

Not ready for the bench: LLM legal interpretation is unstable and out of step with human judgments

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Abstract

Legal interpretation frequently involves assessing how a legal text, as understood by an ‘ordinary’ speaker of the language, applies to the set of facts characterizing a legal dispute in the U.S. judicial system. Recent scholarship has proposed that legal practitioners add large language models (LLMs) to their interpretive toolkit. This work offers an empirical argument against LLM interpretation as recently practiced by legal scholars and federal judges. Our investigation in English shows that models do not provide stable interpretive judgments: varying the question format can lead the model to wildly different conclusions. Moreover, the models show weak to moderate correlation with human judgment, with large variance across model and question variant, suggesting that it is dangerous to give much credence to the conclusions produced by generative AI. We publicly release our code to facilitate future work.¹

1 Introduction

Legal decisions often come down to the interpretation of written text (e.g., in a statute or contract). Often, legal interpretation is straightforward. At times, however, the text is imprecise or ambiguous, leading to disputes about how to apply a written provision to a particular set of circumstances. In the U.S. legal system, judges often place considerable weight on what they deem the ‘ordinary meaning’ (meaning as would be understood by ordinary speakers of American English) of a legal term (Slocum, 2015).² One recent case (*Snell*

John is a contractor with insurance that covers property loss, damage, or personal injury claims that arise due to his ‘landscaping’ work.

John is employed by a family, the Smiths, to install an in-ground trampoline in the family’s backyard. A few years after John completes the project, the Smiths successfully sue John for injuries that their daughter sustained while playing on the trampoline. John files a claim with his insurance company to recover losses incurred from the lawsuit.

Considering just how “landscaping” would be understood by ordinary speakers of English, is John covered by the insurance—yes or no?

Figure 1: A legal interpretation scenario represented as QA task with binary questions. The example is based on the case *Snell v. United Specialty Insurance Co.* and constructed in the style of our task.

v. United Specialty Insurance Co.) required interpretation of the term ‘landscaping’ concerning an in-ground trampoline (see Figure 1). A second case (*U.S. v. Deleon*) turned on the interpretation of the phrase ‘physically restrained’ and whether the phrase describes indirectly restricting movement by threatening someone with a gun.

How can a judge ascertain ‘ordinary meaning’ of a legal text? Often, the judge will deploy armchair intuition buttressed by hypotheticals and dictionaries (Krishnakumar, 2024). On occasion, judges reference corpora (Solan and Gales, 2017; Gries and Slocum, 2017) and surveys (Tobia, 2024).

Enter LLMs. The advent of LLMs has produced considerable excitement in the legal field, and textual interpretation is no exception. Hoffman and Arbel (2024) opined that LLMs are good resources for ascertaining ordinary meaning. Federal judicial opinions (Newsom, 2024a,b) have included ‘direct queries’ to LLMs on the ordinary meaning

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¹<https://github.com/bwaldon/llms-legal-interp>

²In this paper, we are officially agnostic as to whether ‘ordinary meaning’ is a useful legal analytical construct (or even a coherent linguistic concept). Our focus is on whether LLMs are useful for pursuing interpretative methodologies that are purportedly grounded in ‘ordinary meaning.’

of ‘landscaping’ and ‘physically restrained’³ on the grounds that these models supposedly capture and reflect patterns of ordinary language use.

These developments have been met with critical scholarship assessing the benefits, risks, and impact of these technologies from the standpoint of both legal theory and practice. [Waldon et al. \(2025\)](#) and [Choi \(2025\)](#) demonstrated that LLMs’ interpretive judgments are highly sensitive to how queries about the meaning are phrased and suggest that LLMs are not reliable tools for legal interpretation; however, the studies were limited in scope, testing only a few models and interpretive questions.⁴

In this work, we investigate the stability of LLM-based legal interpretation methods, with a focus on LLMs’ ability to reflect ‘ordinary meaning’ as understood by human native speakers. In light of recent research (§2), our hypotheses are as follows:

1. LLM judgments are highly sensitive to subtle manipulations in how ordinary meaning queries are posed to the model.
2. LLM judgments are poorly correlated to human judgment.

To test our hypotheses, we create queries based on a previously developed set of 138 scenarios that assess linguistic interpretation in a variety of hypothetical insurance contract disputes ([Waldon et al., 2023](#)), and query 14 open-weight models (and one closed-weight model) across 9 systematic question variants (§3). In line with our hypotheses, we find in §4 that LLM judgments are inconsistent across model family and size, as well as across subtle choices in question phrasing. The outputs of some instruction-tuned models and some model scales are correlated to human judgment only in some question variants; moreover, neither decoded tokens nor probability distributions offer a reliable source of human-like ‘ordinary meaning’ judgment.

Though LLMs possess undeniably fluent generation capabilities, our results add to a growing chorus of skepticism about LLMs as tools for legal interpretation (§5).

³In *Snell* and *Deleon*, Judge Kevin Newsom posed the two following direct queries, respectively: “*What is the ordinary meaning of landscaping?*” and “*What is the ordinary meaning of physically restrained?*”. See [Waldon et al. \(2025\)](#) for a detailed discussion of the direct query method.

⁴[Waldon et al. \(2025\)](#) utilized two scenarios, and [Choi \(2025\)](#) utilized five.

2 Background

In the current era of generative AI enthusiasm, some scholars have expressed optimism that LLMs will revolutionize legal interpretation. Multiple variations of this idea have been put forward.

One argument is that LLMs need not be perfectly accurate, but as long as they are ‘good enough’, they will produce interpretive outcomes that are more consistent and more affordable to the masses than the legal status quo ([Hoffman and Arbel, 2024](#), writing about contract interpretation). This, of course, presumes that LLM judgments are consistent. [Hoffman and Arbel](#) offer a small-scale case study of robustness with 5 scenarios and 20 machine-generated paraphrases of each prompt. This study was expanded by [Choi \(2025\)](#), who generated 2000 paraphrases for each of the 5 scenarios, and found models to be unreliable with prompt variation, in ways that differ across models, training methods, and output processing methods. While this result is suggestive, we will take the study of prompt variation a step further, with more rigorous testing that covers more scenarios and uses tightly controlled prompt variants (instead of LLM paraphrasing) to ensure meaning preservation.

