When Combating Hype, Proceed with Caution

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Abstract

In an effort to avoid reinforcing widespread hype about the capabilities of state-of-the-art language technology, researchers have developed practices in framing and citation that serve to deemphasize the field’s successes. Though well-meaning, these practices often yield misleading or even false claims about the limits of our best technology. This is a problem, and it may be more serious than it looks: It limits our ability to mitigate short-term harms from NLP deployments and it limits our ability to prepare for the potentially enormous impacts of more distant future advances. This paper urges researchers to be careful about these claims and suggests some research directions and communication strategies that will make it easier to avoid or rebut them.

1 Introduction

Over the last few years, natural language processing has seen a wave of surprising negative results overturning previously-reported success stories about what our models can do and showing that widely-used models are surprisingly brittle (Jia and Liang, 2017; Niven and Kao, 2019; McCoy et al., 2019). This shows that many of our standard practices for evaluation and reporting often lead to unrealistically positive initial claims about what we can do. The resulting hype and overclaiming, whether intentional or not, is a problem. Hype and overclaiming can encourage the reckless deployment of NLP systems in high-stakes settings where they can do significant harm. They also threaten the health and credibility of NLP as a research field, and thereby threaten our ability to influence applied stakeholders or attract funding.

Fortunately, these results have led to a surge of research and writing that proposes more thorough and cautious practices for the evaluation of model ability (Ribeiro et al., 2020; Gardner et al., 2020; Kiela et al., 2021; Bowman and Dahl, 2021). While we have only a limited ability to control the public narrative taking place through industry PR and the media, there’s reason to be hopeful that we researchers are getting much better at avoiding the worst forms of overconfidence about our systems.

Less fortunately, these discouraging results seem to have produced a trend toward pessimism about model performance that is often ungrounded from real empirical results. This leaves room for the research community’s consensus about a model’s capabilities to fall short of its actual capabilities.

This is more dangerous than it might seem. It risks our credibility and thereby limits our ability to influence stakeholders in cases where our current systems are doing real harm. It also limits our ability to accurately forecast and plan for the impacts that may result from the deployment of more capable systems in the future. If we can truly reach near-human-level performance on many of the core problems of NLP, we should expect these impacts to be enormous, and potentially catastrophic if not planned for.

I call this issue underclaiming, for lack of a better term. In this paper, I lay out four types of underclaiming, focusing especially on writing and citation practices in NLP. I then argue that this

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Hey #deeplearning skeptics, Why complain about “neural nets hype” when you could be calling it “backpropagenda”? 12:09 PM · Mar 5, 2015 · Twitter Web Client

Figure 1: Hype is a problem. The opposite of hype isn’t necessarily better.

Note: While overclaiming generally refers to overstating the effectiveness of one’s own methods or ideas, the phenomenon that I call underclaiming often involves downplaying the effectiveness of preexisting methods or ideas.
is a problem: It hurts the short-term interests of NLP researchers, it limits our ability to mitigate the current real-world harms of language technology deployments, and it limits our ability to adequately prepare for longer-term harms involving economic disruption or catastrophic alignment failures. I close by sketching some ways of reducing the prevalence of this kind of underclaiming, including straightforward best practices in writing and evaluation, a proposed new rule of thumb for writing and reviewing, improvements to tooling for analysis and benchmarking, and as more ambitious research agendas in model performance forecasting and test set design.

2 Underclaiming: Case Studies

This paper addresses the possibility that in our effort to avoid overconfidence, we have started to err in the opposite direction: Published work increasingly claims or implies that state-of-the-art systems are less capable than they actually are. This has taken several forms, including misleading presentations of valid negative results from weak or dated baseline models, misleading claims about the limits of what is conceptually possible with machine learning, misleading reporting of results on adversarially collected data.

