experiential, demographic, and economic factors. Experiential controls were indicators for selling insurance, being the final decision-maker on a homeowners insurance contract, reporting that they have read and understand their own policy, having switched carriers, and having prior negative views of insurers. Demographic controls were gender, age, race/ethnicity, marital status, and region. Economic controls were education, employment, and income. The symbols * and ** represent significance at the 5% and 1% levels, respectively.

Table A4. Logit Regression Analyses of Confidence in Accuracy of Coverage Assessments

	Earthquake Damage (Coverage & Non-coverage)			Slip-and-Fall Liability (Coverage & Non-coverage)			Deck Collapse (Non-coverage Only)			Electrical Fire (Coverage Only)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Received policy language	1.498**	1.498**	1.448*	1.457**	1.456**	1.345	2.794**	2.799**	2.989**	0.953
	(0.159)	(0.159)	(0.216)	(0.158)	(0.158)	(0.204)	(0.308)	(0.309)	(0.473)	(0.112)
Second vignette		1.205	1.156		0.947	0.859		1.183	1.279	
		(0.126)	(0.187)		(0.100)	(0.145)		(0.126)	(0.220)	
2nd vignette × Rec'd policy language			1.071			1.174			0.880	
			(0.227)			(0.255)			(0.192)	
Baseline odds	0.486**	0.443**	0.453**	0.302**	0.311**	0.327**	0.302**	0.274**	0.263**	0.584**
	(0.127)	(0.117)	(0.124)	(0.085)	(0.090)	(0.098)	(0.084)	(0.078)	(0.077)	(0.112)
No. of observations	1,644	1,644	1,644	1,640	1,640	1,640	1,611	1,611	1,611	1,453
Pseudo R-squared	0.0686	0.0700	0.0701	0.0752	0.0753	0.0756	0.0839	0.0850	0.0851	0.0248

Note: The table reports results from logit regressions in which the dichotomous outcome variable is equal to one if the respondent's confidence in their understanding of insurance coverage is high (i.e., very confident or extremely confident) and equal to zero otherwise. Estimates are shown as odds ratios (i.e., exponentiated coefficients of the logistic regression). Heteroskedasticity-robust standard errors are reported in parentheses. The reported pseudo R-squared is McFadden's R-squared. The analyses controlled for experiential, demographic, and economic factors. Experiential controls were indicators for selling insurance, being the final decision-maker on a homeowners insurance contract, reporting they have read and understand their own policy, having switched carriers, and having prior negative views of insurers. Demographic controls were gender, age, race/ethnicity, marital status, and region. Economic controls were education, employment, and income. The symbols * and ** represent significance at the 5% and 1% levels, respectively.

Table A5. Logit Regression Analyses of Accuracy of Respondents' Confidence in the Accuracy of Their Coverage Assessments

	Earthquake Damage	Slip-and-Fall Liability	Deck Collapse	Electrical Fire
	(1)	(2)	(3)	(4)
Received policy language	0.299**	1.743**	0.443**	0.199**
	(0.065)	(0.342)	(0.099)	(0.038)
Second vignette	0.327**	1.389	1.684**	
	(0.075)	(0.289)	(0.340)	
Confident respondent	1.852*	4.652**	0.890	2.680**
	(0.448)	(1.210)	(0.220)	(0.390)
2nd vignette × Rec'd policy lang.	11.042**	0.874	1.862*	
	(3.443)	(0.238)	(0.565)	
Confident × Rec'd policy lang.	1.178	1.066	0.810	1.047
	(0.366)	(0.360)	(0.275)	(0.290)
2nd vignette × Confident	1.283	0.279**	2.330*	
	(0.430)	(0.099)	(0.800)	
2nd vignette × Confident	1.299	1.582	1.376	
× Rec'd policy lang.	(0.574)	(0.725)	(0.631)	
Baseline odds	0.928	0.488*	0.502*	0.450**
	(0.283)	(0.148)	(0.148)	(0.131)
No. of observations	1,644	1,640	1,611	1,453
Pseudo R-squared	0.115	0.0953	0.119	0.140

Note: The table reports results from logit regressions in which the dichotomous outcome variable is equal to one if the respondent's understanding of insurance coverage is accurate and equal to zero otherwise. Estimates are shown as odds ratios (i.e., exponentiated coefficients of the logistic regression). Heteroskedasticity-robust standard errors are reported in parentheses. The reported pseudo R-squared is McFadden's R-squared. The analyses controlled for experiential, demographic, and economic factors. Experiential controls were indicators for selling insurance, being the final decision-maker on a homeowners insurance contract, reporting they have read and understand their own policy, having switched carriers, and having prior negative views of insurers. Demographic controls were gender, age, race/ethnicity, marital status, and region. Economic controls were education, employment, and income. The symbols * and ** represent significance at the 5% and 1% levels, respectively.

