How well does active learning *actually* work? Time-based evaluation of cost-reduction strategies for language documentation.

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Abstract

Machine involvement has the potential to speed up language documentation. We assess this potential with timed annotation experiments that consider annotator expertise, example selection methods, and suggestions from a machine classifier. We find that better example selection and label suggestions improve efficiency, but effectiveness depends strongly on annotator expertise. Our expert performed best with uncertainty selection, but gained little from suggestions. Our non-expert performed best with random selection and suggestions. The results underscore the importance both of measuring annotation cost reductions with respect to time and of the need for cost-sensitive learning methods that adapt to annotators.

1 Introduction

Data annotated with linguistically interesting labels is used in a wide variety of contexts. Computational linguists generally use annotated data as training and evaluation material for natural language processing systems; corpus linguists use it to test hypotheses about language; documentary linguists create interlinear glossed texts to preserve examples of endangered languages and hypotheses about the grammars of those languages. Regardless of the context, creating annotated data is costly in terms of time and/or money. Since both time and money are undeniably in limited supply, there is a widely shared desire to reduce this cost.

Reducing cost involves strategies that do more with fewer human-annotated labels and/or reduce the per-label cost. An example of the former is active learning, which focuses annotation effort on data points selected by the learner(s) for their expected utility in developing a more accurate model (Settles, 2009). Examples of the latter include providing suggestions from a machine labeler and using extremely cheap human labelers, e.g. with the Amazon Mechanical Turk (Snow et al., 2008). Different techniques may be more or less applicable depending on the language being annotated, the kind of labels which are desired (tags, syntactic structures, etc.), and the desired use of the annotated data (e.g., for training models, testing linguistic hypotheses, or preserving a language).

This paper discusses experiments that measure the effectiveness of machine-aided annotation for language documentation using both active learning simulation experiments and annotation experiments which involve actual documentary linguists interacting with machine example selection and label suggestion. Specifically, we deal with the task of labeling morphemes of the Mayan language Uspanteko with fine-grained parts-of-speech. We also run active learning simulation experiments for part-of-speech tagging for Danish, Dutch, English, Swedish, and Uspanteko to show the validity of our models and methods in a standard setting. For Uspanteko, we provide results from annotation experiments in which annotation cost is measured in terms of the actual annotation time required while varying three factors: (1) example selection, (2) machine label suggestions, and (3) annotator expertise.

Our findings indicate that there is considerable promise for reducing the cost of producing IGT, but they also demonstrate considerable variation due to the interaction of these factors. This suggests different prescriptions for appropriate strategies in different contexts. Most clearly, the worst performing strategy—by far—is that used in nearly all documentary work: sequential annotation without automation. Also, our expert annotator did best with examples picked by uncertainty selection, while our non-expert did best with random selection aided by machine label sug-

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Language	#words-tr	#words-dev	#tags	#sents-tr	#sents-dev	Avg.sent	Avg.tr.sent	Avg.dev.sent
Danish	62825	31561	10	3570	1618	18.18	17.60	19.50
Dutch	129586	65483	13	9365	3982	14.61	13.84	16.44
English	167593	131768	45	6945	5527	24.00	24.13	23.84
Swedish	127684	63783	41	7326	3714	17.34	17.43	17.17
Uspanteko	43473	19906	69	7423	3288	5.92	5.86	6.05

Table 1: Corpora: number of words and sentences, number of possible tags, and average sentence length.

gestions. This difference confirms the importance of cost-sensitive active learning strategies that are not just learner-guided, but also take into account modeling of the annotators (Settles et al., 2008; Haertel et al., 2008; Vijayanarasimhan and Grauman, 2008). Finally, we confirm the importance of using actual annotation time to measure annotation cost: a unit-cost assumption—even at a fine-grained level—can dramatically misrepresent the actual effectiveness of different strategies.

2 Task and data

Annotation task: language documentation The amount of money spent on obtaining human annotations is an extremely important concern in much language annotation. However, there is a further urgency for annotation in the case of language documentation: languages are dying at the rate of two each month. By the end of this century, half of the approximately 6000 extant spoken languages will cease to be transmitted effectively from one generation of speakers to the next (Crystal, 2000). Recorded and transcribed texts annotated with detailed linguistic information create an important multi-faceted record of these languages, but there are few trained linguists with adequate time and appropriate levels of funding relative to the size of the problem. Annotation cost—in both time and money—is thus keenly felt in the work of documenting and describing endangered languages. Active learning and automated label suggestions could help deal with this language documentation bottleneck.