A second line of reasoning holds that for difficult interpretive problems, for investigating ordinary meaning in English, LLMs are potentially *better* than humans because they have been trained on so much ordinary English data (we can call this the ‘omniscience’ argument). Judge Kevin Newsom in his opinion in the *Snell* case gives voice to this idea, citing the diversity of ordinary usage that these models might draw upon: “models train on a mind-bogglingly enormous amount of raw data . . . as I understand LLM design, those data run the gamut from the highest-minded to the lowest, from Hemmingway [sic] novels and Ph.D. dissertations to gossip rags and comment threads. Because they cast their nets so widely, LLMs can provide useful statistical predictions about how, in the main, ordinary people ordinarily use words and phrases in ordinary life” ([Newsom, 2024a](#)). He appears to tone down the omniscience argument in a subsequent case, saying LLMs “may well serve a valuable auxiliary role as we aim to triangulate ordinary meaning,” complementing “traditional interpretive tools [such as] dictionaries” ([Newsom, 2024b](#)).

Is it plausible that a careful judge, armed with established tools of ordinary meaning analysis and a bevy of law clerks, would arrive at a better-

informed result because an LLM was consulted? Newsom’s rationale is predicated on certain assumptions about LLMs that have been called into question (Waldon et al., 2025). In particular, his comment about the vast training data behind LLM chatbots suggests he believes systems are capable of conducting *metalinguistic reasoning* about the language in their training data in order to synthesize an interpretive conclusion. But recent analyses have put in doubt the proposition that LLMs are capable of deep metalinguistic reflection (Behzad et al., 2023; Thrush et al., 2024; Cheng and Amiri, 2025). Instead, it seems likely that LLMs are good at imitating or summarizing metalinguistic text in the training data—be it dictionary definitions, textbooks, or online language forum discussions (Behzad et al., 2023; Waldon et al., 2025). Thus, to the extent that an LLM produces plausible-sounding responses to an interpretive prompt, it likely draws on what humans say about language (Almeman et al., 2024), rather than what they do as language users. Still, if LLMs could accurately synthesize ordinary people’s opinions about meaning, then they might be a reliable and cost-effective tool for ordinary meaning analysis, as hypothesized by Hoffman and Arbel (2024).

It is crucial, then, to test whether LLMs’ interpretive judgments are reliably correlated to human interpretations. By some measures, LLM probability models exhibit human-like sensitivity to grammatical phenomena in English sentences: these include linguistic or syntactic acceptability (Gauthier et al., 2020; Warstadt et al., 2019, 2020), and semantic plausibility (Kauf et al., 2023, 2024). However, there is increasing behavioral evidence of differences between LLMs and humans when it comes to learning and processing language (Oh et al., 2024; Aoyama and Wilcox, 2025; McCoy and Griffiths, 2025) and answering questions (Srikanth et al., 2025). To test whether LLMs and humans arrive at similar interpretive conclusions, we use the human judgments from Waldon et al.’s (2023) to evaluate LLMs’ interpretive judgments.

3 Legal Interpretation with LLMs

Consider the landscaping example in Figure 1. A term within the contractor’s insurance, ‘landscaping,’ must be interpreted to determine whether the insurance covers the described scenario. The task explicitly demands a judgment as to how ordinary speakers would understand the contract language

in context. This judgment may not cohere with one’s beliefs regarding the ‘correct’ interpretation of the provision, or regarding how a judge will actually resolve the legal dispute at hand. Rather, our investigation is perspectivist in that we are not assessing model behavior against a single, fixed ‘ground truth’ linguistic interpretation or judicial outcome (Frenda et al., 2025). Instead, our assessment metrics assume that ‘ordinary interpretation’ is both highly varied and to some extent subjective. Our first metric—robustness to variation—evaluates the stability of model judgments across multiple prompt formulations. Our second metric—human correlation (see §4.3)—evaluates the extent to which model judgments cohere with the intuitions of a relevant human population.

In the remainder of this section, we describe a study designed to investigate LLMs’ legal interpretation capabilities as assessed against these two metrics.

Vague Contracts. Our study adapts materials originally developed by Waldon et al. (2023) for a human study of legal interpretation and consists of 138 items based on real-world insurance contracts. Table 1 provides an example item. Each item names a category of insurance coverage (e.g., Wind Damage) and provides a definition of that category. The item then describes a policyholder’s loss, which may or may not be covered by the named category.

Waldon et al. (2023) analyzed human responses from 1,338 U.S.-based native English speakers recruited via Prolific. The participants were shown a vignette with the scenario and insurance text, and were asked to answer questions. The first of the three questions elicited interpretive judgments, **Yes**, **No**, or **CAN’T DECIDE**. A sample vignette (Figure 4) can be found in §A.

Interpretation as binary QA. Each query ends with a cue designed to induce single-token judgment that addresses the binary question. To measure model response, we examine first-token continuation probabilities. Since the first token probabilities may not be fully representative of model judgment, as highlighted by Wang et al. (2024), we review output text (decoded with temperature set to zero) to check for such inconsistency between the first token probabilities and the model’s intended judgment. Additionally, we consider several types of **yes** and **no** tokens across casing as indicators of coverage judgment, and operationalize their re-

Query element	Example (‘Vehicle Damage’)
Insurance text	Steve’s car insurance policy includes coverage for “Vehicle Damage,” defined as “loss or damage to the policy holder’s 1) car; or 2) car accessories (while in or on the car)”
Scenario	One day, Steve is involved in a minor accident. His GPS navigation system, which was in the car at the time, was damaged. Steve files a claim with his insurance company for the damage.
Framing for ‘ordinary meaning’	Considering just how “accessory” would be understood by ordinary speakers of English,
Question	is Ken covered by the insurance—yes or no?
Cue	Final answer is:

Table 1: Elements of the interpretive queries. ‘Insurance text’ and ‘Event’ are sourced from the study item, and the ‘Framing’ and ‘Question’ are added to solicit interpretive judgment. The ‘Cue’ was found with exploratory tests to induce high rate of first token judgment.

spective sums as judgment probabilities, which we further describe in §E.1.

Formulation of prompt variants. To investigate whether LLMs’ interpretive judgments are robust to minor changes in prompt design, we constructed a template of 9 question variants and applied that template to each of the 138 items. An example paradigm is presented in Table 2.

Some variants reflect LLM behavior that is already attested to be challenging and unlike humans. [García-Ferrero et al. \(2023\)](#) and [Truong et al. \(2023\)](#) show, for example, that LLMs find natural language negation words challenging and lack a deep understanding of the phenomenon.

Moreover, [Sharma et al. \(2024\)](#) and [Hong et al. \(2025\)](#) demonstrate that in some contexts, LLM outputs are modulated by prompts that overtly solicit agreement (e.g., *Do you agree that...?*). Our study builds upon these previous findings in a novel domain of legal interpretation; we also investigate variants that, to our knowledge, remain understudied (e.g., overt solicitation of disagreement) or that combine multiple previously-studied phenomena (e.g., negation plus agreement solicitation).

