Revisiting Uncertainty Quantification Evaluation in Language Models: Spurious Interactions with Response Length Bias Results

Andrea Santilli[*†] Sapienza University of Rome
Adam Goliński[*] Apple
Michael Kirchhof Apple
Federico Danieli Apple
Arno Blaas Apple
Miao Xiong[†] National University of Singapore
Luca Zappella Apple
Sinead Williamson Apple

Abstract

Uncertainty Quantification (UQ) in Language Models (LMs) is key to improving their safety and reliability. Evaluations often use metrics like AUROC to assess how well *UQ methods* (e.g., negative sequence probabilities) correlate with task *correctness functions* (e.g., ROUGE-L). We show that mutual biases-when both *UQ methods* and *correctness functions* are biased by the same factors-systematically distort evaluation. First, we formally prove that any mutual bias non-randomly skews AUROC rankings, compromising benchmark integrity. Second, we confirm this happens empirically by testing 7 widely used *correctness functions*, from lexical-based and embedding-based metrics to LM-as-a-judge approaches, across 4 datasets $\times$ 4 models $\times$ 8 *UQ methods*. Our analysis shows that length biases in *correctness functions* distort UQ assessments by interacting with length biases in *UQ methods*. We identify LM-as-a-judge methods as the least length-biased, offering a promising path for a fairer UQ evaluation.

1 Introduction

Language Models (LMs) excel at natural language generation but often produce factually incorrect outputs, or "hallucinations" (Guerreiro et al., 2023; Huang et al., 2025). These hallucinations are typically associated with high uncertainty about the correct output (Xiao and Wang, 2021), leading to the emergence of *Uncertainty Quantification (UQ) methods* as a compelling approach to detect errors (Farquhar et al., 2024; Baan et al., 2023). A fundamental challenge in evaluating *UQ methods* is the lack of ground truth uncertainty labels. Consequently, benchmarks commonly rely on *UQ performance metrics* such as AUROC, assessing how effectively *UQ methods* distinguish correct from incorrect outputs as determined by a *correctness function*. Thus, the accuracy and reliability of UQ evaluations inherently depend on the quality of the correctness assessments.

In this paper, we critically analyze how errors and biases in *correctness functions* impact *UQ performance metrics*. First, we provide a formal analysis showing that: i) if errors in the *correctness function* are random and independent from the *UQ method*, AUROC is noisy but unbiased; ii) conversely, if there exists a *mutual bias*—i.e. if the *correctness function* errors correlate systematically with the uncertainty scores—then AUROC rankings are inherently skewed. Our formal results demonstrate that **any mutual bias** introduces systematic distortions into AUROC evaluations, artificially advantaging certain methods and fundamentally undermining the reliability of benchmarks.

We confirm this happens empirically by benchmarking 7 widely-used *correctness functions*, including lexical-based metrics (e.g., ROUGE metrics (Lin, 2004)), embedding-based metrics (e.g., BERTScore (Zhang et al., 2020)), and LM-as-a-judge approaches (Zheng et al., 2024) across 4 datasets $\times$ 4 models $\times$ 8 *UQ methods*. We reveal two key issues: (i) the *correctness function* choice significantly impacts UQ results and (ii) widely used lexical-based and embedding-based *correctness functions* (Farquhar et al., 2024; Fadeeva et al., 2023) introduce systematic biases that distort the perceived effectiveness of certain *UQ methods*.

A human evaluation of 450 LM samples reveals that this bias stems, at least in part, from the mutual dependence of certain *UQ methods* and *correctness functions* on the output length. Building on this, we identify *correctness functions* that mitigate bias by avoiding such confounding, finding LM-as-a-judge approaches best suited for UQ evaluation and most aligned with human judgment. Overall, this study highlights pitfalls in UQ evaluation and charts a path toward a more reliable evaluation protocol.

[*]Equal contribution
[†]Work done during an internship at Apple.

Proceedings of the 63rd Annual Meeting of the Association for Computational Linguistics (Volume 2: Short Papers), pages 743–759
July 27 – August 1, 2025

2 Evaluating uncertainty

In this section, we review common *UQ methods*, *correctness functions* and *UQ performance metrics*.

2.1 *UQ methods*

Given an input x to an LM, which generates an output sequence $\hat{y}$, a *UQ method* estimates a measure of the model's uncertainty about $\hat{y}$, denoted as $\hat{g}(\hat{y}, x)$. These methods can be broadly categorized into three types: (i) single-sample, (ii) multi-sample, and (iii) learned. One simple single-sample approach is negative sequence probability,

$$\hat{g}(\hat{y}, x) = -\hat{p}(\hat{y}|x) = -\prod_{i=1}^{L} \hat{p}(\hat{y}_i|\hat{y}_{<i}, x), \quad (1)$$

where L is the length of the generated answer and $\hat{p}$ the output probabilities assigned by the model. Note that in Eq. 1, $\hat{g}$ increases with L. Multiple-sample approaches derive uncertainty scores by sampling multiple responses for the same input x and measuring a metric (e.g., variance) across samples. Notably, several *UQ methods* in this second group, like Naïve Entropy and Semantic Entropy (Farquhar et al., 2024), use Eq. 1 to compute the probability of a sequence of generated tokens. Lastly, learned methods train a binary classifier via supervised learning on a correctness-labeled dataset (Kadavath et al., 2022). We refer readers to App. A for more details on each family.

2.2 *Correctness functions*

Correctness functions $\hat{h}(\hat{y}, x, y)$ compare a generated answer $\hat{y}$ to a reference answer y to estimate a correctness score, and can be categorized as lexical-based, embedding-based, or LM-as-a-judge.

Lexical-based *correctness functions*, such as SQuAD (Rajpurkar et al., 2016) and ROUGE variants (Lin, 2004), are based on lexical overlap between $\hat{y}$ and y. While limitations of these metrics have been studied in areas like summarization and Question Answering (QA) (Guo and Vosoughi, 2023; Chen et al., 2019; Cohan and Goharian, 2016; Fabbri et al., 2021; Reiter and Belz, 2009), their impact on UQ evaluation remains largely unexplored.

Embedding-based *correctness functions*, such as BERTScore (Zhang et al., 2020) and Sentence-BERT cosine similarity (Reimers and Gurevych, 2019), assess similarity by encoding both $\hat{y}$ and y using a language model, typically BERT-based.

Correctness function	Used in UQ eval protocol	Threshold t
ROUGE-1 (F1)	Aichberger et al. (2025)	$0.1 - 1.0$
ROUGE-L (F1)	Fadeeva et al. (2023); Kuhn et al. (2023)	0.5
	Duan et al. (2024); Chen et al. (2024a)	0.5
	Qiu and Miikkulainen (2024)	$0.1 - 1.0$
	Aichberger et al. (2025)	$0.1 - 1.0$
SQuAD (F1)	Farquhar et al. (2024)	0.3
BERTScore (F1)	Fadeeva et al. (2023)	N/A
SentenceBERT	Chen et al. (2024a)	0.9
AlignScore	Vashurin et al. (2025)	0.5
LM-as-a-judge (Prompt)	Farquhar et al. (2024)	N/A

Table 1: *Correctness functions* used in UQ evals.

LM-as-a-judge *correctness functions* evaluate correctness by using another LM to judge the accuracy of $\hat{y}$ against y. Examples include AlignScore (Zha et al., 2023), which uses a specifically trained LM, and prompt-based variants of LM-as-a-judge (Zheng et al., 2024).

Table 1 summarizes common *correctness functions* used in recent UQ papers. AUROC requires binary labels, so a certain threshold t is typically applied to binarize continuous correctness scores. Some *correctness functions* are inherently binary (e.g., LM-as-a-judge), and some *UQ performance metrics* do not require binarization (Fadeeva et al., 2023). This variety in UQ eval protocols raises questions about which combination to trust. App. B offers a broader view on each metric.

2.3 *UQ performance metrics*

The utility of *UQ methods* is typically assessed using a *UQ performance metric* that quantifies how well uncertainty estimates (§2.1) correlate with correctness. Among the various *UQ performance metrics* available in the literature (Malinin and Gales, 2020; Fadeeva et al., 2023), we focus on the Area Under the Receiver Operating Characteristic curve (AUROC) due to its widespread use in UQ benchmarks (Farquhar et al., 2024; Chen et al., 2024a).

Let $\hat{g}_i \equiv \hat{g}(\hat{y}_i, x_i)$ be the uncertainty score assigned by some *UQ method* to the i-th data sample, and let h_i be a binary label denoting (ground truth) correctness of that data sample ($h_i = 1$ if correct, $h_i = 0$ if incorrect). AUROC can be written as

$$\text{AUROC} = P\left(\hat{g}_i < \hat{g}_j \mid h_i = 1,\ h_j = 0\right), \quad (2)$$

i.e., the probability that a randomly chosen *correct* data sample receives a lower uncertainty score than a randomly chosen *incorrect* data sample.

