Abstract

Recent years have seen a growing number of publications that analyse Natural Language Inference (NLI) datasets for superficial cues, whether they undermine the complexity of the tasks underlying those datasets and how they impact those models that are optimised and evaluated on this data. This structured survey provides an overview of the evolving research area by categorising reported weaknesses in models and datasets and the methods proposed to reveal and alleviate those weaknesses for the English language. We summarise and discuss the findings and conclude with a set of recommendations for possible future research directions. We hope it will be a useful resource for researchers who propose new datasets, to have a set of tools to assess the suitability and quality of their data to evaluate various phenomena of interest, as well as those who develop novel architectures, to further understand the implications of their improvements with respect to their model’s acquired capabilities.

1 Introduction

Research in areas that require natural language inference (NLI) over text, such as Recognizing Textual Entailment (RTE) (Dagan et al., 2006) and Machine Reading Comprehension (MRC) is advancing at an unprecedented rate. On the one hand, novel architectures (Vaswani et al., 2017) enable efficient unsupervised training on large corpora to obtain expressive contextualised word and sentence representations for a multitude of downstream NLP tasks (Devlin et al., 2019). On the other hand, large-scale datasets (Bowman et al., 2015; Rajpurkar et al., 2016; Williams et al., 2018) provide sufficient examples to optimise large neural models that are capable of outperforming the human baseline on multiple tasks (Raffel et al., 2019; Lan et al., 2020).

Recent work, however, has questioned the seemingly superb performance for some of the tasks. Specifically, training and evaluation data may contain exploitable superficial cues, such as syntactic constructs (McCoy et al., 2019), specific words (Poliak et al., 2018) or sentence length (Gururangan et al., 2018) that are predictive of the expected output. After having been evaluated on data in which those cues have been removed, the performance of those models deteriorated significantly (McCoy et al., 2019; Niven and Kao, 2019), showing that they are in fact relying on the existing cues rather than learning to understand meaning or perform inference. In other words, those well-performing models tend to obtain optimal performance on a particular dataset, i.e. overfitting on it, rather than generalising for the underlying task. This issue, in fact, remains concealed, if a model is compared to a human baseline by means of a single number that reports the average score on a held-out test set, which is typically the case with contemporary benchmark leaderboards.

To reveal and overcome these issues mentioned above, a growing number of approaches has been proposed in the past. All those methods contribute towards a fine-grained understanding of whether the existing methodology actually evaluates the required inference capabilities, what existing models learn from available training data and, more importantly, which capabilities they still fail to acquire, thus providing targeted suggestions for future research.

To make sense of this growing body of literature and help researchers new to the field to navigate it, we present a structured survey of the recently proposed methods and report the trends, applications
Figure 1: Number of premise-hypothesis pairs in an RTE dataset following lexical patterns, spuriously skewed towards Entailment (McCoy et al., 2019).

Figure 2: Models’ over-stability towards common words in question and paragraph, revealed by adversarially inserting distracting sentences (Jia and Liang, 2017).

and findings. In the remainder of this paper, we first establish terminology, set the objectives and the scope of the survey and describe the data collection methodology. We then present a categorisation of the surveyed methods with their main findings, and finally discuss the arising trends and open research questions.

1.1 Terminology

Tasks: The task of Recognising Textual Entailment (RTE) is to decide, for a pair of natural language sentences (premise and hypothesis), whether given the premise the hypothesis is true (Entailment), false (Contradiction) or whether the two sentences are unrelated (Neutral) (Dagan et al., 2013).

We refer to the task of finding the correct answer to a question over a passage of text as Machine Reading Comprehension (MRC), also known as Question Answering (QA). Usual formulations of the task require models to select a span from the passage, select from a given set of alternatives or generate a free-form string (Liu et al., 2019b).

In this paper, we use the term “NLI” in its broader sense, referring to the requirement to perform inference over natural language. Thus we expand the usual textual entailment-based definition to also include MRC, as answering a question can be framed as finding an answer that is entailed by the question and the provided context, and the tasks can be transformed vice versa (Demszky et al., 2018).

Model and Architecture: We refer to the neural network architecture of a model as “architecture”, e.g. BiDAF (Seo et al., 2017). We refer to a (statistical) model of a certain architecture that was optimised on a given set of training data simply as “model”. It is important to make this distinction, as an optimised model’s systematic failures can either be traced to biases in the training data (and can potentially be different for a model optimised on different data) or affiliated with the model class (and exist for all models with the same architecture) (Liu et al., 2019a; Geiger et al., 2019).

Spurious Correlations: We call correlations between input data and the expected prediction as “spurious” if they are not indicative for the underlying task but rather an artefact of the data at hand (as illustrated in Figure 1). The exploitation of those correlations in order to produce the expected prediction is known as the “Clever Hans Effect”, named after a horse that was believed to perform arithmetic tasks but was shown to react to subtle body language cues of the asking person (Pfungst and Rahn, 1911).

Adversarial: Szegedy et al. (2014) define “adversarial examples” as (humanly) imperceptible perturbations to images that cause a significant drop in the prediction performance of neural models. Similarly for NLP, we refer to data as “adversarial” if it is designed to minimise prediction performance for a class of models, while not impacting the human baseline. Examples include appending irrelevant information (Jia and Liang, 2017), illustrated in Figure 2, or paraphrasing (Ribeiro et al., 2019).

