Boundary-based MWE segmentation with
text partitioning

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In this article we present a novel algorithm for the task of comprehensively segmenting texts into MWEs. With the basis for this algorithm (referred to as text partitioning) being recently developed, these results constitute its first performance-evaluated application to a natural language processing task. A differentiating feature of this single-parameter model is its focus on gap (i.e., punctuation) crossings as features for MWE identification, which uses substantially more information in training than is present in dictionaries. We show that this algorithm is capable of achieving high levels of precision and recall, using only type-level information, and then extend it to include part-of-speech tags to increase its performance to state-of-the-art levels, despite a simple decision criterion and general feature space (which makes the method directly applicable to other languages). Since the existence of comprehensive MWE annotations are what drive this segmentation algorithm, these results support their continued production. In addition, we have updated and extended the strength-averaging evaluation scheme, allowing for a more accurate and fine-grained understanding of model performance, and leading us to affirm the differences in nature and identifiability between weakly- and strongly-linked MWEs, quantitatively.

1. Introduction

Multiword expressions (MWEs) constitute a mixed class of complex lexical objects that exhibit a variety of morphological properties and often behave in syntactically unruly ways. The unifying thread that ties this touchy class together is the lexicalization of multiple words into a single unit, and the main discrimination that initiates a rough—there are gradations in this property (Bannard, Baldwin, and Lascarides 2003)—taxonomy is semantic compositionality, where MWEs either do, or don’t express meanings that can be derived by from those of the their words and the rules of grammar. For all of their strangeness they appear across natural languages (Jackendoff 1997; Sag et al. 2001), though generally not for common meanings, and often with opaque etymologies that confound non-native speakers.

We focus on the task of comprehensive MWE segmentations of text. While other studies initially focus the identification task toward specific categories or sizes of MWEs only (Tsdevkov and Wintner 2011), our method advances the engineering challenge of comprehensive (both in category and and size) segmentations, recently established in (Schneider et al. 2014). The identification of MWEs and collocations is an area of study that has seen notable attention in recent years, (Seretan 2008; Pecina 2010; Newman et al. 2012; Ramisch 2015; Schneider and Smith 2015), and has a strong history of attention (both directly and through related work) in the literature (Becker 1975; Church and Hanks 1990; Sag et al. 2001). In accord with some of the more recent work (Schneider et al. 2014), we accept the definition of an MWE to be “a group of
tokens in a sentence that cohere more strongly than ordinary syntactic combinations,” which is
nicely general and leaves space for a broad range of idiomatic classes.

The automatic and efficient identification of MWEs holds promise for numerous applica-
tions, notably including machine translation (Carpuat and Diab 2010) (where idioms can be
translated whole), sense disambiguation (Finlayson and Kulkarni 2011) (where polysemy is often
reduced in an aggregate expression), information retrieval (Newman et al. 2012) (for keyphrase
extraction), and second language learning (Ellis, Simpson-Vlach, and Maynard 2008) (where
blocked text can direct learners to idiomatic constructions), to name a few.

Using the the recent work of (Schneider et al. 2014), who developed the first comprehen-
sively MWE-annotated corpus, we explore and apply our concurrently developed methodology
of text partitioning (Williams et al. 2015). While text partitioning has shown theoretical promise
at improving naïve, multinomial representations of texts through increased term-term indepen-
dence (exhibiting stronger connections to stochastic models of language generation (Williams et
al. 2016)), the methodologies remain largely unevaluated in the context of MWE identification.
Consequently, the general and comprehensive nature of the MWE-annotated corpus provides a
novel opportunity to perform an evaluation of text partitioning, once adequately tuned for the
MWE identification task.

2. Model

2.1 General description

Our approach to MWE identification directly descends from the text partitioning framework
developed in (Williams et al. 2015, 2016). In the framework’s most recent instantiation, a
partition would be drawn between a pair of words \((w_l, w_r)\) with probability proportional to
the number times the pair had been observed to be partitioned across some training data. For
example, if presented with the text “Out to lunch in New York City,” extraction of the MWE
New York City
would require information from training that the pairs (New, York) and (York,
City) are frequently not partitioned, and that the pair (in, New) frequently is. However, as we
will continue with here, it is important to create a default behavior, biasing towards splitting, for
the case when no knowledge of partition frequency is known, since the feature space of pairs
is large, and training data is generally sparse (especially on information regarding the edges of
MWEs).

Here, we build an additional layer of sophistication by observing partition frequencies across
arbitrary gaps, i.e., across any blocks of non-word characters. This provides a larger degree of
sensitivity, ensures that arbitrary forms are constructible, and has the appeal of processing text
exactly as it is recorded, since whitespace is not assumed and punctuation is not ignored. Furth-
ernore, instead of partitioning pairs of words according to empirical probabilities stochastically,
we set a threshold partition probability \(q\) (the single model parameter), and partition only those
pairs whose empirical partition probabilities are at least \(q\), thereby building a tunability into the
model.