For most variants, an affirmative **yes** response would correspond to the **COVERED** judgment, but in some variations (e.g., the Negation variant in Table 2), the **COVERED** judgment would be expressed with a **no** token. For readability, we report and discuss probabilities corresponding to **COVERED** and **NOTCOVERED** judgments, rather than discussing **yes** and **no** token probabilities.

Models. We collect judgments from base and instruction-tuned open-weight LLMs of varying size up to 70B parameters and GPT-4.⁵ They include GPT-2 ([Radford et al., 2019](#)), Llama ([Grattafiori et al., 2024](#)), OLMo ([Groeneveld](#)

[et al., 2024](#)), Mistral ([Karamcheti et al., 2021](#)), and Gemma ([Team et al., 2024](#)). The full list is in Table 3, and implementation details §E. Given our evaluation data’s release in 2023 we note that data contamination is a possibility, which we discuss in §B.

4 Results and Analysis

From these models, we collect both categorical (**COVERED** or **NOTCOVERED**) and distributional (probabilistic) judgments over tokens that represent the **COVERED** judgment (**yes**, **Yes**, **YES** for Yes/No variant), tokens that represent the **NOTCOVERED** judgment (**no**, **No**, **NO** for Yes/No variant), and the residual other tokens. We use these metrics to analyze how robust the models are with respect to model family, size, and question variation. Additionally, we examine correlation (or lack thereof) between human and LLM judgment.

4.1 Analysis of judgments for Yes/No prompts

We begin by focusing on what is arguably the most basic question variant: Yes/No. The categorical and distributional for all models with the Yes/No question variant are reported in Table 4.

Some models can be stopped clocks. We observe that many models repeat the same categorical judgment no matter the scenario, or are highly biased towards one judgment: 6 of the 15 models tested provide the same response in all question variant conditions for at least 127 (≈92%) of the 138 tested scenarios. In these cases, it is doubtful that the model judgment reflects substantive engagement with the provided prompt. These models, rather, are only as useful as the proverbial stopped clock that correctly tells the time twice a day.

We also observe that models allocate judgments over different probability ranges from each other and instruction tuning has inconsistent effects,

⁵This is a closed weight model, whose judgment was obtained via API. See §E.2 for details.

Variant	Question
	Considering just how the word ‘landscaping’ would be understood by ordinary speakers of English,
Yes/No	is John covered by the insurance—yes or no?
No/Yes	is John covered by the insurance—no or yes?
Negation	is John not covered by the insurance—yes or no? Final answer is:
Agreement	do you agree with the statement, “John is covered by the insurance”—yes or no?
AgrWithNeg	do you agree with the following statement: “John is not covered by the insurance”— yes or no?
Disagreement	do you disagree with the following statement: John is covered by the insurance”— yes or no?
DisagrWithNeg	do you disagree with the following statement: John is not covered by the insurance”— yes or no?
Options	is John covered by the insurance? Options: A. John is covered. B. John is not covered.
OptionsFlipped	is John covered by the insurance? Options: A. John is not covered. B. John is covered.

Table 2: Systematic variation of the question (Table 1) in the interpretive queries. Further discussion of these variants is available in §D.

Family	Models
Llama-3	1B, 1B-Inst (3.2) 3B, 3B-Inst (3.2) 8B, 8B-Inst (3.1) 70B (3.1), 70B-Inst (3.3)
GPT	GPT-2-medium, GPT-4 ⁵
OLMo-2	7B, 7B-Inst
Minstral	8B-Inst
Gemma	7b, 7b-it

Table 3: We query a total of 15 models across 5 families. None are considered reasoning or thinking models.

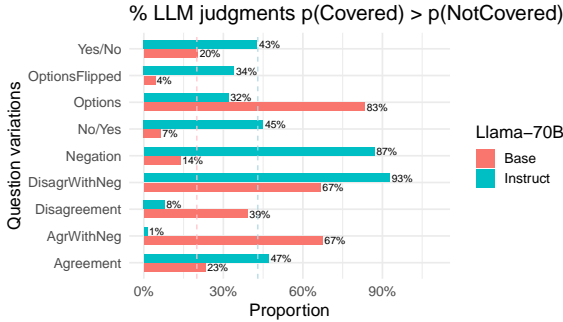


Figure 2: Llama-70B model responses across question variants, each of which results in a large shift away from values in either directions given the Yes/No variant, indicated with the dotted lines.

which also vary by the models and sizes. Detailed discussion is available in §C.

4.2 Robustness to question variation

For a model to be considered reliable at answering interpretive questions, it should be robust to variation in how the question is phrased. As described in §3, we measure model responses while varying the phrasing of the question in the prompt and leaving the content unchanged, and analyze how the variation affects the models’ categorical and distributional judgments. We report three major findings in this section of our study.

Given the nine question variants, for each item and model, one of the categorical judgments

Model	Categorical Counts		Distributional Spread	
	Cov.	NOTCov.	Min	Max
Llama-70B	28	110	0.21	0.48
+Inst	59	79	0.02	0.85
Llama-8B	80	58	0.14	0.37
+Inst	0	138	0.11	0.74
Llama-3B	127	11	0.09	0.52
+Inst	53	85	0.16	0.69
Llama-1B	138	0	0.06	0.29
+Inst	138	0	0.15	0.59
OLMo-2-7B	70	68	0.19	0.56
+Inst	53	85	0.00	0.99
Minstral-8B-Inst	75	63	0.21	0.59
gemma-7b	39	99	0.19	0.49
+it	131	7	0.00	1.00
GPT-4	77	61	0.00	1.00
GPT-2-medium	5	133	0.13	0.31
Human Majority	84	54		

Table 4: Count and probability range for each model’s COVERED and NOTCOVERED judgments in response to Yes/No questions. Both distribution and the effect of instruction tuning vary significantly across models. The human majority is mentioned for reference.

(COVERED, NOTCOVERED) will be the majority judgment and the other the minority judgment. The strength of the majority can vary—from 5 of 9 variants (an indication of brittleness) to 9 of 9 variants (unanimity, indicating robustness). A frequency table for majority judgments collated by items for each model is shown in Table 5.

