Do Language Models Learn Commonsense Knowledge?

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Abstract

Language models (LMs) trained on large amounts of data (e.g., Brown et al., 2020; Patwary et al., 2021) have shown impressive performance on many NLP tasks under the zero-shot and few-shot setup. Here we aim to better understand the extent to which such models learn commonsense knowledge— a critical component of many NLP applications. To that end, we conduct a systematic and rigorous zero-shot and few-shot commonsense evaluation of pre-trained LMs, where we: (i) carefully control for the LM’s ability to exploit potential surface cues and annotation artefacts, and (ii) account for variations in model performance that arise from non-commonsense related factors. Our findings highlight the limitations of pre-trained LMs in acquiring commonsense knowledge without task-specific supervision; furthermore, using larger models— or augmenting the LMs with commonsense knowledge bases at test-time — did not substantially improve their performance. More broadly, our findings offer valuable lessons and best practices for conducting more rigorous multiple-choice evaluations of pre-trained LMs.

1 Introduction

Common sense— the implicit knowledge about everyday situations that is shared by humans — is an important prerequisite for developing general-purpose intelligent systems (McCarthy et al., 1960; Liu and Singh, 2004; Gunning, 2018). Intriguingly, recent large LMs (Brown et al., 2020; Patwary et al., 2021) have achieved remarkable performance at various common sense benchmarks (e.g., Sakaguchi et al., 2020; Zellers et al., 2019; Bisk et al., 2020b; Sap et al., 2019b), even when they are evaluated in a zero-shot or few-shot fashion, without explicit commonsense supervision. Here we revisit this apparent success, and conduct a rigorous study that aims to better understand the extent to which such pre-trained LMs are able to capture commonsense knowledge. We argue that this research question is an important one: As pre-trained LMs constitute a foundational building block of NLP today, any deficiencies in their commonsense understanding can thus adversely manifest in downstream NLP applications (Bommasani et al., 2021).

Concretely, our work differs from prior work on commonsense evaluation of LMs (Brown et al., 2020; Patwary et al., 2021) by way of a more rigorous evaluation, in which we: (i) carefully control for the LM’s ability to exploit potential surface cues and annotation artefacts to predict the answer, without reasoning over the context. We further (ii) account for the variations in factors influencing the LM’s performance, which arise from certain evaluation design choices, independently of commonsense knowledge in the models. To our best knowledge, our work is the first such systematic investigation: We conduct our study on four commonsense benchmarks, five model sizes (up to 7B
parameters), and multiple evaluation settings (e.g., different score functions and prompt format).

We first focus on zero-shot evaluation and ask: How would the LM’s zero-shot performance compare to a strong baseline (§3)? Controlling for the LM’s ability to guess the correct answer, without even looking at the question (Poliak et al., 2018; Trichelair et al., 2019, Answer-only baseline, top of Fig. 1), we find that, despite the LM’s strong zero-shot performance, the Answer-only baseline can nevertheless perform surprisingly well on most benchmarks. We thus emphasize the importance of strong baselines that can account for superficial cues or annotation artefacts in benchmarks; such baselines are at times absent from recent work (Zhou et al., 2020; Brown et al., 2020).

To what extent does the model’s zero-shot performance vary depending on certain evaluation design choices, such as the format of the prompt, or the score function (§4)? We find that these design choices — though they have little to do with common sense — can result in large fluctuations in performance (up to 19%). This finding challenges the notion that LMs are largely able to work well out-of-the-box with minimal task-specific tuning. Furthermore, as different papers have used different design choices, we emphasize the need to carefully select such choices, explicitly state them to enable fair comparison, and quantify the robustness of the observed results across different design choices.

Finally, can increasing model size, using few-shot evaluation, or leveraging commonsense knowledge bases (CSKBs) help improve the LM’s performance? When controlling for the Answer-only baseline, increasing model size yields marginal improvements (§5). Furthermore, using a few-shot evaluation setup (instead of zero-shot) did not substantially improve the 7B LM’s performance (§6). Lastly, our best zero-shot performance matches, or surpasses, that of prior models with CSKBs (Shwartz et al., 2020; Bosselut et al., 2021; Bauer and Bansal, 2021); however, we do not observe notable gains from augmenting our LMs with CSKBs (§7). This finding reiterates the importance of comparing with strong baselines (Melis et al., 2018).

