

# Experimental Standards for Deep Learning in Natural Language Processing Research

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## Abstract

The field of Deep Learning (DL) has undergone explosive growth during the last decade, with a substantial impact on Natural Language Processing (NLP) as well. Yet, compared to more established disciplines, a lack of common experimental standards remains an open challenge to the field at large. Starting from fundamental scientific principles, we distill ongoing discussions on experimental standards in NLP into a single, widely-applicable methodology. Following these best practices is crucial to strengthen experimental evidence, improve reproducibility and support scientific progress. These standards are further collected in a public repository to help them transparently adapt to future needs.

## 1 Introduction

Spurred by the advances in Machine Learning (ML) and Deep Learning (DL), the field of Natural Language Processing (NLP) has seen immense growth over the span of the last ten years, as illustrated by the number of publications in Figure 2. While such progress is remarkable, rapid growth comes at a cost: Akin to concerns in other disciplines (John et al., 2012; Jensen et al., 2021), several authors have noted major obstacles with reproducibility (Gundersen and Kjensmo, 2018; Belz et al., 2021) and a lack of significance testing (Marie et al., 2021) or published results not carrying over to different experimental setups, for instance in text generation (Gehrmann et al., 2022) and with respect to new model architectures (Narang et al., 2021). Others have questioned commonly-accepted procedures (Gorman and Bedrick, 2019; Søggaard et al., 2021; Bouthillier et al., 2021; van der Goot, 2021) as well as the (negative) impacts of research on society (Hovy and Spruit, 2016; Mohamed et al., 2020; Bender et al., 2021; Birhane et al., 2021) and environment (Strubell et al., 2019; Schwartz et al., 2020; Henderson et al., 2020). These problems

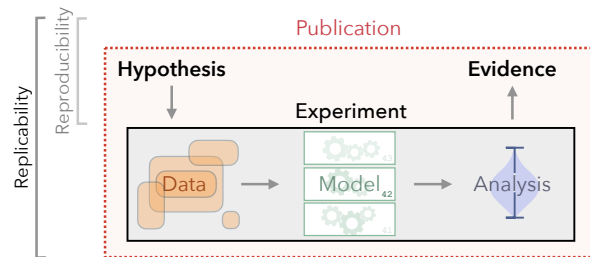


Figure 1: **Visualization of the Scientific Process in Deep Learning.** Uncertainty is introduced at each step, influencing the resulting evidence as well as the documentation required for reproducibility or replicability.

have not gone unnoticed—many of the mentioned works have proposed a cornucopia of solutions. In a quickly-moving environment however, keeping track and implementing these proposals becomes challenging. In this work, we weave these open issues together into a cohesive methodology for gathering stronger experimental evidence, that can be implemented with reasonable effort.

Based on the scientific method (Section 2), we divide the empirical research process—obtaining evidence from data via modeling—into four steps, which are depicted in Figure 1: *Data* (Section 3), including dataset creation and usage, *Codebase & Models* (Section 4), *Experiments & Analysis* (Section 5) and *Publication* (Section 6). For each step, we survey contemporary findings and summarize them into actionable practices for empirical research. Using insights from adjacent sub-fields of ML / DL, we extract useful insights to help overcome current challenges with replicability in NLP.

**Contributions** ① We survey and summarize a wide array of proposals regarding the improvement of the experimental (and publishing) pipeline in NLP research into a single accessible methodology applicable for a wide and diverse readership. At the end of every section, we provide a summary with the most important points, marked with  $\diamond$  to indicate that they should be seen as a minimal require-

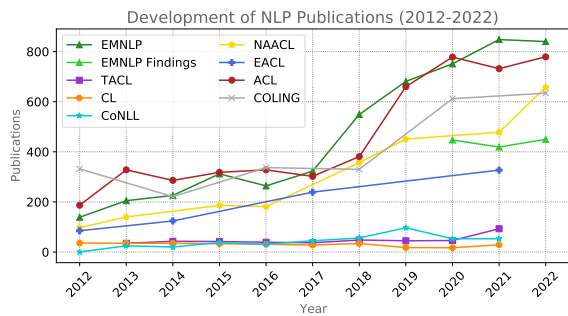


Figure 2: **Development of NLP publications.** Shown is the development of NLP measured by the number of peer-reviewed publications between 2012–2022 based on the data by [Rei \(2022\)](#) and manual additions.

ment, and  $\star$  for additional recommended actions. ② We create, point to, or supply useful resources to support everyday research activities and improve soundness of research in the field. We furthermore provide examples and case studies illustrating these methods in [Appendix A](#). We also provide an additional list of resources in [Appendix C](#). The same collection as well as checklists derived from the actionable points at the end of sections are also maintained in an open-source repository,<sup>1</sup> and we invite the research community to discuss, modify and extend these resources. ③ We discuss current trends and their implications, hoping to initiate a more widespread conversation about them in the NLP community to facilitate common standards and improve the quality of research.

## 2 Preliminaries

Our proposed methodology must be built on the scientific principles for generating strong evidence for the general advancement of knowledge, as defined by the following terms:

**The Scientific Method** Knowledge can be obtained through several ways including theory building, qualitative methods, and empirical research ([Kuhn, 1970](#); [Simon, 1995](#)). Here, we focus on the latter aspect, in which (exploratory) analyses lead to falsifiable hypotheses that can be tested and iterated upon ([Popper, 1934](#)).<sup>2</sup> This process requires that *anyone* must be able to back or dispute these hypotheses in the light of new evidence.

<sup>1</sup><https://github.com/Kaleidophon/experimental-standards-deep-learning-research>

<sup>2</sup>While such hypothesis-driven science is not always applicable or possible ([Carroll, 2019](#)), it is a strong common denominator that encompasses most empirical ML research.