2.4 Mutual biases in *UQ performance metric*

In practice, we estimate h_i using a *correctness function* $\hat{h}_i \equiv \hat{h}(\hat{y}_i, x_i, y_i)$ from §2.2. This means that

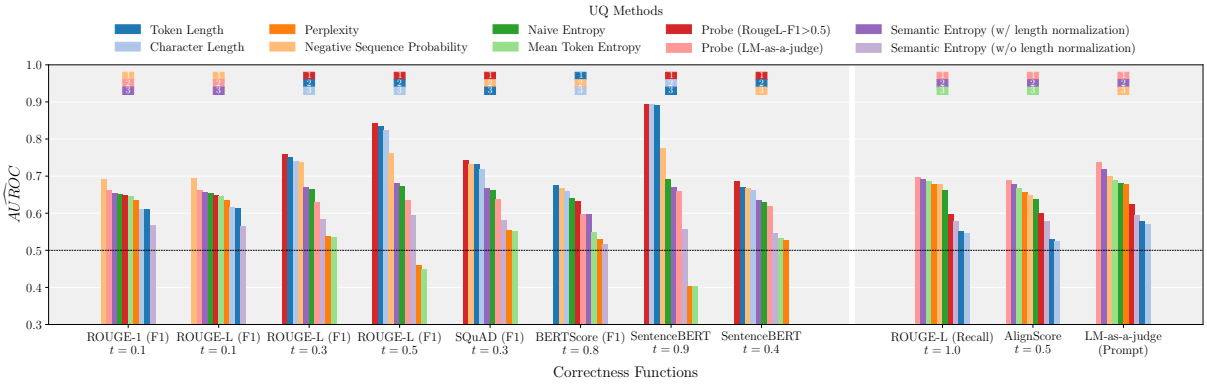


Figure 1: $\widehat{AUROC}$ of various *UQ methods* across *correctness functions* averaged over models and datasets. The ranking of *UQ methods* (top row) changes across *correctness functions*, raising questions about which one to trust.

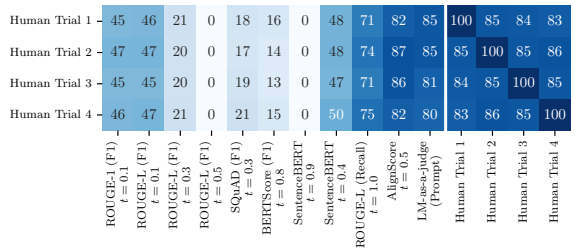


Figure 2: Cohen Kappa agreement rates between annotators and *correctness functions*. Per dataset: Fig. 6.

AUROC is not computed on ground-truth labels, but rather on a potentially biased surrogate:

$$\widehat{\text{AUROC}} = P\left(\hat{g}_i < \hat{g}_j \mid \hat{h}_i = 1,\ \hat{h}_j = 0\right). \quad (3)$$

Eq. 3 highlights a key challenge: the measured performance is merely an *estimate* of the true AUROC and is subject to *correctness function* errors, which can interact with and distort the final outcome. Specifically, two scenarios may arise:
i) Uncorrelated errors. If errors in $\hat{h}$ are independent of $\hat{g}$, $\widehat{\text{AUROC}}$ is a noisy but *unbiased* estimator of the real AUROC. While scores, in the worst case, regress toward the 0.5 random baseline, no *UQ method* is systematically favored or penalized.
ii) Mutually biased errors. If errors in $\hat{h}$ correlate with $\hat{g}$, the estimated performance of $\hat{g}$ will be *systematically biased*. Depending on the direction of correlation, some *UQ methods* may appear more or less effective than they truly are—leading to inflated or deflated evaluations and ultimately compromising the validity of performance comparisons. These two scenarios are formally characterized and analyzed in App. C, which provides a theoretical foundation for understanding how *correctness function* errors propagate into *UQ performance metrics*.

In practice, in §3 we find that both *UQ methods* and *correctness functions* can exhibit biases over the answer length L, falling into scenario (ii). Understanding the impact of these biases is crucial to ensuring fair and reliable comparisons in UQ.

3 Experiments

In this section, we evaluate several *UQ methods* following evaluation protocols in line with previous related works (Lin et al., 2024; Fadeeva et al., 2023; Farquhar et al., 2024) while varying just the *correctness function*. We consider generative QA tasks, as they are standard in UQ literature and their single-answer format simplifies correctness evaluation relative to more open-ended tasks like summarization.

Experimental setup. We evaluate the performance of 8 *UQ methods* across 4 datasets, 4 models, and 7 *correctness functions*. For details, see: App. A on *UQ methods*; App. B on *correctness functions*; App. D on models, datasets and prompts.

3.1 Impact of the *correctness function* on UQ

Fig. 1 illustrates the estimated performance of different *UQ methods* when varying only the *correctness function*. For each method, we report average $\widehat{\text{AUROC}}$ across datasets and models, focusing on the most commonly used *correctness functions* from Table 1. Fig. 1 reveals that changing the *correctness function* affects not only the estimated AUROC value but also the ranking of *UQ methods*. This, however, raises the question of which metric to trust and what is causing the disagreement.

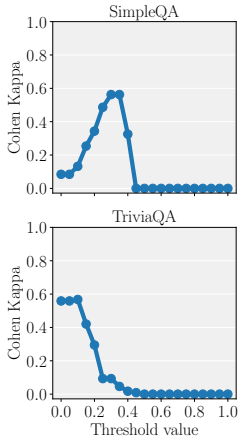


Figure 3: Cohen Kappa score w.r.t. human annotators for Rouge-L (F1) against thresholds.

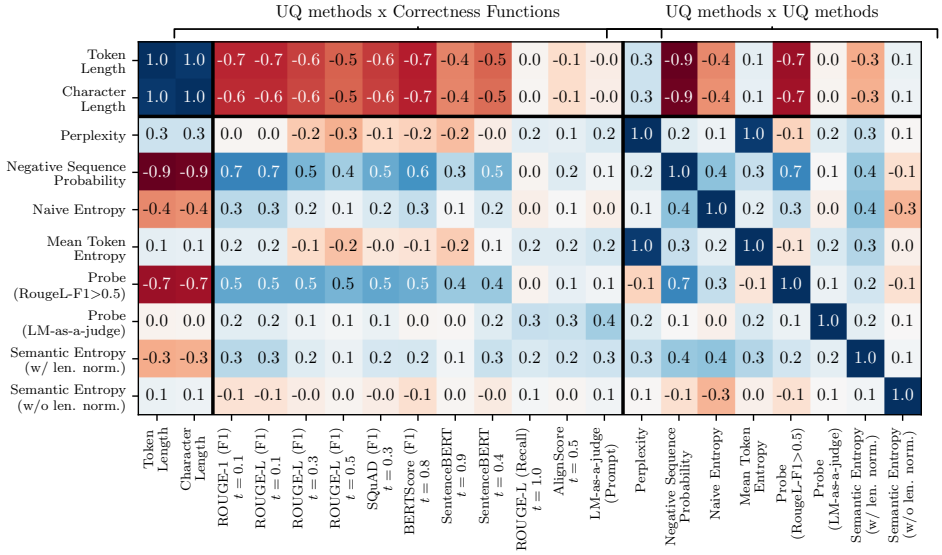


Figure 4: Spearman's rank correlation coefficients.

3.2 Evaluating *correctness functions* for UQ

The previous section shows how *correctness functions* choices impact benchmarking conclusions, but it is unclear which function yields reliable UQ results. To investigate, we evaluate several *correctness functions* against human annotations (four annotators per sample for 450 samples, see App. E). The Cohen's Kappa (Cohen, 1960; Artstein and Poesio, 2008) values in Fig. 2[1] show that LM-as-a-judge approaches (prompt-based and AlignScore) align best with human labelers, followed by ROUGE-L (Recall) with $t = 1$.

Revisiting Fig. 1, we see that these three *correctness functions* show more stable orderings, with some variability in AUROC magnitudes—consistent with expectations for small errors that are mostly uncorrelated with *UQ methods*, as per case (i) in §2.4. Conversely, most previously-used lexical- and embedding-based *correctness functions* poorly reflect human judgment.

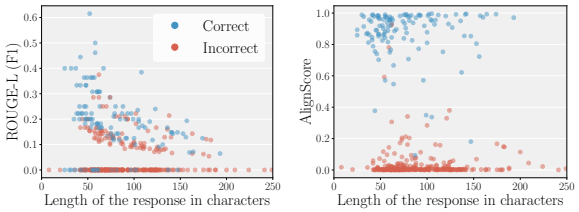
Impact of threshold choice. A major source of error in lexical- and embedding-based *correctness functions* stems from the thresholding strategy used to binarize scores for AUROC computation. As shown in Table 1, prior work often applies standard thresholds or experiments with a small set of options. However, Figs. 2 and 3 illustrate that metrics like ROUGE-L (F1) and SentenceBERT are highly sensitive to threshold choices, as assessed by the resulting agreement with humans. Poor thresholding can lead to degenerate outcomes—e.g., assigning nearly identical labels to all predictions—which drastically reduces alignment with human annotators. The issue is further exacerbated by the fact that optimal thresholds vary across tasks (Figs. 3 and 8) and are heavily influenced by response verbosity (Fig. 5), making it challenging to select a single effective threshold. In contrast, metrics such as ROUGE-L Recall, AlignScore, and LM-as-a-judge exhibit considerably less sensitivity to threshold selection, as shown in Fig. 7 of the Appendix.

