An Evaluation of the Human-Interpretability of Explanation

Isaac Lage*[1], Emily Chen*[1], Jeffrey He*[1], Menaka Narayanan*[1], Been Kim[2], Sam Gershman[1] and Finale Doshi-Velez[1]

[1]Harvard University
[2]Google Brain

Abstract

Recent years have seen a boom in interest in machine learning systems that can provide a human-understandable rationale for their predictions or decisions. However, exactly what kinds of explanation are truly human-interpretable remains poorly understood. This work advances our understanding of what makes explanations interpretable under three specific tasks that users may perform with machine learning systems: simulation of the response, verification of a suggested response, and determining whether the correctness of a suggested response changes under a change to the inputs. Through carefully controlled human-subject experiments, we identify regularizers that can be used to optimize for the interpretability of machine learning systems. Our results show that the type of complexity matters: cognitive chunks (newly defined concepts) affect performance more than variable repetitions, and these trends are consistent across tasks and domains. This suggests that there may exist some common design principles for explanation systems.

1 Introduction

Interpretable machine learning systems provide not only decisions or predictions but also explanation for their outputs. Explanations can help increase trust and safety by identifying when the recommendation is reasonable and when it is not. While interpretability has a long history in AI [Michie, 1988], the relatively recent widespread adoption of machine learning systems in real, complex environments has lead to an increased attention to interpretable machine learning systems, with applications including understanding notifications on mobile devices [Mehrotra et al., 2017, Wang et al., 2016], calculating stroke risk [Letham et al., 2015], and designing materials [Raccuglia et al., 2016]. Techniques for ascertaining the provenance of a prediction are also popular within the machine learning community as ways for us to simply understand our increasingly complex models [Lei et al., 2016, Selvaraju et al., 2016, Adler et al., 2016].

The increased interest in interpretability has resulted in many forms of explanation being proposed, ranging from classical approaches such as decision trees [Breiman et al., 1984] to input gradients or other forms of (possibly smoothed) sensitivity analysis [Selvaraju et al., 2016, Ribeiro et al., 2016, Lei et al., 2016], generalized additive models [Caruana et al., 2015], procedures [Singh et al., 2016], falling rule lists [Wang and Rudin, 2015], exemplars [Kim et al., 2014, Frey and Dueck, 2007] and decision sets [Lakkaraju et al., 2016]—to name a few. In all of these cases, there is a face-validity to the proposed form of explanation: if the explanation was not human-interpretable, clearly it would not have passed peer review.

That said, these works provide little guidance about when different kinds of explanation might be appropriate, and within a class of explanations—such as decision-trees or decision-sets—what factors most influence the ability of humans to reason about the explanation. For example, it is hard to imagine that a human would find a 5000-node decision tree as interpretable as a 5-node decision tree for any reasonable notion of interpretable, but it is not clear whether it is more urgent to regularize for fewer nodes of shorter average path lengths. In Doshi-Velez and Kim [2017], we point to a growing need for the interpretable machine learning community to engage with the human factors and cognitive science of interpretability: we can spend enormous efforts optimizing all kinds of models and regularizers, but that effort is only worthwhile if those models and regularizers actually solve the original human-centered task of providing explanation.

Determining what kinds of regularizers we should be using, and when, require carefully controlled human-subject experiments. In this work, we make modest but concrete strides towards providing an empirical grounding for what kinds of explanations humans can utilize. Focusing on decision sets, we determine how three different kinds of explanation complexity—clause and explanation lengths, number and presentation of cognitive chunks (newly defined concepts), and variable repetitions—affect the ability of humans to use those explanations across three different tasks, two different domains, and three different performance metrics. We find that the type of complexity matters: cognitive chunks affect performance more than variable repetitions, and these trends are consistent across tasks and domains. This suggests that there may exist some common design principles for explanation systems.