Table A6. Logit Regression Analyses of Respondents' Accuracy of Coverage Assessments by Sophistication Level

	Earthquake Damage	Slip-and-Fall Liability	Deck Collapse	Electrical Fire
	(1)	(2)	(3)	(4)
Received policy language	0.360**	1.977**	0.395**	0.215**
	(0.056)	(0.307)	(0.067)	(0.030)
Second vignette	0.403**	0.938	2.308**	
	(0.068)	(0.157)	(0.390)	
Sophisticated respondent	1.549	3.326*	0.750	1.677
	(0.706)	(1.602)	(0.374)	(0.450)
2nd vignette. × Rec'd policy lang.	11.217**	1.030	2.552**	
	(2.485)	(0.225)	(0.584)	
Sophisticated × Rec'd policy lang.	0.885	0.345	0.720	1.071
	(0.500)	(0.216)	(0.535)	(0.499)
2nd vignette × Sophisticated	0.586	0.312	0.908	
	(0.372)	(0.216)	(0.575)	
2nd vignette × Sophisticated	1.791	1.481	1.857	
× Rec'd policy lang.	(1.522)	(1.312)	(1.695)	
Baseline odds	1.253	1.052	0.602*	0.732
	(0.303)	(0.252)	(0.147)	(0.177)
No. of observations	1,644	1,640	1,611	1,453
Pseudo R-squared	0.0829	0.0451	0.104	0.103

Note: The table reports results from logit regressions in which the dichotomous outcome variable is equal to one if the respondent's understanding of insurance coverage is accurate and equal to zero otherwise. Estimates are shown as odds ratios (i.e., exponentiated coefficients of the logistic regression). Heteroskedasticity-robust standard errors are reported in parentheses. The reported pseudo R-squared is McFadden's R-squared. The analyses controlled for experiential, demographic, and economic factors. Experiential controls were indicators for selling insurance, being the final decision-maker on a homeowners insurance contract, having switched carriers, and having prior negative views of insurers. Demographic controls were gender, age, race/ethnicity, marital status, and region. Economic controls were employment and income. The symbols * and ** represent significance at the 5% and 1% levels, respectively.

Table A7. Logit Regression Analyses of Accuracy of Coverage Assessments by Income

	Earthquake Damage	Slip-and-Fall Liability	Deck Collapse	Electrical Fire
	(1)	(2)	(3)	(4)
Received policy language	0.299**	1.743**	0.355**	0.219**
	(0.065)	(0.342)	(0.062)	(0.031)
Second vignette	0.327**	1.389	2.033**	
	(0.075)	(0.289)	(0.355)	
High-income respondent	1.161	1.714	0.421*	0.948
	(0.403)	(0.578)	(0.181)	(0.187)
2nd vignette × Rec'd policy lang.	12.026**	0.921	2.825**	
	(2.791)	(0.210)	(0.671)	
High income × Rec'd policy lang.	0.811	0.465	2.383	0.967
	(0.351)	(0.201)	(1.339)	(0.385)
2nd vignette × High income	1.705	0.831	2.930*	
	(0.796)	(0.399)	(1.529)	
2nd vignette × High income	0.918	2.647	0.525	
× Rec'd policy lang.	(0.572)	(1.668)	(0.365)	
Baseline odds	1.109	0.717	0.496*	0.518*
	(0.314)	(0.205)	(0.139)	(0.146)
No. of observations	1,644	1,640	1,611	1,453
Pseudo R-squared	0.115	0.0489	0.109	0.106

Note: The table reports results from logit regressions in which the dichotomous outcome variable is equal to one if the respondent's understanding of insurance coverage is accurate and equal to zero otherwise. Estimates are shown as odds ratios (i.e., exponentiated coefficients of the logistic regression). Heteroskedasticity-robust standard errors are reported in parentheses. The reported pseudo R-squared is McFadden's R-squared. The analyses controlled for experiential, demographic, and economic factors. Experiential controls were indicators for selling insurance, being the final decision-maker on a homeowners insurance contract, reporting they had read and understand their homeowners insurance policy, having switched carriers, and having prior negative views of insurers. Demographic controls were gender, age, race/ethnicity, marital status, and region. Economic controls were education and employment. The symbols * and ** represent significance at the 5% and 1% levels, respectively.

Table A8. Logit Regression Analyses of Accuracy of Respondents' Coverage Assessments by Race

	Earthquake Damage	Slip-and-Fall Liability	Deck Collapse	Electrical Fire
	(1)	(2)	(3)	(4)
Received policy language	0.297**	1.943**	0.418**	0.230**
	(0.056)	(0.371)	(0.083)	(0.038)
Second vignette	0.410**	0.721	2.601**	
	(0.082)	(0.144)	(0.519)	
Nonwhite respondent	0.863	0.653	0.985	1.114
	(0.217)	(0.163)	(0.245)	(0.164)
2nd vignette × Rec'd policy lang.	13.510**	1.097	2.276**	
	(3.603)	(0.290)	(0.615)	
Nonwhite × Rec'd policy lang.	1.620	0.903	0.773	0.809
	(0.514)	(0.284)	(0.272)	(0.228)
2nd vignette × Nonwhite	0.814	1.745	0.668	
	(0.285)	(0.603)	(0.231)	
2nd vignette × Nonwhite	0.726	0.826	1.658	
× Rec'd policy lang.	(0.328)	(0.368)	(0.784)	
Baseline odds	1.104	0.802	0.423**	0.562*
	(0.318)	(0.235)	(0.124)	(0.161)
No. of observations	1,644	1,640	1,611	1,453
Pseudo R-squared	0.0862	0.0494	0.106	0.108

Note: The table reports results from logit regressions in which the dichotomous outcome variable is equal to one if the respondent's understanding of insurance coverage is accurate and equal to zero otherwise. Estimates are shown as odds ratios (i.e., exponentiated coefficients of the logistic regression). Heteroskedasticity-robust standard errors are reported in parentheses. The reported pseudo R-squared is McFadden's R-squared. The analyses controlled for experiential, demographic, and economic factors. Experiential controls were indicators for selling insurance, being the final decision-maker on a homeowners insurance contract, reporting they had read and understand their homeowners insurance policy having switched carriers, and having prior negative views of insurers. Demographic controls were gender, age, marital status, and region. Economic controls were education, employment, and income. The symbols * and ** represent significance at the 5% and 1% levels, respectively.