Stress-test: The evaluation of trained models and neural architectures in a controlled way with regard to a particular type of reasoning (e.g. logic inference (Richardson and Sabharwal, 2019)) or linguistic capability (e.g. lexical semantics (Naik et al., 2018)) is referred to as “stress-testing” (Naik et al., 2018). Measuring the prediction performance of a model with a particular architecture that was trained on a particular dataset on an evaluation-only stress-test (Glockner et al., 2018) allows to draw conclusions
about the capabilities the model obtains from the training data. Stress-tests with a training set allow for more general conclusions whether a model with a specific architecture is capable of obtaining the capability, even when optimised with sufficient examples (Kaushik et al., 2020; Geiger et al., 2019).

Robustness: In line with the literature (Wang and Bansal, 2018; Jia et al., 2019), we call a model “robust” against a method that alters the underlying (unknown) distribution of the evaluation data when compared to the training data, such as introduced by adversarial evaluation or stress-tests, if the out-of-distribution performance of the model is similar to that on the original evaluation set. The opposite of robustness is referred to as “brittleness”.

1.2 Objectives and Scope

We aim to provide a comprehensive overview of issues in NLI data and models that are trained and evaluated upon them as well as the methodology used to report them. We set out to address the following questions:

• Which NLI tasks and corresponding datasets have been investigated?
• Which types of weaknesses have been reported in NLI models and their training and evaluation data?
• What types of methods have been proposed to detect those weaknesses and their impacts on model performance and what methods have been proposed to overcome them?
• How have the proposed methods impacted the creation of novel datasets (that were described in published papers)?

1.3 Data collection methodology

To answer the first three questions we collect a literature body using the “snowballing” technique. Specifically, we initialise the set of surveyed papers with Gururangan et al. (2018), Poliak et al. (2018) and Jia and Liang (2017), because their impact helped to motivate further studies and shape the research field. For each paper in the set we follow its citations and works that have cited it according to Google Scholar and include papers that describe methods and/or their applications to report either (1) qualitative evaluation of training and/or test data; (2) superficial cues present in data and the tendency of models to pick them up; (3) systematic issues with task formulations and/or data collection methods; (4) analysis of specific linguistic and reasoning phenomena in data and/or models’ performance on them; or (5) enhancements of models’ architecture or training procedure in order to overcome data-specific or model-specific issues, related to phenomena and cues described above. We exclude a paper if its target task does not fall under the NLI definition established above, was published before the year 2014 or the language of the target dataset is not English; otherwise, we add it to the set of surveyed papers. With this approach, we obtain a total of 69 papers from the years 2014-2017 (6), 2018 (17), 2019 (38) and 2020 (8). More than two thirds (48) of the papers were published in venues hosted by the the Association for Computational Linguistics, whereas five and three were presented in AAAI and ICLR conferences, respectively. The remaining papers were published in other venues (3) or are available as an arXiv preprint (10). The papers were examined by the first author; for each paper the target task and dataset(s), the method applied and the result of the application was extracted and categorised.

To answer the final question, we took those publications introducing any of the datasets that were mentioned by at least one paper in the pool of surveyed papers and extended that collection by additional state-of-the-art NLI dataset resource papers (for detailed inclusion and exclusion criteria, see Appendix B). This approach yielded 73 papers. For those papers, we examine whether any of the previously collected methods were applied to report spurious correlations or whether the dataset was adversarially pruned against some model.

Although related, we deliberately do not include work that introduces adversarial attacks on NLP systems or discuss their fairness. For an overview thereof, we refer the interested reader to respective surveys conducted by Zhang et al. (2019c) or Xu et al. (2019) for the first, and by Mehrabi et al. (2019) for the latter.
2 Weaknesses in NLI data and models

Here, we report the types of weaknesses found in state-of-the-art NLI data and models.

2.1 Data

We identify three main types of weakness found in the data that was utilised in training and evaluating models and outline them below:

**Spurious Correlations**  In span extraction tasks such as MRC, question (Rychalska et al., 2018), passage wording and the position of the answer span in the passage is indicative of the expected answer for various datasets (Kaushik and Lipton, 2018). In the ROC stories dataset, (Mostafazadeh et al., 2016) where the task is to choose the most plausible ending to a story, the endings exhibit exploitable cues (Schwartz et al., 2017). These cues are even noticeable by humans (Cai et al., 2017).

For sentence pair classification tasks, such as RTE, Poliak et al. (2018) and Gururangan et al. (2018) showed that certain n-grams, lexical and grammatical constructs in the hypothesis and its length correlate with the expected label for a multitude of RTE datasets. The latter study referred to these correlations as “annotation artifacts”. McCoy et al. (2019) showed that lexical features like word overlap and common subsequences between the hypothesis and premise, are highly predictive of the entailment label in the MNLI dataset. Beyond RTE, the choices in the COPA (Roemmele et al., 2011) dataset, where the task is to finish a given passage (similar to ROC Stories), and ARCT (Habernal et al., 2018) where the task is to select whether a statement warrants a claim, contain words that correlate with the expected prediction (Kavumba et al., 2019; Niven and Kao, 2019).

**Task unsuitability**  Chen and Durrett (2019a) demonstrated that selecting from answers in a multiple choice setting considerably simplifies the task when compared to selecting a span from the context. They further showed that for large parts of the popular HotPOTQA dataset the answer can be found when deliberately not integrating information from multiple sentences (“multi-hop” reasoning), replicated by Min et al. (2019).