We focus on one stage of language documentation, the production of interlinear glossed text (IGT), a standard form of annotation that involves both morphological and grammatical analysis. IGT is generally created following transcription and translation of recorded speech, with the annotations often being provided by trained annotators with varying levels of expertise. The result is generally a small amount of IGT annotated data and a greater amount of unannotated data.

Data We use a collection of 32 interlinear glossed texts (IGT) in the Mayan language Uspanteko. This corpus was cleaned up and adapted by Palmer et al. (2009) from an original collection of 67 texts that were collected, transcribed, translated and annotated by the OKMA language documentation project (Pixabaj et al., 2007).

Two core tasks in creating IGT are morphological analysis and tagging morphemes with their glosses (labels indicating part-of-speech and/or grammatical function). We deal with the latter task and assume texts are morphologically segmented. Standard four-line IGT has morphemes on one line and their glosses on the next. The gloss line includes labels for grammatical morphemes (e.g. `PL` or `COM`) and translations of stems (e.g. *hablar* or *idioma*). The following is an Uspanteko example:

(1)

TEXT:	`Kita'`	`tinch'ab'ej`	`laj`	`inyolj`	`iin`
MORPH:	`kita'`	`t-in-ch'abe-j`	`laj`	`in-yol-j`	`iin`
GLOSS:	NEG	INC-E1S-hablar-SC	PREP	A1S-idioma-SC	yo
POS:	PART	TAM-PERS-VT-SUF	PREP	PERS-S-SUF	PRON
TRANS:	'No le hablo en mi idioma.'				

We use a single layer that is a combination of the GLOSS and POS layers (Palmer et al., 2009). For (1), the morphemes and labels for our task are:

(2)

kita'	t-	in-	ch'abe	-j	laj	in-	yol	-j	iin
NEG	INC	E1S	VT	SC	PREP	A1S	S	SC	PRON

We also consider POS-tagging for Danish, Dutch, English, and Swedish; the English is from sections 00-05 (as training set) and 19-21 (as development set) of the Penn Treebank (Marcus et al., 1993), and the other languages are from the CoNLL-X dependency parsing shared task (Buchholz and Marsi, 2006).[1] We split the original training data into training and development sets. Table 1 shows the number of words and sentences in each split of each dataset, as well as the number of possible labels and the average sentence length. The Uspanteko data is counted in morphemes rather than words; also, the Uspanteko texts are divided at the clause rather than sentence level. This gives the corpus a much lower average clause length than the other languages (Table 1).

[1]The subset of the Penn Treebank was chosen to be of comparable size to the CoNLL datasets.

3 Model and methods

Classification model. We use a standard maximum entropy classifier for tagging Danish, Dutch, English, and Swedish words with POS-tags and tagging Uspanteko morphemes with Gloss/POS tags. The label for a word/morpheme is predicted based on the word/morpheme itself plus a window of two units before and after. Standard part-of-speech tagging features (Ratnaparkhi, 1998; Curran and Clark, 2003) are extracted from the morpheme to help with predicting labels for previously unseen morphemes. This is a strong but standard model; better, more complex models could be used, but the gains are likely to be small. Thus, we opted for simplicity in our model so as to focus more on the interaction between the annotator and different levels of machine involvement.

The accuracy of the tagger on the datasets when trained on all available training material is given in the following table, along with accuracy of a unigram model (learned from the training set and constrained by a tag dictionary for known words).

	Unigram	Model
Danish	91.62%	95.58%
Dutch	90.92%	93.57%
English	87.87%	93.25%
Swedish	84.91%	87.74%
Uspanteko	77.84%	79.39%

Sample selection. We consider three sample selection methods: **seq**uential, **rand**om, and **unc**ertainty. Sequential selection is important to consider as it is the default in documentary projects. It is sub-optimal for corpora with contiguous sub-domains, since it necessitates working through many similar examples before getting to possibly more informative examples. Random selection is a model-free method that avoids the sub-domain trap by sampling freely from the entire corpus. It generally works better than sequential selection and provides a strong baseline against which to compare learner-guided selection.

Uncertainty selection (Cohn et al., 1995) identifies examples the model is least confident about. We measure uncertainty as the entropy of the label distribution predicted by the maximum entropy model for each example. Uncertainty for a clause is calculated as the average entropy per morpheme; clauses with the highest average entropy are selected for labeling.

A recent development in active learning is cost-sensitive selection that is guided not only by the learner but also by the expected cost of labeling an example based on its likely complexity and/or the reliability of the annotator. Settles et al. (2008) provide empirical validation for cost-related intuitions; for example, that cost of annotation is static neither per example nor per annotator. Also, they show that taking annotation cost into account can improve active learning effectiveness, but that learning to predict annotation cost is not yet well-understood. A cost-sensitive Return on Investment heuristic is developed in Haertel et al. (2008) and tested in a simulated POS-tagging context. Our experiments do not employ cost-sensitive selection, but our results—from live (non-simulated) active learning experiments of real-world scale—empirically support the need to consider cost-sensitive selection if better cost reductions are to be achieved.