[1]Some approaches show no agreement at all due to poor thresholding choices.

3.3 Mutual bias in *correctness functions* and *UQ methods*

We concluded that LM-as-a-judge approaches achieve higher agreement with humans than other *correctness functions*. However, this alone does not explain the shift in *UQ method* rankings observed in §3.1. If errors were random, no systematic effect would emerge, falling into case (i) of our error analysis (§2.4). However, this is not the case: the relative performance of negative sequence probability, perplexity, and probes varies dramatically. This is indicative of a spurious correlation between *UQ methods* and errors in the *correctness functions*.

Many *UQ methods* are biased by length. Many *UQ methods* explicitly or implicitly depend on the length of a response. In particular, negative sequence probability assigns higher uncertainty to longer responses, as each term in Eq. 1 is < 1. Other *UQ methods* that incorporate Eq. 1 in their computation (§2.1), such as Naïve Entropy and



(a) ROUGE-L (F1) (b) AlignScore

Figure 5: *Correctness function* vs response length. The color indicates human correctness judgments. Results for other *correctness functions* in the Appendix, Fig. 7.

Semantic Entropy, may also be impacted. To investigate this relationship, we compute Spearman correlation between the scores from various *UQ methods* and the length of generated answers (measured in tokens and characters). In Fig. 4, we see that multiple estimators exhibit significant positive or negative correlations with length.

Many *correctness functions* are biased by length Many *correctness functions* are also known to exhibit length bias when assessing summaries (Guo and Vosoughi, 2023). We demonstrate that this issue also affects QA. In Fig. 5, we analyze the relationship between response length and *correctness function* output, showing correctness values for responses where all annotators agree on the label. The ROUGE-L (F1) score is highly dependent on response length, favoring shorter sentences and making threshold selection challenging. In contrast, AlignScore is length-independent and clearly separates correct and incorrect samples. App. F presents similar findings for other *correctness functions*.

Spurious interaction. Mutual correlation between *UQ methods* and *correctness functions* on answer length can systematically inflate or deflate *UQ performance metrics* (App. C). This effect is evident in Fig. 1, where length-based baselines (token and character length—blue bars) perform competitively on lexical- and embedding-based *correctness functions* but rank last under LM-as-a-judge metrics. This may also explain discrepancies in prior works, such as the inflated ranking of negative sequence probability in Fadeeva et al. (2023). We recommend using LM-as-a-judge where possible, as its lower error is less likely to impact *UQ performance metrics*. While ROUGE-L (Recall) is inherently independent of the generated answer's length, it offers lower correlation with human judgment, leading to noisier AUROC estimates and offering a higher likelihood of additional confounding variables. For example, it is vulnerable to exploitation by models that produce multiple off-target answers alongside the correct one.

4 Beyond Length Bias

In this paper, we argue that **any biases**—not just length—present **simultaneously** in both the *UQ method* and the *correctness function* induce a spurious correlation that systematically biases the *UQ performance metric* (AUROC). Crucially, this effect does not merely introduce random noise (i.e., increased variance) to the AUROC estimate; rather, it leads to consistent bias, producing misleading results that can artificially favor certain *UQ methods* over others. We support this claim through both analytical derivation (App. C) and empirical evidence (Fig. 1). Importantly, this is a general result. While we use length bias as a running example—because it is the most severe and already impacting benchmarks—the underlying issue extends to **any confounding variable that correlates with both *UQ methodscorrectness functions***. Identifying such confounders, which are less obvious than length, is inherently difficult, which makes their presence particularly dangerous for evaluation protocols. For instance, in settings that combine LM-as-a-judge approaches with verbalized uncertainty methods (e.g., Huang et al. (2024); Band et al. (2024); Yang et al. (2024)), one might hypothesize less obvious and harder-to-detect sources of confounding like vocabulary used or writing style of the response (Feuer et al., 2025). Our goal is not to enumerate all possible biases but to establish the existence of a broader class of systematic evaluation failures—of which length bias is a concrete and empirically validated case. Identifying and mitigating other such biases remains an important direction for future work.

5 Conclusion

We prove that *UQ performance metrics* are systematically biased when the *UQ method* and *correctness function* share a confounder. Empirically, we identify response length as a concrete instance of such mutual bias which is affecting existing benchmarks and undermines their reliability.

Our results highlight that lexical- and embedding-based *correctness functions*, commonly used in prior work, frequently introduce these distortions. In contrast, LM-as-a-judge

approaches exhibit greater robustness and stronger alignment with human judgments, making them a more reliable choice for UQ evaluation. That said, we recommend validating any LM-as-a-judge setup against human annotations before applying it to new tasks or datasets (Bavaresco et al., 2024).

Limitations

In this work, we critically examine the role of the *correctness function* in the evaluation of *UQ methods* using *UQ performance metrics*. While our analysis sheds light on biases introduced by *correctness functions*, certain limitations remain.

Our analysis is focused on the context of QA, as it is a standard task in UQ literature and provides well-defined single-answer questions, making the definition of a *correctness function* easier compared to open-ended tasks like machine translation and summarization where even objective human judgment of correctness is difficult. However, previous work suggests that the length bias of errors in *correctness functions* is not unique to the QA setting (Guo and Vosoughi, 2023), suggesting that *UQ performance metrics* will face similar issues in such tasks.

Our recommendation is to use LM-as-a-judge as a potential *correctness function*. While using another LM to judge correctness has demonstrated advantages (Zheng et al., 2024), it also comes with known limitations (Wang et al., 2024; Chen et al., 2024b). The reliability of the correctness assessment may vary depending on the choice of the judging LM and the prompt formulation. More concerningly, if the same LM is used as both the *correctness function* and as part of the *UQ method*, we are likely to have correlations between the LM-as-a-judge's errors and the *UQ method*, which could inflate the *UQ method*'s performance—although this is mitigated by the relatively low frequency of such errors. Additionally, while our analysis on QA is based on widely used QA datasets, we do not know whether the same LM judge and prompts would generalize effectively to other tasks and datasets. Ideally, an LM judge should be rigorously evaluated against human annotators before being employed in new tasks and datasets (Bavaresco et al., 2024). Furthermore, LM-as-a-judge introduces significant computational overhead compared to traditional *correctness functions*, making it less practical for resource-constrained applications.

Finally, our study identifies response length as a confounding factor in UQ benchmarking, but other latent variables may also influence *UQ methods* and *correctness functions* in subtle ways, as discussed in App. C. A deeper understanding of these biases is crucial for refining UQ evaluation protocols and ensuring more reliable assessments of model uncertainty.

Acknowledgements

We would like to thank Xavier Suau, Miguel Sarabia, Pau Rodríguez, and Eugene Ndiaye for their feedback on an earlier draft of this manuscript.

References

Lukas Aichberger, Kajetan Schweighofer, Mykyta Ielanskyi, and Sepp Hochreiter. 2025. Improving uncertainty estimation through semantically diverse language generation. In *International Conference on Learning Representations*.

Ron Artstein and Massimo Poesio. 2008. Survey article: Inter-coder agreement for computational linguistics. *Computational Linguistics*, 34(4):555–596.

Joris Baan, Nico Daheim, Evgenia Ilia, Dennis Ulmer, Haau-Sing Li, Raquel Fernández, Barbara Plank, Rico Sennrich, Chrysoula Zerva, and Wilker Aziz. 2023. Uncertainty in natural language generation: From theory to applications. *arXiv preprint arXiv:2307.15703*.

Lalit R. Bahl, Frederick Jelinek, and Robert L. Mercer. 1983. A maximum likelihood approach to continuous speech recognition. *IEEE Transactions on Pattern Analysis and Machine Intelligence*, PAMI-5(2):179–190.

Neil Band, Xuechen Li, Tengyu Ma, and Tatsunori Hashimoto. 2024. Linguistic calibration of long-form generations. In *International Conference on Machine Learning*.

Anna Bavaresco, Raffaella Bernardi, Leonardo Bertolazzi, Desmond Elliott, Raquel Fernández, Albert Gatt, Esam Ghaleb, Mario Giulianelli, Michael Hanna, Alexander Koller, André F. T. Martins, Philipp Mondorf, Vera Neplenbroek, Sandro Pezzelle, Barbara Plank, David Schlangen, Alessandro Suglia, Aditya K Surikuchi, Ece Takmaz, and Alberto Testoni. 2024. LLMs instead of human judges? a large scale empirical study across 20 NLP evaluation tasks. *Preprint*, arXiv:2406.18403.

Anthony Chen, Gabriel Stanovsky, Sameer Singh, and Matt Gardner. 2019. Evaluating question answering evaluation. In *2nd Workshop on Machine Reading for Question Answering*, pages 119–124.

Chao Chen, Kai Liu, Ze Chen, Yi Gu, Yue Wu, Mingyuan Tao, Zhihang Fu, and Jieping Ye. 2024a.

INSIDE: LLMs' internal states retain the power of hallucination detection. In *International Conference on Learning Representations*.