2.1 Negative Results with Weak or Dated Baselines

In spite of many surprises and setbacks, NLP research seems to have made genuine progress on many problems over the last few years. In light of this, discussions about the limitations of systems from past years don’t straightforwardly apply to present systems. The first two cases that I present involve failures to contextualize claims about the failures of weaker past systems:

Adversarial Examples for SQuAD Jia and Liang (2017) published one of the first prominent demonstrations of adversarial failures in neural-network-based NLP, showing that a simple algorithm could automatically augment examples from the SQuAD benchmark (Rajpurkar et al., 2016) in a way that would fool many state-of-the-art systems, but not humans. This work prompted a wave of much-needed analysis and a corresponding lowering of expectations about the effectiveness of neural network methods.

However, citations to this work have since taken on a strange role in the literature. In particular, the results in this paper predate the development of modern pretraining methods in NLP (Peters et al., 2018; Radford et al., 2018; Devlin et al., 2019). The best systems studied in Jia and Liang have more than twice the error rate of current state-of-the-art systems, and while I am not aware of any results from current state-of-the-art systems on this data, results from 2019 systems suggest that we are making substantial progress (Table 1). We have no reason to expect, then, that the failures documented in this work are quantitatively or qualitatively similar to the failures of current systems.

However, papers that cite these results often present them with no discussion of the model under study, yielding misleading implications. For example, the award-winning work of Linzen (2020) uses the Jia and Liang result to justify this claim:

Table 1: F1 results for the best-performing SQuAD model studied by Jia and Liang—ReasoNet (Shen et al., 2017)—and for thenewer BERT and XLNet models (Devlin et al., 2019; Yang et al., 2019) reported by Zhou et al. (2020) on the original SQuAD development set and the two Jia and Liang adversarial evaluation sets. While I am not aware of results from more recent models on the this data, progress through 2019 had already cut error rates in half.
tribution that differs from the one they were trained on, their behavior is unpredictable and inconsistent with that of humans.

The chief concern in this context is the claim that this failure applies to current deep learning systems in general, and the corresponding unjustified implication that these failures are a fundamental or defining feature of neural network language models. Looking only to highly-cited works from the last two years that cite Jia and Liang, similar statements can be found in Xu et al. (2020), Zhang et al. (2020), and others.

**The Long Shadow of BERT** While the case of Jia and Liang is especially striking since it deals with models that predate pretraining entirely, a similar effect is much more common in a subtler form: Most analysis papers that identify limitations of a system come out well after the system description paper that claims the initial (typically positive) results. BERT, first released in Fall 2018, has been a major locus for this kind of analysis work, and continues to be a focus of such work long after its release. Looking to a random sample of ten papers from the NAACL 2021 analysis track that study existing systems, none of them analyze models that have come out since summer 2019, and five only study BERT, representing a median lag of nearly three years from the release of a model to the publication of the relevant analysis.

This trend has consequences for the conclusions that one would draw from a broad review of the recent literature on some problem: A review of that literature will contrast the successes of the best current systems against the weaknesses of the best systems from an earlier period. In many cases, these weaknesses will be so severe as to challenge the credibility of these successes if they are not properly recognized as belonging to different model generations.

This analysis work is nonetheless often valuable and these long timelines are sometimes justifiable: Good analysis work takes time, and researchers doing analysis work often have an incentive to focus on older models to ensure that they can reproduce previously observed effects. Even so, this three-year lag makes it easy to seriously misjudge our progress.

The BERT-only results, though, represent a clear missed opportunity: There exist newer models like RoBERTa and DeBERTa (Liu et al., 2019; He et al., 2020) which follow nearly identical APIs and architectures to BERT, such that it should generally be possible to reuse any BERT-oriented analysis method on these newer models without modification. In many cases, these newer models are different enough in their performance that we should expect analyzing them to yield different conclusions: For example, BERT performs slightly worse than chance on the few-shot Winograd Schema commonsense reasoning test set in SuperGLUE (Levesque et al., 2011; Wang et al., 2019), while the newer DeBERTa reaches a near-perfect 96% accuracy.