To conceptualize the process of text partitioning, it is most helpful to view a text initially as
a mixed sequence of word (here, alphabetics and the apostrophe) and complimentary non-word
blocks. For example, the text New York, N.Y. may be tokenized as the sequence

\[
\text{New} \quad \_ \quad \text{York} \quad \_ \quad \text{N} \quad \_ \quad \text{Y} \quad .
\]

Predictive features are then taken as those contiguous (for now) sequences with at most one word
block on the left and one word block on the right. Note then that this resolves non-word blocks
(“colored” by surrounding word blocks) as the predictive features of partitioning. For example,
the observed features that determine whether or not New York, N.Y. is constructed are
Since text partitioning (in its current form) only focuses on separating text with knowledge of immediate local information (surrounding word pairs), it can have the tendency of leaving too many boundaries unpartitioned. For example, if presented with the text “I go for take out there, frequently.” there is a good possibility that the algorithm would leave the non-MWE segment 
*take out there* unpartitioned, since both *take out* and *out there* are known MWEs and may have been observed sufficiently in training. To balance this tendency, we apply a directional, lookup-based algorithm to all candidate MWEs, which we refer to as the longest first defined (LFD) algorithm (see Alg. 1). At a high level, this algorithm prunes candidates by clipping off the longest known (MWE) references (or individual blocks, when references are not found) from left to right. Continuing with this example, the LFD would find *take out there* unreferenced, and start working on the next shortest segments from left to right, to discover *take out*, which, being defined, would be accepted, leaving the process to repeat on the remainder. Putting this all together, our model consists of two main steps—locally driven pairwise partitioning, and pruning for defined terms by the LFD algorithm.

**Algorithm 1** Pseudocode for the longest first defined (LFD) algorithm. Here, candidate MWEs are pruned from left to right for the longest lexemes referenced in a training lexicon, *lex*. When no form is found in *lex*, the first block is automatically pruned from the candidate, (accepting it as an expression), which then starts from the next block.

1: procedure LFD(candidate)
2:     lexemes ← (·)
3:     N ← length(candidate)
4:     while N do
5:         indices ← (N + 1) : 1
6:         for i in indices do
7:             form ← join(candidate[0 : i])
8:             remaining ← candidate[i : N]
9:             if form ∈ lex or not i − 1 then
10:                lexemes ← lexemes ^ form
11:             if length(candidate) = 1 then
12:                 candidate ← (·)
13:             else
14:                 candidate ← remaining
15:         break
16:     N ← length(candidate)
17: return lexemes

### 2.2 Gappy expressions

Prediction of gappy expressions is handled in a similar manner to contiguous expressions, where predictive features have at most one word block on the left and one word block on the right. However, since gappy expressions cross over other word blocks, we create a special, unique punctuation block, which simply indicates that a gap is present. For example, if presented with the text *putting me at my ease*, the predictive features of gap-size 1 are

*putting <gap> at me <gap> my at <gap> ease*
and the predictive features of gap-size 2 are

\[ \text{putting} \ <\text{gap}> \ my \ \text{me} \ <\text{gap}> \ ease \]

with larger gaps modeled similarly\(^1\).

### 2.3 Link strength

In the comprehensively-annotated data set, links between tokens in MWEs are given two strength classifications—weak (compositional) and strong (non-compositional). We handle strength classification simplistically, by recording the frequency at which a given predictive feature is bound by a weak or strong link in training, and predict the more likely strength, with a default of strong (since there are far and away more strong links in the annotations) if a feature was either not observed, or a was observed as weak and strong with the same frequency in training.

### 2.4 Feature composition

For our base model we employ un-cased, type-level information. The only differently composed features we leverage are in an enhancement of the base model, where part-of-speech tags are used. In this enhancement, the part-of-speech-based model runs alongside the base model and predicts any MWEs (that it can) which have not already been predicted by the base model. When predicting MWEs by part-of-speech tags, the text partitioning and LFD algorithms operate in precisely the same conceptual manner, with type-level information replaced by tags inside of features (creating a special, unique tag for the space character, and inserting whitespace between tags when composing features).

We specifically choose to compose features by elements that avoid specificity to the English language. Given sufficient training data, this model can be used to predict MWEs in other languages in precisely the same manner. The main detail which must be sorted out in order to apply this model to other languages is the determination of an appropriate dichotomy of word- and non-word blocks, which ultimately define features. This is in contrast to several of the (many) features used in (Schneider and Smith 2015), which are tailored to English. For example, relying on capitalization for the prediction of proper noun compounds and named entities will be unhelpful if applied to German (where all nouns are capitalized), and irrelevant in many Asian languages (where case generally does not exist).

### 3. Materials

#### 3.1 Preprocessing

To evaluate our model we use the newest version (Schneider and Smith 2015) of the comprehensively annotated MWE corpus of 55,000 words originally developed in (Schneider et al. 2014). In order to work with this data, we require associated information on the locations of whitespace (recall non-word blocks are predictive features). Since whitespace was removed from the corpus annotations, we use the part of speech annotations present to assist in the inference of the locations of whitespace, e.g., the labeling of double-quotes as left or right indicates the

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\(^1\) We forego using the sizes of gaps in the special punctuation blocks (where one could create a unique punctuation block for each gap size) both for simplicity and the expectation that variation of gap size exists across instances of a given gappy expression, e.g., *putting my mind at ease*. 
presence of whitespace at left or right, respectively. Note that this does not in any way impact the coding of MWEs in the data set, and leaves our results entirely comparable to those of others.