In the following, we focus on the evidence-based evaluation of hypotheses and how to ensure the scientific soundness of the experiments which gave rise to the original empirical evidence, with a focus on *replicability* and *reproducibility*. In computational literature, one term requires access to the original code and data in order to re-run experiments exactly, while the other requires sufficient information in order to reproduce the original findings even in the absence of code and original data (see also [Figure 1](#)).<sup>3</sup>

**Replicability** Within DL, we take replicability to mean the exact replication of prior reported evidence. In a computational environment, access to the same data, code and tooling should be sufficient to generate prior results. However, many factors, such as hardware differences, make exact replication difficult to achieve. Nonetheless, we regard experiments to be replicable if a practitioner is able to re-run them to produce the same evidence within a small margin of error dependent on the environment, without the need to approximate or guess experimental details.

**Reproducibility** In comparison, we take reproducibility to mean the availability of all necessary and sufficient information such that an experiment’s findings can be independently reaffirmed when the same research question is asked. As discussed later, the availability of all components for replicability is rare—even in a computational setting. An experiment then is reproducible if anyone with access to the publication is able to re-identify the original evidence, i.e. exact results differing, but patterns across experiments being equivalent.

We assume that the practitioner aims to follow these principles in order to find answers to a well-motivated research question by gathering the strongest possible evidence for or against their hypotheses. The following methods therefore aim to reduce uncertainty in each step of the experimental pipeline in order to ensure reproducibility and/or replicability (visualized in [Figure 1](#)).

## 3 Data

Frequently, it is claimed that a model solves a particular cognitive task, however in reality it merely

<sup>3</sup>Strikingly, these central terms already lack agreed-upon definitions ([Peng, 2011](#); [Fokkens et al., 2013](#); [Lieberman, 2015](#); [Cohen et al., 2018](#)), however we follow the prevailing definitions in the NLP community ([Drummond, 2009](#); [Dodge and Smith, 2020](#)) as the underlying ideas are equivalent.

scores higher than others on some specific dataset according to some predefined metric (Schlangen, 2021). Of course, the broader goal is to improve systems more generally by using individual datasets as proxies. Admitting that our experiments cover only a small slice of the real-world sample space will help more transparently measure progress towards this goal. In light of these limitations and as there will always be private or otherwise unavailable datasets which violate replicability, a practitioner must ask themselves: *Which key information about the data must be known in order to reproduce an experiment’s findings?* In this section we define requirements for putting this question into practice during dataset creation and usage such that anyone can draw the appropriate conclusions from a published experiment.

**Choice of Dataset** The choice of dataset will arise from the need to answer a specific research question within the limits of the available resources. Such answers typically come in the form of comparisons between different experimental setups while using the equivalent data and evaluation metrics. Using a publicly available, well-documented dataset will likely yield more comparable work, and thus stronger evidence. In absence of public data, creating a new dataset according to guidelines which closely follow prior work can also allow for useful comparisons. Should the research question be entirely unexplored, creating a new dataset will be necessary. In any case, the data itself must contain the information necessary to generate evidence for the researcher’s hypothesis. For example, a model for a classification task will not be learnable unless there are distinguishing characteristics between data points and consistent labels for evaluation. Therefore, an exploratory data analysis is recommended for assessing data quality and anticipating problems with the research setup. Simple baseline methods such as regression analyses or simply manually verifying random samples of the data may provide indications regarding the suitability and difficulty of the task and associated dataset (Caswell et al., 2021).

**Metadata** At a higher level, data sheets and statements (Gebru et al., 2020; Bender and Friedman, 2018) aim to standardize metadata for dataset authorship in order to inform future users about assumptions and potential biases during all levels of data collection and annotation—including the

research design (Hovy and Prabhumoye, 2021). Simultaneously, they encourage reflection on whether the authors are adhering to their own guidelines (Waseem et al., 2021). Generally, higher-level documentation should aim to capture the dataset’s *representativeness* with respect to the global population. This is especially crucial for “high-stakes” environments in which subpopulations may be disadvantaged due to biases during data collection and annotation (He et al., 2019; Sap et al., 2021). Even in lower-stake scenarios, a model trained on only a subset of the global data distribution can have inconsistent behaviour when applied to a different target data distribution (D’Amour et al., 2020; Koh et al., 2020). For instance, domain differences have a noticeable impact on model performance (White and Cotterell, 2021; Ramesh Kashyap et al., 2021). Increased data diversity can improve the ability of models to generalize to new domains and languages (Benjamin, 2018), however diversity is difficult to quantify (Gong et al., 2019) and full coverage is unachievable. This highlights the importance of documenting representativeness in order to ensure reproducibility—even in absence of the original data. For replicability using the original data, further considerations include long-term storage and versioning, as to ensure equal comparisons in future work (see Appendix A.1 for case studies).

**Instance Annotation** Achieving high data quality entails that the data must be accurate and relevant for the task to enable effective learning (Pustejovsky and Stubbs, 2012; Tseng et al., 2020) and reliable evaluation (Bowman and Dahl, 2021; Basile et al., 2021). Since most datasets involve human annotation, a careful annotation design is crucial (Pustejovsky and Stubbs, 2012; Paun et al., 2022). Ambiguity in natural language poses inherent challenges and disagreement is genuine (Basile et al., 2021; Specia, 2021; Uma et al., 2021). As insights into the annotation process are valuable, yet often inaccessible, we recommend to release datasets with individual-coder annotations, as also put forward by Basile et al. (2021); Prabhakaran et al. (2021) and to complement data with insights like statistics on inter-annotator coding (Paun et al., 2022), e.g., over time (Braggaa and van der Goot, 2021), or coder uncertainty (Bassignana and Plank, 2022). When creating new datasets such information strengthens the reproducibility of future findings, as they transparently communicate the inherent variability instead of obscuring it.