### 2.2 Strong Claims about Limits to Understanding

The influential work of Bender and Koller (2020) is centered on the argument that:

> The language modeling task, because it only uses form as training data, cannot in principle lead to learning of meaning.

The proof of this claim is straightforward and convincing under some (but not all) mainstream definitions of the word meaning in the context of NLP: If meaning deals with the relationship between language and some external nonlinguistic reality, then a system that can only interact with the world through language cannot manipulate meanings.

This argument does not on its own make any prediction about the behavior of these models on tasks that take place entirely through the medium of language. On the definitions of meaning that must be assumed for this argument, tasks like summarization or translation can in principle be solved without reference to meaning: Under this definition, a translation model would be acting without reference to meaning even if it has a rich, structured internal model of the world, and it interprets sentences with reference to that model when translating: As long as that model of the world was developed solely using language, and does not otherwise ground out to perception or some other nonlinguistic modality, no meaning has been used. See
Merrill et al. (2021) for some limits on how closely such a model can correspond to the real world and Bommasani et al. (2021, §2.6.3) for further discussion of the implications of Bender and Koller’s arguments for NLP.

In addition, this argument does justify any strong prediction about the behavior of models which are trained primarily, but *not exclusively*, on a language modeling objective, as with models that are fine-tuned to produce non-textual outputs like labels, or models which are trained in a multimodal language-and-vision regime.

While this core claim is sound and important, public discussion of the paper has often repeated the claim in ways that imply stronger conclusions about model behavior. Utama et al. (2020), for example, write that:

Researchers have recently studied more closely the success of large fine-tuned LMs in many NLU tasks and found that models are simply better in leveraging biased patterns instead of capturing a better notion of language understanding for the intended task (Bender and Koller, 2020).

In another vein, Jang and Lukasiewicz (2021) make the straightforward claim that Bender and Koller (2020) show that it is impossible to learn the meaning of language by only leveraging the form of sentences.

but they then use that claim to motivate a new regularization technique for language models, which does nothing to change the fact that they are trained on form alone. In this context, it is hard to avoid the inference that Bender and Koller must have shown something specific and contingent about recent language models. In fact, they make a more general point about the nature of meaning that applies equally to any purely form-based language model, no matter what objective it uses or how well it performs.

Similar claims can be found in many other citing works (Utama et al., 2020; van Noord et al., 2020; Hovy and Yang, 2021; Sayers et al., 2021; Peti-Stantić et al., 2021; Jang and Lukasiewicz, 2021). While Bender and Koller raise important points for discussion, these strong inferences in citing works are misleading and potentially harmful.

2.3 Adversarially Collected Data

*Adversarially collected* test sets (Bartolo et al., 2020; Nie et al., 2020; Kiela et al., 2021)—or test sets composed of examples that some target system gets wrong—have recently become a popular tool in the evaluation of NLP systems. Datasets of this kind are crowdsourced in a setting where an example-writer can interact with a model (or ensemble) in real time and is asked to explore possible ways of writing examples until they find an example on which the model fails. Writers are generally paid or rewarded only for these failure cases, and the test section(s) of the final released dataset will generally consist *exclusively* of these failure cases.

This process produces difficult test sets, and it can be a useful tool in understanding the limits of existing training sets or models (Williams et al., 2020). However, the constraint that a specified system *must* fail on the test examples makes it difficult to infer much from absolute measures of test-set performance. In particular, as long as a model makes *any* errors at all on some possible inputs, then we expect it to be possible to construct an adversarial test set against it, and we expect it to achieve zero test accuracy on that test set. We can further infer that any models that are sufficiently similar to the adversary should also perform very poorly on this test set, regardless of their ability.