APPENDIX B. ROBUSTNESS CHECKS

A. Definition of Accurate Responses

Comparing the results in Tables A3 and A9 indicates that our results for the earthquake, slip-and-fall, and electrical fire vignettes were not impacted by defining accurate responses in each vignette as "definitely" covered or not covered, as opposed to "probably" or "definitely" covered or not covered, as in the model reported in Table A3. For the first deck collapse vignette, we detect no significant difference between the control and treatment groups, indicating that our main results in that analysis are driven by respondents who responded "probably not covered."

Table A9. Logit Regression Analyses of Accuracy of Respondents' Coverage Assessments Using Only "Definitely" Covered / Not Covered Responses

	Earthquake Damage	Slip-and-Fall Liability	Deck Collapse	Electrical Fire
	(1)	(2)	(3)	(4)
Received policy language	0.712	1.475*	1.176	0.339**
	(0.124)	(0.263)	(0.297)	(0.066)
Second vignette	0.781	0.915	3.339**	
	(0.148)	(0.190)	(0.807)	
2nd vignette × Rec'd policy language	7.575**	1.821*	1.981*	
	(1.828)	(0.459)	(0.596)	
Baseline odds	0.198**	0.114**	0.047**	0.049**
	(0.062)	(0.040)	(0.018)	(0.020)
No. of observations	1,644	1,640	1,611	1,453
Pseudo R-squared	0.116	0.0580	0.126	0.0826

Note: The table reports results from logit regressions in which the dichotomous outcome variable is equal to one if the respondent's understanding of insurance coverage is accurate and equal to zero otherwise. Estimates are shown as odds ratios (i.e., exponentiated coefficients of the logistic regression). Heteroskedasticity-robust standard errors are reported in parentheses. The reported pseudo R-squared is McFadden's R-squared. The analyses controlled for experiential, demographic, and economic factors. Experiential

controls were indicators for selling insurance, being the final decision-maker on a homeowners insurance contract, reporting they have read and understand their own policy, having switched carriers, and having prior negative views of insurers. Demographic controls were gender, age, race/ethnicity, marital status, and region. Economic controls were education, employment, and income. The symbols * and ** represent significance at the 5% and 1% levels, respectively.

B. Definition of Sophisticated Respondents

Comparing the results in Tables A6 and A10 indicates that the results were not impacted when we used an alternative definition of sophisticated respondents as those with bachelor's degrees or higher who had read the most relevant portions of their own homeowners insurance policy. Our primary definition required that respondents must also have reported (1) understanding their policy terms "very well" or "completely," and (2) understanding "mostly" or "completely" what their homeowners policy does and does not cover.

Table A10. Logit Regression Analyses of Respondents' Accuracy of Coverage Assessments Using Alternative Sophisticated Consumer Definition

	Earthquake Damage	Slip-and-Fall Liability	Deck Collapse	Electrical Fire
	(1)	(2)	(3)	(4)
Received policy language	0.354**	1.847**	0.411**	0.225**
	(0.060)	(0.307)	(0.074)	(0.034)
Second vignette	0.393**	1.159	1.120	
	(0.072)	(0.468)	(0.457)	
Sophisticated respondent	0.953	1.265	0.938	1.167
	(0.273)	(0.366)	(0.280)	(0.191)
2nd vignette × Rec'd policy lang.	10.779**	1.149	2.369**	
	(2.588)	(0.272)	(0.579)	
Sophisticated × Rec'd policy lang.	1.030	0.936	0.745	0.859
	(0.370)	(0.355)	(0.320)	(0.268)
2nd vignette × Sophisticated	0.925	1.159	1.120	
	(0.368)	(0.468)	(0.457)	
2nd vignette × Sophisticated × Rec'd policy lang.	1.551	0.709	1.896	
	(0.828)	(0.376)	(1.076)	
Baseline odds	1.305	1.092	0.598*	0.728
	(0.324)	(0.268)	(0.149)	(0.177)
No. of observations	1,644	1,640	1,611	1,453
Pseudo R-squared	0.0826	0.0416	0.105	0.100

Note: The table reports results from logit regressions in which the dichotomous outcome variable is equal to one if the respondent's understanding of insurance coverage is accurate and equal to zero otherwise. Estimates are shown as odds ratios (i.e., exponentiated coefficients of the logistic regression). Heteroskedasticity-robust standard errors are reported in parentheses. The reported pseudo R-squared is McFadden's R-squared. The analyses controlled for experiential, demographic, and economic factors. Experiential controls were indicators for selling insurance, being the final decision-maker on a homeowners insurance contract, having switched carriers, and having prior negative views of insurers. Demographic controls were gender, age, race/ethnicity, marital status, and region. Economic controls were employment and income. The symbols * and ** represent significance at the 5% and 1% levels, respectively.