**Pre-processing** Given a well-constructed or well-chosen dataset, the first step of an experimental setup will be the process by which a model takes in the data. This must be well documented or replicated—most easily by publishing the associated code—as perceivably tiny pre-processing choices can lead to huge accuracy discrepancies (Pedersen, 2008; Fokkens et al., 2013). Typically, this involves decisions such as sentence segmentation, tokenization and normalization. In general, the data setup pipeline should ensure that a model “observes” the same kind of data across comparisons. Next, the dataset must be split into representative subsamples which should only be used for their intended purpose, i.e., model training, tuning and evaluation (see Section 5). In order to support claims about the generality of the results, it is necessary to use a test split without overlap with other splits. Alternatively, a tuning / test set could consist of data that is completely foreign to the original dataset (Ye et al., 2021), ideally even multiple sets (Bouthillier et al., 2021). It should be noted that even separate, static test splits are prone to unconscious “overfitting” if they have been in use for a longer period of time, as people aim to beat a particular benchmark (Gorman and Bedrick, 2019). If a large variety of resources are not available, it is also possible to construct challenging test sets from existing data (Ribeiro et al., 2020; Kiela et al., 2021; Søggaard et al., 2021). Finally, the metrics by which models are evaluated should be consistent across experiments and thus benefit from standardized evaluation code (Dehghani et al., 2021). For some tasks, metrics may be driven by community standards and are well-defined (e.g., classification accuracy). In other cases, approximations must stand in for human judgment (e.g., in machine translation). In either case—but especially in the latter—dataset authors should inform users about desirable performance characteristics and recommended metrics.

**Appropriate Conclusions** The results a model achieves on a given data setup should first and foremost be taken as just that. Appropriate, broader conclusions can be drawn using this evidence provided that biases or incompleteness of the data are addressed (e.g., results only being applicable to a subpopulation). Even with statistical tests for the significance of comparisons, properties such as the size of the dataset and the distributional characteristics of the evaluation metric may influence

the statistical power of any evidence gained from experiments (Card et al., 2020). It is therefore important to keep in mind that in order to claim the reliability of the obtained evidence, for example, larger performance differences are necessary on less data than what might suffice for a large dataset, or across multiple comparisons (see Section 5). Finally, a practitioner should be aware that a model’s ability to achieve high scores on a certain dataset may not be directly attributable to its capability of simulating a cognitive ability, but rather due to spurious correlations in the input (Ilyas et al., 2019; Schlangen, 2021; Nagarajan et al., 2021). By for instance only exposing models to a subset of features that should be inadequate to solve the task, we can sometimes detect when they take unexpected shortcuts (Fokkens et al., 2013; Zhou et al., 2015). Communicating the limits of the data helps future work in reproducing prior findings more accurately.

#### Best Practices: Data

- ◇ Consider dataset and experiment limitations when drawing conclusions (Schlangen, 2021);
- ◇ Document task adequacy, representativeness and pre-processing (Bender and Friedman, 2018);
- ◇ Split the data such as to avoid spurious correlations;
- ◇ Publish the dataset accessibly & indicate changes;
- ★ Perform exploratory data analyses to ensure task adequacy (Caswell et al., 2021);
- ★ Publish the dataset with individual-coder annotations, besides aggregation;
- ★ Claim significance considering the dataset’s statistical power (Card et al., 2020).

## 4 Codebase & Models

The NLP community has historically taken pride in promoting open access to papers, data, code, and documentation, but some have also noted room for improvement (Wieling et al., 2018; Belz et al., 2020). One practice has been to open-source all components of the experimental procedure in a repository, consisting of all code, necessary scripts, and detailed documentation. The benefit of such a repository is in its ability to enable direct *replication*. In particular, a comprehensive code base directly enables replicability. In practice, such documentation is often communicated through a README file, in which user-oriented information is described.<sup>4</sup> In DL, full datasets can be large

<sup>4</sup>In Appendix B, we propose minimal requirements for a README file and give pointers on files and code structure.

and impractical to share. Due to their importance however, it is essential to carefully consider how one can share the data with researchers in the future. Therefore, repositories for long-term data storage backed by public institutions should be preferred (e.g., LINDAT/CLARIN by [Váradi et al., 2008](#), more examples in [Appendix C](#)). Nevertheless, practitioners often can not distribute data due to privacy, legal, or storage reasons. In such cases, practitioners must instead carefully consider how to distribute data and tools to allow future research to produce accurate replications of the original data ([Zong et al., 2020](#)).

**Hyperparameter Search** Hyperparameter tuning strategies remain an open area of research (e.g., [Bischl et al., 2021](#)), but are central to the replication of contemporary models. The following rules of thumb exist: Grid search or Bayesian optimization can be applied if few parameters can be searched exhaustively under the computation budget. Otherwise, random search is preferred, as it explores the search space more efficiently ([Bergstra and Bengio, 2012](#)). Advanced methods like Bayesian Optimization ([Snoek et al., 2012](#)) and bandit search-based approaches ([Li et al., 2017](#)) can be used as well if applicable ([Bischl et al., 2021](#)). To avoid unnecessary guesswork, the following information is expected: Hyperparameters that were searched per model (including options and ranges), the final hyperparameter settings used, number of trials, and settings of the search procedure if applicable. As tuning of hyperparameters is typically performed using specific parts of the dataset, it is essential to note that any modeling decisions based on them automatically invalidate their use as *test* data.

**Models** Contemporary models (e.g., [Vaswani et al., 2017](#); [Devlin et al., 2019](#); [Dosovitskiy et al., 2021](#); [Chen et al., 2021](#)) have very large computational and memory footprints. To avoid retraining models, and more importantly, to allow for replicability, it is recommended to save and share model weights. This may face similar challenges as those of datasets (namely, large file sizes), but it remains an impactful consideration. In most cases, simply sharing the best or most interesting model could suffice. It should be emphasized that distributing model weights should always complement a well-documented repository as libraries and hosting sites might not be supported in the future.