2 Related Work

Interpretable Machine Learning Interpretable machine learning methods aim to optimize models for both succinct explanation and predictive performance. Common types of explanation include regressions with simple, human-simulatable functions [Caruana et al., 2015, Kim et al., 2015a, Rüping, 2006, Bucilu et al., 2006, Ustun and Rudin, 2016, Doshi-Velez et al., 2015, Kim et al., 2015b, Krakovna and Doshi-Velez, 2016, Hughes et al., 2016, Jung et al., 2017], various kinds of logic-based methods [Wang and Rudin, 2015, Lakkaraju et al., 2016, Singh et al., 2016, Liu and Tsang, 2016, Safavian and Landgrebe, 1991, Wang et al., 2017], techniques for extracting local explanations from black-box models [Ribeiro et al., 2016, Lei et al., 2016, Adler et al., 2016, Selvaraju et al., 2016, Smilkov et al., 2017, Shrikumar et al., 2016, Kindermans et al., 2017, Ross et al., 2017], and visualization [Wattenberg et al., 2016]. There exist a range of technical approaches to derive each form of explanation, whether it be learning sparse models [Mehmood et al., 2012, Chandrashekar and Sahin, 2014], monotone functions [Canini et al., 2016], or efficient logic-based models [Rivest, 1987]. Related to our work, there also exists a history of identifying human-relevant concepts from data, including disentangled representations [Chen et al., 2016] and predicate invention in inductive logic programming [Muggleton et al., 2015]. While the algorithms are sophisticated, the measures of interpretability are often not—it is common for researchers to simply appeal to the face-validity of the results that they find (i.e., "this result makes sense to the human reader") [Caruana et al., 2015, Lei et al., 2016, Ribeiro et al., 2016].

Human Factors in Explanation In parallel, the literature on explanation in psychology also offers several general insights into the design of interpretable AI systems. For example, humans prefer explanations that are both simple and highly probable [Lombrozo, 2007]. Human explanations typically appeal to causal structure [Lombrozo, 2006] and counterfactuals [Keil, 2006]. Miller [1956] famously argued that humans can hold about seven items simultaneously in working memory, suggesting that human-interpretable explanations should obey some kind of capacity limit (importantly, these items can correspond to complex *cognitive chunks*—for example, 'CIAFBINSA' is easier to remember when it is recoded as 'CIA', 'FBI', 'NSA.'). Orthogonally, Kahneman [2011] notes that humans have different modes of thinking, and larger explanations might push humans into a more careful, rational thinking mode. Machine learning researchers can convert these concepts into notions such as sparsity or simulatability, but answering questions such as "how sparse?" or "how long?" requires empirical evaluation in the context of machine learning explanations.

A/B Testing for Interpretable ML Existing studies evaluting the human-interpretability of explanation often fall into the A-B test framework, in which a proposed model is being compared to some competitor, generally on an intrinsic task. For example, Kim et al. [2014] showed that human subjects' performance on a classification task was better when using examples as representation than when using non-example-based representation. Lakkaraju et al. [2016] performed a user study in which they found subjects are faster and more accurate at describing local decision boundaries based on decision sets rather than rule lists. Subramanian et al. [1992] found that users prefer decision trees to tables in games, whereas Huysmans et al. [2011] found users prefer, and are more accurate, with decision tables rather than other classifiers in a credit scoring domain. Hayete and Bienkowska [2004] found a preference for non-oblique splits in decision trees (see Freitas [2014] for a more detailed survey). These works provide quantitative evaluations of the human-interpretability of explanation, but rarely identify which properties are most essential for which contexts—which is critical for generalization.