Annotation setup. We compare results from two annotators with different levels of exposure to Uspanteko. Both are documentary linguists with extensive field experience. Our **expert annotator** is a native speaker of K'ichee', a closely related Mayan language, and has worked extensively on Uspanteko. Our **non-expert annotator** had no prior experience with Uspanteko and only limited exposure to Mayan languages. During annotation, he used an Uspanteko-Spanish dictionary.

For each selection method, we consider two conditions for providing classifier labels: a **do-suggest** (**ds**) condition where the labels predicted by the machine learner are shown to the annotator, and a **no-suggest** (**ns**) condition where the annotator does not see the predictions. With **ds**, the annotator is shown the most probable label and a ranked list of all labels assigned a probability greater than half that of the best label. For **ns**, the annotator sees a frequency-ranked list of labels previously seen in training data for the given morpheme.

Annotators improve as they see more examples. To minimize the impact of this learning process, annotation is done in rounds. Each round consists of sixty clauses—six batches of ten each for the six experimental cases. The annotator is free to break between batches. Following annotation, the newly-labeled clauses are added to the training data, and a new model is trained and evaluated. Both annotators completed fifty-six rounds of annotation. See Palmer et al. (2009) for more details on the annotation setup.

Measuring annotation cost. Active learning studies usually *simulate* annotation and use a unit cost assumption that each word, sentence, constituent, document, etc. takes the same time to annotate. This is often the only option since corpora typically do not retain annotation time, but it is likely to exaggerate the annotation cost reductions achieved. This is exacerbated with active learning: the informative examples it seeks to find are typically harder to annotate (Hachey et al., 2005).

Baldridge and Osborne (2008) correlate a unit cost in terms of *discriminants* (decisions made by annotators about valid parses) to annotation time. This is a better approximation than unit costs where such a relationship cannot be established. However, it is based on a *static* measurement of annotation time, and clearly the time taken to annotate an example is not a function of the example alone. Annotation time is actually *dynamic* in that it is dependent on how many and what kinds of examples have already been annotated. An "informative" example is likely to take longer to annotate if selected early than it would after the annotator has seen many other examples.

Thus, it is important to measure annotation time *embedded in the context* of a particular annotation experiment with the sample selection/labeling strategies of interest. In our annotation experiments, we measure the exact time taken to annotate each example by each annotator and use this as the cost metric, inspired by Ngai and Yarowsky (2000). In the simulation studies, as we are unable to measure time, we measure cost by sentence/clause and word/morpheme.

Learning curve comparison. We are interested in comparative evaluation of many different experimental settings, across which we vary selection methods, use of label suggestions, and annotators. To achieve this, it is useful to have a summary value for comparing the results from two individual experiments. One such measure is the percentage error reduction (PER), measured over a discrete set of points on the first 20% of the points on the learning curve (Melville and Mooney, 2004).[2]

We use a new related measure, which we call the *overall* percentage error reduction (**OPER**), that uses the *entire* area under the curves given by fitted nonlinear regression models rather than averaging over a subset of data points. Specifically, we fit a modified Michaelis-Menton model:

$$f(cost, (K, V_m, A)) = \frac{V_m(A + cost)}{K + cost}$$

The (original) parameters V_m and K respectively correspond to the horizontal asymptote and the cost where accuracy is halfway between 0 and V_m. The additional parameter A allows for a better fit to our data by allowing for less sharp elbows and letting *cost* be zero. Model parameters were determined with `nls` in R (Ritz and Streibig, 2008).

With the fitted regression models, it is straightforward to calculate the area under the curve between a start cost c_i and end cost c_j by taking the integral from c_i to c_j. The overall accuracy for the experiment is given by dividing that area by $100 \times (c_j - c_i)$. Call this the overall curve accuracy (OCA). Then, for experiment A compared to experiment B, OPER(A,B) = $\frac{OCA_A - OCA_B}{100 - OCA_B}$. For the simulation experiments we calculate OPER for only the first 20% of cost units, like Melville and Mooney. For the annotation experiments, we calculate it for the minimum amount of time spent on any of the experiments (which ended up using less than 10% of all available morphemes).