What’s more, in many NLU data collection efforts, a significant fraction of cases of annotator disagreement represents subjective judgment calls, rather than annotator errors (Pavlick and Kwiatkowski, 2019). This means that even a perfectly careful and perfectly well-qualified human annotator should be expected to disagree with the majority judgment on some examples, and will thereby be coded as having made errors. It is therefore possible to create an adversarial test set for which our perfect human annotator would reliably achieve 0% accuracy.

Adversarially collected datasets are increasingly commonly used as test sets in standard experimental paradigms, and these caveats about the interpretation of results are not always clear when numbers are presented. I focus on Nie et al. (2020) as a case study: Sampling papers that cite this work, it is easy to find references that do not mention the adversarial design of the data and that make claims
that are hard to justify. Talmor et al. (2020) use the results from Nie et al. to claim that “LMs do not take into account the presence of negation in sentences”, and Hidey et al. (2020) use them to justify the claim that “examples for numerical reasoning and lexical inference have been shown to be difficult.” Liu et al. (2020) similarly use absolute results on the adversary models to back up the trivial but easily-misread claim that BERT-style models “may still suffer catastrophic failures in adversarial scenarios.” McClelland et al. (2019) use absolute results on Nie et al.’s ANLI data to make similar claims about the limitations of GPT-3 (Brown et al., 2020).

3 A Word on Hype

The previous section has laid out some ways in which the mainstream NLP research community often makes unjustifiable claims about the limitations of state-of-the-art methods. These claims do not make the opposite phenomenon, hype, any less real or any less harmful.

While hype is likely most severe in industry PR and in the media, it is nonetheless still prevalent in the research literature. In one especially clear example, a prominent paper claiming of human parity in machine translation performance (Hassan et al., 2018) severely overstates what has been accomplished relative to commonsense intuitions about what a human-level translation system would do (Toral et al., 2018; Läubli et al., 2018; Zhang and Toral, 2019; Graham et al., 2020).

I do not aim to argue that overclaiming or hype is acceptable or safe. However, unlike the underclaiming that this paper focuses on, hype is receiving substantial attention, and we have some established tools and norms within peer review to combat it. Finally, combating hype should be fully compatible with the goals laid out in this paper, and broad-based efforts to improve our practices in evaluation, analysis, writing, and forecasting should help reduce both underclaiming and hype.

4 Why Underclaiming is Harmful

Research papers are generally most useful when they’re true and informative, and a research field that allows misleading claims to go unchallenged is likely to lose credibility with serious funders, reporters, or industry stakeholders. This is the most obvious reason that we should be concerned about underclaiming, but it is not the whole story.

This loss of insight and loss of credibility can seriously challenge our ability to anticipate, understand, and manage the impacts of deploying NLP systems. This is especially true of impacts that are contingent on NLP technologies actually working well, which we should expect will become more substantial as time goes on.

4.1 Present-Day Impact Mitigation

The deployment of modern NLP systems has had significant positive and negative impacts on the world. Technical researchers in NLP have an ethical obligation to inform (and if necessary, pressure) stakeholders about how to avoid or mitigate the negative impacts while realizing the positive ones.

Most prominently, most applied NLP models show serious biases with respect to legally protected attributes like race and gender (Bolukbasi et al., 2016; Rudinger et al., 2018; Nangia et al., 2020; Bender et al., 2021). We have no reliable mechanisms to mitigate these biases, and no reason to believe that they will be satisfactorily resolved with larger scale. Worse, it is not clear that even superhuman levels of fairness on some measures would be satisfactory: Fairness norms can conflict with one another, and in some cases, a machine decision-maker will be given more trust and deference than a human decision-maker would in the same situation (see, e.g., Rudin et al., 2020; Fazelpour and Lipton, 2020). We are standing on shaky moral and political grounds when we deploy present systems in high-impact settings, but they are being widely deployed anyway (e.g. Dastin, 2018; Nayak, 2019; Dansby et al., 2020). Beyond bias, similar present-day concerns can be seen around issues involving minority languages and dialects, deceptive design, and the concentration of power (Joshi et al., 2020; Bender et al., 2021; Kenton et al., 2021, §3.3).