**Data Quality issues**  Pavlick and Kwiatkowski (2019) argue that when training data are annotated using crowdsourcing, a fixed label representing the ground truth, usually obtained by majority vote between annotators, is not representative of the uncertainty which can be important to indicate the complexity of an example or the fact that its correctness is debateable. Neural networks are, in fact, unable to pick up such uncertainty. Furthermore, both Schlegel et al. (2020) and Pugaliya et al. (2019) report the existence of factual errors in MRC evaluation data, where the expected answer to a question is actually wrong. Finally, Rudinger et al. (2017) show the presence of gender and racial stereotypes in crowd-sourced RTE datasets.

2.2 Models

These data weaknesses contribute to brittleness in trained models themselves. Below, we outline those and other issues reported in the literature:

**Exploitation of Cues**  Given the existence of spurious correlations in NLI data, it is worthwhile knowing whether models optimised on data containing those correlations actually exploit them. In fact, multiple studies confirm this hypothesis, demonstrating that evaluating models on a version of the same dataset where the correlations do not exist, results in poor prediction performance (McCoy et al., 2019; Niven and Kao, 2019; Kavumba et al., 2019).

**Semantic Over-stability**  Another weakness, particularly shown for MRC models, is that they appear to not capture the semantics of text beyond superficial lexical features. Neural models struggle to distinguish important from irrelevant sentences that share words with the question (Jia and Liang, 2017), disregard syntactic structure (Basaj et al., 2018; Rychalska et al., 2018) and semantically important words (Mudrakarta et al., 2018). For RTE, they may disregard the composition of the sentence pairs (Nie et al., 2019a).
Figure 3: Taxonomy of investigated methods. Dashed arrows indicate conceptually related types of methods, i.e. a method of one type are commonly applied with another method of the related type. Labels (a), (b) and (c) correspond to the coarse grouping discussed in Section 3.

Generalisation Issues Some issues hint at limited generalisation capabilities of models beyond a particular dataset. A reason lies in the typical machine learning strategy whereby data used for evaluation is drawn from the same distribution as the training data. In the case of NLP, the distribution is determined by the design of the data collection method, usually crowd-sourced annotation of a large corpus of documents in natural language, e.g. SQuAD (Rajpurkar et al., 2016), MNLI (Williams et al., 2018). A related problem is that datasets contain spurious correlations that are inherent to a particular dataset rather than to the underlying task, and that optimised models learn to exploit them as discussed above. The implications are, firstly, that models overfit to a specific dataset and do not generalise well to other examples drawn from the (unknown) task-specific distribution. Secondly, they fail to acquire linguistic and reasoning capabilities that were not explicitly required in the training sets (Glockner et al., 2018; Richardson and Sabharwal, 2019; Yanaka et al., 2019a). Evaluation data drawn from the same distribution as the training data is unsuitable for revealing both of those issues.

3 Methods that reveal weaknesses in NLI

In the following section we categorise the surveyed papers, briefly describe the categories and illustrate the methodologies by reference to respective papers. On a high level, we distinguish between methods that (a) reveal systematic issues with existing training and evaluation data such as the spurious correlations mentioned above, (b) investigate what inference and reasoning capabilities models optimised on these data acquire when evaluated on samples not drawn from the training distribution and (c) propose architectural (Sagawa et al., 2020) and training procedure (Wang and Bansal, 2018) improvements in order to achieve more robust generalisation beyond data drawn from the training distribution. A schematic overview of the taxonomy of the categories is shown in Figure 3.

3.1 Data-investigating Methods

Methods in this category analyse flaws in data such as cues in input that are predictive of the output (Gururangan et al., 2018). As training and evaluation data from state-of-the-art NLI datasets are assumed to be drawn from the same distribution, models that were fitted on those cues achieve high performance in the evaluation set, without being tested on the required inference capabilities. Furthermore, methods that investigate the evaluation data in order to gain a deeper understanding of the assessed capabilities (Chen et al., 2016) fall under this category as well. In the analysed body of work, we identified the following three types of methods:

Partial Baselines These methods seek to verify that every input modality provided by the task is actually required to make the right prediction (e.g. both question and passage for MRC, and premise and hypothesis for RTE). Training and evaluating a classifier on parts of the input only suggests that those parts exhibit cues that correlate with the expected prediction, if the measured performance is significantly
higher than randomly guessing. Both Gururangan et al. (2018) and Poliak et al. (2018) demonstrated near state-of-the-art performance on multiple RTE datasets, such as SNLI (Bowman et al., 2015) and MNLI (Williams et al., 2018), when training a classifier with hypothesis-only input. Kaushik and Lipton (2018) even surpass state-of-the-art MRC models on various datasets when training and evaluating only on parts of the provided input. Methods that mask, drop or shuffle input words or sentences fall under this category as well. Using them, Sugawara et al. (2020) reach performance comparable to that of a model that is trained on full input on a variety of MRC datasets. Similarly, Nie et al. (2019a) reach near state-of-the-art performance on the SNLI and MNLI datasets when shuffling the words in the premise and hypothesis. Finally, we include methods here that seek to verify whether the data or task formulation is fit to evaluate a particular capability, as they involve training models that are architecturally restricted to obtain said capability, e.g. models that process documents strictly independently to answer questions that require information synthesis from multiple documents (Min et al., 2019; Chen and Durrett, 2019a). Good performance of those impaired models indicates that the task can be solved without the required capability to a certain extent.

Above-chance performance of partial input baselines hints at spurious correlations in the data and suggests that models learn to exploit them; it does not however reveal their precise nature. The opposite does not hold true either: near-chance performance on partial input does not warrant cue-free data, as Feng et al. (2019) illustrate on synthetic examples and published datasets.