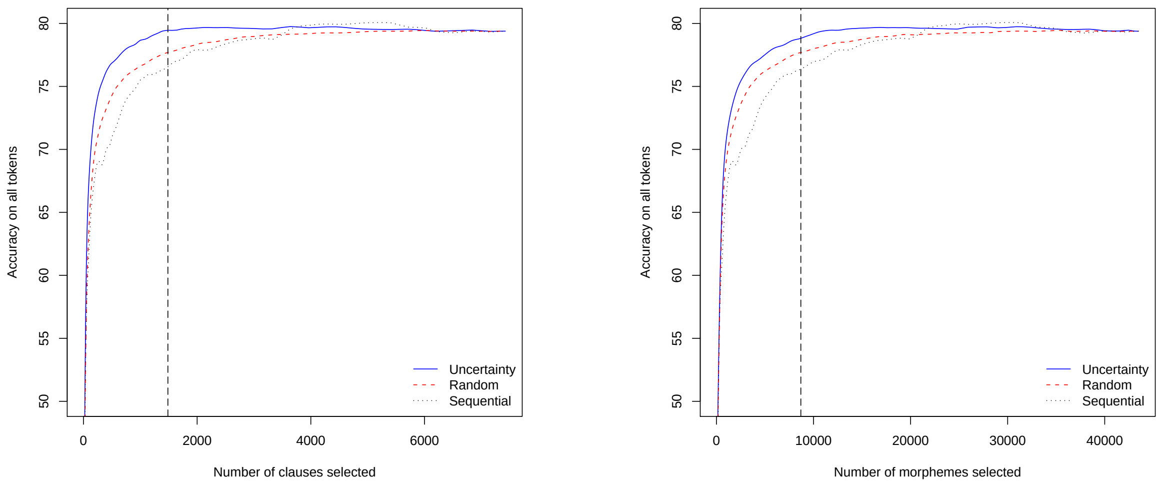
4 Simulation experiments

We verify that our tagger and dataset behave as expected in standard active learning experiments by running simulations on the Uspanteko data set, and on POS-tagging for Danish, Dutch, English, and Swedish. Here, we vary only the selection method: **seq**uential, **rand**om, or **unc**ertainty.

For each language, we randomly select a seed set of 10 labeled sentences. The number of examples selected to be labeled in each round begins at 10 and doubles after every 20 rounds. For **rand** and **unc**, each batch of examples is selected from a pool (size of 1000) that is itself randomly selected from the entire set of remaining unlabeled examples. **rand** and **unc** experiments for each language are replicated 5 times; splines and regressions are computed over all runs for each condition.

Figure 1 gives learning curves for the Uspanteko simulations, with cost measured in terms of (a) clauses and (b) morphemes. Both graphs show the usual behavior found in active learning experiments. **rand** and **unc** both rise more quickly than **seq**, and **unc** is well above **rand**. The relationship between the methods is the same regardless

[2]This is justified in standard conditions, sampling from a finite corpus: active learning runs out of interesting examples after considering a fraction of the data, so the curve is *artificially* pulled down by the remaining, boring examples.



(a) (b)

Figure 1: Learning curves for simulations; (a) clause cost and (b) morphemes cost. The dashed vertical lines indicate (a) #clauses=1485 and (b) #morphemes=8695 (to compare OPER values).

	$\frac{\textbf{rand}}{\textbf{seq}}$	$\frac{\textbf{unc}}{\textbf{seq}}$	$\frac{\textbf{unc}}{\textbf{rand}}$
Uspanteko-Clauses	5.86	13.27	7.86
Uspanteko-Morphs	7.47	11.68	4.55

Table 2: OPER values for Uspanteko simulations, comparing **clause** and **morpheme** cost. $\frac{A}{B}$ indicates we compute OPER(A,B).

	$\frac{\textbf{rand}}{\textbf{seq}}$	$\frac{\textbf{unc}}{\textbf{seq}}$	$\frac{\textbf{unc}}{\textbf{rand}}$
Danish	4.58	6.95	2.48
Dutch	21.95	23.68	2.20
English	6.55	8.00	1.56
Swedish	9.56	9.29	-0.30
Uspanteko	7.47	11.68	4.55

Table 3: OPER values for **morpheme** cost for simulations. $\frac{A}{B}$ indicates we compute OPER(A,B).

of the cost metric, but the relative differences in cost-savings are not, which we see when we look at OPER values.