All in all, our findings suggest that acquiring human-level commonsense knowledge, without relying on surface cues or task-specific supervision, remains beyond the reach of current LMs. Given the small improvements from increasing model size, we conjecture that other techniques, such as explicit commonsense supervision, multimodal grounding, or physical embodiment (Bisk et al., 2020a), are potential promising ways forward. More broadly, we expect our findings — particularly on the importance of Answer-only baselines for multiple-choice evaluation, alongside careful tuning of certain evaluation design choices — to offer valuable lessons for the burgeoning field of large LM evaluation.

2 Experimental Setting

We begin by outlining our experimental setup, and describe the benchmarks, model, baselines, and other relevant experimental settings.

2.1 Commonsense Benchmarks

Commonsense knowledge spans many different categories, such as physical common sense (e.g., a car is heavier than an apple), social common sense (e.g., a person will feel happy after receiving gifts), and temporal common sense (e.g., cooking an egg takes less time than baking a cake). Given this diverse nature of commonsense knowledge, various benchmarks have been proposed to test these different types of knowledge (e.g., Zellers et al., 2019; Sakaguchi et al., 2020; Sap et al., 2019b; Bisk et al., 2020b; Lin et al., 2020; Boratko et al., 2020).

Commonsense benchmarks broadly consist of two tasks: (a) multiple-choice evaluation (Zellers et al., 2018, 2019; Sap et al., 2019b; Bisk et al., 2020b), where a model needs to choose the correct answer from a list of plausible answers; (b) generative evaluation (Boratko et al., 2020; Lin et al., 2020, 2021), which requires a model to generate an answer given a question and some additional context. Here we focus on multiple-choice benchmarks, since they provide a more reliable automatic metric (i.e., accuracy), whereas automated metrics used to evaluate language generation (e.g., BLEU, Papineni et al., 2002) do not correlate perfectly with human judgment (Liu et al., 2016; Novikova et al., 2017).1 We use a diverse set of four representative multiple-choice commonsense benchmarks to better understand the extent to which pre-trained LMs are able to acquire different types of commonsense knowledge. We use the validation split of each benchmark, as their test splits are not public.

HellaSwag (Zellers et al., 2019) is designed to

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1Human judgment of LM output is not only costly to obtain, but also imperfect (Clark et al., 2021), compounding the difficulty of commonsense evaluation in a generation setup.
evaluate a model’s ability to understand physical, grounded, and temporal common sense. Given a four-sentence story, the model must choose the correct ending from four candidates. The stories are either video captions from ActivityNet (Heilbron et al., 2015), or WikiHow passages (Koupaee and Wang, 2018). When evaluating LMs on a similar dataset (Zellers et al., 2018), incorrect answers can be easy to distinguish from correct ones; hence in constructing HellaSwag, Zellers et al. (2019) removed easy negatives through adversarial filtering.

WinoGrande (Sakaguchi et al., 2020) is a co-reference resolution benchmark that mainly examines physical and social common sense. Each example consists of a sentence (e.g., “The trophy did not fit the suitcase because it is too big.”) and two candidate entities (e.g., “trophy” or “suitcase”). The task is to choose the correct entity for the pronoun, e.g., “it” refers to “trophy” in the example.

Social IQa (Sap et al., 2019b) focuses on evaluating social commonsense, in particular theory of mind — the capacity to reason about others’ mental states (Flavell, 2004). Given context sentences and a corresponding question, the task is to choose the correct response from three candidates. Annotators use the ATOMIC knowledge base (Sap et al., 2019a) to create context sentence and questions; the answers are provided by additional annotators.

PIQA (Bisk et al., 2020b), short for physical interaction question answering, mainly covers the physical aspect of common sense. Each data point consists of a task and two alternative solutions to finish the task; one of which is correct. The tasks are curated from a website with instructions for everyday tasks (e.g., separating egg yolks from eggs); the solutions are provided by human annotators.

### 2.2 Pre-trained Language Model

We use a pre-trained autoregressive LM with a Transformer architecture (Vaswani et al., 2017). Our model has 32 transformer layers — each with 4096-dimensional states, 16384-dimensional feed-forward layer, and 32 attention heads — translating to 7 billion parameters. In terms of model size, our model is comparable to the open-sourced GPT-J model (Wang and Komatsuzaki, 2021), and is ~5x larger than the largest GPT2-XL model with 1.5 billion parameters (Radford et al., 2019). Following the T5 model (Raffel et al., 2019), we train our model on the C4 corpus, which consists of 800 GB of data.