C. Definition of Higher-Income Respondents

Comparing the results in Tables A7 and A11 indicates that our results were not impacted by using an alternative definition of high-income respondents as those with gross household annual incomes of $200,000 or greater. Our primary definition of high income was those with gross household incomes of $150,000 or more.

Table A11. Logit Regression Analyses of Respondents' Accuracy of Coverage Assessments with Alternative High-Income Definition

	Earthquake Damage	Slip-and-Fall Liability	Deck Collapse	Electrical Fire
	(1)	(2)	(3)	(4)
Received policy language	0.333**	1.974**	0.376**	0.218**
	(0.052)	(0.307)	(0.063)	(0.030)
Second vignette	0.344**	0.908	2.227**	
	(0.059)	(0.153)	(0.375)	
High-income respondent	0.598	2.657	0.400	1.134
	(0.253)	(1.352)	(0.239)	(0.294)
2nd vignette × Rec'd policy lang.	13.141**	0.967	2.525**	
	(2.935)	(0.211)	(0.574)	
High income × Rec'd policy lang.	1.999	0.358	1.265	1.099
	(1.110)	(0.236)	(1.239)	(0.563)
2nd vignette × High income	4.443*	0.528	2.093	
	(2.753)	(0.367)	(1.483)	
2nd vignette × High income	0.296	3.660	2.563	
× Rec'd policy lang.	(0.262)	(3.374)	(2.920)	
Baseline odds	1.158	0.713	0.476**	0.506*
	(0.325)	(0.202)	(0.133)	(0.143)
No. of observations	1,644	1,640	1,611	1,453
Pseudo R-squared	0.0858	0.0485	0.110	0.106

Note: The table reports results from logit regressions in which the dichotomous outcome variable is equal to one if the respondent's understanding of insurance coverage is accurate and equal to zero otherwise. Estimates are shown as odds ratios (i.e., exponentiated coefficients of the logistic regression). Heteroskedasticity-robust standard errors are reported in parentheses. The reported pseudo R-squared is McFadden's R-squared. The analyses controlled for experiential, demographic, and economic factors. Experiential controls were indicators for selling insurance, being the final decision-maker on a homeowners insurance contract, reporting they had read

and understand their homeowners insurance policy, having switched carriers, and having prior negative views of insurers. Demographic controls were gender, age, race/ethnicity, marital status, and region. Economic controls were education and employment. The symbols * and ** represent significance at the 5% and 1% levels, respectively.

D. Analyzing Differences by Race Using Only Black and White Respondents

Comparing the results in Tables A8 and A12 indicates that the results in our primary analysis were not impacted by restricting the analysis sample to only Black and white respondents when evaluating the differences by race. This analysis excludes those who identified their race as neither Black nor white, who were included in our primary analysis.

Table A12. Logit Regression Analyses of Respondents' Accuracy of Coverage Assessments Using Only Black and White Respondents

	Earthquake Damage	Slip-and-Fall Liability	Deck Collapse	Electrical Fire
	(1)	(2)	(3)	(4)
Received policy language	0.295**	1.941**	0.407**	0.226**
	(0.056)	(0.371)	(0.082)	(0.037)
Second vignette	0.414**	0.732	2.636**	
	(0.083)	(0.146)	(0.535)	
Nonwhite respondent	0.770	0.527	0.884	1.007
	(0.268)	(0.200)	(0.321)	(0.220)
2nd vignette × Rec'd policy lang.	13.567**	1.067	2.319**	
	(3.635)	(0.283)	(0.634)	
Nonwhite × Rec'd policy lang.	1.864	1.134	0.880	0.668
	(0.825)	(0.528)	(0.440)	(0.284)
2nd vignette × Nonwhite	0.596	1.686	0.409	
	(0.341)	(0.851)	(0.207)	
2nd vignette × Nonwhite	0.841	0.570	1.829	
× Rec'd policy lang.	(0.588)	(0.368)	(1.227)	
Baseline odds	1.063	0.684	0.318**	0.653
	(0.339)	(0.221)	(0.104)	(0.212)
No. of observations	1,285	1,292	1,287	1,132
Pseudo R-squared	0.0928	0.0562	0.112	0.115

Note: The table reports results from logit regressions in which the dichotomous outcome variable is equal to one if the respondent's understanding of insurance coverage is accurate and equal to zero otherwise. Estimates are shown as odds ratios (i.e., exponentiated coefficients of the logistic regression). Heteroskedasticity-robust standard errors are reported in parentheses. The reported pseudo R-squared is McFadden's R-squared. The analyses controlled for experiential, demographic, and economic factors. Experiential controls were indicators for selling insurance, being the final decision-maker on a homeowners insurance contract, reporting they had read and understand their homeowners insurance policy, having switched carriers, and having prior negative views of insurers. Demographic controls were gender, age, marital status, and region. Economic controls were education, employment, and income. The symbols * and ** represent significance at the 5% and 1% levels, respectively.