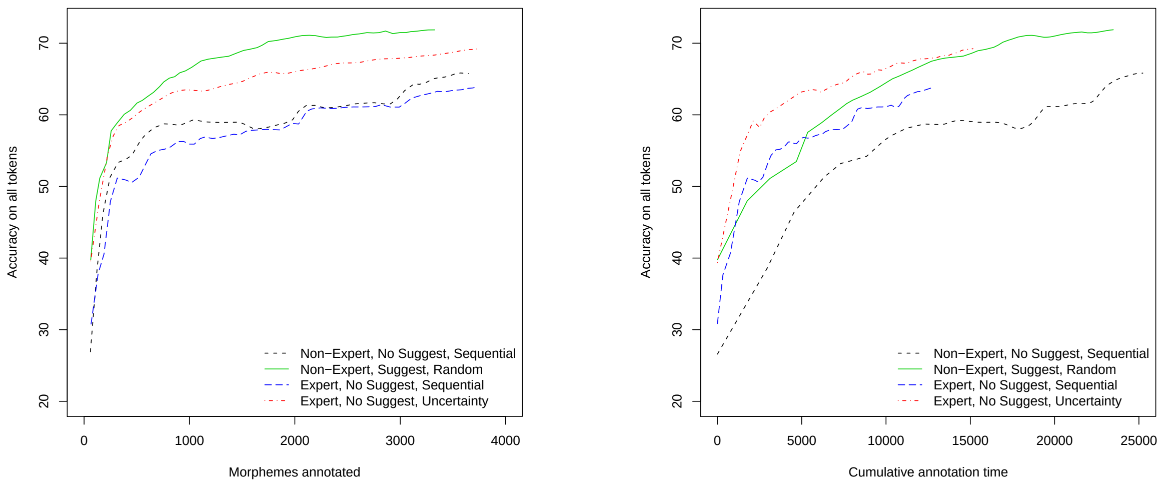
The dashed vertical lines in the two graphs correspond to the 20% mark used to calculate OPER values, which are given in Table 2. Most importantly, note the much larger OPER for **unc** over **rand** with clause cost (7.86 vs 4.55). Also note that OPER(**rand**,**seq**) is *lower* with clause cost—this indicates that the beginning portions of the corpus contain longer sentences with more morphemes, an accident which overstates how well **seq** would likely work in general.

Since **rand** is unbiased with respect to picking longer sentences, the large increase of OPER(**unc**,**rand**) from 4.55 to 7.86 is a clear indication of the well-known—but not always attended to—tendency of uncertainty sampling to select longer sentences. Consequently, one should at least use sub-sentence cost in order not to overstate the gains from active learning. The annotation experiments in the next section take this word of caution one step further: even sub-sentence cost (morpheme cost, in our setting) can overestimate gains since the morphemes selected are actually harder to annotate and thus take more time.

Table 3 gives overall percentage error reductions (OPER) between different selection methods based on word/morpheme cost, for each language. For all languages, **rand** and **unc** are better than **seq**. Only in the case of Swedish is there no benefit from **unc** over **rand**. For Dutch, the large gains over **seq** for both **rand** and **unc** accurately reflect the heterogeneity of the underlying Alpino corpus.[3] Most importantly, for Uspanteko, there are large reductions from **unc** to **rand** to **seq**, mirroring the clear trends in Figure 1b.

These simulations have an unrealistic "perfect" annotator, the corpus. Next, we discuss results with real annotators—who may be fallible or may (reasonably) beg to differ with the corpus analysis.

[3] `http://www.let.rug.nl/vannoord/trees/`



(a) (b)

Figure 2: A sample of the learning curves with (a) morpheme cost and (b) time cost. Morpheme cost ranks strategies for a given annotator similarly to time cost, but it gives dramatically different results from time cost when used to compare different annotators.

5 Annotation experiments

With two annotators (**expert**, **non-expert**), three selection methods (**seq**, **rand**, **unc**), and two machine labeling settings (**ns**, **ds**), we obtain 12 different experiments. Each experiment measures accuracy in terms of all words and unknown words and cost in terms of clauses, morphemes and time; this produces six views on every experiment. In this paper we focus on one view: accuracy over all words with time-based evaluation of cost.

As with the simulations, clause cost in the annotation experiments overestimates the cost reductions. For morpheme cost, the annotation experiments show that (a) it also overstates cost reductions compared to time, and (b) it can mis-state relative effectiveness when comparing annotators.