Heuristics and Correlations These aim to unveil specific cues and spurious correlations between input and expected output that enable models to learn the task more easily. For sentence pair classification tasks, Gururangan et al. (2018) use the PMI measure between words in a hypothesis and the expected label, while Poliak et al. (2018) use the conditional probability of a label given a word. In contrast, Tan et al. (2019) use word bigrams instead of single words to model their correlation. McCoy et al. (2019) count instances of (subsequently) overlapping words and mutual subtrees of the syntactic parses in a given premise and hypothesis pair, and show that their label distribution is heavily skewed towards entailment. Nie et al. (2019a) optimise a logistic regression model on lexical features and use its confidence to predict a wrong label for a given premise-hypothesis pair as a score for the requirement of inference beyond lexical matching. Niven and Kao (2019) define productivity and coverage to measure how likely and for what proportion of the dataset an n-gram is indicative of the expected label. Cai et al. (2017) propose simple rules based on length, negation and off-the-shelf sentiment analyser scores to select the most probable ending for the ROC story completion task.

To show that models actually learn to react to the cues, the data analysis is usually followed by an evaluation on a balanced evaluation set where those correlations are not present anymore (e.g. by balancing the label distribution for a correlating cue, as described in Section 3.2).

Manual Analyses These methods intend to qualitatively analyse the data, if automated approaches as those mentioned above are unsuitable due to the complexity of the phenomena of interest. To some extent, all papers describing experiment results on evaluation data or introducing new datasets are expected to perform a qualitative error or data analysis. We highlight a comparative qualitative analysis of state-of-the-art models on multiple MRC datasets (Pugaliya et al., 2019). Furthermore, Schlegel et al. (2020) perform a qualitative analysis of popular MRC datasets reporting evaluated linguistic phenomena and reasoning capabilities as well as existing factual errors in data.

3.2 Model-investigating Methods

Rather than analysing data, approaches described in this section directly evaluate the models with respect to their inference capabilities with regard to various phenomena of interest. Furthermore, methods that improve a model’s generalisation beyond potential biases it encounters during training, either by augmenting the training data, or by altering the architecture or the training procedure, are described here as well.

Stress-test is an increasingly popular way to assess trained models and architectures. Naik et al. (2018) automatically generate NLI evaluation data based on an analysis of observed state-of-the-art model er-
ror patterns, introducing the term “stress-test”. Stress-tests have since been proposed to evaluate the capabilities of handling monotonicity (Yanaka et al., 2019a), lexical inference (Glockner et al., 2018), definitions (Richardson and Sabharwal, 2019) and compositionality (Nie et al., 2019a) for RTE models and semantic equivalence (Ribeiro et al., 2019) for MRC. Liu et al. (2019a) propose an evaluation methodology to rightfully attribute the stress test performance to either missing examples in training data or the model’s inherent incapability to capture the tested phenomenon by optimising the trained model on portions of the stress test data.

**Adversarial Evaluation** refers to generating data with the aim to “fool” a target model. Jia and Liang (2017) showed that models across the leaderboard exhibit over-stability to keywords shared between a given question and passage pair in the SQuAD (Rajpurkar et al., 2016) dataset. These models change their prediction after the addition of distracting sentences, even if they do not alter the semantics of the passage (therefore keeping the validity of the expected answer). Wallace et al. (2019) further showed that adversaries generated against a target model tend to be universal for a whole range of neural architectures. Methods that evaluate whether models that are trained on data exhibiting spurious correlations inherit those, belong to this category as well. McCoy et al. (2019) use patterns to generate an adversarial evaluation set with controlled distribution, such that lexical cues in the training data are not indicative of the label anymore. Niven and Kao (2019) and Kavumba et al. (2019) add mirrored instances (i.e. modify the semantics of the sentences in a way such that the opposite label is true) of the biased data to create a set with balanced distribution of examples that contain words that otherwise correlate with the expected label in the original data.

### 3.3 Model-improving Methods

Here we discuss methods that improve the robustness of models against adversarial and out-of-distribution evaluation, by either modifying the available training data or making adjustments to the training procedure.

**Training data augmentation** methods improve the training data to train a model that is robust against a given adversary type. Thus they are inherently linked with the adversarial data generation methods. However, simply training the model on parts of the adversarial evaluation set is not always sufficient, as adversarially robust generalisation increases the sample complexity, and therefore “requires more (training) data” (Schmidt et al., 2018). Wang and Bansal (2018) introduce various improvements to the original ADDSENT algorithm, in order to generate enough training data to obtain robustness for the adversarial evaluation set introduced by Jia and Liang (2017). Geiger et al. (2019) propose a method to estimate the required size of the training set for any given adversarial evaluation set and apply their theory on evaluating the capability of neural networks to learn compositionality. As an alternative to augmenting training data, Sakaguchi et al. (2019) introduce AFLITE, a method to automatically detect and remove data points that contribute to arbitrary spurious correlations. It has been since empirically validated and theoretically underpinned by Bras et al. (2020).

Furthermore, we include the application of adversarial data generation when employed during the construction of a new dataset: in crowd-sourcing, where humans act as adversary generators and an entry is only accepted if it triggers a wrong prediction by a trained target model (Nie et al., 2019b; Dua et al., 2019b), or when automatically generating multiple choice alternatives until a target model cannot distinguish between human-written and automatically generated options, called **Adversarial Filtering** (Zellers et al., 2018; Zellers et al., 2019).