Guiming Hardy Chen, Shunian Chen, Ziche Liu, Feng Jiang, and Benyou Wang. 2024b. Humans or LLMs as the judge? a study on judgement bias. In *Empirical Methods in Natural Language Processing*.

Arman Cohan and Nazli Goharian. 2016. Revisiting summarization evaluation for scientific articles. In *International Conference on Language Resources and Evaluation*.

Jacob Cohen. 1960. A coefficient of agreement for nominal scales. *Educational and psychological measurement*, 20(1):37–46.

Jinhao Duan, Hao Cheng, Shiqi Wang, Alex Zavalny, Chenan Wang, Renjing Xu, Bhavya Kailkhura, and Kaidi Xu. 2024. Shifting attention to relevance: Towards the predictive uncertainty quantification of free-form large language models. In *Proceedings of the 62nd Annual Meeting of the Association for Computational Linguistics (Volume 1: Long Papers)*.

Alexander R Fabbri, Wojciech Kryściński, Bryan McCann, Caiming Xiong, Richard Socher, and Dragomir Radev. 2021. Summeval: Re-evaluating summarization evaluation. *Transactions of the Association for Computational Linguistics*, 9:391–409.

Ekaterina Fadeeva, Roman Vashurin, Akim Tsvigun, Artem Vazhentsev, Sergey Petrakov, Kirill Fedyanin, Daniil Vasilev, Elizaveta Goncharova, Alexander Panchenko, Maxim Panov, Timothy Baldwin, and Artem Shelmanov. 2023. LM-polygraph: Uncertainty estimation for language models. In *Empirical Methods in Natural Language Processing: System Demonstrations*.

Sebastian Farquhar, Jannik Kossen, Lorenz Kuhn, and Yarin Gal. 2024. Detecting hallucinations in large language models using semantic entropy. *Nature*, 630(8017):625–630.

Benjamin Feuer, Micah Goldblum, Teresa Datta, Sanjana Nambiar, Raz Besaleli, Samuel Dooley, Max Cembalest, and John P. Dickerson. 2025. Style outweighs substance: Failure modes of LLM judges in alignment benchmarking. *Preprint*, arXiv:2409.15268.

Marina Fomicheva, Shuo Sun, Lisa Yankovskaya, Frédéric Blain, Francisco Guzmán, Mark Fishel, Nikolaos Aletras, Vishrav Chaudhary, and Lucia Specia. 2020. Unsupervised quality estimation for neural machine translation. *Transactions of the Association for Computational Linguistics*, 8:539–555.

Nuno M Guerreiro, Duarte M Alves, Jonas Waldendorf, Barry Haddow, Alexandra Birch, Pierre Colombo, and André FT Martins. 2023. Hallucinations in large multilingual translation models. *Transactions of the Association for Computational Linguistics*, 11:1500–1517.

Xiaobo Guo and Soroush Vosoughi. 2023. Length does matter: Summary length can bias summarization metrics. In *Empirical Methods in Natural Language Processing*.

Lei Huang, Weijiang Yu, Weitao Ma, Weihong Zhong, Zhangyin Feng, Haotian Wang, Qianglong Chen, Weihua Peng, Xiaocheng Feng, Bing Qin, and Ting Liu. 2025. A survey on hallucination in large language models: Principles, taxonomy, challenges, and open questions. *ACM Trans. Inf. Syst.*, 43(2).

Yukun Huang, Yixin Liu, Raghuveer Thirukovalluru, Arman Cohan, and Bhuwan Dhingra. 2024. Calibrating long-form generations from large language models. In *Findings of the Association for Computational Linguistics: EMNLP 2024*.

Mykyta Ielanskyi, Kajetan Schweighofer, Lukas Aichberger, and Sepp Hochreiter. 2025. Addressing pitfalls in the evaluation of uncertainty estimation methods for natural language generation. *ICLR Workshop: Quantify Uncertainty and Hallucination in Foundation Models: The Next Frontier in Reliable AI*.

Albert Q Jiang, Alexandre Sablayrolles, Arthur Mensch, Chris Bamford, Devendra Singh Chaplot, Diego de las Casas, Florian Bressand, Gianna Lengyel, Guillaume Lample, Lucile Saulnier, et al. 2023. Mistral 7b. *arXiv preprint arXiv:2310.06825*.

Mandar Joshi, Eunsol Choi, Daniel Weld, and Luke Zettlemoyer. 2017. TriviaQA: A large scale distantly supervised challenge dataset for reading comprehension. In *Association for Computational Linguistics (Volume 1: Long Papers)*.

Saurav Kadavath, Tom Conerly, Amanda Askell, Tom Henighan, Dawn Drain, Ethan Perez, Nicholas Schiefer, Zac Hatfield-Dodds, Nova DasSarma, Eli Tran-Johnson, Scott Johnston, Sheer El-Showk, Andy Jones, Nelson Elhage, Tristan Hume, Anna Chen, Yuntao Bai, Sam Bowman, Stanislav Fort, Deep Ganguli, Danny Hernandez, Josh Jacobson, Jackson Kernion, Shauna Kravec, Liane Lovitt, Kamal Ndousse, Catherine Olsson, Sam Ringer, Dario Amodei, Tom Brown, Jack Clark, Nicholas Joseph, Ben Mann, Sam McCandlish, Chris Olah, and Jared Kaplan. 2022. Language models (mostly) know what they know. *arXiv preprint arXiv:2207.05221*.

Sanyam Kapoor, Nate Gruver, Manley Roberts, Katherine Collins, Arka Pal, Umang Bhatt, Adrian Weller, Samuel Dooley, Micah Goldblum, and Andrew Gordon Wilson. 2024. Large language models must be taught to know what they don't know. In *Neural Information Processing Systems*.

Lorenz Kuhn, Yarin Gal, and Sebastian Farquhar. 2023. Semantic uncertainty: Linguistic invariances for uncertainty estimation in natural language generation. *International Conference on Learning Representations*.

Kenton Lee, Ming-Wei Chang, and Kristina Toutanova. 2019. Latent retrieval for weakly supervised open

domain question answering. In *Association for Computational Linguistics*.

Chin-Yew Lin. 2004. ROUGE: A package for automatic evaluation of summaries. In *Text Summarization Branches Out*, pages 74–81, Barcelona, Spain. Association for Computational Linguistics.

Zhen Lin, Shubhendu Trivedi, and Jimeng Sun. 2024. Generating with confidence: Uncertainty quantification for black-box large language models. *Transations on Machine Learning Research*.

Yinhan Liu, Myle Ott, Naman Goyal, Jingfei Du, Mandar Joshi, Danqi Chen, Omer Levy, Mike Lewis, Luke Zettlemoyer, and Veselin Stoyanov. 2019. Roberta: A robustly optimized bert pretraining approach. *arXiv preprint arXiv:1907.11692*.

Andrey Malinin and Mark Gales. 2020. Uncertainty estimation in autoregressive structured prediction. *arXiv preprint arXiv:2002.07650*.

Guilherme Penedo, Quentin Malartic, Daniel Hesslow, Ruxandra Cojocaru, Hamza Alobeidli, Alessandro Cappelli, Baptiste Pannier, Ebtesam Almazrouei, and Julien Launay. 2023. The refinedweb dataset for falcon llm: Outperforming curated corpora with web data only. *Advances in Neural Information Processing Systems*.

Xin Qiu and Risto Miikkulainen. 2024. Semantic density: Uncertainty quantification for large language models through confidence measurement in semantic space. In *Neural Information Processing Systems*.

Qwen, :, An Yang, Baosong Yang, Beichen Zhang, Binyuan Hui, Bo Zheng, Bowen Yu, Chengyuan Li, Dayiheng Liu, Fei Huang, Haoran Wei, Huan Lin, Jian Yang, Jianhong Tu, Jianwei Zhang, Jianxin Yang, Jiaxi Yang, Jingren Zhou, Junyang Lin, Kai Dang, Keming Lu, Keqin Bao, Kexin Yang, Le Yu, Mei Li, Mingfeng Xue, Pei Zhang, Qin Zhu, Rui Men, Runji Lin, Tianhao Li, Tianyi Tang, Tingyu Xia, Xingzhang Ren, Xuancheng Ren, Yang Fan, Yang Su, Yichang Zhang, Yu Wan, Yuqiong Liu, Zeyu Cui, Zhenru Zhang, and Zihan Qiu. 2025. Qwen2.5 technical report. *Preprint*, arXiv:2412.15115.

Pranav Rajpurkar, Jian Zhang, Konstantin Lopyrev, and Percy Liang. 2016. SQuAD: 100,000+ questions for machine comprehension of text. In *Empirical Methods in Natural Language Processing*.

Nils Reimers and Iryna Gurevych. 2019. Sentence-BERT: Sentence embeddings using Siamese BERT-networks. In *Proceedings of the 2019 Conference on Empirical Methods in Natural Language Processing and the 9th International Joint Conference on Natural Language Processing (EMNLP-IJCNLP)*.

Ehud Reiter and Anja Belz. 2009. An investigation into the validity of some metrics for automatically evaluating natural language generation systems. *Computational Linguistics*, 35(4):529–558.