Domain Specific Human Factors for Interpretable ML Specific application areas have also evaluated the desired properties of an explanation within the context of the application. For example, Tintarev and Masthoff [2015] provides a survey in the context of recommendation systems, noting differences between the kind of explanations that manipulate trust [Cosley et al., 2003] and the kind that increase the odds of a good decision [Bilgic and Mooney, 2005]. In many cases, these studies are looking at whether the explanation has an effect on performance on a downstream task, sometimes also considering a few different kinds of explanation (actions of similar customers, etc.). Horsky et al. [2012] describe how presenting the right clinical data alongside a decision support recommendation can help with adoption and trust. Bussone et al. [2015] found that overly detailed explanations from clinical decision support systems enhance trust but also create over-reliance; short or absent explanations prevent over-reliance but decrease trust. These studies span a variety of extrinsic tasks, and again given the specificity of each explanation type, identifying generalizable properties is challenging.

General Human Factors for Interpretable ML Closer to the objectives of this work, Kulesza et al. [2013] performed a qualitative study in which they varied the soundness (nothing but the truth) and the completeness (the whole truth) of an explanation in a recommendation system setting. They found completeness was important for participants to build accurate mental models of the system. Allahyari and Lavesson [2011], Elomaa [2017] also find that larger models can sometimes be more interpretable. Schmid et al. [2016] find that human-recognizable intermediate predicates in inductive knowledge programs can sometimes improve simulation time. Poursabzi-Sangdeh et al. [2017] manipulate the size and transparency of an explanation and find that longer explanations and black-box models are harder to simulate accurately (even given many instances) on a real-world application predicting housing prices. Our work fits into this category of empirical study of explanation evaluation; we perform controlled studies on a pair of synthetic applications to assess the effect of a large set of explanation parameters.

3 Methods

Our central research question is to determine which properties of decision sets are most relevant for human users to be able to use the explanations for a set of synthetic tasks described below. In order to carefully control various properties of the explanation and the context, we generate explanations by hand that mimic those learned by machine learning systems. We emphasize that while our explanations are not machine-generated, our findings provide suggestions to the designers of interpretable machine learning systems about which parameters are most urgent to optimize when producing explanations since they affect usability most heavily.

The question of how humans utilize explanation is broad. For this study, we focused on explanations in the form of *decision sets* (also known as rule sets). Decision sets are a particular form of procedure consisting of a collection of cases, each mapping some function of the inputs to a collection of outputs. An example of a decision set is given below

calm or nodding and **sunny** → **spices or vegetables** and **grains**
rainy and grumpy or **calm** → **dairy** or **vegetables**

Figure 1: Example of a decision set explanation.

where each line contains a clause in disjunctive normal form (an or-of-ands) of the inputs (blue words), which, if true, maps to the output (orange words–also in disjunctive normal form).

Decision sets make a reasonable starting point for a study on explanation because there exist many techniques to optimize them given data [Frank and Witten, 1998, Cohen, 1995, Clark and Boswell, 1991, Lakkaraju et al., 2016]; they are easy for humans to parse since they can scan for the rule that applies and choose the accompanying output [Lakkaraju et al., 2016]; and there are many parameters to tune that may influence how easy it is to parse a specific decision set, including the number of lines, number of times variables are repeated, and whether terms represent intermediate concepts. Finally, decision sets can either be trained as the machine learning model for a given

Setting: Domain	Setting: Choice of Task	Explanation Variation	Metrics
Recipe	Verification	V1: Explanation Size	Response Time
Clinical	Simulation	V2: Cognitive Chunks	Accuracy
	Counterfactual	V3: Repeated Terms	Subjective Satisfaction

Table 1: We conduct experiments in 2 parallel domains–one low-risk (recipe) and one high risk (clinical). In each domain, we conduct 3 experiments testing different types of explanation variation. For each of those, we vary 1-2 factors (described in detail in Section 3.3). For each setting of these factors, we ask people to perform 3 core cognitive tasks and for each of these, we record 3 metrics measuring task performance.

application, or they can be trained to locally approximate a more complex model like a neural network using procedures like the one described in Ribeiro et al. [2016].