3.2 Training data

To train our model, we integrate multiple sources of data. Since the model is based on the frequency at which boundaries are cut or bridged, the comprehensive annotations in (Schneider et al. 2014) provide the most significant source of information on the boundaries between high-frequency words. So, while we utilize the example usages of MWEs held inside of the Wiktionary (Wiktionary 2016) and WordNet (Miller 1995) dictionaries to help build information on a larger diversity of MWEs into the model, it is essential to combine the information from these with that of the comprehensive annotations. Rehashing an example from (Schneider et al. 2014), the phrase eat in, as held by WordNet, means to eat at home, but is quite often not meant as such (e.g., a document states“One of my top 5 places to eat in Baltimore.”) when the two words are adjacent, which is recorded well in the comprehensive annotations. To balance and combine the different sources of training data, we scale the gap-bridging frequencies of each training corpus by the total numbers of observed gaps (both bridged and cut). In addition to the Wiktionary and WordNet dictionaries, we similarly utilize the hyperlinks (the text of the target, not the displayed text) present in all Wikipedia articles (Wikipedia 2016) (without using any term extraction measures for filtering, unlike in (Hartmann, Szarvas, and Gurevych 2012)) to provide access to the large number of noun and proper noun compounds held therein.

4. Methods

4.1 Evaluation

To maintain comparability with the results in (Schneider and Smith 2015), we proceed with the MUC (Vilain et al. 1995) evaluation method, and likewise apply the strength-averaging scheme to assess overall, precision ($P$), recall ($R$), and $F_1$ performance of our methods. While in the discussion of our results we also find it instructive to consider other methods for strength averaging, we stick to the original scheme for comparability. For a more detailed discussion of how strength averaging can be generalized and extended, see Appendix 1.1.

4.2 Part-of-speech tagging

To derive features from part-of-speech information it is best to have the most accurate information possible. The part-of-speech tags bundled with the comprehensively-annotated data set are a strong, human-coded gold standard (Oracle), and in all cases are used for training. To maintain comparability with (Schneider and Smith 2015), we test on the Oracle tags as well, but to fully understand the likely performance of our model in a real-world scenario, we also test on automatically-predicted tags, using the default, averaged perceptron tagger in the Python NLTK (Loper and Bird 2002) package (using the ‘PerceptronTagger()’ method).

4.3 Experimental design

To measure the performance of our model we proceed with the 8-fold (101 test document) cross-validations scheme, maintaining comparability with the work in (Schneider and Smith 2015). However, we note that this performance evaluation method can be fraught. The MWEs that appear in documents vary greatly, and in the comprehensively-annotated data set it is worth noting that 60.33% of all MWEs appear exactly once (these cover 66.00% of all MWE-internal
boundaries, i.e., the features we use for prediction). As a result, this puts a hard upper limit near 35% on the recall potential for in-domain type-level information (we will return to this fact later on, when considering our results). Perhaps more to the point is the fact that setting aside only 101 of 3,812 (roughly 3.8%) documents for a test set injects a huge degree of uncertainty into the nature of the final test set (especially when more than 60% of MWEs are once-occurring), and the reliability of any numbers measured therefrom. To combat this issue in our own experiment (while maintaining comparability to the results in (Schneider and Smith 2015)), we leverage the speed and simplicity of our method, and repeat the final step—testing on 101 randomly drawn documents with a model trained on the remaining 3,711, though using the q-parameterization from only one run of the full 8-fold cross validation—for 100 randomizations of the data set. By doing this, we are able to reasonably quantify the uncertainty in our own results, and draw insight for comparability with the results from (Schneider and Smith 2015).

5. Results

We test our algorithm on the comprehensively annotated collection of web reviews originally produced in (Schneider et al. 2014), but in the updated format bundled in the STREUSLE data set (Schneider and Smith 2015) (once adequately preprocessed for whitespace, as discussed in Sec. 3).

First, we explore the base form of our model (type-level information, only). For each of the eight folds in the training set, we evaluate the performance of our model across 100 evenly-spaced values of $q \in (0, 1]$. Averaging across the eight folds, we find a non-trivial $F_1$-optimal parameterization at $q = 0.82$, where $P = 71.73\%$, $\sigma_P = 2.60\%$; $R = 45.99\%$, $\sigma_R = 2.41\%$; and $F_1 = 55.98\%$, $\sigma_{F_1} = 2.18\%$ (see red dashed lines, Fig. 1). While this portion of the experiment sets $q$ for the model, we only compare our results to other models when ours is trained on all eight folds and applied to left-out test sets of 101 documents as discussed in Sec. 4.3. In this scenario, the mean values across 100 test runs were $P = 74.31\%$, $\sigma_P = 5.62\%$; $R = 45.49\%$, $\sigma_R = 5.39\%$; and $F_1 = 56.24\%$, $\sigma_{F_1} = 5.03\%$ which show precision somewhat improved (see Base box plots in Fig. 1), whether by the $\approx 15\%$ increases of in-domain training data, or better estimation of uncertainty from bootstrapping over 100 repetitions. Notably, we observe that the values of standard deviation are approximately doubled, which speaks to the importance of bootstrapping this portion of the experiment. When comparing these results to the numbers in (Schneider and Smith 2015) ($P = 72.97\%$, $R = 55.55\%$, and $F_1 = 63.01\%$), we see that our model (in this form) has not achieved the same level of recall, though must take stock of the fact that their model was trained on many more specialized types of features (including part-of-speech, which we explore next), and was run on a test sample once, for one randomization of the data set, and therefore might exhibit different performance on average$^2$.