**Model Evaluation** The exact model and task evaluation procedure can differ significantly (e.g. [Post, 2018](#)). It is important to either reference the exact evaluation script used (including parameters, citation, and version, if applicable) or include the evaluation script in the codebase. Moreover, to ease error or post-hoc analyses, we highly recommend saving model predictions whenever possible and making them available at publication ([Card et al., 2020](#); [Gehrmann et al., 2022](#)).

**Model Cards** Apart from quantitative evaluation and optimal hyperparameters, [Mitchell et al. \(2019\)](#) propose model cards: A type of standardized documentation, as a step towards responsible ML and AI technology, accompanying trained ML models that provide benchmarked evaluation in a variety of conditions, across different cultural, demographic, or phenotypic and intersectional groups that are relevant to the intended application domains. They can be reported in the paper or project, and can help to collect important information for reproducibility, such as preprocessing and evaluation results. We refer to [Mitchell et al. \(2019\)](#); [Menon et al. \(2020\)](#) for examples of model cards.

#### Best Practices: Codebase & Models

- ◇ Publish a code repository with documentation and licensing to distribute for replicability;
- ◇ Report all details about hyperparameter search and model training;
- ◇ Specify the hyperparameters for replicability;
- ◇ Publish model predictions and evaluation scripts.;
- ★ Use model cards;
- ★ Publish models;

## 5 Experiments & Analysis

Experiments and their analyses constitute the core of most scientific works, and empirical evidence is valued especially highly in ML research ([Birhane et al., 2021](#)). Therefore, we discuss the most common issues and counter-strategies at different stages of an experiment.

**Model Training** For model training, it is advisable to set a random seed for replicability, and train multiple initializations per model in order to obtain a sufficient sample size for later statistical tests. The number of runs should be adapted based on the observed variance: Using for instance bootstrap power analysis, existing model scores are raised by a constant compared to the original

sample using a significance test in a bootstrapping procedure (Yuan and Hayashi, 2003; Tufféry, 2011; Henderson et al., 2018). If the percentage of significant results is low, we should collect more scores.<sup>5</sup> Bouthillier et al. (2021) further recommend to vary as many sources of randomness in the training procedure as possible (i.e., data shuffling, data splits etc.) to obtain a closer approximation of the true model performance. Nevertheless, any drawn conclusion are still surrounded by a degree of statistical uncertainty, which can be combated by the use of statistical hypothesis testing.

**Significance Testing** Using deep neural networks, a number of (stochastic) factors such as the random seed (Dror et al., 2019) or even the choice of hardware (Yang et al., 2018) or framework (Leventi-Peetz and Östreich, 2022) can influence performance and need to be taken into account. First of all, the size of the dataset should support sufficiently powered statistical analyses (see Section 3). Secondly, an appropriate significance test should be chosen. We give a few rules of thumb based on Dror et al. (2018): When the distribution of scores is known, for instance a normal distribution for the Student’s t-test, a *parametric* test should be chosen. Parametric tests are designed with a specific distribution for the test statistic in mind, and have strong statistical power (i.e. a lower Type II error). The underlying assumptions can sometimes be hard to verify (see Dror et al., 2018 §3.1), thus when in doubt *non-parametric* tests can be used. This category features tests like the Bootstrap, employed in case of a small sample size, or the Wilcoxon signed-rank test (Wilcoxon, 1992), when plenty observations are available. Depending on the application, the usage of specialized tests might furthermore be desirable (Dror et al., 2019; Agarwal et al., 2021). We also want to draw attention to the fact that comparisons between multiple models and/or datasets, *require* an adjustment of the confidence level, for instance using the Bonferroni correction (Bonferroni, 1936), which is a safe and conservative choice and easily implemented for most tests (Dror et al., 2017; Ulmer et al., 2022). Azer et al. (2020) provide a guide on how to adequately word insights when a statistical test was used, and Greenland et al. (2016) list common pitfalls and misinterpretations of results. Due to spatial constraints, we here refer to Appendix A.4 for

<sup>5</sup>The resulting tensions with modern DL hardware requirements are discussed in Section 7.

a number of easy-to-use software packages and further reading on the topic.

**Critiques & Alternatives** Although statistical hypothesis testing is an established tool in many disciplines, its (mis)use has received criticism for decades (Berger and Sellke, 1987; Demšar, 2008; Ziliak and McCloskey, 2008). For instance, Wasserstein et al. (2019) criticize the *p*-value as reinforcing publication bias through the dichotomy of “significant” and “not significant”, i.e., by favoring positive results (Locascio, 2017). Instead, Wasserstein et al. (2019) propose to report it as a continuous value and with the appropriate scepticism.<sup>6</sup> In addition to statistical significance, another approach advocates for reporting *effect size* (Berger and Sellke, 1987; Lin et al., 2013), so for instance the mean difference, or the absolute or relative gain in performance for a model compared to a baseline. The effect size can be modeled using Bayesian analysis (Kruschke, 2013; Benavoli et al., 2017), which better fit the uncertainty surrounding experimental results, but requires the specification of a plausible statistical model producing the observations<sup>7</sup> and potentially the usage of Markov Chain Monte Carlo sampling (Brooks et al., 2011; Gelman et al., 2013). Benavoli et al. (2017) give a tutorial for applications to ML and supply an implementation of their proposed methods in a software package (see Appendix C) and guidelines for reporting details are given by Kruschke (2021), including for instance the choice of model and priors.

#### Best Practices: Experiments & Analysis

- ◇ Report mean & standard dev. over multiple runs;
- ◇ Perform significance testing or Bayesian analysis and motivate your choice of method;
- ◇ Carefully reflect on the amount of evidence regarding your initial hypotheses.