Persuading the operators of deployed systems to take these issues seriously, and to mitigate harms or scale back deployments when necessary, will be difficult. Intuitively, researchers concerned about

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3 Focus here about claims about the absolute performance level of models. Whether adversarially collected test sets are appropriate for comparing the relative effectiveness of models is a largely orthogonal issue (Bowman and Dahl, 2021; Kaushik et al., 2021).

4 Consider the 2017 Huffington Post headline “Facebook Shuts Down AI Robot After It Creates Its Own Language.”
these harms may find it appealing to emphasize the limitations of models in the hope that this will discourage the deployment of harmful systems. This is appropriate when done carefully, and could be a way in which the field’s increasing focus on model analysis could reduce some of these harms.

However, this kind of strategic underclaiming can backfire if done sloppily: Models are often both useful and harmful, especially when the operator of the system is not the one being harmed. If the operator of some deployed system sees firsthand that the system is effective for their purposes, they have little reason to trust researchers who argue that that same system does not understand language, or who argue something similarly broad and negative. They will then be unlikely to listen to those researchers’ further claims that such a system is harmful, even if those further claims are accurate.

4.2 Longer-Term Risks

We can reasonably expect NLP systems to improve over the coming decades. Even if intellectual progress from research were to slow, the dropping price of compute should allow us to continue to reap the benefits of larger-scale training (Kaplan et al., 2020; Brown et al., 2020). This improvement in capabilities is likely to amplify both the harms and benefits of language technology.

We have good reason to expect that this further progress in NLP will lead to upheavals in areas like education, medicine, law, and the service sector more broadly, as well as making mass surveillance and misinformation campaigns far more effective, and opening up additional new use cases that will be hard for us to foresee (Brundage et al., 2018; Tamkin et al., 2021; Bommasani et al., 2021). One can reasonably expect that the positive and negative impacts of these upheavals will far exceed the impacts that our technologies have produced to date. In turn, NLP researchers who want to ensure that their career has a net-positive impact on the world should be concerned with these longer-term developments.

It will be difficult to do the necessary technical, social, and governance work to prepare for these advances if we do not have a clear picture of our current capabilities, and it will be difficult to convince outside stakeholders to act appropriately to mitigate these risks if we don’t acknowledge that we have made, and are making, real progress toward effective language technology.

Looking somewhat further into the future, a substantial community of philosophers, economists, and general ML researchers are concerned that highly-capable AI systems, of the kind that could plausibly be developed through existing ML research paradigms, are catastrophically dangerous by default (Bostrom, 2012; Critch and Krueger, 2020; Christian, 2020; Ord, 2020; Russell and Norvig, 2020). Expert forecasts suggest that this could take place within a few decades (Grace et al., 2018). If these hypotheses hold, and if we are poorly prepared for these developments, the worst-case outcomes could be catastrophic, even threatening the continued existence of human civilization on some views.

Investments in research into these potential catastrophic risks from advanced machine learning systems have become substantial: Funding from one charitable foundation alone has totaled over $75M USD.5 Concerns about risks from AI have also been a major stated motivation for a significant fraction of the work from DeepMind and OpenAI, which both have access to even greater amounts of private funding.

Spurred on in particular by the shift in emergent capabilities from GPT-2 to GPT-3, the attention of these AI risk researchers has also been increasingly centered on language models and similar multi-modal models (Irving et al., 2018; Stiennon et al., 2020; Hendrycks et al., 2020; Kenton et al., 2021; Wu et al., 2021; Bommasani et al., 2021, §4.9). Despite the scale of this research, and its recent shift of focus toward language models, there has been little interaction between the AI risk research community and mainstream academic NLP to date.

To the extent that these concerns are valid, they represent an urgent call for reprioritization within NLP research to favor safety-relevant areas like interpretability, control, and evaluation over scaling, and to push for better oversight and regulation of large-scale research (Dafoe, 2018): Even a small risk of a globally significant catastrophe warrants a dramatic response.