When we apply our model with the part-of-speech enhancement we see substantially improved results. Through cross-validating in a similar manner (optimizing for $F_1$ with two independent q-parameters over 10,000 evenly-gridded points, $(q_{\text{type}}, q_{\text{POS}}) \in (0, 1] \times (0, 1]$), we find an optimal parameterization of $(q_{\text{type}}, q_{\text{POS}}) = (0.76, 0.45)$ that when applied to the 100 test repetitions (with Oracle part-of-speech tags) yields $P = 76.50\%$, $\sigma_P = 4.83\%$; $R = 54.23\%$, $\sigma_R = 5.46\%$; and $F_1 = 63.27\%$, $\sigma_{F_1} = 4.57\%$ (see Oracle box plots in Fig. 1), which exhibits higher precision and slightly lower recall than the results in (Schneider and Smith 2015),

\footnote{While one might (with more certainty) compare the results from our eight-fold cross-validation (which are roughly the same) to that reported in (Schneider et al. 2014), the updated version of the data set (Schneider and Smith 2015) includes over 474 changes to the 2,526 annotated MWEs (roughly 20%), making results between the two data sets not directly comparable, as is evidenced by significant variation in precision and recall between the results in (Schneider et al. 2014) and (Schneider and Smith 2015).}
Figure 1
Line-plot envelopes exhibiting the results of an 8-fold cross-validation (left column), where gray lines indicate minimum and maximum values, and black lines indicate means for each of the 100 evenly-spaced values of $q$ in the single-parameter, base model where only type-level information is used. The $F_1$-optimal case is indicated by red dashed lines, and sets the parameter $q$ for experimentation on test sets of 101 documents (right column, leftmost column of 'Base' box plots), where the segmentation algorithm is trained on all but 101 test documents for 100 randomizations. In the two adjacent box plot columns are the results of training and testing on the same 100 randomizations, but with the model extended to two $q$-parameters—one each for both type and part-of-speech information, with the center box plots testing with knowledge of human-annotated, 'Oracle' tags, and the far right box plots testing with 'Percept.' predicted tags. Each of the rightmost columns of box plots were similarly cross-validated, though optimizing for $F_1$ in the two-dimensional parameter space over 10,000 evenly-gridded points.

amounting to a slight overall $F_1$ improvement over the state-of-the-art, despite being trained on fewer MWE lexica.

While it is important to understand the performance of our model in a scenario that is most comparable to (Schneider and Smith 2015), we also stress the importance of understanding the performance of our enhanced model in a more realistic setting, when only imperfect part-of-speech information is possessed. So, instead of the provided Oracle part-of-speech tags, we
repeat the same experiment using the out-of-the-box averaged perceptron (see Percept. box plots in Fig. 1) part-of-speech tagger from Python’s NLTK package (Loper and Bird 2002). Doing so (on the same 100 test runs), we see similar performance in recall, but a definite decrease in precision: \( \overline{P} = 66.59\%, \sigma_P = 7.33\%; \overline{R} = 52.96\%, \sigma_R = 5.45\%; \) and \( \overline{F_1} = 58.66\%, \sigma_{F_1} = 4.92\% \), as compared to the results by Oracle tags, and note further that from the cross-validation the model’s parameterization changed slightly to be \((\theta_{type}, \theta_{POS}) = (0.76, 0.41)\), reflecting the increased uncertainty around part-of-speech information.

6. Discussion

6.1 How fast is text partitioning?

The base system runs the fastest, segmenting text at more than 22,000 words per second. When enhanced with known part-of-speech tags the only additional cost to speed is running the model twice, which amounts to processing at about 12,000 words per second. Compared to the final system in (Schneider and Smith 2015) that was observed to process roughly 140 words per second (though supersense class labeling was done there, too), ours is quite fast. However, since part-of-speech information is not generally known, we also report the increase in runtime when using NLTK’s default averaged perceptron classifier, which puts the total system speed at about 6,500 words per second. All experiments were run on a single core of a 3.1 Ghz processor.

6.2 Are gappy expressions learned?

We are able to assess the extent to which our model recovers gappy expressions by rerunning the Oracle-enhanced system without training on gappy features (described in 2.2). In this scenario, average precision remains the same, but recall decreases by about 1.5 points, indicating that gappy expressions are learned and predicted with no greater uncertainty than contiguous expressions. From the increase in recall, the learned gappy expressions provide an average overall increase in \( F_1 \) of 1 point\(^3\).