On the multiple-choice benchmarks, we evaluate the pre-trained LM by calculating the score for each answer choice under the model, and select the answer $\hat{y}$ with the highest score:

$$\hat{y} = \arg \max_{y \in Y(x)} s_\theta(y|x);$$

here $x$ denotes the question or prompt, $Y(x)$ the set of answer choices for a given question, and $s_\theta(\cdot)$ the score of an answer choice $y$ given $x$, under the pre-trained LM with parameters $\theta$. We provide some examples in Table 2. For Social IQa, we convert questions to natural text using the rules of Shwartz et al. (2020); we find this natural text format to yield better results, as discussed in §4.

Unless otherwise stated, we use cross-entropy (or token-level log prob) to score each answer:

$$s_\theta(y|x) = \sum_{i=0}^{\|y\|} \log(p_\theta(y_i|x, y_0...y_{i-1})) \quad \|y\|. \quad (1)$$

This score function reduces the impact of length; without dividing by $\|y\|$, longer answers might have lower probabilities (Stahlberg and Byrne, 2019). GPT3 (Brown et al., 2020) also employs this score function for zero-shot evaluation.

Table 1: Benchmark Statistics. For each benchmark, “Choices” and “Questions” show the number of candidate answers for each question and the number of questions in the validation split, respectively.

| Benchmark       | Choices | Main Knowledge Types        | Questions |
|-----------------|---------|----------------------------|-----------|
| HellaSwag (Zellers et al., 2019) | 4       | Temporal, Physical         | 10042     |
| WinoGrande (Sakaguchi et al., 2020) | 2       | Social, Physical           | 1267      |
| Social IQa (Sap et al., 2019b)   | 3       | Social                     | 1954      |
| PIQA (Bisk et al., 2020b)        | 2       | Physical                   | 1838      |

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2https://www.instructables.com/  
3A cleaned version of the Common Crawl corpus: https://huggingface.co/datasets/c4  
4For Social IQa, we concatenate the context sentence and question together to form the prompt $x$. 

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### Dataset

| Dataset  | Prompt: x                                                                 | Answer: y                                                                 |
|----------|---------------------------------------------------------------------------|---------------------------------------------------------------------------|
| HellaSwag | A woman is outside with a bucket and a dog. The dog is running around trying to avoid a bath. She gets the dog wet, then it runs away again. | The home that my parents had when I was in school was a lot nicer than my house now because the house is trashy. |
| WinoGrande| None                                                                      | horrible that he let his friends down on the camping trip.                |
| Social IQa | Jordan was in charge of taking the food on the camping trip and left all the food at home. Jordan felt horrible that he let his friends down on the camping trip. |                                                                 |
| PIQA     | Make Halloween lanterns.                                                  | Draw ghost faces on empty milk bottles, put a candle in each one.         |

Table 2: Examples of the prompt $x$ and the correct answer $y$ in different benchmarks.

### 2.3 The Case for Zero & Few-shot Evaluation

Following prior work (Brown et al., 2020; Patwary et al., 2021), we perform zero-shot or few-shot evaluations of pre-trained LMs, without fine-tuning the model on task-specific commonsense supervision. We outline two reasons for this choice: First, we aim to evaluate the extent to which the foundation model (i.e., the underlying pre-trained LM itself) is able to acquire commonsense knowledge. Fine-tuning the LM would make it hard to disentangle how much of the commonsense knowledge is acquired by the underlying LM, as opposed to the task-specific supervision afforded by the benchmark (Yogatama et al., 2019). Second, human-annotated commonsense datasets are expensive to collect due to the vast, diverse, and growing nature of commonsense knowledge (Elazar et al., 2021). By conducting our evaluation in a zero-shot and few-shot setup, we aim to understand whether training LMs on vast amounts of data can circumvent the need to collect large amounts of commonsense annotations.

### 2.4 Baselines

We compare the performance of pre-trained LMs with two baselines. The first, simple baseline is to randomly select an answer from the answer candidate choices, where the chance of selecting the correct answer is $\frac{1}{\text{number of choices}}$. We henceforth refer to this as the Random Baseline. We experiment with two other baselines: Either choosing the majority label from the training data, or choosing the longest answer. We omit these baselines as they perform similarly to the Random Baseline.