Roman Vashurin, Ekaterina Fadeeva, Artem Vazhentsev, Lyudmila Rvanova, Akim Tsvigun, Daniil Vasilev, Rui Xing, Abdelrahman Boda Sadallah, Kirill Grishchenkov, Sergey Petrakov, Alexander Panchenko, Timothy Baldwin, Preslav Nakov, Maxim Panov, and Artem Shelmanov. 2025. Benchmarking uncertainty quantification methods for large language models with lm-polygraph. *Preprint*, arXiv:2406.15627.

Leandro Von Werra, Lewis Tunstall, Abhishek Thakur, Sasha Luccioni, Tristan Thrush, Aleksandra Piktus, Felix Marty, Nazneen Rajani, Victor Mustar, and Helen Ngo. 2022. Evaluate & evaluation on the hub: Better best practices for data and model measurements. In *Empirical Methods in Natural Language Processing: System Demonstrations*.

Peiyi Wang, Lei Li, Liang Chen, Zefan Cai, Dawei Zhu, Binghuai Lin, Yunbo Cao, Lingpeng Kong, Qi Liu, Tianyu Liu, and Zhifang Sui. 2024. Large language models are not fair evaluators. In *Association for Computational Linguistics (Volume 1: Long Papers)*.

Jason Wei, Nguyen Karina, Hyung Won Chung, Yunxin Joy Jiao, Spencer Papay, Amelia Glaese, John Schulman, and William Fedus. 2024. Measuring short-form factuality in large language models. *Preprint*, arXiv:2411.04368.

Yijun Xiao and William Yang Wang. 2021. On hallucination and predictive uncertainty in conditional language generation. In *European Chapter of the Association for Computational Linguistics*.

Ruihan Yang, Caiqi Zhang, Zhisong Zhang, Xinting Huang, Sen Yang, Nigel Collier, Dong Yu, and Deqing Yang. 2024. Logu: Long-form generation with uncertainty expressions. *arXiv preprint arXiv:2410.14309*.

Yuheng Zha, Yichi Yang, Ruichen Li, and Zhiting Hu. 2023. AlignScore: Evaluating factual consistency with a unified alignment function. In *Association for Computational Linguistics (Volume 1: Long Papers)*.

Tianyi Zhang, Varsha Kishore, Felix Wu, Kilian Q. Weinberger, and Yoav Artzi. 2020. Bertscore: Evaluating text generation with bert. In *International Conference on Learning Representations*.

Lianmin Zheng, Wei-Lin Chiang, Ying Sheng, Siyuan Zhuang, Zhanghao Wu, Yonghao Zhuang, Zi Lin, Zhuohan Li, Dacheng Li, Eric P. Xing, Hao Zhang, Joseph E. Gonzalez, and Ion Stoica. 2024. Judging llm-as-a-judge with mt-bench and chatbot arena. In *Neural Information Processing Systems*.

A Details of *UQ methods*

There are several methods for generating uncertainty estimates that help assess the in-correctness of LM outputs. These approaches can be broadly classified into three main categories: **1) Single-sample methods**: methods that require a single forward pass from the model and that generally use directly the logits and probability distributions over the vocabulary space provided as output from the model; **2) Multiple-sample methods**: methods that, given a prompt x, sample multiple possible outputs for the same prompt and compute an uncertainty score based on these outputs; **3) Learned methods**: usually probes or small networks directly trained to predict the accuracy of the model given the prompt and the answer.

We denote with x the sequence of tokens corresponding to the prompt. This usually includes the instruction prompt (e.g., "Answer the following question") together with the question and additional context. The L generated tokens are indicated as $\hat{y}_i$. Additionally, a superscript $\hat{y}^{(s)}$ is used for multiple-sample methods to indicate the s-th sample (out of S_{UQ} samples) sampled for a given prompt. $\hat{p}(\cdot)$ denotes the probability assigned by the model.

Single-sample methods. Single-sample methods estimate the uncertainty score using the logits that the models output. These logits are usually computed on the greedy decoded output or on a low-temperature sample decoded from the model given the prompt x.

Negative Sequence Probability. Sequence probability computes the cumulative probability of the sequence. This can be used as an uncertainty score by flipping the sign and considering $-\hat{p}(\hat{y}|x)$. When evaluated on the greedy decoding samples, this method is sometimes referred to in the literature as *Maximum Sequence Probability* (MSP) (Fadeeva et al., 2023; Vashurin et al., 2025). Aichberger et al. (2025) investigate the difference between the performance of MSP estimated using greedy decoding and estimated using the Beam Search decoding algorithm, which yields sequences with higher likelihood.

$$\hat{p}(\hat{y}|x) = \prod_{i=1}^{L} \hat{p}(\hat{y}_i|\hat{y}_{<i}, x). \tag{4}$$

Perplexity. Perplexity computes the uncertainty score via the exponential of the mean token likelihood (Bahl et al., 1983). Compared to sequence probability, perplexity normalizes the underlying probability by the number of the generated tokens,

$$\exp\left(-\frac{1}{L}\sum_{i=1}^{L}\log \hat{p}(\hat{y}_i|\hat{y}_{<i}, x)\right). \tag{5}$$

Mean Token Entropy. Mean token entropy (Fomicheva et al., 2020; Malinin and Gales, 2020) computes the mean of the per-token entropies over the vocabulary distribution,

$$\mathcal{H}_T(\hat{y}, x) = \frac{1}{L}\sum_{i=1}^{L}\mathcal{H}\left[\hat{p}(\hat{y}_i|\hat{y}_{<i}, x)\right]. \tag{6}$$

Multiple-Sample methods. Multiple-sample methods compute an uncertainty score by sampling S_{UQ} times for a single prompt. Since it is accessing (more of) the full probability distribution, this class of methods should provide better uncertainty scores than single-sample methods, albeit at the expense of an increased computational cost at inference time. The exact number of samples S_{UQ} is a hyperparameter that usually depends on the specific *UQ method*.

Naive Entropy. Naive Entropy computes the entropy over the different generated samples. The sequence probability of each generation is computed using the chain rule of probability, like in the Sequence Probability method,

$$-\sum_{s=0}^{S_{\text{UQ}}}\hat{p}(\hat{y}^{(s)}|x)\log \hat{p}(\hat{y}^{(s)}|x). \tag{7}$$

Semantic Entropy. Semantic entropy computes the entropy over the different semantic clusters C of the generated samples (Farquhar et al., 2024). Semantic clusters are generated using a Natural Language Inference (NLI) model, which evaluates bidirectional entailment between pairs of answers in S_{UQ}. This process group answers with equivalent meanings into clusters $c^{(i)}$. Each cluster probability $\hat{p}(c^{(i)})$ is computed by summing the Sequence Probabilities of the unique generations that fall into that cluster (Farquhar et al., 2024). The probability of each generated sequence is computed using Eq. 4, either directly for Semantic Entropy (without length normalization), or normalized by the sequence token-length L for length-normalized Semantic Entropy.

$$SE(x) = -\sum_{i=1}^{C}\hat{p}(c^{(i)}|x)\log \hat{p}(c^{(i)}|x). \tag{8}$$

Learned methods. Learned methods leverage the model's internal activations or its entire architecture to train additional networks or classifiers that predict the correctness of the answer.

Probes are the most common form of learned method. The most prominent variety of probe is *P(IK)*, also known as P(I Know) (Kadavath et al., 2022), which finetunes the entire model to predicts a binary score whether the model can answer the question correctly or not. This is accomplished by attaching a classifier to the embedding of the final token in the last layer. The training set is collected by labeling some generations from the model with the task *correctness function*. In this paper, we follow the implementation of (Farquhar et al., 2024; Kapoor et al., 2024) that does not train the full model but just a logistic regression classifier on top of the representation of the final answer token as in (Chen et al., 2024a). We trained probes using two different *correctness functions*: LM-as-a-judge (using `Qwen/Qwen2.5-72B-Instruct`), and ROUGE-L (Recall) with a 0.5 threshold. When reporting results, we specify the *correctness function* used to label the dataset and train the probe in round brackets—for example, `Probe(LM-as-a-judge)`, where LM-as-a-judge denotes the judging model. Probes are trained until convergence on each training dataset with L-BFGS and a tolerance value of 0.0001 and maximum number of optimization iterations of 10000.

B Details of *Correctness functions*

In this section, we describe in detail the *correctness functions* used in our experiments. Many of these metrics return a continuous score, which is binarized for calculating AUROC; we detail the thresholds t used in this binarization below.

B.1 Lexical-based

Lexical-based metrics assess similarity by measuring lexical overlap between the generated sentence and the ground truth. These metrics are among the most widely used due to their low computational cost and long-standing history in QA evaluation.

It is important to note that these metrics were originally used to evaluate QA in *trained systems*, where the output distribution of generated sequences has been aligned with the expected distribution of ground-truth answers in the dataset. However, in common LM zero-shot evaluation settings, this alignment is no longer guaranteed. Consequently, these metrics may fail to accurately assess correctness, requiring careful consideration when applying them. While techniques like incorporating few-shot examples, as demonstrated by Farquhar et al. (2024), can mitigate this issue to some extent, this does not fully address the fundamental limitations of lexical-based metrics.