The recovery rate for gappy expressions \((\approx 10\%)\) is substantially lower than that for contiguous expression \((\approx 50\%)\). We understand this as follows. Many gappy expressions are compositional (we note this difficulty with recovering weakly-linked expressions in Secs. 6.3, 6.6, and in Appendix 1.1), and are usually only once-appearing in the comprehensive annotations. Since our methods presently cannot use the out-of-domain data sets to model gappy expressions, we propose their coding for gappiness as a means of model improvement. For example, the Wiktionary entry for put at ease could be annotated to indicate the general points of gappy flexibility\(^4\), somewhat like is what is already done in the snowclones’ appendix (https://en.wiktionary.org/wiki/Appendix:English_snowclones).

6.3 Which MWEs are missed?

Most of the missed MWE links tend to appear only once in the annotations (as was noted in Sec 4.3). These are simply not identifiable at testing, unless they fit a common part-of-speech pattern, or are present in the out-of-domain training data. Many of these are actually gappy

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\(^3\) This test is more transparent than in that of (Schneider et al. 2014), where the identification of gappy expressions was assessed by running an experiment in which the annotation scheme (i.e., the data set) was modified to exclude gappy expressions, which reduced the total number expressions that their model was required to recover.

\(^4\) Notably, the listed example for put at ease in the Wiktionary—put my mind at ease—exhibits gappiness, but unfortunately does not code for this property generally.
expressions (as discussed in Sec. 6.2), and some are missed as a result of over-annotation, e.g., *was a no brainer* is linked strongly, instead of just *no brainer* (the non-compositional part, which the Wiktionary covers). There are a number of links missed on account of training sparsity in casing and tense, e.g., *Branch out* at the start of a sentence and *shopped around* in the middle of another (though variants of both are held in the Wiktionary), indicating the possibility for enhancement of recall if lemmatization and casing were incorporated in the model (though precision would suffer). Occasionally, links are are missed because of the simplicity of the model. For example, the model misses the link bridging *and every one of* them because the comprehensive annotations indicate that *and every* has a very high probability of being partitioned. This is unfortunate, but could be combatted through a higher-order partitioning model that considers larger contexts than just surrounding words. Also, many weak links are missed on account of their compositional nature. For example *back for more* and *found a spot* are rare, and possess links between words that are frequently partitioned (the former is also inconsistently annotated as a non-MWE, which we discuss in Secs. 6.4 and 6.6).

For some MWEs held in the Wiktionary no accompanying examples are listed. This leaves them out of the model’s training, and makes them inaccessible if they are once-appearing in the comprehensive annotations. However, this indicates the possibility that performance may increase over time as these sources continue to grow and more examples are included.

### 6.4 Which MWEs are incorrectly predicted?

Surprisingly, a large portion of precision loss (perhaps half) is actually due to annotation inconsistencies. For example *went in* is annotated as a strongly-linked MWE 3 out of 5 times in the data set, despite a usage that is generally the same across the 5. Perhaps more problematic is the annotation of *a few*, which is a strongly-linked MWE 13 out of 25 times, despite a usage that is generally the same across the lot. From just one run of our test procedure, we can also report (inconsistencies) similarly about instances of *or so, and yet, Trust me, took him back,* and *Not only.* A number of false positives also resulted from MWEs that are present in the out-of-domain training lexicons, but not in the annotations. For example, the document “Just had our car returned *this morning.*” refers to the day of writing implicitly. In some English dialects it is more common to use the literalism *today morning,* which is often a direct translation from mother tongues, and strangely unacceptable in English, despite the acceptance of related expressions like *yesterday morning* and *tomorrow night.* Given these considerations, both precision and recall are likely underestimated in our work and others.

There are a number of truly false positives. As mentioned in Sec. 6.5, relying on an automatic part-of-speech tagger can be fickle, since flippant capitalizations are often confused with proper nouns. Additionally, there are some cases when MWEs were predicted incorrectly as a result of polysemy and incorrect semantic attribution (as discussed in Sec. 3.2), like “I could nt fit in it”, “...the common continental affair *at most* cheap hotels.”, “He had a robe that was made back in the’60s.”, “...takes his time to *make it* look right.”, “...have them *come to* the house...”, and “It made me *feel good* to see.” While these cases are actually somewhat rare, they are very important from a modeling perspective, and if successfully parsed, would indicate a high level of sophistication.

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5 In principle, one could train on the internal boundaries of these un-exemplified entries, or those from the other dictionaries that don’t have examples to make them identifiable. However, this could leave the model unbalanced, operating more like a lookup-based algorithm, which were shown in (Schneider et al. 2014) to exhibit much lower precision. However, so long as this (training on un-exemplified entries) were balanced with training on more comprehensive annotations, the net result could potentially increase overall performance.
6.5 How valuable is part-of-speech information?