More importantly, we consider an Answer-only Baseline, where we select the highest-scoring answer choice under the LM, without conditioning on the question. More formally, this baseline considers $s_\theta(y)$, as opposed to $s_\theta(y|x)$ in Eq. 1. This baseline reveals the extent to which a pre-trained LM conducts the appropriate reasoning over the provided context to select the answer, as opposed to relying on potential surface cues or annotation artefacts that make the correct answer $a$ priori more probable than the rest. We illustrate this baseline at the top of Fig. 1; for WinoGrande, we calculate the cross-entropy of each candidate entity (e.g., “house” in Table 2). Ideally, each answer choice should be equally likely if we do not consider the question, and the Answer-only performance should be close to the Random baseline. Similar hypothesis-only baselines are well-studied for natural language inference datasets (Poliak et al., 2018); Trichelair et al. (2019) further explored such an Answer-only baseline, albeit only on the SWAG commonsense benchmark (Zellers et al., 2018).

### 3 Zero-shot Performance

In Fig. 2, we report the zero-shot performance of our pre-trained LM (with 7B parameters, §2.2) on the four commonsense benchmarks, alongside: (i) the Random and Answer-only baselines, and (ii) the current state-of-the-art (SOTA) result. The SOTA results are achieved by the UNICORN (Lourie et al., 2021) model with 11B parameters, which is pretrained on 6 existing commonsense datasets (Zellers et al., 2019; Bisk et al., 2020b; Sap et al., 2019b; Sakaguchi et al., 2020; Bhagavatula et al., 2020; Huang et al., 2019).

**Zero-shot performance.** At first glance, we observe strong zero-shot results, outperforming the Random Baseline in all benchmarks (compare “Rand” and “ZS” in Fig. 2). Nevertheless, the gap between the stronger Answer-only baseline and the zero-shot result is much smaller (compare “An-
Answer-only bias. As shown in Fig. 3, the performance gap between the Random and Answer-only baselines is large for HellaSwag and PIQA, where the Answer-only baseline outperforms the Random baseline by more than 26% and 21%, respectively. This large performance gap highlights an existing answer-only bias in these benchmarks: The correct answer can, in fact, be selected by the LM without conducting the appropriate commonsense reasoning over the provided context. On the other hand, the Answer-only baseline performs similarly to the random baseline on WinoGrande and Social IQa; hence the zero-shot performance on these datasets yields a more reliable estimate of the model’s acquisition of the relevant knowledge. Hence, given the existing (and sometimes inevitable) answer-only biases in some benchmarks, it is important to contextualize the zero-shot results by comparing with strong baselines, although such comparisons are sometimes missing from recent work (e.g., Zhou et al., 2020; Brown et al., 2020).

Familiarity with the evaluation benchmarks. To what extent does the model’s zero-shot performance depend on the LM’s familiarity with the evaluation benchmark (e.g., because the benchmark is more similar to the pre-training data), which is a potential confounder for commonsense evaluation? To this end, for each benchmark, we calculate the perplexity of each question and the correct answer choice under the pre-trained LM; a lower perplexity indicates that the model is likely more familiar with the style, domain, and format of the evaluation benchmark. To calibrate results across different datasets, we report the gap between the zero-shot and the Random Baseline, because achieving a higher performance is more difficult for benchmarks that have more answer choices. As shown in Fig. 4, lower perplexity often correlates with a higher zero-shot performance. Hence, the LM’s
performance is also a function of its familiarity with the style, domain, and format of the evaluation tasks, which can be difficult to disentangle from the LM’s commonsense knowledge.

4 Robustness of Reported Results

Different evaluation design choices — such as the format of the prompt or the choice of score functions — can impact the LM’s zero-shot performance, and crucially result in different conclusions about a model’s commonsense understanding ability. Moreover, the lack of a standardized zero-shot LM evaluation protocol makes direct comparisons between papers difficult (Bosselut et al., 2021; Shwartz et al., 2020). Here we perform a set of experiments to shed light on the effects of these design choices on model performance.

Score functions. Previous work employed different score functions to assess the plausibility of each answer choice given a question (Brown et al., 2020; Shwartz et al., 2020; Bosselut et al., 2021; Holtzman et al., 2021), which makes a direct comparison between different results challenging. Here we investigate the impact of different score functions on the reported performance. In addition to cross-entropy (defined in §2.2), we experiment with two other score functions. The first is sequence log probability, defined as the log probability of the answer choice \( y \) given the question \( x \), where \( y_i \) is the \( i \)-th token in the answer \( y \).

\[
s(y|x) = \sum_{i=0}^{||y||} \log(p(y_i|x, y_0...y_{i-1}))
\]  