Ubiquitous inconsistency across question variants. We observe inconsistency across the 9 question variants in each LLM we study. As illustrated in Table 5, in 2,061 of 2,070 model responses (138 scenarios for each of the 15 models), both judgments can be found across the question variants with each model; only in 9 item-model combinations is the judgment fully consistent across question variants. Model judgments are sensitive even to simple variations like reversing the order of the provided answer choices, as seen in Fig-

Majority Count Model	5	6	7	8	9
Frequency for count					
Llama-70B	33	68	36	1	0
+Inst	25	33	78	2	0
Llama-8B	40	52	46	0	0
+Inst	6	39	59	31	3
Llama-3B	95	40	3	0	0
+Inst	75	48	15	0	0
Llama-1B	12	57	69	0	0
+Inst	129	9	0	0	0
Ministral-8B-Inst	24	65	30	18	1
OLMo-2-7B	46	65	20	7	0
+Inst	51	57	30	0	0
gemma-7b	44	58	29	7	0
+it	9	31	79	19	0
GPT-4	4	9	57	63	5
GPT-2-medium	50	83	5	0	0

Table 5: Number of items by number of question variants that yielded the majority judgment for the model. For example, there were 33 items for which Llama-70B produced one judgment for 5 variants, and the other judgment for 4 variants. Each judgment is a binary choice between COVERED and NOTCOVERED.

ure 2 for Llama-70B models, where the swap from Agreement to AgrWithNegation produces a 64% absolute shift in the base model, and a 46% absolute shift in the instruction-tuned model, with respect to the rate of categorical COVERED judgments. This ubiquity of inconsistency exposes a generalization failure on the part of the model. Worse, it invites users invested in a particular outcome to engage in “prompt shopping,” varying the prompt until the desired response is produced (Waldon et al., 2025).

Some questions are more likely to elicit minority judgments. Four variants Disagreement, AgreementWithNegation, DisagrWithNegation and Options elicit $\approx 67\%$ of minority responses. Even in the absence of these four variants leads to unanimity only in $\approx 33\%$ of cases, although up from less than 1% of model responses and reducing the predominance of inconsistency. We provide additional details on question variants that induce minority judgments in §D.1, and details of their effect on distributional judgments in §D.2.

The lack of robustness supports our first hypothesis, and should be considered a limitation of LLMs for use in legal interpretation, since such ubiquitous inconsistency shows models are brittle when used for legal interpretation.

4.3 Correlation to human judgment

Finally, we compare LLMs’ responses and distributional judgments to human judgment data from

Waldon et al. (2023) to test our hypothesis that model responses are poorly correlated to human judgments. In Waldon et al.’s (2023) study, each human participant was asked for a response of **yes**, **no**, or **cantdecide** (where **yes** and **no** correspond to COVERED and NOTCOVERED judgments, respectively).⁶

The dataset contains 138 items, and a total of 4,140 judgments from 1460 participants, for an average of 30 judgments per scenario.

For our correlation analysis, we operationalize human judgment as a rate—the %COVERED of all human responses on the same item. We operationalize model judgment as a difference in model probabilities: $\Delta = p(\text{COVERED}) - p(\text{NOTCOVERED})$. We consider this linear linking hypothesis, which accounts for the probability judgments for both COVERED and NOTCOVERED with the decision boundary at 0. With this hypothesis (further discussed in §F), a perfectly correlated model would represent human judgment with $\%COVERED = 0.5\Delta + 0.5$.

Only some LLMs are moderately correlated to human judgment some of the time. Our results show that only larger instruction-tuned models’ judgment show some correlation to human judgment for some specific question variants. This is despite evidence for scaling—models do show better fit to human data and stronger performance in predicting human consensus as the number of parameters (and likely the amount of training data) increases. In particular, only instruction-tuned models with 70 billion parameters or more report an R^2 value greater than 0.5, as shown in Table 6. Correlation between Llama-70B-Inst response and human judgment across question variants is illustrated in Figure 3, where one variant yields a negative correlation ($m < 0$) with a R^2 value of 0.18. The significant variation in R^2 values across question variants suggests that the models’ responses are not, in general, representative of ordinary meaning or human-like judgment, but rather prompt design and question form. This highlights a second limitation to LLMs in legal interpretation, despite achieving up to moderate correlation to human judgment in select model-question variant pairs.

Furthermore, even when there is correlation between token probabilities and human judgment,

⁶Participants were also asked to rate the confidence of their response on a Likert scale from 0 to 4 (can be seen in Figure 4). We do not scale human judgments with the confidence response.

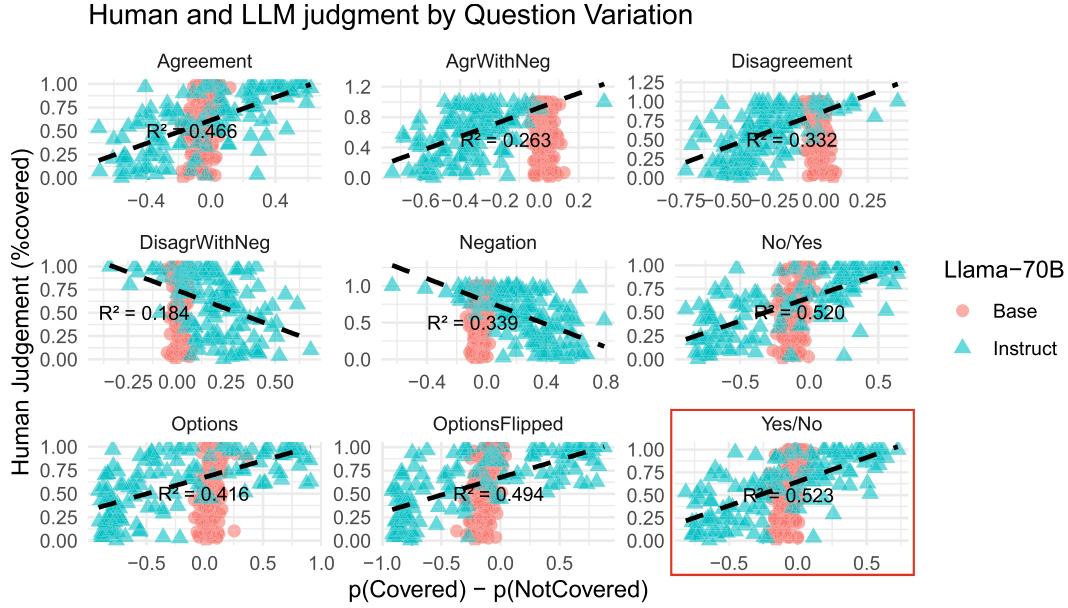


Figure 3: Llama-70B model probabilities versus human consensus across question variants. Dotted lines and the corresponding R^2 are best best-fit lines between human and instruction-tuned LLM. The Yes/No question variant, highlighted in red, represents the highest R^2 value in our study.

the output tokens can be overwhelmingly biased. Llama-70B-Inst’s responses to the AgrWithNeg variant shown in Figure 3 illustrates that despite a (weak) correlation of $R^2 = 0.26$ when examining gradations of probabilities, the actual responses are overwhelmingly **NOTCOVERED**, where $\Delta < 0$, further raising caution to LLMs’ use for legal interpretation.