## 6 Publication

In this section, we discuss some additional trends in the DL field that researchers should consider when publishing their work, even though they might not directly be related to reproducibility & replicability.

<sup>6</sup>Or, as Wasserstein et al. (2019) note: “*statistically significant*—don’t say it and don’t use it”.

<sup>7</sup>Here, we are *not* referring to a neural network, but instead to a process generating experimental observations, specifying a prior and likelihood for model scores. Conclusions are drawn from the posterior distribution over parameters of interest (e.g., the mean performance), as demonstrated by Benavoli et al. (2017).

**Citation Control** While frequently, researchers cite non-archival versions of papers, the published version of a paper is peer-reviewed, increasing the probability that any mistakes or ambiguities have been resolved. In [Appendix C](#), we suggest tools to verify the version of any cited papers.

**Hardware Requirements** The paper should report the computing infrastructure used. At minimum, the specifics about the CPU and GPU. This is for indicating the amount of compute necessary for the project, but also for the sake of replicability issues due to the non-deterministic nature of the GPU ([Jean-Paul et al., 2019](#); [Wei et al., 2020](#)). Moreover, [Dodge et al. \(2019\)](#) demonstrate that test performance scores alone are insufficient for claiming the dominance of a model over another, and argue for reporting additional performance details on validation data as a function of computation budget, which can also estimate the amount of computation required to obtain a given accuracy.

**Environmental Impact** The growth of computational resources required for DL over the last decade has led to financial and carbon footprint discussions in the AI community. [Schwartz et al. \(2020\)](#) introduce the distinction between *Red AI*—AI research that seek to obtain state-of-the-art results through the use of massive computational power—and *Green AI*—AI research that yields novel results without increasing computational cost. In the paper the authors propose to add *efficiency* as an evaluation criterion alongside accuracy measures. [Hershcovich et al. \(2022\)](#) advocate for the usage of a *climate performance model card*, in which energy and emission statistics are being detailed. [Strubell et al. \(2019\)](#) approximate financial and environmental costs of training a variety of models (e.g., BERT, GPT-2). In conclusion, to reduce costs and improve equity, they propose (1) *Reporting training time and sensitivity to hyperparameters*, (2) *Equitable access to computation resources*, and (3) *Prioritizing computationally efficient hardware and algorithms* ([Appendix C](#) includes a tool for CO<sub>2</sub> estimation of computational models).

**Social Impact** The widespread of DL studies and their increasing use of human-produced data (e.g., from social media and personal devices) means the outcome of experiments and applications have direct effects on the lives of individuals. Addressing and mitigating biases in ML is near-impossible as subjectivity is inescapable and thus converging in a

universal truth may further harm already marginalized social groups ([Waseem et al., 2021](#); [Parmar et al., 2022](#)). As a follow-up, [Waseem et al., 2021](#) argue for a reflection on the consequences the imaginary objectivity of ML has on political choices. [Hovy and Spruit \(2016\)](#) analyze and discuss the social impact research may have beyond the more explored privacy issues. They make an ethical analysis on social justice, i.e., equal opportunities for individuals and groups, and underline three problems of the mutual relationship between language, society and individuals: exclusion, over-generalization and overexposure.

**Ethical Considerations** There has been effort on the development of concrete ethical guidelines for researchers within the ACM Code of Ethics and Professional Conduct ([Association for Computing Machinery, 2022](#)). The Code lists seven principles stating how fundamental ethical principles apply to the conduct of a computing professional (like DL and NLP practitioners) and is based on two main ideas: computing professionals’ actions change the world and the public good is always the primary consideration. [Mohammad \(2021\)](#) discusses the importance of going beyond individual models and datasets, back to the ethics of the task itself. As a practical recommendation, he presents *Ethics Sheets for AI Tasks* as tools to document ethical considerations *before* building datasets and developing systems. In addition, researchers are invited to collect the ethical considerations of the paper in a cohesive narrative, and elaborate them in a paragraph, usually in the Introduction/Motivation, Data, Evaluation, Error Analysis or Limitations section ([Mohammad, 2020](#); [Hardmeier et al., 2021](#)).

#### Best Practices: **Publication**

- ◇ Avoid citing pre-prints (if applicable);
- ◇ Describe the computational requirements;
- ◇ Consider the potential ethical & social impact;
- ★ Consider the environmental impact and prioritize computational efficiency;
- ★ Include an Ethics and/or Bias Statement.

## 7 Discussion

Since previous sections have emphasized the need to overhaul some experimental standards, we dedicate this last section to discuss some structural issues that might pose obstacles to this.

**Compute Requirements** Specifically with regard to statistical significance in Section 5, there is a stark tension between the hardware requirements of modern methods (Sevilla et al., 2022) and the computational budget of the average researcher. Only the best-funded research labs can afford the increasing computational costs to account for the statistical uncertainty of results and to reproduce prior works (Hooker, 2021). Under these circumstances, it becomes difficult to judge whether the results obtained via larger models and datasets *actually* constitute substantial progress or just statistical flukes. At the same time, such experiments can create environmental concerns (Strubell et al., 2019; Schwartz et al., 2020).<sup>8</sup> The community must decide collectively whether these factors, including impeded reproducibility and weakened empirical evidence, constitute a worthy price for the knowledge obtained from training large neural networks.