**Architecture and Training Procedure Improvements** deviate from the idea of data augmentation and seek to train robust and de-biased models from potentially biased data. These methods include joint training (and discarding) of robust models together with models that are designed to exploit the dataset biases (Clark et al., 2019; He et al., 2019), re-weighting the loss function to incorporate the bias in the data (Schuster et al., 2019; Zhang et al., 2019b), parameter regularisation (Sagawa et al., 2020) and the use of external resources, such as linguistic knowledge (Zhou et al., 2019; Wu et al., 2019) or logic (Minervini and Riedel, 2018).
4 Results and Discussion

We report the result of our categorisation of the literature in this section. More than half of the surveyed papers (35) are focusing on the RTE task, followed by analysis of the MRC (25) task with 4 and 5 investigating other and multiple tasks, respectively. Looking at the breakdown by type of analysis according to our taxonomy (Figure 4) we see that most approaches concern adversarial evaluation and propose improvements for robustness against biased data and adversarially generated test data. This is not surprising, as robustness against a type of adversary can only be empirically validated via evaluation on the corresponding adversarial test set.

It is worth highlighting that there is little work analysing MRC data with regard to spurious correlations. We attribute this to the fact, that it is hard to conceptualise the correlations of input and expected output for MRC beyond very coarse heuristics (such as sentence position or lexical answer type), as the input is a whole paragraph and a question and the expected output is typically a span anywhere in the paragraph. For RTE, by contrast, where the input consists of two sentences and the expected output is one of three fixed class labels, possible correlations are easier to unveil. In fact, the sole paper (included in our survey) which reports spurious correlations in MRC data, investigated a dataset where the goal is to predict the right answer given four alternatives, thus considerably constraining the expected output space (Yu et al., 2020). Finally, there are few (4) stress-tests for the task of MRC. Those focus on prediction consistency (Ribeiro et al., 2019), acquired knowledge (Richardson and Sabharwal, 2019), unanswerability (Nakanishi et al., 2018) or multi-dataset evaluation (Dua et al., 2019a) rather than performing an analysis of acquired linguistic or reasoning capabilities.

Regarding the datasets used in the surveyed papers most analyses were done on the SNLI and MNLI datasets (20 and 22 papers, respectively) For RTE. For MRC, the most analysed dataset is SQuAD. 17 RTE and 30 MRC datasets were analysed at least once; we attribute the difference to the existence of various different MRC datasets and the tendency of performing multi-dataset analyses in papers that investigate MRC datasets (Kaushik and Lipton, 2018; Sugawara et al., 2020; Si et al., 2019). For a full list of investigated datasets and the weaknesses reported on them, please refer to Appendix A.

We report, whether the existence of spurious correlations was investigated in the original or a later publication, by applying quantitative methods such as those discussed in Section 3.1: Partial Baselines and Heuristics and Correlations, or whether the dataset was generated adversarially against a neural model. The results are shown in Figure 5. We observe that the publications we use as our seed papers for the survey (c.f. Section 1.3) in fact seem to impact how novel datasets are presented, as after their publication (in years 2017 and 2018) a growing number of papers report partial baseline results and advanced correlations in their data (three in 2018 and seven in 2019). Furthermore, newly proposed resources are progressively pruned against neural models (eight in 2018 and 2019 cumulative). However, for nearly a half (36 out of 75) of the datasets under investigation there is no information about potential spurious correlations and biases yet.
A noteworthy corollary of the survey is that – perhaps unsurprisingly – neural models’ notion of complexity does not necessarily correlate with that of humans. In fact, after creating a “hard” subset of their evaluation data that is clean of correlations, Yu et al. (2020) report a better human performance than on the biased version, directly contrary to neural models they evaluate. Partial baseline methods suggest a similar conclusion: without the help of statistics, humans will arguably not be able to infer, whether a sentence is entailed by another sentence they never see, whereas neural networks excel at it (Poliak et al., 2018; Gururangan et al., 2018). Additionally, models’ prediction confidence does not correlate with human confidence as approximated by inter-annotator agreement on a variety of RTE datasets (Pavlick and Kwiatkowski, 2019).

Finally, results suggest that models can benefit from different types of knowledge that enables them to learn to perform the task even when trained on biased data. Models that incorporate structural biases (Battaglia et al., 2018), e.g. by operating on syntax trees rather than plain text, are more robust to syntactic adversaries (McCoy et al., 2019). In the case of models that build upon large pre-trained language models, the number of the parameters and the size of the corpus used for language model training appear beneficial (Kavumba et al., 2019).

5 Conclusion

We present a structured survey of methods that reveal heuristics and spurious correlations in datasets, methods which show that neural models inherit those correlations or assess their capabilities otherwise, and methods that mitigate this by adversarial training, data augmentation and model architecture or training procedure improvements. Various NLI datasets are reported to contain spurious correlations between input and expected output, might be unsuitable to evaluate some task modality due to dataset design or suffer from quality issues. RTE is a popular target task for these data-centred investigations with more than half of the surveyed papers focusing on it. NLI models, in turn, are shown to exploit those correlations and to rely on superficial lexical cues. Furthermore, they lack generalisation beyond the evaluation set resulting in poor performance on out-of-distribution evaluation sets, generated adversarially or targeted at a specific capability. Efforts to achieve robustness include augmenting the training data with adversarial examples, making use of external resources and modifying the neural network architecture or training objective.

Based on these findings, we formulate the following recommendations for possible future research directions:

• There is a need for an empirical study that systematically investigates the benefits of type and amount of prior knowledge on neural models’ out-of-distribution stress test performance.

• We believe the scientific community will benefit from an application of the quantitative methods that have been presented in this survey to the remaining 36 recently proposed NLI datasets that have not been examined for spurious correlations yet.