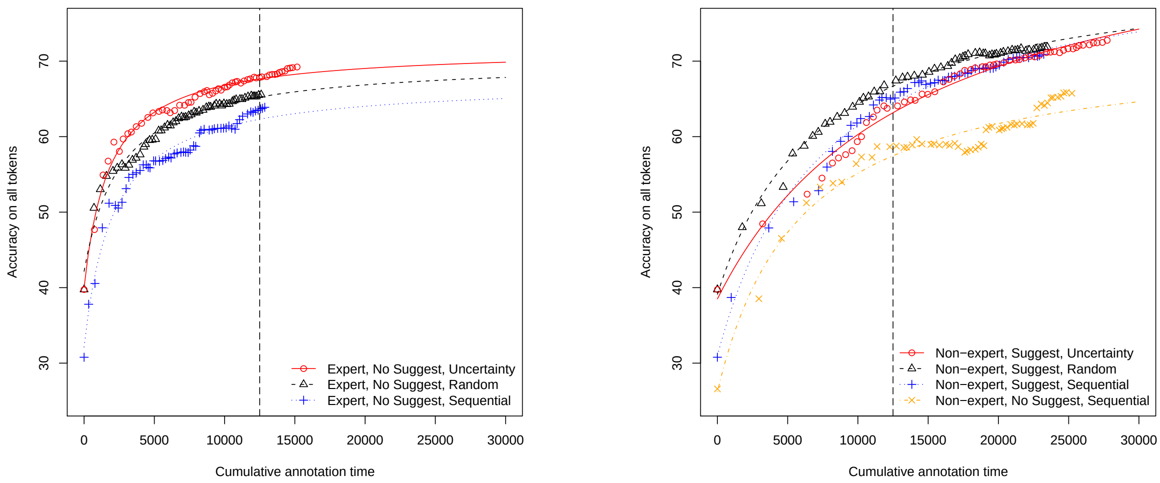
The big picture. Figure 2 shows curves for four experiments: **seq**-**ns** for both annotators[4] and the most effective overall condition for each annotator. Figure 2a uses morpheme cost evaluation; on that metric, both annotators appear to be about equally effective with **seq**-**ns** and much more effective with machine involvement (**unc** or **ds**) than without. Additionally, the non-expert's **rand**-**ds** appears to beat the expert's **unc**-**ns**. However, the time cost evaluation in Figure 2b tells a dramatically different story. Each annotator's machine-involved experiment is much better than their **seq**-**ns**, but now the expert's best is clearly better than the non-expert's. We see this as clear evidence for the need for cost-sensitive learning over vanilla active learning (as we do here).[5]

The non-expert with **rand**-**ds** caught up to and surpassed the unaided expert in about six hours total annotation time, and he caught up to her **unc**-**ns** curve after 35 hours. This is encouraging since often language documentation projects have participants with a wide range of expertise levels, and these results suggest that assistance from machine learning, if done properly, may increase the effectiveness of participants with less language-specific expertise. We are also encouraged, with respect to the effectiveness of active learning, that the expert's best performance is obtained with uncertainty-based selection.

Within annotator comparisons. Figure 3 shows both actual measurements and the fitted nonlinear regression curves used to compute OPER. Figure 3a, the expert without suggestions, exhibits typical active learning behavior similar to that seen in the simulation experiments. Figure 3b,

[4]Recall that sequential annotation is the default mode for producing IGT, so this strategy is of particular interest.

[5]It is also clear to see that, unsurprisingly, the expert spent much less time to complete the 56 rounds than the non-expert. In general, the expert annotator was much quicker, particularly in early rounds, averaging 4.1 seconds per morpheme annotated against the non-expert's 8.0 second average. See Palmer et al. (2009) for more details.



(a) (b)

Figure 3: Sample measurements and fitted nonlinear regression curves for (a) the expert and (b) the non-expert. Note that the scale is consistent for comparability. The dashed vertical lines indicate 12,500 seconds (about 35 hours), which is the upper limit used in computing OPER values for Table 4.

the non-expert *with* suggestions, shows that in the **ds** conditions the non-expert was less effective with **unc**. This is not unexpected: uncertainty selects harder examples that will either take longer to annotate or are easier to get wrong, especially if the annotator trusts the classifier and *especially* on examples the classifier is uncertain about. Nonetheless, in all **ds** cases, the non-expert performs better than with **seq-ns**.

OPER. Table 4 provides OPER values from time 0 to 12,500 seconds (about 35 hours), the minimum amount of annotation time logged in any one of the twelve experiments.[6] The table mixes three types of comparison: (1) the boxed values on the diagonal give OPER for the expert versus the non-expert given the same selection and suggestion conditions; (2) the upper (right) triangle gives OPER for the expert versus herself for different conditions; and (3) the lower (left) triangle is the non-expert versus himself. For example: (1) the expert obtained an 11.52 OPER versus the non-expert when both used **rand-ns**; (2) the expert obtained a 10.52 OPER by using **rand-ds** rather than **seq-ns**; and (3) the non-expert obtained a 5.93 OPER over **rand-ns** by using **rand-ds**.