**Incentives in Publishing** As demonstrated by Figure 2, NLP has gained traction as an empirical field of research. At such a point, more rigorous standards are necessary to maintain high levels of scholarship. Unfortunately, we see this process lagging behind, illustrated by repeated calls for improvement (Gundersen and Kjensmo, 2018; Narang et al., 2021). Why is that so? We speculate that the reason for many of these problems are caused by adverse incentives set by the current publishing environment: As the career of researchers hinges on their publications and more rigorous experimental standards are often not required to get published, reproducing and reproducible works are not rewarded. Instead, actors are tempted to “rig the benchmark lottery” (Dehghani et al., 2021), since achieving state-of-the-art results remains important for publishing (Birhane et al., 2021). As of now, better experimental standards often do not increase the acceptance probability: The more details are provided for replicability purposes, the more potential points of criticism are exposed to reviewers. This state of affairs might still seem like progress to some, but Chu and Evans (2021) show how an increased amount of papers actually leads to *slowed* progress in a field, making it harder for new, promising ideas to break through. Furthermore, Raff (2022) shows that reproducible work can actually have a positive impact on a paper’s citation rates, and thus should be more embraced.

<sup>8</sup>E.g., GPT-3’s training was estimated to have cost ca. 12M USD (Turner, 2020) or 188,702 kWh (Anthony et al., 2020).

**Culture Change** How can we change this trend? **As researchers**, we can start implementing the recommendations in this work in order to drive bottom-up change and reach a critical mass (Centola et al., 2018). **As reviewers**, we can shift focus from results to more rigorous methodologies (Rogers and Augenstein, 2021), and allow more critiques and reproductions of past works and meta-reviews to be published (Birhane et al., 2021; Lampinen et al., 2021). **As a community**, we can change the incentives around research and experiment with new initiatives. Rogers and Augenstein (2020) and Su (2021) give recommendations on how to improve the peer-review process by better paper-reviewer matching and paper scoring. Other attempts are currently undertaken to encourage reproduction of past works.<sup>9</sup> Other ideas change the publishing process more fundamentally, for instance by splitting it into two steps: The first part, where authors are judged solely on the merit of their research question and methodology; and the second one, during which the analysis of their results is evaluated (Loscascio, 2017). In a similar vein, van Miltenburg et al. (2021) recommend a procedure similar to clinical studies, where whole research projects are pre-registered, i.e., specifying the parameters of research before carrying out any experiments (Nosek et al., 2018). The implications of these ideas are not only positive, however, as a slowing rate of publishing might disadvantage junior researchers (Chu and Evans, 2021).

## 8 Conclusion

Being able to (re-)produce empirical findings is critical for scientific progress, particularly in fast-growing fields like NLP (Manning, 2015). To reduce the risks of a reproducibility crisis and unreliable research findings (Ioannidis, 2005), experimental rigor is imperative. Being aware of possible harmful implications and to avoid them is therefore important. Every step carries possible biases (Hovy and Prabhumoye, 2021; Waseem et al., 2021). This paper aims at providing a toolbox of actionable recommendations *for each research step*, and a reflection and summary of the ongoing broader discussion. With concrete best practices to raise awareness and call for uptake, we hope to aid researchers in their empirical endeavors.

<sup>9</sup>See for instance the reproducibility certification of the TMLR journal (TMLR, 2022) or NAACL 2022 reproducibility badges (Association for Computational Linguistics, 2022).



## Limitations

This work comes with two main limitations: On the one hand, it can only take a snapshot of an ongoing discussion. On the other hand, this work was aimed to primarily serve the NLP community, although other disciplines using DL might also profit from these guidelines. With these limitations in mind, we invite members of the community to contribute to our open-source repository.

## Ethics Statement

We do not foresee any immediate negative ethical consequences in lieu with our work.

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## A Case Studies & Further Reading

The implementation of the methods we advocate for in our work can be challenging. This is why we dedicate this appendix to listing further resources and pointing to examples that illustrate their intended use.

### A.1 Data

**Data Statement** Following [Bender and Friedman \(2018\)](#), the long form data statement should outline CURATION RATIONALE, LANGUAGE VARIETY, SPEAKER DEMOGRAPHIC, ANNOTATOR DEMOGRAPHIC, SPEECH SITUATION, TEXT CHARACTERISTICS and a PROVENANCE APPENDIX. A good example of a long form data statement can be found in Appendix B in [Plank et al. \(2020\)](#), where each of the former mentioned topics are outlined. For example, with respect to ANNOTATOR DEMOGRAPHIC, they mention “three students and one faculty (age range: 25-40), gender: male and female. White European. Native language: Danish, German. Socioeconomic status: higher-education student and university faculty.” This is a concise explanation of the annotators involved in their project.

**Data Quality** Text corpora today are building blocks for many downstream NLP applications like question answering and summarization. In the work of [Caswell et al. \(2021\)](#), they audit the quality of quality of 205 language-specific corpora released within major public datasets. At least 15 of these 205 corpora have no usable text, and a large fraction contains less than 50% sentences of acceptable quality. The tacit recommendation is looking at samples of any dataset before using it or releasing it to the public. A good example is [Varab and Schluter \(2020, 2021\)](#), who filter out low-quality news articles from their summarization dataset with empty summaries or bodies, removing duplicates, and removing summaries that are long than their main body of text. More wide varieties of data filtering can be applied, like filtering on length-ratio, LangID, and TF-IDF wordlists ([Caswell et al., 2020](#)). Note that there is no easy solution—data cleaning is not a trivial task ([Caswell et al., 2021](#)).

**Universal Dependencies** [Nivre et al. \(2020\)](#) aims to annotate syntactic dependencies in addition to part-of-speech tags, morphological features etc. for as many languages as possible within a consistent set of guidelines. The dataset which consists of tree-

banks contributed by various authors is updated in a regular half-yearly cycle and is hosted on the long-term storage LINDAT/CLARIN repository ([Váradí et al., 2008](#)). Each release is clearly versioned such that fair comparisons can be made even while guidelines are continuously adapted. Maintenance of the project is conducted on a public git repository, such that changes to both the data and the guidelines can be followed transparently. This allows for contributors to suggest changes via pull requests.