ROUGE-L. ROUGE-L measures the longest common subsequence (LCS) between the generated response and the reference answer, allowing for non-contiguous matches (Lin, 2004). In UQ the F1-score (*ROUGE-L (F1)*) of this metric is typically used, balancing precision and recall (Fadeeva et al., 2023; Kuhn et al., 2023; Duan et al., 2024; Chen et al., 2024a; Qiu and Miikkulainen, 2024; Aichberger et al., 2025). *ROUGE-L (Precision)* measures the ratio of the longest common subsequence (LCS) length to the number of unigrams in the generated answer. *ROUGE-L (Recall)* measures the ratio of the LCS length to the number of unigrams in the reference answer. ROUGE-L (F1) is the harmonic mean of ROUGE-L precision and recall. It is important to note that ROUGE-L recall is not affected by the length of the generated answer, whereas precision and F1 metrics are influenced by it. In the experiments of this paper, we consider *ROUGE-L (F1)* and *ROUGE-L (Recall)* variants, with both metrics computed using the Python package `rouge_scorer`. Both ROUGE-L variants return continuous scores; where a binary score is used, we consider thresholds $t \in \{0.1, 0.3, 0.5\}$ for ROUGE-L (F1), and $t = 1.0$ for ROUGE-L (Recall).

ROUGE-1. ROUGE-1 measures the unigrams overlap between the generated response and the ground truth (Lin, 2004). ROUGE-1 captures similarity based on single-tokens overlap. This metric has been widely used in QA evaluations and in UQ benchmarks in Aichberger et al. (2025). As in Aichberger et al. (2025), we use the F1 variant (*ROUGE-1 (F1)*). In the experiments of this paper, the metric has been computed using the Python package `rouge_scorer`. ROUGE-1 (F1) returns continuous scores; where a binary score is used, we use a threshold $t = 0.1$.

SQuAD. This metric has been introduced in Rajpurkar et al. (2016) to measure the performance of systems trained on the homonymous dataset. The metric computes the F1 score based on word overlap between the prediction and ground truth,

treating them as unordered bags of tokens, selecting the highest F1 among multiple references per question, and averaging across all questions. This metric has been used to evaluate correctness for UQ benchmarks in Farquhar et al. (2024). To compute the metric we used the implementation from Von Werra et al. (2022). SQuAD returns continuous scores; where a binary score is used, we use a threshold $t = 0.3$.

B.2 Embedding-based

Embedding-based metrics assess similarity by encoding both the ground truth and generated text using a neural model, typically BERT-based. The goal is to measure semantic similarity rather than surface-level overlap.

BERTScore. BERTScore (Zhang et al., 2020) evaluates generated answers by embedding both the generated text and the ground truth using a BERT pretrained model. It then computes the pairwise cosine similarity between tokens. For each token in the generated text, the highest similarity score with any token in the reference text is selected. Finally, precision, recall, and F1-score are calculated, with the F1-score commonly used in UQ to balance precision and recall. This metric has been used to evaluate correctness for UQ benchmarks in Fadeeva et al. (2023). In the experiments of this paper, the metric has been computed using the Python package `bert_score` as in Fadeeva et al. (2023). BERTScore returns continuous scores; where a binary score is used, we use a threshold $t = 0.8$, which we empirically found to yield the highest agreement with human raters in Fig. 2.

SentenceBERT Similarity. A SentenceBERT model (Reimers and Gurevych, 2019) is used to encode both the generated answer and the ground truth answer. Specifically, following Chen et al. (2024a), we use *nli-roberta-large*[2]. The cosine similarity is then calculated between the ground truth and generated answer embeddings. This metric has been used to evaluate correctness for UQ benchmarks in Chen et al. (2024a). SentenceBERT returns continuous scores; where a binary score is used, we use a threshold $t \in \{0.4, 0.9\}$.

B.3 LM-as-a-judge methods

LM-as-a-judge metrics evaluate correctness by using another LM to judge the accuracy of a generated answer against the reference answer from the dataset. The evaluating LM may be specifically trained for this task or not.

AlignScore. AlignScore is a metric designed to evaluate the factual consistency of generated text with respect to a ground truth answer (Zha et al., 2023). It employs a RoBERTa model (Liu et al., 2019) trained to assess the alignment between two text pieces, determining how well the generated content corresponds to the source information. The training process integrates data from several NLP tasks—natural language inference, question answering, paraphrasing, fact verification, information retrieval, semantic similarity, and summarization—resulting in a model trained specifically to evaluate correctness. This metric has been used to evaluate correctness for UQ benchmarks in Vashurin et al. (2025). AlignScore returns continuous scores; where a binary score is used, we use a threshold $t = 0.5$.

LM-as-a-judge (Prompt). LM-as-a-judge (Zheng et al., 2024) encompasses a set of approaches that rely on a large language model to provide a human-like assessment of generated content by comparing it against a reference answer. Generally, different prompting strategies can be applied to guide the evaluation process. In our experiments, we used the same prompt as Farquhar et al. (2024) with `Qwen/Qwen2.5-72B-Instruct` as the judging model. LM-as-a-judge returns binary scores, so no thresholds are used.

[2]https://huggingface.co/sentence-transformers/nli-roberta-large

C Impact of Correlated and Uncorrelated Errors in the *Correctness function* on AUROC Estimation

In this section, we analyze the impact of correlated or uncorrelated errors in the *correctness function* on AUROC estimation. We note that a similar analysis is performed in Ielanskyi et al. (2025), a concurrent work that explores impact of bias and variance of *correctness functions* on AUROC estimation.

Let $\hat{g}_i \equiv \hat{g}(\hat{y}_i, x_i) \in \mathbb{R}$ be the uncertainty (UQ) score assigned to the answer $\hat{y}_i$ given the question x_i. We use $h_i \equiv h(\hat{y}_i, x_i) \in \{0, 1\}$ to denote the *ground-truth correctness* of $\hat{y}_i$, i.e., $h_i = 1$ if the answer is correct and 0 otherwise. The *estimated correctness* under some *correctness function* $\hat{h}$ is $\hat{h}_i \equiv \hat{h}(\hat{y}_i, x_i, y_i) \in \{0, 1\}$, possibly using a reference answer y_i.

We define:

$$\text{TPR} \;=\; P\big(h = 1 \mid \hat{h} = 1\big), \quad \text{FPR} \;=\; 1 - \text{TPR},$$

$$\text{TNR} \;=\; P\big(h = 0 \mid \hat{h} = 0\big), \quad \text{FNR} \;=\; 1 - \text{TNR}.$$

The *true AUROC* of $\hat{g}$, based on ground-truth labels, is

$$\text{AUROC}(\hat{g}) \;=\; P\big(\hat{g}_i < \hat{g}_j \;\mid\; h_i = 1,\; h_j = 0\big).$$

When correctness is measured by $\hat{h}$, we obtain

$$\widehat{\text{AUROC}}(\hat{g}) \;=\; P\big(\hat{g}_i < \hat{g}_j \;\mid\; \hat{h}_i = 1,\; \hat{h}_j = 0\big).$$

We additionally assume $P(\hat{g}_i = \hat{g}_j) = 0$ for $i \neq j$, implying $\text{AUROC}(\hat{g}) = 1 - \text{AUROC}(-\hat{g})$.

Expanding $\widehat{\text{AUROC}}$. We rewrite $\widehat{\text{AUROC}}(\hat{g})$ by conditioning on both the true labels (h_i, h_j) and the estimated labels $(\hat{h}_i, \hat{h}_j)$:

$$\begin{aligned}
&\widehat{\text{AUROC}}(\hat{g}) \\
&= \sum_{a,b \in \{0,1\}} P\big(g_i < g_j \mid \hat{h}_i = 1,\; \hat{h}_j = 0,\; h_i = a,\; h_j = b\big) \\
&\quad \times P\big(h_i = a \mid \hat{h}_i = 1\big) \;\cdot\; P\big(h_j = b \mid \hat{h}_j = 0\big)
\end{aligned}$$

We discuss below two cases: (*i*) when $\hat{h}$'s errors are *independent* of $\hat{g}$, and (*ii*) when they are *correlated*.