Of course, within decision sets there are many possible variations, tasks, and metrics. Table 1 summarizes the core aspects of our experiments. We considered three main kinds of explanation variation—variations in explanation size (number of lines, length of lines), variations in introducing new cognitive chunks (newly defined concepts), and variations in whether terms repeat. The effect of these variations were tested across three core cognitive tasks in two domains, for a total of six settings. We also considered three metrics—accuracy, response time, and subjective satisfaction—as measures of task performance that we may care about. In the following, we first describe each of these core experimental aspects and then we detail the remaining aspects of the experimental design.

3.1 Setting: Domain

The question of what kinds of explanation a human can use implies the presence of a setting, task, and metric that the explanation will facilitate. Examples include improving safety, where a user might use the explanation to determine when the machine learning system will make a mistake; and increasing trust, where a user might be convinced to use a machine learning system if it justifies its actions in plausible ways. While evaluating how well explanations facilitate real world tasks is the ultimate goal, it is challenging because we must control for the knowledge and assumptions that people bring with them when making these decisions. For example, even if the task is the same—improving safety—people may bring different assumptions based on aspects of the domain (e.g., perceived risk of the decision).

To rigorously control for outside knowledge that could influence our results, we created two domains—recommending recipes and medicines to aliens —to which humans could not bring any prior knowledge. Further, each question involved a supposedly different alien to further encourage the users to not generalize within the experiment. All non-literals (e.g., what ingredients were spices) were defined in a dictionary so that all participants would have the same concepts in both experiments. Although designed to feel very different, this synthetic set-up also allowed us to maintain exact parallels between inputs, outputs, categories, and the forms of explanations.

- **Recipe** Study participants were told that the machine learning system had studied a group of aliens and determined each of their individual food preferences in various settings (e.g., weekend). This scenario represents a setting in which customers may wish to inspect product recommendations. This domain was designed to feel like a low-risk decision. Here, the inputs are settings, the outputs are groups of food, and the recommendations are specific foods.

- **Clinical** Study participants were told that the machine learning system had studied a group of aliens and determined personalized treatment strategies for various symptoms (e.g., sore throat). This scenario closely matches a clinical decision support setting in which a doctor might wish to inspect the decision support system. This domain was designed to feel like a high-risk decision. Here, the inputs are symptoms, the outputs are classes of drugs, and the recommendations are specific drugs. We chose drug names that start with the first letter of the drug class (e.g., antibiotics were Aerove, Adenon and Athoxin) so as to replicate the level of ease and familiarity of food names.

We hypothesized that the trends would be consistent across both domains.

3.2 Setting: Choice of Task

In any domain, there are many possible tasks that an explanation could facilitate. For example, one could be interested in error in recipe suggestions or a mechanism for alien disease. To stay general, as well as continue to rigorously control for outside knowledge and assumptions, we follow the suggestion of Doshi-Velez and Kim [2017] and consider a core set of three cognitive tasks designed to test how well humans understand the explanation:

- **Simulation** Predicting the system's recommendation given an explanation and a set of input observations. Participants were given observations about the alien and the alien's preferences and were asked to make a recommendation that would satisfy the alien. See Figure 9.

- **Verification** Verifying whether the system's recommendation is consistent given an explanation and a set of input observations. Participants were given a recommendation as well as the observations and preferences and asked whether it would satisfy the alien. See Figure 2.

- **Counterfactual** Determining whether the system's recommendation changes given an explanation, a set of input observations, and a perturbation that changes one dimension of the input observations. Participants were given a change to one of the observations in addition to the observations, preferences and recommendation and asked whether the alien's satisfaction with the recommendation would change. See Figure 10.

As before, we hypothesized that while some tasks may be easier or harder, the trends of what makes a good explanation would be consistent across tasks.