E. Testing for the Impact of Differing Cognitive Load

As discussed in Subsection II.A.2, one of the limitations of our experiment involved subjecting treatment group respondents to an overall higher cognitive load than their control group counterparts. Our survey instrument was programmed to randomize the order in which the main vignettes (i.e., earthquake, deck collapse, and slip-and-fall) were distributed to respondents. We leverage this mechanism of our survey design to test whether the potential impact of differing cognitive load significantly impacted our results. We do this by limiting the analyses to only the first-encountered vignette for all respondents, reported in Table A13. Notably, we cannot run the same test for the electrical fire vignette because it was excluded from the randomization algorithm, but we believe the analyses reported in Table A13 indicate that the differing cognitive load between surveys likely does not drive any of our main results.

Table A13. Logit Regression Analyses of Respondents' Accuracy of Coverage Assessments Using Only First-Encountered Vignettes

	Earthquake Damage	Slip-and-Fall Liability	Deck Collapse
	(1)	(2)	(3)
Received policy language	0.294**	2.331**	0.343**
	(0.080)	(0.616)	(0.101)
Second vignette	0.588	0.691	2.390**
	(0.171)	(0.203)	(0.707)
2nd vignette × Rec'd policy language	11.686**	1.069	3.677**
	(4.595)	(0.402)	(1.481)
Baseline odds	1.232	0.587	0.269*
	(0.622)	(0.283)	(0.142)
No. of observations	533	542	518
Pseudo R-squared	0.116	0.0704	0.139

Note: The table reports results from logit regressions in which the dichotomous outcome variable is equal to one if the respondent's understanding of insurance coverage is accurate and equal to zero otherwise. Estimates are shown as odds ratios (i.e., exponentiated coefficients of the logistic regression). Heteroskedasticity-robust standard errors are reported in parentheses. The reported pseudo R-squared is McFadden's R-squared. The analyses controlled for experiential, demographic, and economic factors. Experiential controls were indicators for selling insurance, being the final decision-maker on a homeowners insurance contract, reporting that they have read and understand their own policy, having switched carriers, and having prior negative views of insurers. Demographic controls were gender, age, race/ethnicity, marital status, and region. Economic controls were education, employment, and income. The symbols * and ** represent significance at the 5% and 1% levels, respectively.

APPENDIX C. IMPACT WHEN THE APPLICATION POLICY LANGUAGE IS AMBIGUOUS OR UNENFORCEABLE

In addition to testing the impact of viewing policy language when the event described in a vignette was clearly covered or clearly not covered,

we also tested the impact of providing policy language when it was unclear whether the situation in the vignette would be covered.[171] As described in more detail in Part IV, consumers' ability to detect potential uncertainties or ambiguities about whether a loss is covered is quite important as a policy matter because policyholders are generally entitled to coverage when the operative policy language is ambiguous.

A. Policy Language and Vignettes

To better understand the extent to which consumers could recognize when policy language was potentially ambiguous or unenforceable in a specific situation, we used two of the policy excerpts reported in the primary analyses, pertaining to earthquake damage and slip-and-fall liability. In each case, however, we provided respondents with vignettes modified intentionally to describe situations in which it was unclear whether the policy language would cover the loss or was unenforceable.

1. Earthquake Policy Language: Unclear Coverage Vignette

In the revised earthquake vignette, the insured's loss was caused more directly by the negligence of a local city in erecting a utility pole than by an earthquake.[172] Although the plain meaning of the ISO policy language appears to deny coverage, the enforceability of this language is unclear and depends on whether the jurisdiction allows insurers to contract out of the "efficient proximate cause rule."[173] One common formulation of the efficient proximate cause rule is that, in the event that a covered peril and an uncovered peril both cause a loss, coverage depends on which is the dominant cause of the loss. In some jurisdictions, this rule supersedes policy language attempting to contract

[171] These experiments more closely mirror those conducted by Ben-Shahar and Strahilevitz. See Ben-Shahar & Strahilevitz, supra note 18, at 1783–84.

[172] This vignette was loosely modeled on *State Farm Fire and Casualty Co. v. Bongen*, 925 P.2d 1042, 1043 (Alaska 1996).

[173] See Vonda Mallicoat Laughlin, The Efficient Proximate Cause Doctrine—What Is It, and Why Should I Care?, 73 Baylor L. Rev. 311, 313 (2021) ("The efficient proximate cause doctrine sets forth a method to determine policy coverage in situations in which two or more identifiable causes contribute to a loss and both covered and excluded causative factors are involved."); see also Murray v. State Farm Fire & Cas. Co., 509 S.E.2d 1, 12 (W. Va. 1998) ("The efficient proximate cause . . . is not necessarily the last act in a chain of events, nor is it the triggering cause. The efficient proximate cause doctrine looks to the quality of the links in the chain of causation. The efficient proximate cause is the predominating cause of the loss."). For research on the use of unenforceable terms, see sources cited supra note 136.

out of that rule. Figure A1 provides the text of the earthquake policy language (which matches the excerpted policy language used in the experiments reported in the primary analyses).