### A.2 Models

There are several libraries that allow for model hosting or distribution of model weights for “mature” models. HuggingFace ([Wolf et al., 2020](#)) is an example of hosting models for distribution. It is an easy-to-use library for practitioners in the field. Other examples of model distribution is Keras Applications<sup>10</sup> or TensorFlow Model Garden ([Yu et al., 2020](#)). Other ways of distributing models is setting hyperlinks in the repository (e.g., [Joshi et al., 2020](#)), to load the models from the checkpoints they have been saved to. A common denominator of all the aforementioned libraries is to list relevant model performances (designated metrics per task), the model size (in bytes), model parameters (e.g., in millions), and inference time (e.g., any time variable).

### A.3 Codebase

At the code-level, there are several examples of codebases with strong documentation and clean project structure. We define documentation and project structure in [Appendix B](#). Here, we give examples going from smaller projects to larger Python projects:

The codebase of CateNETS ([Curth and van der Schaar, 2021b,a](#); [Curth et al., 2021](#))<sup>11</sup> shows a clear project structure. This includes unit tests, versioning of the library, and licensing. In addition, there are specific files for each published work to replicate the results.

Not all projects require a pip installation or unit tests. For example—similar to the previous project—MaChAmp ([van der Goot et al., 2021](#))<sup>12</sup> shows detailed documentation, including several reproducible experiments shown in the paper (including files with model scores) and a clear project

<sup>10</sup><https://keras.io/api/applications/>

<sup>11</sup><https://github.com/AliciaCurth/CATENets>

<sup>12</sup><https://github.com/machamp-nlp/machamp>

structure. Here, one possible complication lies in possible dependency issues once the repository grows, with unit tests as a mitigation strategy.

AdapterHub (Pfeiffer et al., 2020)<sup>13</sup> demonstrates the realization of a large-scale project. This includes tutorials, configurations, and hosting of technical documentation (<https://docs.adapterhub.ml/>), as well as a dedicated website for the library itself.

#### A.4 Experimental Analysis

**Statistical Hypothesis Testing** A general introduction to significance testing in NLP is given by Dror et al. (2018); Raschka (2018); Azer et al. (2020). Furthermore, Dror et al. (2020) and Riezler and Hagmann (2021) provide textbooks around hypothesis testing in an NLP context. Japkowicz and Shah (2011) describe the usage of statistical test for general, classical ML classification algorithms. When it comes to usage, Zhang and Plank (2021) describe the statistical test used with all parameter and results alongside performance metrics. Shimorina et al. (2021) report p-values alongside test statistics for the Spearman’s  $\rho$  test, using the Bonferroni correction due to multiple comparisons. Apidianaki et al. (2018) transparently report the p-values of a approximate randomization test (Riezler and Maxwell III, 2005) between all the competitors in an argument reasoning comprehension shared task and interpret them with the appropriate degree of carefulness.

**Bayesian analysis** Bayesian Data Analysis has a long history of application across many scientific disciplines. Popular textbooks about the topic are given by Kruschke (2010); Gelman et al. (2013) with a more gentle introduction by Kruschke and Liddell (2018). Benavoli et al. (2017) supply an in-depth tutorial for Bayesian Analysis for Machine Learning, by using a Bayesian signed ranked test (Benavoli et al., 2014), an extension of the frequentist Wilcoxon signed rank test and a Bayesian hierarchical correlated t-test (Corani and Benavoli, 2015). Applications can be found for instance by Nilsson et al. (2018), who use the Bayesian correlated t-test (Corani and Benavoli, 2015) to investigate the posterior distribution over the performance difference to compare different federated learning algorithms. To evaluate deep neural networks on road traffic forecasting, Manibardo et al. (2021)

<sup>13</sup><https://github.com/Adapter-Hub/adapter-transformers>

employ Bayesian analysis and plot Monte Carlo samples from the posterior distribution between pairs of models. The plots include ROPEs, i.e., regions of practical equivalence, where the judgement about the superiority of a model is suspended.

#### A.5 Publication Considerations

**Replicability** Gururangan et al. (2020) report in detail all the computational requirements for their adaptation techniques in a dedicated sub-section. Additionally, following the suggestions by Dodge et al. (2019), the authors report their results on the development set in the appendix.

**Environmental Impact** By introducing MultiBERTs (Sellam et al., 2021), the authors include in their paper an *Environmental Statement*. In the paragraph they estimate the computational cost of their experiments in terms of hours, and consequential tons of CO<sub>2</sub>e. They release the trained models publicly with the aim to allow subsequent studies by other researchers without the computational cost of training MultiBERTs to be incurred.

Hershcovich et al. (2022), instead, propose a *climate performance model card* as a way to systematically report the climate impact of NLP research.

**Social and Ethical Impact** Brown et al. (2020) present GPT-3 and include a whole section on the *Broader Impacts* language models like GPT-3 have. Despite improving the quality of text generation, they also have potentially harmful applications. Specifically, the authors discuss the potential for deliberate misuse of language models, and the potential issues of bias, fairness and representation (focusing on the gender, race and religion dimensions).

The work of Hardmeier et al. (2021) assists the researcher in writing a bias statement, by recommending to provide explicit statements of why the system’s behaviors described as “bias” are harmful, in what ways, and to whom, then to reason on them. In addition, they provide an example of a bias statement from Basta et al. (2019).

## B Contents of Codebase

**The README** First, the initial section of the README would consist of the name of the repository—to what paper or project is this code base tied to? Including a hyperlink to the paper or project itself. Second, developers also indicate the structure of the repository—what and where are

the files, folders, code, et cetera in the project and how would they be used.