3.3 Explanation Variation

While decision sets are interpretable, there are a large number of ways in which they can be varied that potentially affect how easy they are for humans to use. Following initial pilot studies (see Appendix 6), we chose to focus on three main sources of variation:

- **V1: Explanation Size.** We varied the size of the explanation across two dimensions: the *total number of lines* in the decision set, and the *number of terms within the output clause*. The first corresponds to increasing the vertical size of the explanation—the number of cases—while the second corresponds to increasing the horizontal size of the explanation—the complexity of each case. We focused on output clauses because they were harder to parse: input clauses could be quickly scanned for setting-related terms, but output clauses had to be read through and processed completely to determine the correct answer. We hypothesized that increasing the size of the explanation across either dimension would increase response time. For example, the explanation in Figure 2 has 4 lines (in addition to the first 3 lines that define what we call explicit cognitive chunks), and 3 terms in each output clause.

- **V2: Cognitive Chunks.** We varied the number of cognitive chunks, and whether they were implicitly or explicitly defined. We define a cognitive chunk as a clause in disjunctive normal form of the inputs that may recur throughout the decision set. Explicit cognitive chunks are mapped to a name that is then used to reference the chunk throughout the decision set, while implicit cognitive chunks recur throughout the decision set without ever being explicitly named. On one hand, creating new cognitive chunks can make an explanation more succinct, while on the other hand, the human must now process an additional idea. We hypothesized that explicitly introducing cognitive chunks instead of having long clauses that implicitly contained them would reduce response time. For example, the explanation in Figure 2 has 3 explicit cognitive chunks, and the explanation in Figure 10 has 3 implicit cognitive chunks.

- **V3: Repeated Terms.** We varied the number of times that input conditions were repeated in the decision set. If input conditions in the decision list have little overlap, then it may be faster to find the appropriate one because there are fewer relevant cases to consider than if the input conditions appear in each line. Repeated terms was also a factor used by Lakkaraju et al. [2016] to measure interpretability. We hypothesized that if an input condition appeared in several lines of the explanation, this would increase the time it took to search for the correct rule in the explanation. For example, each of the observations in Figure 2 appears twice in the explanation (the observations used in the explicit cognitive chunks appear only once, but the final chunk appears twice).

3.4 Metrics

In a real domain, one may have a very specific metric, such as false positive rate given no more than two minutes of thinking or the number of publishable scientific insights enabled by the explanation. In our experiments, we considered three basic metrics that are likely to be relevant to most tasks: response time, accuracy, and subjective satisfaction:

- **Response Time** was measured as the number of seconds from when the task was displayed until the subject hit the submit button on the interface.

- **Accuracy** was measured if the subject correctly identified output consistency for verification questions, the presence or absence of a change in recommendation correctness under the perturbation for counterfactual questions, and any combination of correct categories for simulation questions.

- **Subjective Satisfaction** was measured on a 5-point Likert scale. After submitting their answer for each question, but before being told if their answer was correct, the participant was asked to subjectively rate the quality of the explanation on a scale from 1 to 5 where 1 was very easy to use, 3 was neutral and 5 was very hard to use.

3.5 Experimental Design and Interface

The three kinds of variation and two domains resulted in six total experiments. The experiments had parallel structures across the domains. For each question, we ask a simulation, a verification and a counterfactual version with parallel logic structures but different observations. Question order was block-randomized for every participant so participants were always shown a verification, then a simulation, then a counterfactual question, but which condition these came from was randomly determined. Domain and experiment were between subject variables, while task and explanation variation are within subjects variables. Each participant completed a single, full experiment.

Conditions The experiment levels were as follows:

- **V1: Explanation Size.** We manipulated the length of the explanation (varying between 2, 5, and 10 lines) and the length of the output clause (varying between 2 and 5 terms). Each combination was tested once with each question type, for a total of 18 questions. All inputs appeared 3 times in the decision set.

- **V2: Cognitive Chunks.** We manipulated the number of cognitive chunks (repeated clauses in disjunctive normal form of the inputs) introduced (varying between 1, 3 and 5), and whether they were embedded into the explanation (implicit) or abstracted out into new cognitive chunks and later referenced by name (explicit). Each combination was tested once with each question type, for a total of 18 questions. 1 input was used in each experiment to evaluate the answer directly, and 2 were used to evaluate the cognitive chunks, which was used to evaluate the answer. All of the cognitive chunks were used to determine the correct answer to ensure that the participant had to traverse all concepts instead of skimming for the relevant one. For explicit cognitive chunks, the input used to evaluate it appeared only once, but the chunk appeared 2 times. All other inputs appeared twice. All decision sets had 4 lines in addition to any explicit cognitive chunks. All output clauses had 3 elements.