Figure A1. Instruction and Policy Language Distributed in the Earthquake Unclear Coverage Vignette

Earthquake Coverage Scenario	Control Group (No policy language)	Treatment Group (ISO HO3 Policy Language)
Unclear Coverage Scenario: A magnitude 6.0 earthquake strikes near your home. The shaking from the earthquake knocks down an electrical pole in front of your home, which lands on your home and caves in the roof. The local utility company hadn't maintained the pole. If it had, the pole wouldn't have fallen. Major repairs are required.	Respondents instructed to answer coverage scenario based on their existing understanding of what a typical homeowners policy would cover, without looking at any specific insurance policy language or conducting any research.	We insure against direct physical loss to covered property. We do not insure for loss excluded under the Exclusions Section. **Section I -- Exclusions** A. We do not insure for loss caused directly or indirectly by any of the following. Such loss is excluded regardless of any other cause or event contributing concurrently or in any sequence to the loss. These exclusions apply whether or not the loss event results in widespread damage or affects a substantial area. 1. Earth Movement Earth Movement means: a. Earthquake, including land shock waves or tremors before, during or after volcanic eruption; b. Landslide, mudslide or mudflow; c. Subsidence or sinkhole; or d. Any other earth movement including earth sinking, rising or shifting. This Exclusion A.1 applies regardless of whether any of the above, in A.1.a. through A.1.d., is caused by an act of nature or is otherwise caused. However, direct loss by fire, explosion or theft resulting from any of the above, in A.1.a through A.1.d., is covered.

2. Slip-and-Fall Policy Language: Unclear Coverage Vignette

Turning to the slip-and-fall liability scenario, we introduced ambiguity into the revised vignette by specifying that the homeowner was hosting a small gathering of work colleagues at their home for the dual purpose of discussing a work-related project and fostering friendships. The precise details of the vignette and the operative policy language are contained in Figure A2. As above, we view the question of whether the slip-and-fall lawsuit would be covered to be ambiguous. On one hand, the insurer has a good argument that the bodily injury was "in connection" with a "business . . . engaged in by an insured," given that it involved colleagues gathering for work-related discussions, and the phrase "in connection" is deliberately broad. But on the other hand, there is a good argument that even a gathering of colleagues at one's home is not really "in connection" with one's work, especially given some of the surrounding language of the clause that seems to focus on the prospect of an insured conducting business operations in their home.

Figure A2. Instruction and Policy Language Distributed in the Slip-and-Fall Liability Unclear Coverage Vignette

Slip-and-Fall Coverage Vignette	Control Group (No policy language)	Treatment Group (ISO HO3 Policy Language)
Unclear Coverage Scenario: It snows the night before you are scheduled to host a small gathering at your home of people you work with. The point of the gathering is to build better work relationships, to discuss a controversial project at work that hasn't been resolved, and to foster friendships that will continue beyond the workplace. Although you shoveled your front walkway, you didn't shovel the steps leading up from the sidewalk to your front walkway. One of your guests slips on these steps, suffers a broken leg and concussion, and sues you for negligence.	Respondents instructed to answer coverage scenario based on their existing understanding of what a typical homeowners policy would cover, without looking at any specific insurance policy language or conducting any research.	If a claim is made or a suit is brought against an insured for damages because of bodily injury or property damage caused by an occurrence to which this coverage applies, we will pay up to our limit of liability for the damages for which an insured is legally liable. **Exclusions** Liability Coverage does not apply to the following: (1) "Business" a. Bodily injury or property damage arising out of or in connection with a "business" conducted from an insured location or engaged in by an insured, whether or not the "business" is owned or operated by an insured or employs an insured. This Exclusion applies but is not limited to an act or omission, regardless of its nature or circumstance, involving a service or duty rendered, promised, owed, or implied to be provided because of the nature of the "business". **DEFINITIONS** "Business" means: a. A trade, profession or occupation engaged in on a full-time, part-time or occasional basis; or b. Any other activity engaged in for money or other compensation, except the following: (1) One or more activities, not described in (2) through (4) below, for which no insured receives more than $2,000 in total compensation for the 12 months before the beginning of the policy period; (2) Volunteer activities for which no money is received other than payment for expenses incurred to perform the activity; (3) Providing home day case services for which no compensation is received, other than the mutual exchange of such services; or (4) The rendering of home day care services to a relative of an insured.

B. Results of Unclear Coverage Vignettes

Table A14 reports the results of providing the relevant policy language to the treatment group for each of the unclear coverage vignettes described above. Unlike in the results reported in the main body of the paper, these results define an accurate response to be one in which the respondent recognized uncertainty by selecting any response other than "definitely covered" or "definitely not covered."

Table A14. Accurate Perceptions of Insurance Coverage for Unclear Coverage Vignettes

Coverage Context	No Policy Language (Control Group)			Policy Language (Treatment Group)			Difference
	N	*Accurate*	*%*	*N*	*Accurate*	*%*	*PP*
Earthquake	323	219	67.8	472	232	49.2	-18.6
Slip-and-Fall	313	234	74.8	487	370	76.0	1.2

Note: Percentages denote the proportion of the respective survey subsample that accurately predicted policy coverage. We defined accurate responses in unclear coverage scenarios as those that recognized uncertainty in the likely coverage outcome for a typical homeowners insurance policy by choosing responses other than "definitely covered" and "definitely not covered."

As reflected in Table A14, in one of the two vignettes, relative to the control group, a *smaller* proportion of the treatment group (who received relevant policy language) accurately evaluated scenarios in which the homeowners insurance coverage was potentially unenforceable (in the earthquake scenario). In the second vignette, involving the slip-and-fall event, the proportions accurately assessing coverage were virtually the same. In Table A15, we report the results of our tests for the significance of these differences after accounting for observable differences using a series of logistic regression analyses.