On the other hand, to the extent that these concerns are unfounded or are built on misunderstandings about the possible trajectories of ML research, it would be quite valuable to correct this misunderstanding. Correcting the record could redirect these resources and, more significantly, reduce the

5https://www.openphilanthropy.org/giving/grants
risk of popular or regulatory pressure that limits large-scale ML research so severely or clumsily as to snuff out the positive potential of NLP technologies.

5 Why Worry?

Because these more speculative concerns about AI risk are rarely discussed in the NLP literature, I will here offer a brief overview of that work. Concerns around dangers from advanced artificial intelligence tend to focus on four clusters of hypotheses:

Powerful Unaccountable Organizations
Highly-capable AI is likely to lead to highly-profitable applications. Highly-capable AI should also be able to displace human labor in technical fields to a large extent, increasing the relative value of capital over labor, and making it easier for the leaders of profitable organizations to take unilateral unpopular actions. In the longer term, highly-capable AI may also contribute to the effectiveness of persuasion campaigns, further insulating these organizations from political or popular pressure. Combined with significant further technological progress, these forces could conspire to produce make the companies or governments that first produce highly-capable AI almost entirely unaccountable, and allowing their decisions to play a major role in the trajectory of humanity as a whole (Ord, 2020).

Alignment and Robustness
Even if a system is deployed by an actor with good intentions and appropriate oversight, good outcomes are not guaranteed. As AI systems become more capable, it becomes more likely that they will be trusted to make high-stakes decisions autonomously. In these cases, it becomes crucial that they behave in ways that we would endorse, even when they are pushed into unfamiliar new situations. This requires both that the systems be optimized for the right objectives and that the systems actually internalize and generalize those objectives correctly.

Specifying and using safe objectives, such that aggressively optimizing them does not produce catastrophic outcomes, is difficult (Critch and Krueger, 2020). Human preferences are complex, making the problem of specifying an objective that rules out unintended bad behavior non-trivial. Goodhart’s law, means that many objectives that serve as good proxies for what we want in in familiar situations can break down in new situations.

Further, optimizing complex systems is difficult, such that even a genuinely safe objective, if optimized powerfully by a system with an imperfect learned representation of the objective, could produce dangerous failure modes (Hubinger et al., 2019).

Finally, we should expect highly-capable AI systems to be useful in the short term, giving potential users a strong incentive to deploy them as soon as they are affordable, even if their safety is not guaranteed. This means that it is not enough that it simply be possible for us to develop safe systems, it is additionally necessary that it be nearly as easy and nearly as affordable as developing unsafe systems (Irving et al., 2018).

Risks May Be Difficult to Spot

Human-level capabilities are likely to emerge first from large machine learning models that, like modern neural networks, are not directly interpretable. This means that it may be difficult to spot ways in which a model is unsafe or forecast ways in which its behavior might change in novel settings (Critch and Krueger, 2020).

Instrumentally-Convergent Subgoals

The instrumental convergence hypothesis holds that systems that are optimizing for benign objectives, once they become sufficiently capable, have a predictable reason to take on dangerous subgoals—like accumulating large amounts of computational, economic, or political power—to maximize the odds that their primary objectives are achieved (Bostrom, 2003; Omohundro, 2008; Bostrom, 2012). Even with merely near-human-like levels of performance, the ability of computational models to be copied and accelerated gives them considerable leeway to act in un-human-like ways. Systems that interact with humans only through text, or systems whose goals are circumscribed to a well-defined task like question answering, are not exempt from this concern (Armstrong et al., 2012).

7 This is exemplified by the thought experiment of the paperclip maximizer, which points out that a machine which tasked with manufacturing as many paperclips as possible, if sufficiently capable, can be expected to turn nearly all matter on earth into paperclips. While this vision of a single system acting alone on a trivial objective is unrealistic, it demonstrates the key hypothesis that almost any reasonable-sounding goal starts to conflict with basic human needs if a sufficiently capable system pursues it single-mindedly.