Even the best-correlated LLMs are unreliable predictors of human judgment. As another measure of correlation, we consider an oracle (threshold) classifier that predicts the binary human judgment as a function of the LLM’s probability-difference Δ . The threshold is set based on the human data, where the best-fit line crosses 0.5 in human judgment. Given a probability difference value exceeding the threshold, the classifier predicts the **COVERED** judgment; otherwise, it predicts **NOTCOVERED**. Intuitively, this tells us how accurate the LLM would be at giving probabilities consistent with the human majority judgment, if only we knew how to interpret the model-specific probability scale. Binary classification accuracies appear in the last column of Table 6.

The model-variant pair best correlated to human judgment, GPT-4 responses to Y/N ($R^2 = 0.60$), yields a linear model that accurately predicts human consensus 83% of the time. Even if this may initially appear satisfactory, we contend that even 1 in 6 binary classification error rate in an inter-

pretative tool as considered in Hoffman and Arbel (2024); Newsom (2024a,b) defeats its purpose. For comparison, a majority-class classifier always voting **COVERED** would be correct 61% of the time.

On some question variants, model responses are sometimes negatively correlated to human judgment when the question includes negation, which suggests that LLMs’ attested difficulties with negation (García-Ferrero et al., 2023; Varshney et al., 2025) persist in legal interpretation. We suspect this may be attributable to LLMs providing answers to a different question than was asked and discuss the phenomenon in §D.3.

On the most optimistic interpretation of the results—that the largest models with certain prompt types achieve nontrivial correlation, which might be improved with further engineering to the point of reliability—we underscore two additional points of caution: (a) we have not ruled out the possibility of data contamination (§B), and (b) our experimental data is specific to insurance contract scenarios. Thus, there is a long road ahead for any efforts to develop trustworthy interpretive LLMs for practical use.

5 Related Work

Legal AI, legal language, and LLM robustness are popular areas of research. Nonetheless, there is limited scholarly work on the utility of LLMs for novel interpretation questions in the law, and most

Model	Variant	R^2	Acc.	Thresh.
Llama-70B	Options F.	0.25	0.69	-0.17
+Inst	Y/N	0.52	0.79	-0.28
Llama-8B	Agr.	0.04	0.59	-0.02
+Inst	Options	0.20	0.67	-0.36
Llama-3B	Disagr.	0.03	0.40	0.11
+Inst	Y/N	0.06	0.59	-0.25
Llama-1B	N/Y	0.01	0.38	0.13
+Inst	Neg.	0.01	0.38	0.34
Minstral-8B-Inst	Y/N	0.27	0.69	-0.11
OLMo-7B	N/Y	0.07	0.64	-0.10
+Inst	Y/N	0.18	0.64	-0.63
gemma-7b	Y/N	0.09	0.59	-0.10
+it	Neg.	0.06	0.60	-0.16
GPT-4	Y/N	0.60	0.83	-0.25
GPT-2-medium	Options	0.01	0.62	-0.19
Majority class baseline			0.61	

Table 6: For each model, we report the question variant with the highest correlation to human judgments along with the R^2 . We also report the accuracy and threshold of an oracle classifier predicting the human majority judgment from the LLM probability difference Δ . We offer the always-COVERED accuracy as a majority class baseline.

of it is based on conceptual arguments and case studies rather than controlled experimentation.

LLM synopsis of legal interpretations. Luo et al. (2025) instruct LLMs to consider a target legal term (say ‘landscaping’) and generate, based on legal documents from previous cases, an interpretive explanation (along with conditions governing the applicability of the interpretation). This task concerns the extraction of a previously articulated interpretation, rather than novel conclusions about meaning. They find the system performance comparable to that of human experts for the task, but do not evaluate models for robustness across prompts.

Instability of LLM legal judgments. Blair-Stanek and Van Durme (2025) investigate LLM instability in the context of a legal judgment prediction task, where the models are prompted with a synopsis of the case and asked to predict which party should prevail. This task demands a full accounting of all the issues in a particular case, rather than a focused interpretive question. There is no indication that the cases were selected with emphasis on language interpretation. Investigating closed commercial models with a reasoning step, they find significant instability, i.e., the answer will not be consistent even within the same model and prompt.

LLMs as arbiters of ordinary meaning. Our study asks whether LLMs can reliably answer novel questions about ordinary meaning. Several voices of legal scholarship have expressed skepti-

cism. Lee and Egbert (2024) contrast LLMs with legal corpus linguistics, arguing that LLM interpretations are less replicable, transparent, and generalizable. Waldon et al. (2025) argue that the way LLMs are designed makes them vulnerable to misinterpretation and misuse. In a similar vein, Grimmelmann et al. (2025) argue that LLMs are unproven for legal interpretation due to a *reliability* gap (LLMs are not always consistent and reproducible), and an *epistemic* gap (interpretive conclusions in text produced by the model are not necessarily accurate measurements of ordinary meaning as understood by humans). Our robustness and correlation evaluations are ways of quantifying the respective gaps.

In the existing literature, experiential claims about LLM behavior have been supported with ad hoc case studies—with one exception: Choi (2025) conducted a set of experiments to test LLMs’ sensitivity to prompt variation. While our goal and conclusions are similar to Choi’s, there are important methodological differences: (i) Choi utilizes 5 contract scenarios from Hoffman and Arbel (2024) while we use 138 insurance policy scenarios (from Waldon et al., 2023); and (ii) we perform controlled, systematic variation of the questions for our studies rather than LLM-generated synthetic variations à la Choi. Our similar conclusion, based on entirely different methodology and non-synthetic data, underscores the need for caution when contemplating applying LLMs to legal interpretation.

6 Conclusion

Given the excitement about and increasing critical legal scholarship on the use of LLMs for legal interpretation, we conducted a systematic study of this capability with a focus on direct LLM judgments regarding the ‘ordinary meaning’ of legal language, formulated as binary-choice QA. We examined the LLM judgments for both robustness to prompt variation and correlation to human judgments, finding:

- Some LLMs can be stopped clocks, with a strong tendency to provide the same judgment for most input, regardless of the scenario.
- Models show a ubiquitous lack of consistency across question variants.
- Correlation with human judgment is at best moderate, and is strongest in larger, instruction-tuned models.