Table A15. Logit Regression Analyses of Respondents' Accuracy of Coverage Assessments for Unclear Vignettes

	Earthquake Damage	Slip-and-Fall Liability
	(1)	(2)
Received policy language	0.457**	1.122
	(0.070)	(0.202)
Baseline odds	5.034**	2.223
	(1.933)	(0.925)
No. of observations	795	799
Pseudo R-squared	0.0484	0.0700

Note: The table reports results from logit regressions in which the dichotomous outcome variable was equal to one if the respondent's understanding of insurance coverage was accurate and equal to zero otherwise. Estimates are shown as odds ratios (i.e., exponentiated coefficients of the logistic regression). Heteroskedasticity-robust standard errors are reported in parentheses. The reported pseudo R-squared is McFadden's R-squared. The analyses controlled for experiential, demographic, and economic factors. Experiential controls were indicators for selling insurance, being the final decision-maker on a homeowners insurance contract, reporting they have read and understand their own policy, having switched carriers, and having prior negative views of insurers. Demographic controls were gender, age, race/ethnicity, marital status, and region. Economic controls were education, employment, and income. The symbols * and ** represent significance at the 5% and 1% levels, respectively.

As reflected in Table A15, the relationship between receiving policy language and accurately assessing coverage in the earthquake unclear coverage vignette persisted even after controlling for potentially confounding factors; as indicated by the odds ratio values less than one, the relationship was negative and statistically significant (at the 1% level). For the slip-and-fall unclear coverage vignette, the evidence of a positive relationship between receiving policy language and ability to identify ambiguity was not statistically significant.

There are various ways to interpret these results depending on how one conceptualizes the tested coverage vignettes. For the earthquake vignette, to the extent that courts apply the policy language we gave respondents as written, then the correct answer to the vignette would be "definitely not covered," which is strongly consistent with how respondents themselves interpreted this language (67% of the treatment group predicted that the loss would not be covered). This impact of the policy language is perhaps not surprising given that the provided policy language points uniformly in the direction of non-coverage. On the other hand, a number of courts/jurisdictions refuse to enforce this policy language in part because they view it as inconsistent with consumers' reasonable expectations of coverage. Although respondents provided with this policy language generally did not have an expectation of coverage, a majority of the respondents who assessed the coverage in this vignette in the absence of policy language did in fact expect coverage (52% of the control group predicted that the loss would be covered).

In the slip-and-fall unclear coverage vignette, contrary to our hypothesis, respondents provided with the relevant policy language were no more likely to recognize the ambiguity of the coverage question than respondents who did not receive the policy language. Instead, the percentage of respondents who believed there was a definitive answer to the coverage question was relatively the same in both groups. One possible explanation for this result is that the vignette itself was sufficiently suggestive of an ambiguity such that respondents intuited the conclusion independently of the policy language. But it is nonetheless striking that respondents who received policy language that in our view clearly confirmed this intuition were no more likely to report this conclusion than the control group.

APPENDIX D. IMPACT OF RECEIVING DIFFERENT POLICY LANGUAGE FOR THE SAME VIGNETTE

In addition to testing whether respondents' understanding of a typical homeowners insurance policy would be impacted if they saw excerpts from the ISO HO3 policy—the "standard form" on which most private insurers base their actual homeowners policies[174]—we also tested

[174] Although the ISO HO3 policy is the presumptive standard contract for homeowners insurance policies in the United States, individual insurers in recent years have increasingly

specific homeowners policy language that departed from the ISO HO3 policy. To do so, we focused on the electrical fire coverage vignette, which is described in Part II. Recall that the terms of the ISO HO3 policy unambiguously covered the damage described in the vignettes we used in Section II.B. Even so, providing respondents with the relevant policy language significantly decreased the accuracy of respondents' coverage assessments, increasing the percentage of respondents who wrongly believed that the loss would not be covered.

To test how variation in policy language might impact these results, we split our treatment sample for this coverage vignette into three groups: Treatment Group A received ISO HO3 policy language, Treatment Group B received Farmers Insurance Company policy language, and Treatment Group C received State Farm Insurance Company policy language.

As described more fully in Figure A3, the Farmers Insurance Company policy language purports to exclude coverage whenever a loss is caused "directly or indirectly" by a "failure to undertake any maintenance." A plain meaning application of this language to the electrical fire vignette would result in a denial of coverage. However, we are deeply uncertain whether most courts would enforce the Farmers policy language, which is, in our view, extremely overly broad and highly atypical of homeowners policy language. In fact, case law from decades ago generally refused to enforce similar, though less obviously overly broad, language in insurance policies that purported to deny coverage due to an increase of risk for the policyholder.[175] Although doctrines like the "reasonable expectations doctrine," which purport to provide courts with the authority to disregard the plain meaning of policy language that is inconsistent with policyholder expectations, have fallen out of favor in recent years, we suspect many courts would find an attempt by Farmers to enforce its outrageously overly broad policy language in a case like the electrical fire vignette to warrant a limited invocation of this doctrine.

The second atypical policy language was extracted from a State Farm policy that purports to exclude coverage when an insured neglects to use all reasonable means to prevent a loss "when property is endangered." Although the State Farm policy language is less favorable to the insured

opted to depart from the ISO terms in their homeowners policies. See Schwarcz, supra note 27, at 1342.