While we observe increased performance when our model is enhanced with part-of-speech information, we unfortunately see some of that gain lost when the more realistic scenario of automatically-tagged (see Percept. in Fig. 1) text is considered. Clearly, accurately-tagged words increase performance. So, understanding that the recovery of multiword proper nouns is among the most improved by the part-of-speech enhancement, the accuracy of their tagging becomes very important. Upon observing some sample output, we note that a flippance for capitalization on the part of writers tricks the tagger into incorrectly tagging many words as proper nouns. While many proper nouns are still correctly recovered by the automatically-produced tags (recall is still up), many other words are incorrectly tagged as proper nouns, leaving what would be a safe rule for building MWEs (sequences of proper nouns) much less precise. However, other rules are reasonably safely utilized by the real-world, part-of-speech enhanced model. For example, verb-particle constructions like stressed out and mocked up are more reliably identified through predicted part-of-speech tags, and the overall increase by about 2.5 points over the base $F_1$ score seems to indicate that even predicted tags are worth utilizing. However, as we discuss in Sec. 6.6, there appears to be a much greater potential for high-precision segmentation through type-level information.

6.6 How valuable is type-level information?

For the model presented, the potential of type-level information is surprisingly high. When part-of-speech information is left out entirely, and the model is allowed to train and test on the same data (which is of course not valid for benchmarking, and does not currently reflect any real-word performance scenario), we are able to see just how much the type-level information is capable of identifying correctly, given the simplicity of the model. In this scenario, the strength-averaged $F_1$ comes out to 85.89%, with $P = 88.19\%$, and $R = 83.73\%$, which is quite high. We are therefore able to say (given a sufficient basis of type-level information for training) that the large majority of MWEs are easily identifiable through simple modeling, and with a high degree of precision. So, what other type-level information exists for training, and what other data needs to be annotated the most (since the annotation task is costly)?

6.7 How should comprehensive annotations be extended?

Much as with the task of part-of-speech tagging, we are now faced with the reality that most predictions are possible through simple modeling. As was noted in (Charniak 1997), assigning the most common tag from some training corpus to each known word and the tag proper noun to all unknowns in a test corpus approaches 90% accuracy. For the other 10% (which are by far the most interesting), errors are made on account of tag ambiguity. This is precisely what our unrealistic experiment (from Sec. 6.6) shows us about MWE tagging.

Since much larger gains are still possible through increased training coverage (present resources are still quite sparse), focusing only on model sophistication is probably unwise at this point. However, model sophistication must be addressed, and for this we see the expansion of comprehensive annotations as the best direction to follow, but in a more targeted fashion. We find direction for the task of expanding annotations through modifying and generalizing the strength-averaging evaluation scheme (see Appendix 1.1 for a details on this generalization) put forward

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6 Given the number of annotation inconsistencies (described in Sec. 6.4), both precision and recall may be substantially higher here, too.
in (Schneider et al. 2014), and used in Sec. 5 for comparison. By assessing the performance of our model on weak and strong links separately we find an interesting lead (once again unrealistically training on all documents to get a fuller picture). Surprisingly, even though the strength-averaged $F_1$ was 85.89%, the $F_1$ score for weak links was substantially lower at 68.83%, which was mostly due to a lower recall value of 59.68% (precision was still high, at 81.30%). We attribute this to weakly-linked MWEs being compositionally constructible—potentially a powerful affirmation of the weak-strong MWE dichotomy.

Word pairs that are commonly not part of MWEs tend to confuse our simple model, which only considers the immediate information surrounding the gaps/points of decision. To operate on information that lies farther out, such a model would require comprehensively-annotated data that is far less sparse. To accomplish this, we suggest a targeted annotation strategy, where for particularly confounding MWEs (like the expression came to), a number of examples are collected and annotated, e.g., “The man came to the house.”, and “The man came to after resting in the house.” Another alternative might be found in simultaneously identifying and leveraging the parts of speech of MWEs, such as was done in (Shigeto et al. 2013) (though not comprehensively), by using transition probabilities to determine if a particular form is appropriate. For example, since the MWE came to acts as a verb, it makes good sense in the latter, presaging the preposition after, but little sense in the former, appearing just before a determiner.

6.8 What other type-level information might prove useful?

As we’ve observed in Sec. 6.6, large gains in performance are immediately possible (i.e., using the present model) through increasing coverage of out-of-domain type-level information. Many of the proper nouns and named entities that our model is successful at identifying come from the processed Wikipedia hyperlinks. However, the majority of proper nouns present in Wikipedia are references for places and notable people. So while this data set is helpful at identifying MWEs like New York, N.Y., center city, and Noam Chomsky, it is less helpful for the more obscure places and names of individuals, and in particular the names of business, which pervade the MWE-annotated data set of business reviews. To cover more obscure (names of) individuals with type-level information, opportunities might be possible through leveraging the lists in baby naming books, whether by enumerating combinations, or devising matching methods, where an algorithm might accept any pattern that fits the form <name> <surname>.

Regarding improvements on obscure businesses, an opportunity might be possible with access to a large database of business names, such as is held by Yelp. However, this type of improvement is really only conforming to the domain of the annotations (business reviews). With this in mind, it actually seems quite difficult to assess the performance of any MWE segmentation method for text from other domains, such as social media or literature, where language is more conversational, and business names may not factor in at all. In other words, without extending the comprehensive annotations to other textual domains, we really can’t speak holistically to our grasp on this task.