Empirical work requires the installation of libraries or software. It is important to install the right versions of the libraries to maintain replicability, and indicate the correct version of the specific package. In Python, a common practice is to make use of virtual environments in combination with a `requirements.txt` file. The main purpose of a virtual environment is to create an isolated environment for code projects. Each project can have its own dependencies (libraries) regardless of what dependencies every other project has to avoid clashes between libraries. For example, this file can be created by piping the output of `pip freeze` to a `requirements.txt` file. For further examples of virtual environment tools, we refer to [Table 1 \(Appendix C\)](#).

To ensure replicability, the practitioner writes a description on how to re-run all experiments that are depicted in a paper to get the same results. For example, these are evaluation scores or graphical plots. This can come in the form of a bash script, that indicates all the commands necessary.<sup>14</sup> Similarly, one can also indicate all commands in the README. To give credit to each others work, the last section of the README is usually reserved for credits, acknowledgments, and the citation. The citation is preferably provided in BibTeX format.

**Project Structure** From the Python programming language perspective, there are several references for initializing an adequate Python project structure.<sup>15</sup> This includes a README, LICENSE, `setup.py`, `requirements.txt`, and unit tests. To quote *The Hitchhiker’s Guide to Python* ([Reitz and Schlusser, 2016](#)) on the meaning of “structure”:

“By ‘structure’ we mean the decisions you make concerning how your project best meets its objective. We need to consider how to best leverage Python’s features to create clean, effective code. In practical terms, ‘structure’ means making clean code whose logic and dependencies are clear as well as how the files and folders are organized in the filesystem.”

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<sup>14</sup>See for instance <https://robvandergh.github.io/blog/repro.htm>

<sup>15</sup>Some examples: <https://docs.python-guide.org/writing/structure/> and <https://coderefinery.github.io/reproducible-research/02-organizing-projects/>

This includes decisions on where functions should go into which modules. Also on how data flows through the project. What features and functions can be grouped together or even isolated? In a broader sense, to answer the question on how the finished product should look like.

## C Resources

An overview over all mentioned resources in the paper is given in [Table 1](#).

Table 1: Overview over mentioned resources.

Name	Description	Link
ACL Anthology	Website hosting all the published proceedings of the ACL.	<a href="https://aclanthology.org">https://aclanthology.org</a>
ACL pubcheck	Tool to check the format and the citations of papers written with the ACL style files.	<a href="https://github.com/acl-org/aclpubcheck">https://github.com/acl-org/aclpubcheck</a>
Anonymous Github	Website to anonymize a Github repository.	<a href="https://anonymous.4open.science">https://anonymous.4open.science</a>
baycomp (Benavoli et al., 2017)	Implementation of Bayesian tests for the comparison of classifiers.	<a href="https://github.com/janezd/baycomp">https://github.com/janezd/baycomp</a>
BitBucket	A website and cloud-based service that helps developers store and manage their code, as well as track and control changes to their code.	<a href="https://bitbucket.org/">https://bitbucket.org/</a>
Conda	Open Source package management system and environment management system.	<a href="https://docs.conda.io/">https://docs.conda.io/</a>
codecarbon (Schmidt et al., 2021)	Python package estimating and tracking carbon emission of various kind of computer programs.	<a href="https://github.com/mlco2/codecarbon">https://github.com/mlco2/codecarbon</a>
dbpl	Computer science bibliography to find correct versions of papers.	<a href="https://dblp.org/">https://dblp.org/</a>
deep-significance (Ulmer et al., 2022)	Python package implementing the ASO test by Dror et al. (2019) and other utilities	<a href="https://github.com/Kaleidophon/deep-significance">https://github.com/Kaleidophon/deep-significance</a>
European Language Resources Association (ELRA, 1995)	Public institution for language and evaluation resources	<a href="http://catalogue.elra.info/en-us/">http://catalogue.elra.info/en-us/</a>
GitHub	A website and cloud-based service that helps developers store and manage their code, as well as track and control changes to their de.	<a href="https://github.com/">https://github.com/</a>
Google Scholar	Scientific publication search engine. Note that the ACL Anthology should be preferred, as Google Scholar often indexes the first occurrence of a paper (which is frequently a pre-print)	<a href="https://scholar.google.com/">https://scholar.google.com/</a>
Hugging Face Datasets (Lhoest et al., 2021)	Hub to store and share datasets	<a href="https://huggingface.co/datasets">https://huggingface.co/datasets</a>
HyBayes (Azer et al., 2020)	Python package implementing a variety of frequentist and Bayesian significance tests	<a href="https://github.com/allenai/HyBayes">https://github.com/allenai/HyBayes</a>
LINDAT/CLARIN (Váradi et al., 2008)	Open access to language resources and other data and services for the support of research in digital humanities and social sciences	<a href="https://lindat.cz/">https://lindat.cz/</a>
ONNX	Open format built to represent Machine Learning models.	<a href="https://onnx.ai/">https://onnx.ai/</a>
Pipenv	Virtual environment for managing Python packages	<a href="https://pipenv.pypa.io/">https://pipenv.pypa.io/</a>
Protocol buffers	Data structure for model predictions	<a href="https://developers.google.com/protocol-buffers/">https://developers.google.com/protocol-buffers/</a>
rebiber	Python tool to check and normalize the bib entries to the official published versions of the cited papers.	<a href="https://github.com/yuchenlin/rebiber">https://github.com/yuchenlin/rebiber</a>
Semantic Scholar	Scientific publication search engine.	<a href="https://www.semanticscholar.org/">https://www.semanticscholar.org/</a>
Virtualenv	Tool to create isolated Python environments.	<a href="https://virtualenv.pypa.io/">https://virtualenv.pypa.io/</a>
Zenodo	General-purpose open-access repository for research papers, datasets, research software, reports, and any other research related digital artifacts	<a href="https://zenodo.org/">https://zenodo.org/</a>