• Partial baselines are conceptually simple and cheap to employ for any given task, so we want to incentivise researchers to apply and report their performance, when introducing a novel dataset. While not a guarantee for the absence of spurious correlations (Feng et al., 2019), they can hint at their presence and serve as an upper bound for the complexity of the dataset.

• Adapting methods applied to RTE datasets or developing novel methodology to reveal cues and spurious correlations in MRC data is a possible future research direction.

• While RTE is increasingly becoming a proxy task to attribute various reading and reasoning capabilities to neural models, the transfer of those capabilities to different tasks, such as MRC, remains to be shown yet. Additionally, the MRC task requires further capabilities that cannot be tested in an RTE setting conceptually, such as selecting the relevant answer sentence from distracting context or integrating information from multiple sentences, both shown to be inadequately tested by current state-of-the-art gold standards (Jia and Liang, 2017; Jiang and Bansal, 2019). Therefore it is
important to develop those “stress-tests” for MRC models as well, in order to gain a more focussed understanding of their capabilities and limitations.

We want to highlight, that albeit exhibiting cues or weaknesses in design, the availability of multiple large-scale datasets is a vital step in order to gain empirically grounded understanding of what the current state-of-the-art NLI models are learning and where they still fail. This is a necessary requirement for building the next iteration of datasets and model architectures and therefore further advance the research in NLP.

While the discussed methods seem to be necessary to make progress and gain a precise understanding of the capabilities and, most importantly, of the limits of existing (deep learning-based) approaches and can guide research towards solving the NLI task beyond leaderboard performance on a single dataset, the question persists whether they are sufficient. It remains to be seen whether the availability of benchmark suites (Wang et al., 2019a; Wang et al., 2019b) consisting of multiple training and evaluation datasets – open-domain or targeted at a specific phenomenon – will provide enough diversity to optimise models that are robust enough to perform any given natural language understanding task, the so called “general linguistic intelligence” (Yogatama et al., 2019).

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## Detailed Survey Results

Figure 6: Word cloud with investigated RTE, MRC and other datasets. Size proportional to the number of surveyed papers investigating the dataset.

The following table shows the full list of surveyed papers, grouped by dataset and method applied. As papers might report the application of multiple methods on multiple datasets, they can appear in the table more than once.