Another widely used score function (Bosselut et al., 2021; Holtzman et al., 2021) is point-wise mutual information. This score function takes into account the probability of the answer choices alone, and the probability of the answer choices conditional on the question. This metric assesses whether the question adds additional information, as commonsense reasoning should be established within the context of the question. As this score function accounts for the prior probability of answer options, it can yield lower accuracy than score functions like cross-entropy that do not account for such factor (Answer-only baseline, §2.4).

\[
s(y|x) = PMI(y, x) = \log \frac{p(y|x)}{p(y)}
\]  

Table 3 shows the performance difference between the worst and best design choices for each benchmark.

| Benchmark       | Worst | Best | Difference |
|-----------------|-------|------|------------|
| HellaSwag       | 50.8  | 70.5 | 19.7       |
| PIQA            | 62.5  | 78.7 | 16.2       |
| Social IQa      | 43.9  | 48.5 | 4.6        |
| WinoGrande      | 59.7  | 62.0 | 2.3        |

Table 3: The performance difference between the worst and best design choices for each benchmark.

Prompt format. Another important factor is the format of the prompt; here we consider a few such choices. In addition to the concatenation of the question and the answer, we experiment with adding special symbols "[Question]" and "[Answer]" to specify the question and the answer (Brown et al., 2020). Moreover, for Social IQa and PIQA, we experiment with a set of predefined rules (taken from Shwartz et al., 2020) to convert the questions into sentences, which are closer to the LM’s pre-training data format. Finally, we find that having the correct lower/upper case and punctuation is important; thus we manually checked all benchmarks to correct for case and punctuation.\(^5\)

Scored text. The next option is whether to score the entire question–answer pair (Shwartz et al., 2020), or only the answer choice (conditional on the given question as prefix) as done by Brown et al. (2020) i.e., whether to calculate \( s(x; y) \) or \( s(y|x) \), where ; implies text concatenation.

4.1 Do These Design Choices Matter?

Table 3 shows the performance difference of using the worst versus the best design choices, which are independently optimized for each task. To sweep over the above design choices, instead of considering all combinations of parameters, we iterate the options in one category (e.g., score function), while fixing the parameters in the other categories.\(^6\)

Overall, we observe a difference between the best and worst settings on all benchmarks; this gap is especially large for HellaSwag and PIQA. This result shows that large language models do

\(^5\)Recent work automatically learns the prefix that would maximize performance (e.g., Li and Liang, 2021). Here we focus on evaluation setups with no parameter updates, and leave this extension to future work. Furthermore, our findings indicate that the choice of score function — which is not covered by lightweight fine-tuning approaches like prefix tuning — is more important than the format of the prompt (§4.1).

\(^6\)This decision saves compute resources, while offering a lower bound on the performance variations. Our goal here is not to seek the highest achievable performance, but to understand how much performance varies across different settings.
not simply work out of the box for some common-sense benchmarks, because for some tasks, these evaluation design choices can account for a large variation in model performance. We find that the score function plays the most important role — cross-entropy yields the highest accuracy values across most benchmarks, but sequence log probability achieves a slightly better performance for WinoGrande. However, as discussed in §3, when using these scores, it is important to account for the Answer-only baseline performance. Moreover, converting questions to sentences makes the largest difference for Social IQa. We also find that scoring the answer conditional on the question works best, except for WinoGrande, which has no questions.

Moving forward, we call on the community to establish a standardized zero-shot evaluation protocol, which enables a fair comparison across papers. Alternatively, when comparing models, it is important to perform similar sweeps for these design choices, explicitly state the choices used for each task, and indicate how much variance exists in the observed results across different design choices.

4.2 The Answer-Length Bias

Intriguingly, for some datasets, both sequence log probability (Eq. 2) and cross-entropy (Eq. 1) are impacted by the answer length. Fig. 5 shows the relationship between answers’ length and their cross-entropy for PIQA. We observe that cross-entropy tends to assign higher scores to longer answers; to varying extent, this pattern holds for 3 out of the 4 datasets, PIQA, Social IQa, and WinoGrande. We attribute this pattern to the higher probability assigned to subsequent tokens in the sequence, as such tokens have the most context and thus can be more easily predicted than tokens in the beginning of the answer. As longer answers have more such easier-to-predict tokens, their cross-entropy tends to be lower.

This pattern is often reversed in metrics such as sequence log probability, where shorter sequences can have higher scores (Koehn and Knowles, 2017; Stahlberg and Byrne, 2019). On these benchmarks, the longest-answer baseline performs on par with the Random baseline, although future work should also check for correlations between answer length and correctness, especially with length-sensitive score functions like cross-entropy and sequence log probability.