7. Conclusion

While comprehensive MWE segmentation remains a very challenging task, the work presented here exhibits a number of hopeful possibilities. We have shown that our model (which is fast-running and of low-complexity) is capable of matching the state-of-the-art in performance, while being trained on fewer MWE lexica. The low-complexity of our model is evident in the features used (unaltered, type-level information and part-of-speech), and the prediction criteria (observed splitting frequencies), which provides transparency and an applicability to other languages that
other models don’t. All together this constitutes a strong evaluation, favoring the continued development of text partitioning methods.

We have also shown that the current levels of performance at accomplishing this task are most strongly an exhibition of a deficiency and sparseness of training data, and that our model (at least) is in fact capable of much higher performance (despite its simplicity), provided more corpora are annotated, ideally from disparate domains. Furthermore, as the comprehensive annotations exist (version 2.1), the performance of all models is likely underestimated, on account of a significant number of observed annotation errors. In addition to these findings, we have updated the strength-averaging evaluation scheme and proposed multiple alternatives, which better illustrate the differences in identifiability between weak and strong links, and have thereby affirmed the distinction between weak and strong links (weak, compositional links posing a much greater challenge). With this article, we have also released an updated version of the python partitioner module, making all of our methods available for general use.

References
Boundary-based MWE segmentation


1. Supplemental Material

1.1 Strength averaging evaluation and multi-class labeling

To maintain comparability between our results and those from (Schneider and Smith 2015), we have followed the strength-averaging technique presented in (Schneider et al. 2014) to assess the overall performance of weak and strong link prediction throughout our main results. However, we find a peculiarity of this measure in that it behaves non-symmetrically between weak and strong links, biasing heavily toward strong links, in particular.

Consider the formula for recall: \( R = \frac{1}{2} (R^\uparrow + R^\downarrow) \), which is equivalent to the method of macro-averaging in the field of multi-class classification (Sokolova and Lapalme 2009)\(^7\). This formula is the arithmetic mean of the cases where weak links are strengthened \((R^\uparrow)\), i.e., converted to strong links, and when weak links are ignored \((R^\downarrow)\). The latter of these is certainly a bias toward strong links, and the former is actually of neutral bias, since one could mathematically equivalently frame \(R^\uparrow\) as the measurement taken when all strong links are weakened, or more

\(^7\) There is one important difference between macro-averaging and the simple averages put forward in (Schneider et al. 2014), where the arithmetic mean was used for a strength-averaged \(F_1\) score, as compared to what should have been taken—the harmonic mean of \(P\) and \(R\) values, each respectively macro-averaged. Though we have continued with taking the arithmetic means of \(F_1\) scores in the main results (strictly for comparability), we avoid them when computing the other averaging methods in this section, and strongly advise the proper (harmonic means) for future work.
generally, when link strength is ignored. This brings to light a third possibility—measurement of performance in the case when all strong links are ignored. For clarity, we relabel the values as:

- $R_{\text{relaxed}}$: all links are assessed, with type ignored
- $R_{\text{strong}}$: only strong links are assessed
- $R_{\text{weak}}$: only weak links are assessed

and momentarily consider a simplistically balanced solution (another macro-average), in which the arithmetic mean of all three is computed:

$$R = \frac{1}{3}(R_{\text{relaxed}} + R_{\text{strong}} + R_{\text{weak}}).$$

However caution must be taken here, since each of the three represents an evaluation of a different number of links! The original strength-averaging scenario mostly reflects information about the strong links, leaving the weak links represented at half the weight they should, and incorporating $R_{\text{weak}}$ simplistically (as above) would place the weak links (which are about $\frac{1}{7}$ of all links) at about 38% of the weight of the calculation, which is nearly triple their presence in the data set.

One option with which we are left would be to simply consider each of $R_{\text{relaxed}}$, $R_{\text{strong}}$, and $R_{\text{weak}}$ (and their $P$ and $F_1$ counterparts) as separate entities. This ultimately provides the most refined information, but there is a definite appeal and utility in having a single number as a measure of performance, e.g., for tuning model parameters (such as we have done in this work). For a more balanced option, we propose weighting each of the three measures proportionally, to the numbers of links (or guesses at links, if precision is considered) represented by each (i.e., the denominators), which is equivalent to micro-averaging in the field of multi-class classification (Sokolova and Lapalme 2009). For example, if weak links are exactly $\frac{1}{7}$ of all, the balanced calculation would be

$$R = \frac{7}{14}R_{\text{relaxed}} + \frac{6}{14}R_{\text{strong}} + \frac{1}{14}R_{\text{weak}}.$$

This example turns out to be quite similar (numerically) to the original strength-averaging calculation (see Tab. 1 for a comparison of the different measures), but only because of the disproportionate numbers of weak and strong links in the data set. The three-way balanced calculation is flexible to any changes in the weak/strong proportionality that may occur, so if more conversational language were observed (which might focus less narrowly on the strongly-linked proper nouns of business reviews), we could still expect a meaningful evaluation.