Our work complements a growing body of legal scholarship on LLMs for legal interpretation, adding to growing skepticism about LLMs as in-

interpreters of ordinary meaning. Our experimental coverage of LLMs is, of course, not exhaustive; perhaps there is a model or approach whose suitability for this task could be demonstrated in rigorous experiments. But the evidence thus far spectacularly fails to meet the burden of proof.

Limitations

Legal interpretation and ‘ordinary meaning’ are complex topics of theory and practice in the legal field. Our work only looks at a specific aspect of LLM usage, posing direct queries for ascertaining ordinary meaning with a binary-choice QA task. This does not represent other mechanisms of using LMs for legal interpretation, such as producing arguments for and against an interpretation (Waldon et al., 2025), or eliciting examples (Almeman et al., 2024).

We use data from a previous study of consensus in legal interpretation. The authors of that study make no claims as to the overall representativeness of their experimental stimuli to questions that come up in day-to-day contractual interpretation. They also explicitly discuss the role of researcher subjectivity in the constructing the stimuli. We do not report any representativeness or coverage information for the data. Hence, despite the diversity of scenarios compared to other related works, it is currently unclear how representative it is of LLM use for legal interpretation in practice.

Our evaluation utilizes a small sample of 138 unique items. This is a small dataset for testing the generalization of language model judgments for the task.

We attempt to use question variants in a controlled manner to investigate how such variation affects judgment. However, the interplay between question variants and LLM judgment may not be easily disambiguated. LLMs’ ability to understand and follow task instructions cannot be guaranteed, especially without targeted post-training (Tamkin et al., 2023).

Our models are not chosen based on their attested performance on natural language benchmarks. Additionally, none of the models evaluated are considered “reasoning” or “thinking” models.

While we attempt to include models with varying size, architecture, and training recipe, we do not present our findings to be comprehensive or conclusive. Larger and more recent models with ‘reasoning’, ‘retrieval’ and/or search methods may be able

to provide stable, reliable sources of human-like legal interpretation. We also do not use “chain-of-thought” or other prompting or in-context learning methods meant to elicit or induce intermediate or reasoning steps. These have been shown to improve model performance in many tasks. Hence, our results may represent a lower estimation of model ability.

We use first token probability as the model response; this has advantages both in computing resources and analysis, but provides a limited representation of model output (Hu and Levy, 2023). Additionally, the first token probability has some known drawbacks (Wang et al., 2024), especially for instruction-tuned models, which we take additional steps to mitigate. Alternatively, non-instruction-tuned models are less likely to provide appropriate responses to our QA formulation.

Due to the complexity of responding to negation and interpreting it from the first token, we check the text to ascertain the correct polarity (COVERED or NOTCOVERED) to use for the judgments. However, this does not guarantee that all scenarios get captured under the polarity judgment. For distributional judgment, we collate the probabilities to only three categories, in which the tail of the probability distribution is reduced and represented as ‘Other’. This approximation reduces the precision and validity of the distance metric. Our correlation analysis uses a specific transformation of model responses and compares it to the COVERED proportion. We did not perform targeted validation for establishing our linking hypothesis in connecting human responses and LLM responses for legal interpretation.

We did not perform checks for data contamination (Sainz et al., 2023) in the language models we have used. Data contamination of the published materials and reference policies used in the previous study (Waldon et al., 2023) could have influenced the models we studied. We catalog the cutoff and release dates in §B.

Ethical considerations

Data sourcing. We create task data and utilize anonymized crowd-sourced human survey data from a previous work (Waldon et al., 2023) which was made available publicly. We did not collect any new human judgment data.

Highlighting scope for abuse. We highlight drawbacks of using LLMs to obtain legal interpreta-

tion and report the effects of variation across input on LLM judgment. However, outside of ‘prompt shopping’ which is a risk of LLM use we discuss, we do not propose or provide any adversarial methods that can be adapted to abuse and exploit LLMs.

Responsible research statement

We utilized ChatGPT (OpenAI et al., 2024), GitHub Copilot, and AI Assistant in Pycharm during the implementation of our experiments.

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A Additional details on vague contracts

Waldon et al. (2023) collected human judgments for interpretive questions about insurance contract vignettes. Here we detail the structure and topics of these vignettes. Figure 4 illustrates the kind of text seen by human subjects in their study:

Steve’s car insurance policy includes coverage for “Vehicle Damage,” defined as “loss or damage to the policy holder’s 1) car; or 2) car accessories (while in or on the car).”

One day, Steve is involved in a minor accident. His GPS navigation system, which was in the car at the time, was damaged. Steve files a claim with his insurance company for the damage.

1. Do you think that the claim is covered under Vehicle Damage as it appears in the policy? [YES / NO / CAN’T DECIDE]
2. You are one of 100 people who have volunteered to answer these questions. How many of the 100 do you think will agree with your answer to question (1)?
3. How confident are you in your answer to question (1)? [(Not at all / Slightly / Moderately / Very / Totally) confident]

Figure 4: An example vignette from the questionnaire provided to the participants by Waldon et al. (2023). The vignette corresponds to one of the 138 items. Since our study focuses on interpretative judgment, question 1 is of interest to us, and responses make up the human judgments used in correlation analysis in §4.3.

Each vignette consists of a hypothetical contract provision with a term and definition, coupled with a hypothetical scenario meant to test the interpretation of that definition (Table 1). The provisions are drawn from a range of insurance types (Table 7). The term at issue, or *locus of uncertainty*, depends on the scenario; the full list of these terms appears in Table 8. We note that these vignettes were artificially constructed, but are meant to imitate real-world insurance contract scenarios.

B Data contamination

Since our source data is from 2023, it is possible that it was part of the training for one or more

Insurance Types
Emergency Damages
Escape of Oil
Escape of Water
Fire
Flooding
Garden Plants
General Damages
Ground Heave
Hail Damage
Hot Work
House Removal
Identity Theft
Loss and Accidental Damage
Loss or Damage to a Goods Carrying Vehicle
Malicious Acts or Vandalism
Personal Accident
Personal Accidents
Public Liability Property Damages
Storm Damage
Trace and Access
Vehicle Damage
Vehicle Fire
Vehicle Glass
Vehicle Theft
Wind Damage

Table 7: The unique insurance types from the vague contracts data. These refer to insurance types that were used to design the various definitions (Insurance text in Table 1). Each such insurance text has exactly three scenarios with the same locus of uncertainty.

models in our selected suite of LLMs, especially the larger models (which show the best correlation).