A number of patterns emerge. Quite unsurprisingly, the values on the diagonal show that the expert is more effective than the non-expert in all conditions. Also, every other condition is more effective than **seq-ns** for both annotators (first row for the expert, first column for the non-expert). **unc-ns** and **rand-ds** are particularly effective for the non-expert, giving OPERs of 19.20 and 18.59 over **seq-ns**, respectively. These reductions, bigger than the expert's reductions of 14.17 and 10.52 for the same conditions, considerably reduce the large gap in **seq-ns** effectiveness between the two annotators (see Figure 2b).

The expert actually gains very little from **ds** for both **rand** and **unc**: adding suggestions gave OPERs of just 1.83 and .39, respectively. In contrast, the non-expert obtains an improvement of 5.93 OPER when suggestions are used with **rand**,

non-exp \ exp	**seq-ns**	**rand-ns**	**unc-ns**	**seq-ds**	**rand-ds**	**unc-ds**
seq-ns	15.99	8.85	14.17	6.34	10.52	14.50
rand-ns	13.46	11.52	5.83	-2.76	1.83	6.20
unc-ns	19.20	6.63	10.76	-9.12	-4.25	0.39
seq-ds	10.24	-3.72	-11.09	12.34	4.46	8.72
rand-ds	18.59	5.93	-0.76	9.30	7.67	4.45
unc-ds	11.19	-2.62	-9.91	1.06	-9.09	19.13

Table 4: Overall percentage error reduction (OPER) comparisons, with timing cost. See explanation of table in the **OPER** subsection.

[6]Stopping at 12,500 seconds ensures a fair comparison, for example, between the expert and the non-expert because it requires no extrapolation of the expert's performance.

but performs *worse* when used with **unc** (-9.91 OPER). Even more striking: the non-expert's **unc-ds** is worse than **rand-ns** (-2.62 OPER), a completely model-free setting. These variations demonstrate the importance of modeling annotator fallibility and sensitivity to cost, as well as characteristics of the annotation task itself, if learner-guided selection and suggestion are to be used (Donmez and Carbonell, 2008; Arora et al., 2009).

Annotator accuracy. Another factor which must be considered when annotation is done by human annotators (rather than being simulated) is the accuracy of the humans' labels. Table 5 shows the overall accuracy of the annotators' labels for each condition (after 56 rounds) as measured against the original OKMA annotations. Unsurprisingly, **unc** selection picks examples that are more difficult to annotate: accuracy for both annotators suffers in both **unc-ns** and **unc-ds**.

It may seem surprising that the non-expert's accuracies are generally higher than the expert's. The main reason for this is that the non-expert took nearly twice as long to annotate his examples, so each one was done with more care. However, this difference also highlights challenges that arise when we bring active learning into non-simulated annotation contexts. The typical assumption is that gold standard labeled data represents a true, fixed target, against which annotator or machine-predicted labels should be measured. In language documentation, though, the analysis of the language is continually evolving, and analysis and annotation each inform the other. In fact, the expert recognized (in the morphological segmentation) several linguistic phenomena for which the analysis has changed since the original OKMA annotations were done. As she changed her analyses, her labels diverged from those of the original corpus—another reason for her "lower" accuracy. This is to say that the ground truth of the current OKMA annotations we had to work with can be viewed as one (valid) stage in the iterative reanalysis process that language documentation is.

Error analysis. Preliminary analysis of 'errors' made by the annotators supports the idea that the results seen in Table 5 are heavily influenced by changes in the expert's analysis of the language. Some duplicate clause annotation occurred for each annotator, because each of the twelve annotator-selection-suggestion conditions drew from the same global set of unlabeled examples. This duplication allows us to measure the consistency of each annotator on labeling such duplicate clauses. Table 6 shows the percentage of morphemes labeled consistently by each annotator. Numbers for the expert appear in the top (right) triangle, and for the non-expert in the bottom (left) triangle. Overall intra-annotator consistency is much higher for the expert (88.38%) than for the non-expert (81.64%), suggesting that the expert maintained a more consistent mental model of the language, but one which disagrees in some areas with the original annotations.

	expert	**non-expert**
seq-ns	73.17%	75.09%
rand-ns	69.90%	74.37%
unc-ns	61.23%	60.04%
seq-ds	67.48%	73.13%
rand-ds	68.34%	73.03%
unc-ds	59.79%	60.27%

Table 5: Overall accuracy of annotators' labels, measured against OKMA annotations.