6 in the formulation of Strathern (1997): “when a measure becomes a target, it ceases to be a good measure.”
So What? None of these hypotheses is simple to prove, but as far as I am aware, all have resisted straightforward attempts at falsification. All four are potentially applicable to neural network-based models and to models which operate primarily through language.

While the nascent field of AI alignment has proposed some mechanisms by which we might mitigate these risks, work in this area is still largely exploratory, with no clear research agenda in place to ensure that powerful ML models will be safe (Hadfield-Menell et al., 2016; Irving et al., 2018; Critch and Krueger, 2020; Kenton et al., 2021). If these hypotheses hold, significant further work is needed to ensure a safe long-term outcome. This will be difficult to achieve without a clear accounting of the abilities and limitations of current and plausible near-future systems. In particular, we will need enough foresight to be able to see substantial progress of this kind coming well in advance, to avoid the complacency that comes with the perception that worrying about impacts from powerful AI is like worrying about “overpopulation on mars” (Garling, 2015, quoting Andrew Ng).

6 Ways to Do Better

The core issue in this paper is one of sloppy communication about results. The most straightforward step that we can take to remedy underclaiming is to simply use the same practices that we already use to avoid overclaiming: The peer-review process already polices overclaiming to a significant extent, and most researchers have learned to be careful about overclaiming in their writing. We should apply high standards of evidence to our own empirical claims and those of others, both in peer-reviewed venues and in more informal scientific communication, even when those claims are negative and cloaked in a frame of individual or field-level modesty.

In addition, there are specific best practices or research directions that can help make these mistakes harder to make:

A New Rule? In light of the issues with negative results on older models discussed in Section 2.1, it could be productive to introduce a new heuristic when reviewing or evaluating papers. The Bender Rule (Bender, 2019) asks authors to explicitly name the natural languages that they study, so as to avoid misleading claims about language or languages on the basis of evidence from one language (usually English) alone. In that spirit, I propose another rule of thumb:

When describing the failure of a machine learning model on some empirical evaluation, make it clear

i. what kind of model has failed,
ii. whether the model is significantly less capable than the current state of the art in the domain, and
iii. whether the evaluation was deliberately set up to trick that model or another model like it.

The latter two can be points can be left unstated when they do not apply.

While a corresponding rule could be helpful in the context of results describing the success of a machine learning system on some evaluation, the asymmetry here is intentional: Successes are likely to be deliberately replicated from one generation of models to the next, creating a trend for a positive finding about older systems to remain true of newer systems in most cases. The opposite is true of model failures.

Better Evaluation The rise of underclaiming can likely be attributed, at least in large part, to the ineffectiveness of current evaluation practices in many areas of NLP. If encouraging numbers on widely-used benchmarks are frequently met with disappointing subsequent evidence, suggesting that good evaluation numbers don’t translate to effective systems, it is rational to treat new encouraging results with extreme skepticism.

Better benchmarks and evaluation practices could help mitigate this by providing a firmer ground on which to make positive claims about system capacities. In practice, research into more effective crowdsourcing and benchmark design (Kiela et al., 2021; Bowman and Dahl, 2021; Nangia et al., 2021; Gehrmann et al., 2021) and research into better statistical reporting and publication norms (Dodge et al., 2019; Card et al., 2020; Rogers and Augenstein, 2020; van Miltenburg et al., 2021) seem especially high-impact under this lens.

Better Analysis We can help address the time-lag issue discussed in Section 2.1 by building tools that make it easier to adapt existing analysis techniques to new models seamlessly. Leaderboards

8Though Raji et al. (2021) point out ways in which better benchmarking alone is unlikely to be fully satisfactory.
that integrate conventional benchmarking with analysis can be especially helpful by making this largely automatic (Wang et al., 2018; Dua et al., 2019; Gehrmann et al., 2021; Ma et al., 2021).