Table 9 catalogs the known cutoff dates for for the models. Any model with the exception of GPT-2 may have been partially or wholly exposed to the dataset.

C Additional analysis of judgments to Yes/No

Different distributions for different models.

We observe that different models allocate their distributional judgments in different ranges. For example, GPT-4 allocates probabilities through the full range (0.00,0.99], while Ministral-8B-Inst allocates it in a narrower range (0.19,0.58]. However, these variations across models do not represent the quality of models’ interpretive judgments. Rather than basing analysis on the absolute values of the probabilities, we look at the distributions separately for each model to determine whether a given model provides a useful signal for interpretation.

Instruction tuning allows a wider range of judgment probabilities while introducing unpredictable bias. Across the board, instruction-

Loci of Uncertainty
accessory
accidental
audio equipment
broken glass
causative “from”
cause
connected with business
connected with occupation
custody or control
damage
deliberate
family or employee
fire damage
first responder
flammable or combustible materials
flow of water
glass
ground heave
hard surface
heating installation
internal source
key theft
leaking
malicious people or vandals
naturally occurring fire
necessary and reasonable
outside the building
perceived emergency
permanent or total loss
political disturbance
professional movers
rapid build-up
reasonable steps
regular working conditions
requires / uses / produces
sudden/unforeseen
temporarily removed
third party
tracking device
traveling in
wear and tear

Table 8: Loci of uncertainty in contract provisions. (Note that these are descriptions in the dataset; the actual text prompts in the Waldon et al.’s (2023) study did not contain metalinguistic terms like *causative*.)

tuned models utilize a wider range of judgment probabilities than their base counterparts, with instruction-tuned OLMo, gemma, and GPT-4 models utilizing the entire space in [0, 1] as shown in Table 4. However, the other changes introduced by instruction tuning are less consistent– the magnitude and direction of change in both categorical and distributional judgment varies by model and question variant. For example, instruction tuning on Llama-8B leads to a predominance of **NOT COVERED** judgments, while instruction tuning of Llama-70B leads to predominance of **COVERED** judgment, when prompted using the Yes/No question variant. However, Figure 3 shows that instruc-

Model	Reported Cutoff Date	Data available before cutoff?
GPT-4	Dec 1, 2023	Yes
GPT-2	Unknown (Released November 2019)	No
Llama-3	December 2023	Yes
OLMo-2	December 2023	Yes
Minstral-8B	Unknown (Released October 2024)	Maybe
Gemma-3	August 2024	Yes

Table 9: Reported cutoff date for training data or if unknown, model release date for each model. It is possible that our source evaluation data was included in the training dataset for 14 out of 15 models we use for our study.

tion tuning the 70B model yields more **COVERED** judgment with the Disagreement question variant, and more **NOTCOVERED** judgment with the Negation question variant.

D Question variants

Our question variants in Table 2 contain styles and phenomena we expect to be common. These include ordering of choices (No/Yes, OptionsFlipped), negation, and asking for agreement or disagreement. Two variants, AgrWithNeg and DisagWithNeg, contain multiple such phenomena, compounding the challenge. These are also the variants that are predominant in minority judgments.

D.1 Minority Judgments

Some question variants are more likely to induce minority judgment, disagreeing with the majority of judgments induced by the same model given the same query. As shown in Table 10, we find that the Disagreement variant yields the minority judgments most frequently, while the Yes/No variant yields it the least frequently.

Variant	Count	Proportion
Disagr.	1256	0.21
Agr. w/ Neg.	1045	0.17
Disagr. w/ Neg.	918	0.15
Options	809	0.14
Agr.	501	0.08
Options F.	493	0.08
Negation	489	0.08
N/Y	275	0.05
Y/N	188	0.03
Total	4975	

Table 10: The number of minority judgments for each question variant, and the percentage proportion in minority judgments. The counts are sorted vertically in descending order. An equal proportion would lead to a 0.09 proportion for each variant.

Majority Count Model	3	4	5
Frequency for count			
Llama-3-70B	22	21	95
+Inst	24	93	21
Llama-3-8B	33	43	62
+Inst	13	98	27
Llama-3-3B	11	113	14
+Inst	57	48	33
Llama-3-1B	4	25	109
+Inst	13	125	0
Minstral-8B-Inst	16	67	55
OLMo-2-7B	71	26	41
OLMo-2-7B-Inst	36	77	25
Gemma-7b	45	50	43
+it	9	47	82
GPT-2-medium	67	71	0
GPT-4	5	41	92
All	426	945	699

Table 11: Number of items by number of question variants that yielded the majority judgment for the model. For example, there were 5 items for which GPT-4 produced one judgment for 3 variants, and the opposite judgment for 2 variants. Each judgment is a binary choice between **COVERED** and **NOTCOVERED**. This is a replication of Table 5 but without the four most minority response inducing variants Disagreement, AgreementWithNegation, DisagrWithNegation and Options in the mix Table 10.

D.2 Some question variants have a stronger distributional effect than others.

To further quantify the effect of question variation, we operationalize the effects of each question variant as a distribution distance between that judgment for the default Yes/No question and the question variants. We measure distribution distance with the Jensen-Shannon distance metric (JSD; [Österreicher and Vajda, 2003](#)), which is based on KL divergence ([Kullback and Leibler, 1951](#)), but is symmetric and provides a distance between 0.0 (identical) and 1.0 (maximally different). The question variant that yields the most distant distribution for each model are listed in Table 12, where aver-

Model	Variant	Mean	Std
Llama-3-70B	Options	0.09	0.04
+Inst	Negation	0.34	0.17
Llama-3-8B	Options F.	0.15	0.04
+Inst	Disagr. w/ Neg.	0.18	0.06
Llama-3-3B	Agr. w/ Neg.	0.19	0.06
+Inst	Options F.	0.22	0.07
Llama-3-1B	Options	0.28	0.05
+Inst	Options F.	0.32	0.04
OLMo-2-7B	Disagr. w/ Neg.	0.37	0.05
+Inst	Agr. w/ Neg.	0.48	0.23
Ministral-8B-Inst	Options	0.16	0.04
Gemma-7B	Disagr. w/ Neg.	0.08	0.03
+it	Options	0.78	0.04
GPT-2-medium	Disagr. w/ Neg.	0.17	0.02
GPT-4	Disagr. w/ Neg.	0.56	0.30

Table 12: The question variant for each model with the largest Jensen-Shannon distance from the Yes/No question. Higher distance indicates greater difference between distributions.

age distance is obtained by calculating JSD at the item level, and then aggregated by question variant for each model. e.g. The overloaded variant Disagreement with negation most frequently (5 of the 15 models) leads to the biggest change in the distributional judgments in the most models, including up to 0.56 for GPT-4. These metrics also show similar pattern as the minority responses, however provide a probably based numerical overview.