[175] See id. at 1283–84.

than the ISO language, it is also not nearly as overly broad as the Farmers policy language. We therefore think that many courts would enforce this exclusion in cases where it plainly applies. The difficulty presented by this language in the context of the electrical fire vignette, however, is that it produces clearly ambiguous results in our view. In particular, it is not at all clear whether the homeowner's property was "endangered" when the electrical switch described in the vignette started sparking and the homeowner decided not to use the associated room. Once again, we view this as a genuine ambiguity whose resolution would turn on additional relevant facts not specified in the vignette, as well as the applicability of doctrines such as *contra proferentem*.

Figure A3. Instruction and Policy Language Distributed to Treatment Groups B and C in the Electrical Fire Unclear Coverage Vignette

Coverage Scenario	Control Group (No policy language)	Treatment Group (Non-ISO Policy Language)
Unclear Coverage Scenario: An electrical switch in your home's guest room starts to spark when you turn on the light. Instead of repairing the switch, you simply decide not to use the room. Two months later, however, you forget about the malfunctioning light switch, go into the room, and flip the switch on. The resulting sparks trigger a fire that burns down your home.	Respondents instructed to answer coverage scenario based on their existing understanding of what a typical homeowners policy would cover, without looking at any specific insurance policy language or conducting any research.	**Farmers Insurance Policy:** We insure against direct physical loss to covered property. We do not insure for loss excluded under the Exclusions Section. **Excluded Causes of Loss or Damage** Except as expressly provided elsewhere in this policy, we do not a. insure property covered by this policy; b. provide Loss of Use coverage; or c. provide coverage in any Extensions of Coverage; for loss or damage which directly or indirectly is caused by, arises out of, or results from any of the excluded causes of loss or damage listed below, whether the loss or damage occurs on or away from the **residence premises**. Acts or omissions of persons or other causes or other events can cause, contribute to, combine with or aggravate any of the excluded causes of loss or damage to cause loss or damage. Loss or damage caused by an excluded cause of loss or damage is not covered regardless of any acts, omissions or decisions of any persons, group, organization, association or governmental body or of any other causes or other events which aggravate or contribute concurrently or in any combination or sequence with the excluded cause of loss or damage. If covered and excluded causes of loss or damage each cause loss or damage to property such that the resulting damage is indistinguishable except as to the timing or sequence of the causes of the damage, then none of the loss or damage is insured by this policy. Excluded Causes of Loss or Damage are excluded whether they are, or are the result of, natural or man-made activities, conditions or events. Excluded Causes of Loss or Damage apply to exclude the loss or damage arising from or as a result of the excluded activity, condition or event, whether the loss or damage is direct or indirect or immediate or consequential. **1. Neglect or Lack of Maintenance or Failure to Make Repairs** is an Excluded Cause of Loss or Damage. Lack of maintenance includes a failure to undertake any maintenance.
		State Farm Insurance Policy: We insure against direct physical loss to covered property. We do not insure for loss excluded under the Exclusions Section. **SECTION I -- LOSSES NOT INSURED** We do not insure for any loss to the property which consists of, or is directly and immediately caused by, one or more of the perils listed below, regardless of whether the loss occurs suddenly or gradually, involves isolated or widespread damage, arises from natural or external forces, or occurs as a result of any combination of these: **1. Neglect**, meaning neglect of the insured to use all reasonable means to save and preserve property at and after the time of a loss, or when property is endangered.

Table A16. Perceptions of Insurance Coverage for Electrical Fire Unclear Coverage Vignettes

Answers	No Policy Language (Control Group)		ISO HO3 Policy Language (Treatment Group A)		Farmers Policy Language (Treatment Group B)		State Farm Policy Language (Treatment Group C)	
	N	*%*	*N*	*%*	*N*	*%*	*N*	*%*
Definitely not covered	65	6.7	113	23.6	172	34.6	146	29.8
Probably not covered	165	16.9	145	30.3	151	30.4	142	29
Could go either way	228	23.4	122	25.5	86	17.3	109	22.2
Probably covered	334	34.3	62	12.9	56	11.3	68	13.9
Definitely covered	182	18.7	37	7.7	32	6.4	25	5.1
Total	974	100	479	100	497	100	490	100
Total Predicting Non-Coverage	230	23.6	258	53.9	323	65	288	58.8
Total Predicting Coverage	516	53	99	20.7	88	17.7	93	19

Note: Answers predicting likely non-coverage include "Definitely not covered" and "Probably not covered"; answers predicting likely coverage include "Probably covered" and "Definitely covered."

Table A16 depicts the response patterns to the question asking whether respondents believed the relevant policy—i.e., the "typical" policy for the control group, the ISO HO3 policy for Treatment Group A, the Farmers policy for Treatment Group B, and the State Farm policy for Treatment Group C—would cover the electrical fire vignette described in Figure A3.

Despite the clear differences in coverage across the language in the three policies, respondents in all three treatment groups largely reacted to receiving policy language in the same way. Respondents who received any policy language were more likely than their counterparts in the control group to predict that the policy does not cover the damages described in the vignette. Consistent with our test of the potentially unenforceable anti-concurrent causation language, the potentially unenforceable Farmers policy language prompted the largest proportion of respondents to determine that the described loss was definitely not covered, as well as a general shift away from pro-coverage assessments and assessments acknowledging ambiguity and toward anti-coverage assessments. As one might expect, respondents appeared not to recognize the prospect that the supplied language might be unenforceable.