Interestingly, measuring $R$ (or $P$) as balanced averages of the three (relaxed, strong, and weak) is precisely the same as the average of two quantities defined by (1) relaxedly assessing each link for presence only (i.e., the usual $R_{\text{relaxed}}$) and (2) strictly assessing each link for presence of both types, which we will refer to as $R_{\text{double strict}}$. More closely, in the double-strict case a true positive is only counted when prediction and truth are exactly the same, i.e., weak-weak or strong-strong, a false positive is counted when any link is predicted incorrectly, i.e., weak-strong, strong-weak, weak-none, strong-none, and a false negative occurs whenever a link is predicted incorrectly, or not at all, i.e., none-strong, none-weak, weak-strong, strong-weak. Under this framing, a mismatch exhibits negative performance doubly (since simultaneous prediction of both link types is being assessed), which encodes a flexibility that would allow us to assess a slightly different prediction problem where both types could be possessed, simultaneously. Since
Table 1
Examples of the different measurement methods applied to the Oracle part-of-speech enhanced model. Note that the double-strict mixed measure is equivalent to a balanced average of strong and weak, which makes the balanced average of relaxed, strong, and weak equivalent to a balanced (or simple) average of relaxed and double-strict.

<table>
<thead>
<tr>
<th>Measurements</th>
<th>P</th>
<th>R</th>
<th>F₁</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Strong measures</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>strong</td>
<td>75.19</td>
<td>55.67</td>
<td>63.81</td>
</tr>
<tr>
<td>weak</td>
<td>51.42</td>
<td>18.37</td>
<td>26.26</td>
</tr>
<tr>
<td><strong>Mixed measures</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>relaxed</td>
<td>77.82</td>
<td>52.76</td>
<td>62.72</td>
</tr>
<tr>
<td>double-strict (balanced avg., strong and weak)</td>
<td>73.18</td>
<td>49.60</td>
<td>58.98</td>
</tr>
<tr>
<td>strict</td>
<td>73.17</td>
<td>51.22</td>
<td>60.01</td>
</tr>
<tr>
<td><strong>Averaging measures</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>relaxed and strong (original, unbalanced)</td>
<td>75.50</td>
<td>51.18</td>
<td>60.85</td>
</tr>
<tr>
<td>relaxed, strong, and weak (relaxed and double-strict)</td>
<td>75.50</td>
<td>52.00</td>
<td>61.42</td>
</tr>
</tbody>
</table>

this is not the case (a link is never annotated or predicted as both weak and strong), it may be best to approach the strict assessment scenario differently.

Since weak and strong type labels are not applied to links simultaneously, we propose one more alternative for assessment of a strict nature (likely the most appropriate for the task at hand). Here, each link is assessed singularly for absolute correctness, i.e., any true prediction of link presence that is a mismatch of type is counted as a false positive and nothing else. Specifically, the scenarios here are:

TP: strong-strong and weak-weak
FP: strong-none, weak-none, strong-weak, and weak-strong
FN: none-strong and none-weak

Taken with the relaxed case, a balanced average with this may be the most appropriate evaluation for our task, but is unfortunately the most removed from previous work.

The different in measurements are all presented together for comparison in Tab. 1. Starting from the top of the table, the first two rows exhibit the clear difference in difficulty the model has at predicting the different link types, which affirms our other findings from Secs. 6.2, 6.3, and 6.6. Clearly there is much more work to be done with predicting weakly-linked expressions. Moving

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8 However, we do note the niceness of the double-strict scenario for several reasons: (1) it extends and condenses the type-specific measures by the method of micro-averaging, described in (Sokolova and Lapalme 2009); (2) it is flexible in being able to assess a more complex prediction problem, where multiple types may be present simultaneously; and (3) the end computation of an averaged (double-strict & relaxed) $F_1$ is equivalent, regardless of how the average is taken:

$$\frac{1}{2} \left( \frac{P_{\text{double-strict}} + P_{\text{relaxed}}}{2} \right) \frac{1}{2} \left( \frac{R_{\text{double-strict}} + R_{\text{relaxed}}}{2} \right) = \frac{1}{2} \left( \frac{P_{\text{relaxed}} + P_{\text{double-strict}}}{2} \right)$$

since the denominators remain constant across both of the relaxed and double-strict precision and recall, respectively (which also makes the simple (macro) and balanced (macro) averages of $P$ and $R$ equivalent).
along to the mixed measures, we see modest variations, with the double-strict assessment numbers the lowest, as expected. In the bottom three rows, we see the results of the different averaging methods. Once again, recall suffers unnecessarily when the double-strict measure is incorporated (since link types are not treated exclusively). Looking at the original, unbalanced averaging method, we see that recall is inflated, since very little information is incorporated about the quality of weak-link prediction. Note that the recall inflation imposed by the original strength-averaging method is particularly bad in this scenario, since the large number of weak-link false negatives (weak-link recall is 18.37%) are erased in the strong-link-only measurement scenario. Since this is the most difficult part of the task, attention should be directed to and not away from these details.