C.1 Case 1: Independent Errors

Analysis $\hat{h}_i \perp\!\!\!\perp \hat{g}_i \mid h_i$. In this setting, we have

$$\begin{aligned}
&\widehat{\text{AUROC}}(\hat{g}) \\
&= \sum_{a,b \in \{0,1\}} P(g_i < g_j | h_i = a, h_j = b) \\
&\quad \cdot P(h_i = a | \hat{h}_i = 1) \cdot P(h_j = b | \hat{h}_j = 0) \\
&= P(g_i < g_j | h_i = 0, h_j = 0) \cdot \text{FPR} \cdot \text{TNR} \\
&\quad + P(g_i < g_j | h_i = 1, h_j = 1) \cdot \text{TPR} \cdot \text{FNR} \\
&\quad + P(g_i < g_j | h_i = 1, h_j = 0) \cdot \text{TPR} \cdot \text{TNR} \\
&\quad + P(g_i < g_j | h_i = 0, h_j = 1) \cdot \text{FPR} \cdot \text{FNR} \\
&= 0.5 \cdot \text{FPR} \cdot \text{TNR} + 0.5 \cdot \text{TPR} \cdot \text{FNR} \\
&\quad + P(g_i < g_j | h_i = 1, h_j = 0) \cdot \text{TPR} \cdot \text{TNR} \\
&\quad + P(g_i < g_j | h_i = 0, h_j = 1) \cdot \text{FPR} \cdot \text{FNR} \\
&= 0.5 \cdot \text{FPR} \cdot \text{TNR} + 0.5 \cdot \text{TPR} \cdot \text{FNR} \\
&\quad + \text{AUROC}(\hat{g}) \cdot \text{TPR} \cdot \text{TNR} \\
&\quad + (1 - \text{AUROC}(\hat{g})) \cdot \text{FPR} \cdot \text{FNR}.
\end{aligned} \tag{9}$$

All terms $\{\text{TPR}, \text{TNR}, \text{FPR}, \text{FNR}\}$ in Eq. 9 are *constant* properties of the *correctness function* $\hat{h}$, and do not depend on the *UQ method* $\hat{g}$. Hence $\widehat{\text{AUROC}}(\hat{g})$ becomes a "noisy" version of the true $\text{AUROC}(\hat{g})$, biased toward 0.5.

Implications. In this uncorrelated setting, the bias introduced by $\hat{h}$ *does not* depend on the UQ metric $\hat{g}$. Consequently, while the estimated AUROC values will be inaccurate, the ranking of UQ methods by $\widehat{\text{AUROC}}(\hat{g})$ will, in expectation, match the ranking by $\text{AUROC}(\hat{g})$, provided $\text{TPR} \cdot \text{TNR} > \text{FPR} \cdot \text{FNR}$. In practice, finite-sample effects can lead to variance, but with appropriate sample sizes, comparisons based on $\widehat{\text{AUROC}}(\hat{g})$ remain valid.

C.2 Case 2: Correlated errors

Analysis $\hat{h}_i \not\perp\!\!\!\perp \hat{g}_i \mid h_i$. In this case,

$$\begin{aligned}
&P(g_i < g_j | h_i = a, h_j = b, \hat{h}_i = 1, \hat{h}_j = 0) \\
&\quad \neq P(g_i < g_j | h_i = a, h_j = b).
\end{aligned}$$

In particular, if the *correctness function*'s errors are negatively correlated with our *UQ method* (i.e., the more confident the *UQ method* is on a task,

the more likely it is to be erroneously marked as correct), then we have

$$P(g_i < g_j | h_i = a, h_j = b, \hat{h}_i = 1, \hat{h}_j = 0) > P(g_i < g_j | h_i = a, h_j = b),$$

for all values of a and b, with the magnitude of the difference increasing with the magnitude of the correlation. This implies that $\widehat{\text{AUROC}}(\hat{g}) > \text{AUROC}(\hat{g})$. Similarly, if the errors are positively correlated with UQ metric, then we have $\widehat{\text{AUROC}}(\hat{g}) < \text{AUROC}(\hat{g})$.

This indicates that we will over-estimate the true AUROC if we have negatively correlated errors, and under-estimate if we have positively correlated errors. This is problematic because it introduces errors in AUROC that *do* depend on the *UQ method* under consideration, leading to potential reordering of metrics.

Sources of Correlation. Since, in general, the *UQ method* does not depend on the output of the *correctness function* or vice versa, any correlation between the *UQ method*, and errors in the *correctness function*, must be due to information in $\hat{y}$ and/or x that a) introduces systemic errors in the *correctness function*, and b) is used by the *UQ method*. In this paper, we look at length as such a confounding variable, but it is not the only possible option. For example, the use of less frequently occurring words in $\hat{y}$ might lead to both an increase in uncertainty scores due to unfamiliar language, and an increase in the probability of erroneously marking an answer as incorrect due to reduced lexical overlap with the reference answer. We leave the exploration of additional confounders as future work.

D Experimental Details

The datasets considered are `TriviaQA` (Joshi et al., 2017), `SQuAD` (Rajpurkar et al., 2016), `NQ-Open` (Lee et al., 2019), and `SimpleQA` (Wei et al., 2024). The models considered are `Falcon-7B` (Penedo et al., 2023), `Qwen2.5-7B` (Qwen et al., 2025), and two versions of `Mistral-7B` (Jiang et al., 2023).

Our evaluation setup closely follows the methodology proposed by Farquhar et al. (2024)[3]. To obtain model responses, we employed the same prompt as in Farquhar et al. (2024) for the long-form setting, instructing the model as follows:

> *Answer the following question in a single brief but complete sentence.*

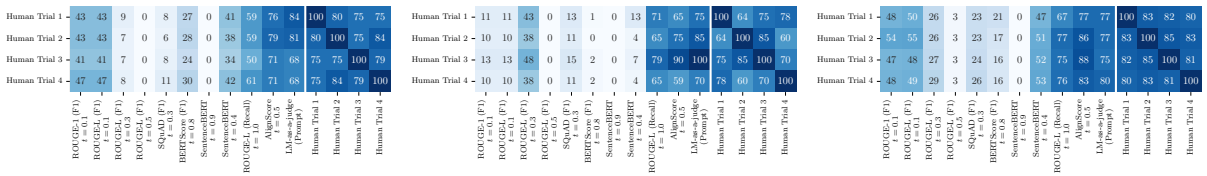
Responses are sampled using greedy decoding. Similarly, for the LM-as-a-judge evaluation, we adhered to the same prompt of Farquhar et al. (2024) and used the model `Qwen/Qwen2.5-72B-Instruct` as the judging model. For our experiments, we employed the following models from the Hugging Face Hub `mistralai/Mistral-7B-Instruct-v0.1`, `mistralai/Mistral-7B-Instruct-v0.3`, `Qwen/Qwen2.5-7B-Instruct`, and `tiiuae/falcon-7b-instruct`. The datasets used in our evaluation consist primarily of closed-book QA datasets, with the exception of `SQuAD` which is an open-book dataset. Specifically, for `SQuAD` we incorporated the available context as part of the prompt. In semantic clustering-based methods (Semantic Entropy), we employed DeBERTa as our Natural Language Inference (NLI) module as in Farquhar et al. (2024).

E Details on Human Annotation process

We used an internal crowdsourcing platform to gather annotations. The raters were fluent English speakers and were compensated at or above the minimum wage. We randomly sampled 450 data points from `TriviaQA` (Joshi et al., 2017), `NQ-Open` (Lee et al., 2019), and `SimpleQA` (Wei et al., 2024). We then generated answers using `Qwen2.5-7B-Instruct`, with with greedy decoding. We excluded `SQuAD` from the human annotation process to avoid incorporating the additional context into the annotation prompt, thereby streamlining and accelerating the annotation process. We then tasked human annotators to evaluate the correctness, collecting four annotations per data point. Below, we present the annotation guidelines provided to each annotator. Each dataset included in the guidelines two manually labeled examples.

In Fig. 2 and Fig. 6, we present the Cohen's Kappa agreement rates among human annotators. The first figure reports agreement computed across all 450 data points, while the second breaks down the agreement rates for each individual dataset (150 data points each). For clearer visualization, Fig. 5 and Fig. 7 display a uniformly sampled subset of 150 data points from the full set of 450. These points represent correctness values for responses where all annotators agreed on the label.

[3] https://github.com/jlko/semantic_uncertainty/



(a) TriviaQA (b) SimpleQA (c) NQ

Figure 6: Cohen Kappa agreement rates between human annotators and *correctness functions*.

Annotation Guidelines

You will be provided with three pieces of text:

- **Question**: A question previously proposed to a system or user. You are not asked to answer this question; it has already been answered (see Proposed Answer).
- **Reference Answer**: The authoritative or "gold standard" answer provided for the Question.
- **Proposed Answer**: The response given by a user for the Question that needs to be evaluated.

Your task is to determine whether the **Proposed Answer** is **equivalent** to the **Reference Answer** in the context of the **Question**.

Evaluation Criteria

- **Focus on Equivalence**: Assess whether the Proposed Answer conveys the same meaning as the Reference Answer, regardless of additional details or alternative phrasings.
- **Ignore Personal Knowledge**: Do not rely on your own knowledge or conduct external research. Base your judgment solely on the given text.
- **Exact Matching is Not Required**: The Proposed Answer does not need to be identical to the Reference Answer, but it must preserve the core meaning.
- **Context Matters**: Ensure that the Proposed Answer is relevant to the Question and correctly aligns with the Reference Answer's meaning.

Rating Scale

Choose one of the following ratings for each evaluation:

- **Equivalent**: The Proposed Answer conveys the same meaning as the Reference Answer.
- **Not Equivalent**: The Proposed Answer does not convey the same meaning, either due to missing essential information, contradictions, or incorrect interpretation.

Additional Notes

- **More Detail vs. Different Information**: Extra information is acceptable as long as the meaning remains the same. However, if the additional details introduce contradictions, the Proposed Answer should be marked **Not Equivalent**.
- **Paraphrasing is Allowed**: The wording of the Proposed Answer does not need to match exactly, but the core meaning must remain intact.
- **Avoid Assumptions**: Do not infer additional meaning beyond what is explicitly stated.