5 Does Increasing Model Size Help?

Recent work has achieved remarkable language modelling progress — in terms of perplexity and downstream performance — by using ever-larger LMs (Kaplan et al., 2020; Brown et al., 2020; Patwary et al., 2021). In light of this trend, should we expect LMs to eventually reach new SOTA performance — which is currently achieved by fine-tuned models that use task-specific commonsense supervision — by training ever-larger models in an unsupervised fashion, and from text input alone?

**Setup.** Here we vary the language model size from 44M up to 7B parameters, where the model with 7B parameters — the basis for our prior experiments — serves as the largest one. We expect our findings to generalize to larger models beyond 7B parameters, although due to computational costs, we leave it to future work to firmly establish whether this is the case.

**Discussion.** We present the findings in Table 4. On all four benchmarks, the LM’s zero-shot performance (Table 4, ZS column) consistently gets better as we use increasingly larger models. At first glance, this finding is consistent with that of Brown et al. (2020), who showed that larger models have better performance at HellaSwag, WinoGrande, and PIQA. But, crucially, we argue that this does not necessarily mean that larger models learn more commonsense: For HellaSwag and PIQA, the Answer-only baseline also substantially improves with model size (Table 4, Ans column). Hence, for these two benchmarks, larger models are also better at exploiting potential surface cues and annotation artefacts to guess the correct answer, without reasoning over the context.

Based on this observation, we reemphasize that, to properly assess commonsense reasoning ability,
we should look beyond the zero-shot performance, and instead focus on the performance difference between the zero-shot setup and the Answer-only baseline (§3). We plot this performance difference with respect to different model sizes in Fig. 6, based on which we remark on two observations. First, larger models generally have better performance across benchmarks — when increasing model size, the zero-shot performance gains are more than the performance gains of the Answer-only baseline. Nevertheless, the magnitude of this improvement varies considerably: We observe substantial improvements on HellaSwag, and smaller improvements on WinoGrande, Social IQa and PIQA.

Crucially, even when model size does improve the gap between the zero-shot result and the Answer-only baseline (Fig. 6), the gains are not necessarily substantial compared to the increase in model size (i.e., diminishing returns): Increasing model size 5-fold from 1.3B to 7B parameters results in an at most 3.5% improvement (Fig. 6, left). This suggests that simply adding more parameters and training ever-larger models may not help us reach substantially better performance, although a rigorous investigation of even-larger models remains within the realm of future work.

6 Few-shot Evaluation

Recent work has shown that large LMs can perform surprisingly well at various tasks in a few-shot fashion (Brown et al., 2020; Patwary et al., 2021). Under this setup, the model is provided with $n$ examples of the downstream task, which are then appended to the prefix. Concretely, for the four commonsense benchmarks, we append $n$ examples that include: (i) the question and (ii) the correct answer; these examples—which are randomly sampled from the training split of each benchmark—appear before the evaluated question, as shown in Fig. 1. This few-shot formulation is indeed an appealing one, as it relies only on a small number of task-specific examples to get the LM accustomed to the task, without any fine-tuning. To what extent can we improve the model performance on commonsense benchmarks, by shifting from the zero-shot to the few-shot evaluation protocol?

Discussion. In Fig. 7, we compare the performance of the same 7B model, under three different evaluation protocols: (i) zero-shot, (ii) few-shot with 1 example, and (iii) few-shot with 10 examples, based on which we remark on two key observations. First, on most datasets (HellaSwag, WinoGrande, and PIQA), we do not observe substantial improvement from few-shot evaluation compared to the zero-shot baseline, which does not benefit from any task-specific examples. In fact, model performance with few-shot (1) is sometimes worse than the zero-shot model, although we find that the few-shot (10) model mostly outperforms its zero-shot counterpart (by small margins). Second, while few-shot evaluation does not help much for most datasets, the only exception is Social IQa, where the few-shot (10) model substantially outperforms the zero-shot model by a $> 4\%$ margin.