D.3 Responses in cases of negation

We noted a mismatch between the expected polarity of the answer, and the greedily decoded text output for many models when it involved negation. The models tended to provide judgment on the positive coverage statement rather than the negated statement. However, our polarity indication at the variant level can be inconsistent with polarity by specific items and models.

E Implementation and Compute

E.1 Load-and-infer pipeline.

We use `vllm` (Kwon et al., 2023) to implement our inference pipeline and use the model implementations available on <https://huggingface.co/models>.

All our inference was completed on Tesla L4 GPUs with 24GB of memory, with the exception of Llama-70B, which were run on 4xA100, each with 40GB of memory. Our inference configuration will be available as part of our public code.

Judgment as a sum of probability. In §3, we consider LLM judgment as a sum of token probabilities that correspond to each judgment. That is,

when answering **yes** to the question would indicate the **COVERED** judgment, we consider $p(\text{COVERED}) = p(\text{Yes}) + p(\text{yes}) + p(\text{YES})$.

Random Seed and Temperature Because a significant portion of our study works with token probabilities, we set temperature to 0, and hence there is no randomness in our inference pipeline.

E.2 OpenAI GPT-4 Inference with APIs

We used OpenAI API platform for inference with GPT-4. We use `temperature=0` to get the highest determinism. We use the GPT-4 model with the model identifier `gpt-4-0613`.

F Linking hypothesis

In §4.3, we assume that our respective operationalizations of human and LLM judgment, namely the proportion of covered judgments **%COVERED** in human responses and the probability difference between LLM covered and not covered judgments Δ , have a linear relationship. While the assumption is difficult to justify, we provide our attempt.

By definition, token probability $p(w | C) = f(C)$ in autoregressive language modeling (Radford et al., 2019) represents the proportion of cases where the next token w occurs given a model f and number of environments with context C . In our implementation, we consider this as a computational analogue of querying a human population and calculating the proportion of which that respond with one judgment, or, the proportion of human judgments in human responses. This is the basis of our assumption that judgment probability as sum of token probabilities $p(\text{COVERED}) = p(\text{Yes}) + p(\text{yes}) + p(\text{YES})$ and proportion of covered judgments have a linear relationship.

However, due to residual token probabilities that linger in LLM probability distributions, we are unable to represent LLM judgment with a single token probability, as a low $p(\text{COVERED})$ does not indicate a high $p(\text{NOTCOVERED})$. We thus take the difference of the two judgments, $\Delta = p(\text{COVERED}) - p(\text{NOTCOVERED})$ to represent LLM judgment that has a linear relationship with human majority judgment. This allows for clear analysis, as the expectation value of a judgment $E(\Delta) = 0$, and $\Delta > 0$ yields the **COVERED** judgment and $\Delta < 0$ the **NOTCOVERED** judgment.

Based on our linking assumption, consider the ideal case where our two variables are perfectly correlated to each other with $R^2 = 1$. In such case

where $\Delta \in [-1, 1]$ and $p(\text{COVERED}) \in [0, 1]$, we predict that proportion of human covered judgments is 0 when probability difference is -1 since $p(\text{COVERED}) = 0$, $p(\text{NOTCOVERED}) = 1$. It follows that proportion of human covered judgments is 1 when probability difference is 1; and 0.5 when probability difference 0. The best fit line would then have $m = 0.5$, $b = 0.5$. Here, all of the variance across human judgment is explained by Δ .

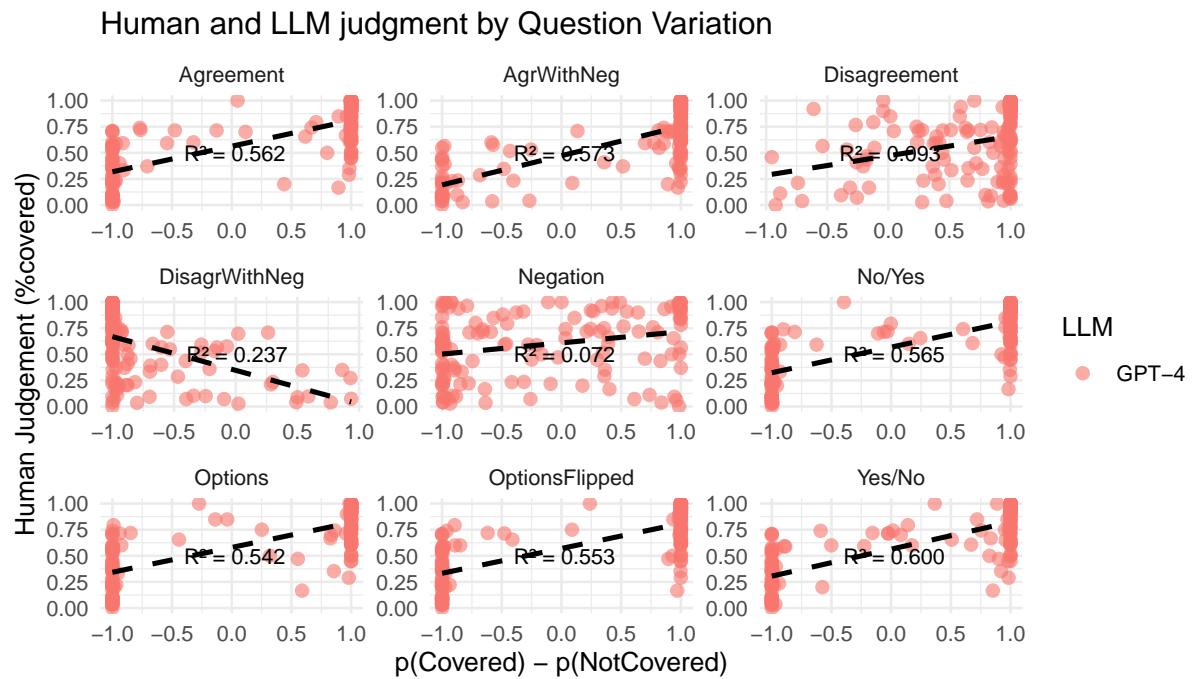


Figure 5: GPT-4 judgment probabilities versus human consensus across question variants. Dotted lines are best best-fit lines between human and instruction-tuned LLM.