Examples

Example 1: Equivalent

<Follows one annotated equivalent example from the dataset>

Example 2: Not Equivalent

<Follows one annotated not equivalent example from the dataset>

F Additional Results

We present here supplementary results that were excluded from the main paper.

Fig. 6 presents the Cohen's Kappa agreement rate with human annotators, broken down by dataset. LM-as-a-judge approaches demonstrate stronger alignment with human judgments, whereas lexical-based and embedding-based *correctness functions* are highly sensitive to the selection of an appropriate threshold.

Fig. 7 illustrates the score assigned by *correctness functions* as a function of the generated answer's length. Among the evaluated approaches, LM-as-a-judge methods (AlignScore and LM-as-a-judge `Qwen/Qwen2.5-72B-Instruct`) appear to be the only robust ones that remain invariant to length while effectively distinguishing between correct and incorrect samples without requiring threshold tuning.

Fig. 8 shows how human-agreement rates vary with the threshold used to binarize different *correctness functions* across datasets. Lexical and embedding-based metrics (e.g., ROUGE-L (F1), BERTScore) show high sensitivity to threshold tuning and inconsistent alignment with human judgments. In contrast, AlignScore yields consistently high agreement across thresholds and datasets.

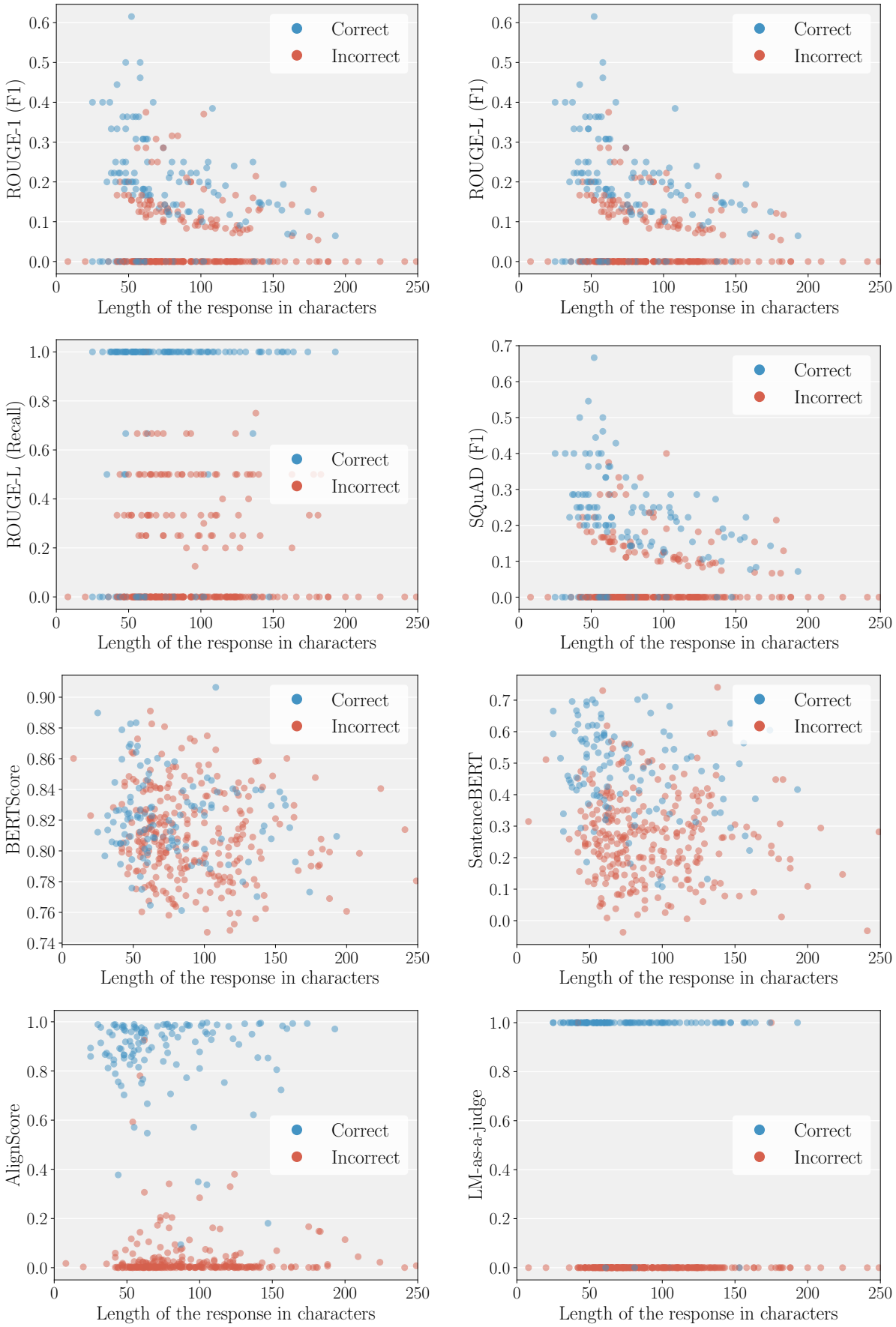


Figure 7: *Correctness function* vs response length. Color indicates human correctness judgments.

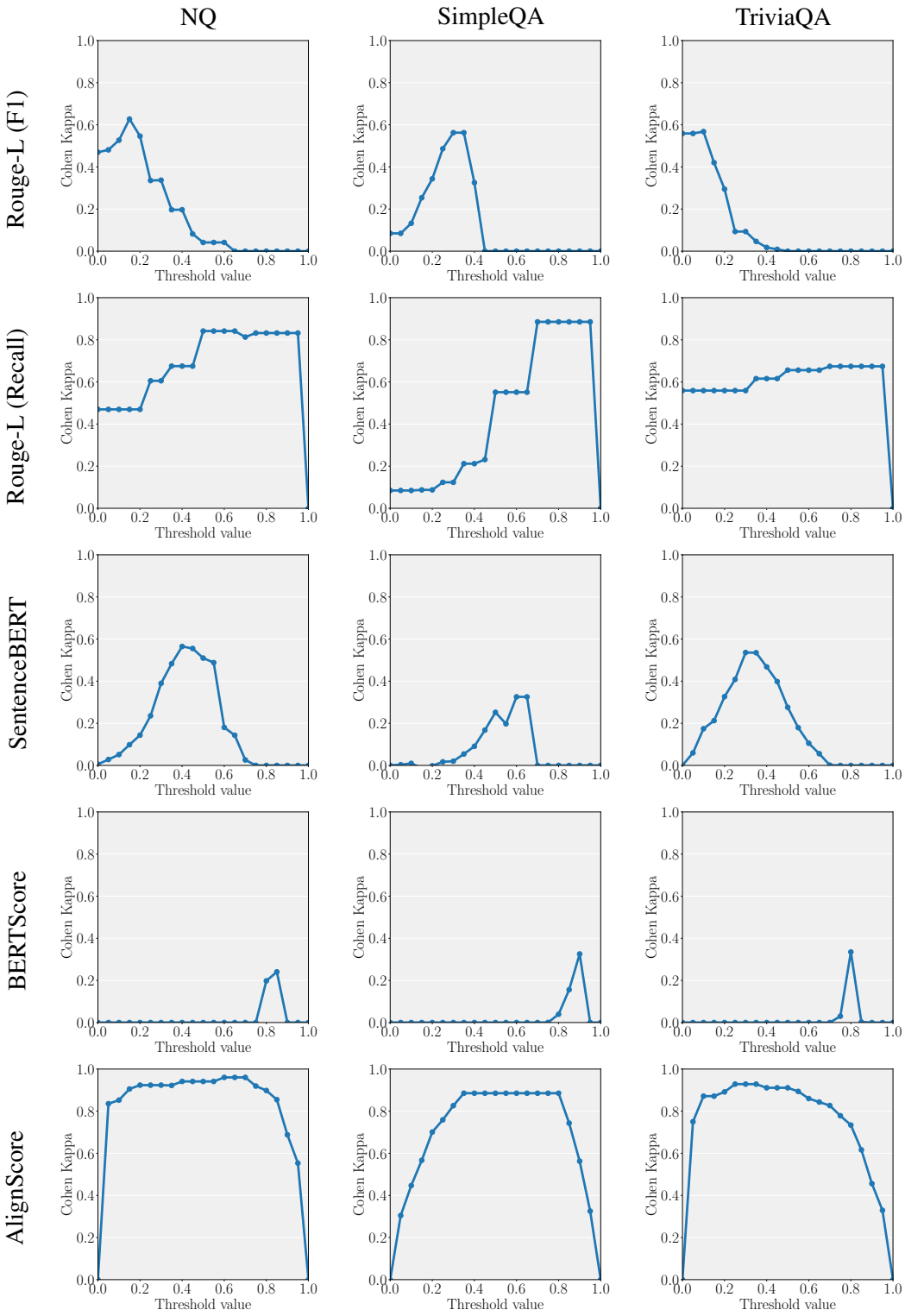


Figure 8: Human-agreement rate as a function of the *correctness function* threshold.