![Figure 6: The difference between zero-shot performance and Answer-only baseline.](image)

| Dataset     | 44M | 117M | 400M | 1.3B | 7B  |
|-------------|-----|------|------|------|-----|
| HellaSwag   | 26.9| 29.6 | 32.1 | 35.2 | 35.2|
| WinoGrande  | 50.4| 52.4 | 53.0 | 53.7 | 52.0|
| Social IQa  | 36.5| 42.3 | 43.6 | 45.5 | 42.6|
| PIQA        | 62.7| 64.5 | 64.2 | 63.7 | 63.7|

Table 4: Performance of all models across the four benchmarks under different experimental settings. **Ans**: Answer-only Baseline; **ZS**: zero-shot performance; **FS($n$)**: few-shot performance where $n$ is the number of examples used.
We attribute this to the less natural text of Social IQa (see the higher perplexity in Fig. 4); hence adding task-specific examples provides valuable information about what is expected of the task.

We acknowledge that our pattern of results differs from that of Brown et al. (2020): They observe larger improvements under the few-shot setup for WinoGrande (7.5% accuracy gain). However, they similarly find small accuracy improvements for PIQA and HellaSwag (less than 1.5%). This difference could be explained by two factors: (i) GPT3 is a much larger LM with 175B parameters (vs. our 7B). We thus conjecture that the large gains from few-shot learning mostly apply to much larger models than the 7B model that we use here. (ii) GPT3 uses a larger number of few-shot samples ($n = 50$), while we limit our experiments to $n = 10$. Except for Social IQa, we observe little improvement from increasing the number of prefixes from $n = 1$ to $n = 10$; hence we do not append more than 10 examples.

7 Commonsense Knowledge Bases

Given the implicit nature of commonsense knowledge, a language model’s pretraining corpora might not contain all of the supporting evidence that is required to answer commonsense understanding questions — a phenomenon widely known as the reporting bias problem (Gordon and Van Durme, 2013). Thus, prior work has proposed to use external knowledge bases for improving the zero-shot performance of LMs on commonsense benchmarks (Bosselut et al., 2021; Bauer and Bansal, 2021). These approaches are particularly interesting, as the knowledge base augmentation only happens at test time, rendering this approach compatible with any pretrained generative LM. While prior work has shown the effectiveness of this approach over the zero-shot baseline that lacks access to commonsense knowledge bases (CSKBs), we find that the performance of the baseline model is highly sensitive to certain evaluation design choices (§4). A natural question, therefore, is the following: If we carefully optimize the evaluation design choices of the baseline model, would we still observe similar improvements through CSKB augmentation?

Setup. To answer this, we replicate prior work by adding commonsense knowledge base entries at test time, as shown in Fig. 1: such knowledge base triplets can potentially provide the relevant implicit commonsense knowledge that makes the correct answer more likely than the rest. To ensure the generality of our findings, we apply this approach to multiple model sizes that we explored in §5. Here we consider the pre-extracted knowledge base triplets that are made publicly available by Shwartz et al. (2020). We use a similar score function as Shwartz et al. (2020), where, for each answer choice $y \in Y(x)$, we choose the knowledge base triplet that yields the highest score:

$$s_{kg}(y|x) = \sum_{t \in T} s(y; t|x) \approx \max_{t \in T} s(y; t|x),$$

where $s(y; t|x)$ denotes the cross-entropy of the concatenated answer choice $y$ and the extracted knowledge base triplet $t$, conditional on the question/context $x$. Here $T$ denotes the set of all extracted commonsense knowledge triplets (see Fig. 1, KG-enhanced for an example, where KG is generated from Comet (Bosselut et al., 2019)). One key difference is that we score the answer and knowledge base triplet conditional on the question, while Shwartz et al. (2020) scored the concatenation of question, answer, and triplet instead.

We experimented with other score functions, such as appending the extracted knowledge base triplets to the question instead of the answer, although this approach does not yield better results than the one proposed by Shwartz et al. (2020).
|        | ZS      | w/t Comet | w/t Atomic | w/t CN |
|--------|---------|-----------|------------|--------|
| 44M    | 42.3    | 42.9      | 42.3       | 40.6   |
| 117M   | 43.6    | 44.0      | 43.6       | 42.2   |
| 400M   | 46.3    | 46.8      | 44.7       | 44.1   |
| 1.3B   | 47.0    | 46.8      | 46.4       | 44.7   |
| 7B     | 48.5    | 48.6      | 47.5       | 46.1   |

Table 5: Zero-shot performance on Social IQa when using different knowledge bases. GPT2 results are taken from Shwartz et al. (2020). ZS: zero-shot performance; CN: ConceptNet.