| Dataset   | Method used                | Used by / Investigated by                                                                 |
|-----------|---------------------------|-----------------------------------------------------------------------------------------|
| HotPotQA  | Partial Baselines         | (Min et al., 2019; Sugawara et al., 2020; Chen and Durrett, 2019b)                      |
|           | Adversarial Evaluation    | (Jiang and Bansal, 2019)                                                                |
|           | Data Improvements         | (Jiang and Bansal, 2019)                                                                |
|           | Arch/Training Improvements| (Jiang and Bansal, 2019)                                                                |
|           | Manual Analyses           | (Schlegel et al., 2020; Pugaliya et al., 2019)                                          |
| MNLI      | Stress-test               | (Naik et al., 2018; Glockner et al., 2018; McCoy et al., 2019; Liu et al., 2019a; Nie et al., 2019a; Richardson et al., 2019) |
|           | Arch/Training Improvements| (Wang et al., 2019a; He et al., 2019; Sagawa et al., 2020; Minervini and Riedel, 2018; Mahabadi and Henderson, 2019; Zhang et al., 2019b; Clark et al., 2019; Mitra et al., 2020; Yaghoobzadeh et al., 2019) |
|           | Heuristics                | (Gururangan et al., 2018; Poliak et al., 2018; McCoy et al., 2019; Zhang et al., 2019a; Nie et al., 2019a; Bras et al., 2020; Tan et al., 2019) |
|           | Partial Baselines         | (Gururangan et al., 2018; Poliak et al., 2018; Nie et al., 2019a)                        |
|           | Manual Analyses           | (Pavlick and Kwiatkowski, 2019)                                                         |
|           | Adversarial Evaluation    | (Chien and Kalita, 2020; Nie et al., 2019a)                                             |
| HELP      | Data Improvements         | (Yanaka et al., 2019b)                                                                  |
| SNLI      | Stress-test               | (Glockner et al., 2018; Nie et al., 2019a; Richardson et al., 2019)                     |
|           | Data Improvements         | (Kang et al., 2018; Mitra et al., 2020; Kaushik et al., 2020)                            |
|           | Heuristics                | (Gururangan et al., 2018; Poliak et al., 2018; Zhang et al., 2019a; Nie et al., 2019a; Rudinger et al., 2017; Bras et al., 2020; Tan et al., 2019) |
|           | Partial Baselines         | (Gururangan et al., 2018; Poliak et al., 2018; Feng et al., 2019; Nie et al., 2019a)    |
|           | Adversarial Evaluation    | (Sanchez et al., 2018; Nie et al., 2019a)                                              |
|           | Manual Analyses           | (Pavlick and Kwiatkowski, 2019)                                                         |
| Dataset       | Evaluation Type       | Baseline Type               | Reference                                                                 |
|--------------|-----------------------|-----------------------------|---------------------------------------------------------------------------|
| SciTail      | Stress-test           | Heuristics                  | (Glockner et al., 2018)                                                   |
|              |                       | Partial Baselines           | (Poliak et al., 2018)                                                     |
| COPA         | Heuristics            |                             | (Kavumba et al., 2019)                                                    |
|              | Stress-test           |                             | (Kavumba et al., 2019)                                                    |
| SICK         | Arch/Training Improvements | Heuristics                  | (Wang et al., 2019a; Zhang et al., 2019b)                                 |
|              |                       | Partial Baselines           | (Poliak et al., 2018; Zhang et al., 2019a)                                |
|              |                       |                             | (Poliak et al., 2018; Lai and Hockenmaier, 2014)                          |
| ADD-1        | Heuristics            |                             | (Poliak et al., 2018)                                                     |
|              |                       | Partial Baselines           | (Poliak et al., 2018)                                                     |
| DPR          | Heuristics            |                             | (Poliak et al., 2018)                                                     |
|              |                       | Partial Baselines           | (Poliak et al., 2018)                                                     |
| FN+          | Heuristics            |                             | (Poliak et al., 2018)                                                     |
|              |                       | Partial Baselines           | (Poliak et al., 2018)                                                     |
| JOCT         | Heuristics            |                             | (Poliak et al., 2018)                                                     |
|              |                       | Partial Baselines           | (Poliak et al., 2018)                                                     |
|              |                       | Manual Analyses             | (Pavlid and Kwiatkowski, 2019)                                            |
|              |                       | Arch/Training Improvements  | (Zhang et al., 2019b)                                                     |
| MPE          | Heuristics            |                             | (Poliak et al., 2018)                                                     |
|              |                       | Partial Baselines           | (Poliak et al., 2018)                                                     |
| SPR          | Heuristics            |                             | (Poliak et al., 2018)                                                     |
|              |                       | Partial Baselines           | (Poliak et al., 2018)                                                     |
| SQuAD        | Adversarial Evaluation|                             | (Rychalska et al., 2018; Wallace et al., 2019; Mudrakarta et al., 2018; Jia and Liang, 2017; Basaj et al., 2018) |
|              |                       | Arch/Training Improvements  | (Min et al., 2018; Wu et al., 2019; Zhou et al., 2019; Clark et al., 2019) |
|              |                       | Stress-test                 | (Liu et al., 2019a; Dua et al., 2019a; Nakanishi et al., 2018; Ribeiro et al., 2019) |
|              |                       | Data Improvements           | (Wang and Bansal, 2018; Nakanishi et al., 2018)                            |
|              |                       | Partial Baselines           | (Sugawara et al., 2020; Kaushik and Lipton, 2018)                         |
|              |                       | Manual Analyses             | (Pugaliya et al., 2019)                                                   |
| DROP         | Adversarial Evaluation|                             | (Dua et al., 2019b)                                                       |
|              |                       | Manual Analyses             | (Schlegel et al., 2020)                                                   |
|              |                       | Stress-test                 | (Dua et al., 2019a)                                                       |
| DNC          | Manual Analyses       |                             | (Pavlid and Kwiatkowski, 2019)                                            |
| RTE2         | Manual Analyses       |                             | (Pavlid and Kwiatkowski, 2019)                                            |
| MSMarco      | Manual Analyses       |                             | (Schlegel et al., 2020; Pugaliya et al., 2019)                            |
| MultiRC      | Manual Analyses       |                             | (Schlegel et al., 2020)                                                   |
|              |                       | Partial Baselines           | (Sugawara et al., 2020)                                                   |
| NewsQA       | Manual Analyses       |                             | (Schlegel et al., 2020)                                                   |
|              |                       | Arch/Training Improvements  | (Min et al., 2018)                                                       |
|              |                       | Stress-test                 | (Dua et al., 2019a)                                                       |
| ReCoRd       | Manual Analyses       |                             | (Schlegel et al., 2020)                                                   |
| ROCStories   | Partial Baselines     |                             | (Schwartz et al., 2017; Cai et al., 2017)                                 |
|              | Heuristics            |                             | (Cai et al., 2017)                                                       |
| Dataset       | Method                  | Reference                                                                 |
|--------------|-------------------------|---------------------------------------------------------------------------|
| TriviaQA     | Arch/Training Improvements | (Min et al., 2018; Clark et al., 2019)                                   |
| FEVER        | Arch/Training Improvements | (Mahabadi and Henderson, 2019; Schuster et al., 2019)                     |
|              | Adversarial Evaluation   | (Thorne et al., 2019)                                                   |
|              | Heuristics               | (Schuster et al., 2019)                                                  |
|              | Data Improvements        | (Schuster et al., 2019)                                                  |
| ARCT         | Heuristics               | (Niven and Kao, 2019)                                                   |
|              | Adversarial Evaluation   | (Niven and Kao, 2019)                                                   |
| ARC          | Stress-test              | (Richardson and Sabharwal, 2019)                                         |
| OBQA         | Stress-test              | (Richardson and Sabharwal, 2019)                                         |
| CoQA         | Partial Baselines        | (Sugawara et al., 2020)                                                  |
|              | Manual Analyses          | (Yatskar, 2019)                                                          |
| DuoRC        | Partial Baselines        | (Sugawara et al., 2020)                                                  |
|              | Stress-test              | (Dua et al., 2019a)                                                      |
| MCTest       | Partial Baselines        | (Sugawara et al., 2020; Si et al., 2019)                                 |
|              | Adversarial Evaluation   | (Si et al., 2019)                                                       |
| RACE         | Partial Baselines        | (Sugawara et al., 2020; Si et al., 2019)                                 |
|              | Adversarial Evaluation   | (Si et al., 2019)                                                       |
| SQuAD 2.0    | Partial Baselines        | (Sugawara et al., 2020)                                                  |
|              | Stress-test              | (Dua et al., 2019a)                                                      |
|              | Manual Analyses          | (Yatskar, 2019)                                                          |
| SWAG         | Partial Baselines        | (Sugawara et al., 2020; Trichelair et al., 2019)                         |
|              | Adversarial Evaluation   | (Zellers et al., 2019; Zellers et al., 2018)                             |
| CNN          | Manual Analyses          | (Chen et al., 2016)                                                      |
|              | Partial Baselines        | (Kaushik and Lipton, 2018)                                               |
| DailyMail    | Manual Analyses          | (Chen et al., 2016)                                                      |
| DREAM        | Partial Baselines        | (Si et al., 2019)                                                        |
|              | Adversarial Evaluation   | (Si et al., 2019)                                                        |
| MCScript     | Partial Baselines        | (Si et al., 2019)                                                        |
|              | Adversarial Evaluation   | (Si et al., 2019)                                                        |
| MCScript 2.0 | Partial Baselines        | (Si et al., 2019)                                                        |
|              | Adversarial Evaluation   | (Si et al., 2019)                                                        |
| Hella-SWAG   | Adversarial Evaluation   | (Zellers et al., 2019)                                                   |
| ANLI         | Adversarial Evaluation   | (Nie et al., 2019b)                                                      |
| Narrative-QA | Stress-test              | (Dua et al., 2019a)                                                      |
| Quoref       | Stress-test              | (Dua et al., 2019a)                                                      |
| ROPES        | Stress-test              | (Dua et al., 2019a)                                                      |
| WikiHop      | Partial Baselines        | (Chen and Durrett, 2019b)                                                |
| QNLI         | Heuristics               | (Bras et al., 2020)                                                      |
| CBT          | Partial Baselines        | (Kaushik and Lipton, 2018)                                               |
|              | Arch/Training Improvements | (Grail et al., 2018)                                         |
| Who-did-What | Partial Baselines        | (Kaushik and Lipton, 2018)                                               |
| bAbI         | Partial Baselines        | (Kaushik and Lipton, 2018)                                               |
The following table shows those 36 datasets from Figure 5 broken down by year, where no quantitative methods to describe possible spurious correlations have been applied yet:

| Year | Dataset |
|------|---------|
| 2015 | MedlineRTE (Abacha et al., 2015), WikiQA (Yang et al., 2015), DailyMail (Hermann et al., 2015) |
| 2016 | MSMarco (Bajaj et al., 2016), BookTest (Bajgar et al., 2016), SelQA (Jurczyk et al., 2016), WebQA (Li et al., 2016) |
| 2017 | SearchQA (Dunn et al., 2017), NewsQA (Trischler et al., 2017), GANNLI (Starc and Mladenić, 2017), TriviaQA (Joshi et al., 2017), CambridgeDialogs (Wen et al., 2017) |
| 2018 | PoiReviewQA (Mai et al., 2018), NarrativeQA (Kočiský et al., 2018), ReCoRd (Zhang et al., 2018), ARC (Clark et al., 2018), QuAC (Choi et al., 2018), emrQA (Pampari et al., 2018), ProPara (Dalvi et al., 2018), MedHop (Welbl et al., 2018), OBQA (Mihaylov et al., 2018), BioASQ (Kamath et al., 2018) |
| 2019 | BiPaR (Jing et al., 2019), NaturalQ (Kwitkowski et al., 2019), ROPES (Lin et al., 2019), SherLiC (Schmitt and Schütze, 2019), CLUTRR (Sinha et al., 2019), PubMedQA (Jin et al., 2019), WIQA (Tandon et al., 2019), HELP (Yanaka et al., 2019b), HEAD-QA (Vilares and Gómez-Rodríguez, 2019), CosmosQA (Huang et al., 2019), TWEET-QA (Xiong et al., 2019), RACE-C (Liang et al., 2019), VGNLI (Mullenbach et al., 2019), CEAC (Liu et al., 2019a) |
B Inclusion Criteria for the Dataset Corpus

We expand the collection of papers introducing datasets that were investigated or used by any publication in the original survey corpus (e.g. those shown in Figure 6 by a Google Scholar search using the queries shown in Table 3. We include a paper if it introduces a dataset for an NLI task according to our definition and the language of that dataset is English, otherwise we exclude it.

```
allintitle: reasoning ("reading comprehension" OR "machine comprehension") -image -visual -"knowledge graph" -"knowledge graphs"
allintitle: comprehension (((set OR dataset) OR corpus) OR benchmark) OR "gold standard") -image -visual -"knowledge graph" -"knowledge graphs"
allintitle: entailment (((set OR dataset) OR corpus) OR benchmark) OR "gold standard") -image -visual -"knowledge graph" -"knowledge graphs"
allintitle: reasoning (((set OR dataset) OR corpus) OR benchmark) OR "gold standard") -image -visual -"knowledge graph" -"knowledge graphs"
allintitle: QA (((set OR dataset) OR corpus) OR benchmark) OR "gold standard") -image -visual -"knowledge graph" -"knowledge graphs"
allintitle: NLI (((set OR dataset) OR corpus) OR benchmark) OR "gold standard") -image -visual -"knowledge graph" -"knowledge graphs"
allintitle: language inference (((set OR dataset) OR corpus) OR benchmark) OR "gold standard") -image -visual -"knowledge graph" -"knowledge graphs"
allintitle: "question answering" (((set OR dataset) OR corpus) OR benchmark) OR "gold standard") -image -visual -"knowledge graph" -"knowledge graphs"
```

Table 3: Google Scholar Queries for the extended dataset corpus