More broadly, careful work on model robustness and interpretation, targeted at capable models, will be valuable in helping to forecast and mitigate the worst risks from future systems.

**Better Forecasting** Scaling laws results in NLP (Hestness et al., 2017; Kaplan et al., 2020; Brown et al., 2020; Zhang et al., 2021) offer the promise that we can predict the performance of future larger-scale machine learning models on at least some metrics. This line of work is still nascent, and successes to date have largely focused on loss values rather than more interpretable measures of capability. Further developing these methods, as well as others that allow us to better forecast near future progress, should be helpful. Better forecasting will provide a useful way to sanity-check future claims (DellaVigna et al., 2019) and will help improve the responsiveness of model analysis by enabling us to prepare analysis methods and datasets that anticipate future capabilities.

7 Additional Related Work

While much of this paper discusses the state of the NLP literature, there are some related works that warrant emphasis as starting points for further reading:

Bender and Koller (2020), Bender et al. (2021), and Raji et al. (2021) discuss the role of hype in driving bad outcomes from the development of language technology. Jin et al. (2021) and Rogers (2021) offer broader discussion of how to ensure that the net impact of near-future NLP deployments on the world is positive. Looking to the longer term, Bommasani et al. (2021, §4.9) provides an introduction to the AI risk and AI alignment literature from a perspective that emphasizes NLP and language. Welty et al. (2019), Linzen (2020), Ribeiro et al. (2020), Raji et al. (2021), Bowman and Dahl (2021), and Dehghani et al. (2021), among many others, discuss the challenges involved in designing evaluations that yield trustworthy and accurate depictions of the capabilities of ML models.

8 Conclusion

Like many research fields that have a tight connection to a consumer products industry, NLP has long struggled to avoid hype and inflated expectations about the capabilities of state-of-the-art technology. This remains a serious issue. However, this paper argues that our attempts to avoid hype have begun to overshot: Instead of merely correcting overly optimistic claims about our capabilities, we have in some cases started to replace them with overly pessimistic claims.

Making misleading claims is generally a bad sign for the health and credibility of a scientific field, and the stakes are high: NLP technologies are implicated in a range of serious real-world harms, and plausible future elaborations of these technologies are potentially much more dangerous still. Our ability to mitigate existing harms will depend on our ability to make reliably credible claims about the limitations of our systems. Our ability to mitigate future harms will depend on our ability to accurately anticipate, recognize, agree upon, and report upon emerging capabilities. Both of these goals are seriously hampered by claims that current technologies are less capable than they in fact are.

Better evaluation, better tooling for model analysis, and better mechanisms for technical forecasting should all contribute to making these pessimistic claims easier to avoid or debunk. However, this problem is ultimately one of scientific communication, and to solve it fully, we will need to use the tools and norms of science to better police false or misleading claims.

Acknowledgements

The ideas and arguments in this work were developed through conversation (live or on Twitter) with more researchers than I can name, including Emiel van Miltenburg, Deb Raji, Paul Christiano, Geoffrey Irving, Rohin Shah, Jacob Steinhardt, Catherine Olsson, Nick Beckstead, Alex Tamkin, Daniel Dewey, Alex Ray, and Robin Jia. Anna Rogers, Owain Evans, Alex Wang, Jacob Steinhardt, Jason Phang, Jared Kaplan, Alex Cristia, Tal Linzen, and Jonathan Uesato provided feedback on drafts. Any errors or dubious rhetoric are my own.

This project has benefited from financial support by Eric and Wendy Schmidt (made by recommendation of the Schmidt Futures program) and Apple. This material is based upon work supported by the National Science Foundation under Grant Nos. 1922658 and 2046556. Any opinions, findings, and conclusions or recommendations expressed in this material are those of the author(s) and do not nec-
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