Another key error source comes from differences in use of one individual label: the annotators could assign a label that does not appear in the original corpus. This is yet another issue that does not—in fact, *cannot*—arise in simulated active learning. The label **ESP** was introduced for labeling Spanish loans or insertions (such as the discourse marker *entonces*) which do not have a clear function in Uspanteko grammar. Such tokens are inconsistently labeled in the original corpus, usually with catch-all categories like particle or adverb. The annotators felt that the best analysis was to mark the tokens as of Spanish origin. The expert annotator used the **ESP** label for 2086 of 24129 tokens (8.65%) versus 221 of 22819 tokens (0.97%) for the non-expert. Any such token labeled with **ESP** is scored as incorrect when compared to the OKMA standard, so this label alone accounts for more than 7% of the expert annotator's total error.

Finally, Table 7 presents inter-annotator agreement measured as percent agreement on morphemes in clauses labeled by both annotators. Note that in general agreement seems to be lowest for clauses duplicated in **unc** conditions, supporting the expected result that uncertainty-based selection does indeed select clauses that are more difficult for human annotators to label.

exp / non	seq-ns	rand-ns	unc-ns	seq-ds	rand-ds	unc-ds
seq-ns	—	95.00% (41)	87.10% (56)	92.39% (60)	91.02% (28)	88.83% (51)
rand-ns	90.11% (49)	—	90.91% (57)	87.57% (35)	90.94% (50)	89.53% (57)
unc-ns	80.80% (44)	81.68% (54)	—	81.35% (41)	89.10% (40)	87.82% (332)
seq-ds	90.00% (54)	87.94% (44)	77.97% (48)	—	86.13% (42)	82.14% (42)
rand-ds	90.15% (52)	86.64% (45)	79.46% (62)	81.43% (44)	—	87.06% (49)
unc-ds	84.15% (47)	78.55% (52)	77.68% (328)	78.81% (35)	77.95% (60)	—

Table 6: Annotation consistency, expert and non-expert, (number of duplicate clauses, of 560 possible)

exp / non	seq-ns	rand-ns	unc-ns	seq-ds	rand-ds	unc-ds
seq-ns	69.91% (523)	70.82% (42)	62.42% (48)	72.35% (54)	74.25% (28)	67.82% (47)
rand-ns	71.32% (48)	83.94% (39)	66.56% (47)	66.15% (43)	73.75% (42)	67.55% (52)
unc-ns	66.31% (48)	67.87% (53)	62.31% (301)	58.87% (51)	73.31% (40)	61.10% (298)
seq-ds	73.35% (60)	75.56% (34)	56.39% (37)	60.02% (540)	66.00% (44)	61.01% (36)
rand-ds	68.67% (50)	76.40% (63)	66.67% (58)	65.88% (47)	76.33% (42)	66.99% (64)
unc-ds	65.41% (50)	67.98% (55)	60.43% (263)	58.13% (38)	70.74% (57)	60.40% (275)

Table 7: IAA: expert v. non-expert, percentage of morphemes in agreement, (number of duplicate clauses, of 560 possible)

6 Conclusion

Through actual annotation experiments that control for several factors, we have evaluated the potential of incorporating active learning and label suggestions to speed up morpheme glossing in a realistic language documentation context. Some configurations of learner-guided example selection and machine label suggestions perform far better than the standard strategy of sequential selection without suggestions. However, the effectiveness of any given strategy depends on annotator expertise. The impact of differences between annotators directly bears on the point made by Donmez and Carbonell (2008) that if cost reductions are to be reliably obtained with active learning techniques, annotators' fallibility, unreliability, and sensitivity to cost must be modeled.

Our results suggest some possible prescriptions for tuning techniques according to annotator expertise. However, even if we can estimate a relative level of expertise, following such broad prescriptions is unlikely to be more robust than an approach which *adapts* selection and suggestion to the individual annotator, perhaps working within an annotation group. Indeed, it seems that dealing with variation in annotators/oracles may be more important than devising better selection strategies.

The difference in performance due to expertise suggests that using multiple annotators to check relative annotation rate and accuracy of different annotators could be a key ingredient in any actually deployed active learning system. This could provide for better modeling of individual annotators as part of an annotation group they can be compared against, allowing the system, for example, to throttle active selection if an annotator appears to be too slow or inaccurate.

Another major issue we highlight is the uncertainty around the question of whether active learning works in practical applications. Respondents to the survey of Tomanek and Olsson (2009) indicated that this uncertainty—will active learning work? what methods or techniques will work best?—is one of the reasons active learning is not widely used in actual annotation. In addition, creating the necessary software infrastructure to build an active learning enabled annotation system—a system which must interface robustly between data, annotator, and machine classifier, yet still be easy to use—is a substantial hurdle. It seems unlikely that there will be much uptake until a) consistent, large cost reductions can be shown in actual annotation studies, and b) appropriate, *tunable*, widely-available software exists.

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