In Table 5, we summarize our results on Social IQa, which has the highest gap between the zero-shot and SOTA performance (Fig. 2). We compare our results with those of Shwartz et al. (2020), who used GPT2 as the base model. Our results in Table 5 provide an interesting contrast to the findings of Shwartz et al. (2020): Our baseline zero-shot model with 1.3B parameters achieves an accuracy of 47.0% on Social IQa, substantially outperforming the reported GPT2 result of Shwartz et al. (2020) — which achieves 41.1% — despite the fact that GPT2 has more parameters (1.5B vs our 1.3B). In fact, the same 1.3B zero-shot model — which does not benefit from any commonsense knowledge base triplets — nearly matches the performance of GPT2 augmented with Comet (Bosselut et al., 2019) (47.0% for our zero-shot 1.3B model vs 47.5% for GPT2 augmented with COMET; Table 5), and also outperforms the GPT2 model that is augmented with self-talk. Nevertheless, we find that adding knowledge base triplets fails to yield substantial improvements for our models; this finding is consistent across three different knowledge bases and five model sizes. On the contrary, adding such knowledge base triplets can occasionally decrease performance compared to the zero-shot baseline.

We remark on two significant aspects of our findings. First, it is important to compare proposed improvements against strong, well-tuned baselines (Henderson et al., 2018; Melis et al., 2018), which can achieve surprisingly competitive performance. We identify the choice of the scored span as a particularly important design choice: While Shwartz et al. (2020) scored the GPT2 model on the concatenation of both question and answer, we instead calculate the cross-entropy of the answer given the question. Second, certain improvements that are observed under a particular set of evaluation design choices may not necessarily be replicated under a different set. This finding reiterates the importance of explicitly stating the evaluation design choices used in each experiment, and identifying whether or not the observed improvements are robust across different evaluation design choices (§4).

### 8 Related Work

While recent work evaluates LMs against commonsense benchmarks in a zero-shot and few-shot fashion, they do not examine the extent to which model performance can be attributed to superficial cues or annotation artefacts in a given dataset (e.g., through an Answer-only baseline); nor do they quantify how robust the model performance is under different evaluation design choices, which can materially impact the LM’s reported performance. Another line of prior work probed for commonsense knowledge in LMs through knowledge base completion (Petroni et al., 2019; Davison et al., 2019) or manually designed probing tasks (Weir et al., 2020; Shwartz and Choi, 2020). In contrast, we focus on the zero and few-shot performance of LMs on a diverse set of established commonsense benchmarks. Another related line of work aims to improve the zero-shot performance of language models on commonsense benchmarks, either using external commonsense knowledge bases or the output generated by the LM (Bosselut et al., 2021; Ma et al., 2021; Shwartz et al., 2020; Paranjape et al., 2021; Holtzman et al., 2021). Recently, Trichelair et al. (2019); Elazar et al. (2021) also investigated the existence of superficial cues or annotation artefacts in commonsense co-reference resolution benchmarks (Levesque et al., 2012; Sakaguchi et al., 2020) and SWAG (Zellers et al., 2018); here we conduct a more comprehensive investigation on four commonsense benchmarks. Lastly, Zhou et al. (2020) evaluated pre-trained LMs against multiple commonsense benchmarks; they then proposed a new evaluation dataset requiring multi-hop reasoning. Here we focus on a systematic study of existing benchmarks, and establish recommendations and best practices for evaluating commonsense understanding in a zero-shot and few-shot fashion.

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*By similarly tuning the evaluation design choices, we achieved 46.7 when evaluating GPT2 in the zero-shot setting.*
9 Conclusion

We conducted a systematic and rigorous study of LM performance on a diverse set of commonsense benchmarks, in a zero-shot and few-shot fashion. While pre-trained LMs can seemingly achieve a good zero-shot performance on these benchmarks, these results can be partially attributed to the LM’s ability to exploit potential surface cues and annotation artefacts to guess the correct answer, without reasoning over the provided context. We further observed that substantially increasing model size yields rather small improvements on commonsense benchmarks. Hence, we conjecture that further progress will rely on innovations that go above and beyond increasing model size, such as multi-modal grounding, explicit commonsense supervision, or physical embodiment. In addition, model performance can be highly sensitive to certain evaluation design choices (particularly the score function and which span of text to score); improvements that hold under a set of evaluation design choices may not necessarily hold under different ones. More broadly, we call on the community to: (i) carefully select these evaluation design choices, (ii) report the variance in performance across different design choices, and (iii) establish a standardized evaluation protocol for evaluating pre-trained generative LMs on multiple choice questions, hence facilitating a fair comparison across different papers.

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