- **V3: Repeated Terms.** We manipulated the number of times the input conditions appeared in the explanation (varying between 2, 3, 4 and 5) and held the number of lines and length of clauses constant. Each combination was tested once within an experiment, for a total of 12 questions. All decision sets had 6 lines. All output clauses had 3 elements.

Figure 2: Screenshot of our interface for the verification task in the recipe domain. The bottom left box shows the observations we give participants about the alien, and a meal recommendation. They must then say whether the machine learning system agrees with the recommendation based on the explanation. Each task is coded in a different color (e.g., yellow) to visually distinguish them.

Experimental Interface Figure 2 shows our interface for the verification task in the Recipe domain. The *observations* section refers to the inputs into the algorithm. The *recommendation* section refers to the output of the algorithm. The *preferences* section contains the explanation—the reasoning that the supposed machine learning system used to suggest the output (i.e., recommendation) given the input, presented as a procedure in the form of a decision set. Finally, the *ingredients* section in the Recipe domain (and the *disease medications* section in the Clinical domain) contained a dictionary defining *concepts* relevant to the experiment (for example, the fact that bagels, rice, and pasta are all grains). Including this list explicitly allowed us to control for the fact that some human subjects may be more familiar with various concepts than others. At the bottom is where the subject completes the task: responses were submitted using a radio button for the verification and counterfactual questions, and using check-boxes for simulation questions.

The choice of location for these elements was chosen based on pilot studies (described in Appendix 6) —while an ordering of input, explanation, output might make more sense for an AI expert, we found that presenting the information in the format of Figure 2 seemed to be easier for subjects to follow in our preliminary explorations. We also found that presenting the decision set as a decision set seemed easier to follow than converting it into paragraphs. Finally, we colored the input conditions in blue and outputs in orange within the explanation. We found that this highlighting system made it easier for participants to parse the explanations for input conditions. We also highlighted the text of each question type in a different color based on informal feedback that it was hard to differentiate between verification and counterfactual questions.

Additional Experimental Details In addition to the variations and settings of interest, there were many details that had to be fixed to create consistent experiments. In the recipe domain, we held the list of ingredients, food categories, and possible input conditions constant. Similarly, in the clinical domain, we held the list of symptoms, medicine categories, and possible input conditions constant. All questions had three observations, and required using each one to determine the correct answer.

Verification questions had two recommendations from two distinct categories. Each recommendation in the verification task matched half of the lines in the decision set. Determining the correct answer for verification questions never required differentiating between OR and XOR.

Counterfactual questions required a perturbation: we always perturbed exactly one observation. The perturbed observation appeared once. The perturbed example always evaluated to a new line of the decision set. Counterfactual questions had a balanced number of each sequence of true/false answers for the original and perturbed input.

Finally, to avoid participants building priors on the number of true and false answers, the verification and counterfactual questions had an equal number of each. This notion of balancing did not apply to the simulation task, but in the results, high accuracies rule out random guessing.

Participants We recruited 150 subjects for each of our six experiments through Amazon Mechanical Turk (900 subjects all together). Participants were given a tutorial on each task and the interface, and were told that their primary goal was accuracy, and their secondary goal was speed. Before completing the task, participants were given a set of three practice questions, one drawn from each question type. If they answered these correctly, they could move directly to the experiment, and otherwise they were given an additional set of three practice questions.

We excluded participants from the analysis who did not get all of one of the two sets of three practice questions correct. While this may have the effect of artificially increasing the accuracy rates overall—we are only including participants who could already perform the task to a reasonable extent—this criterion helped filter the substantial proportion of participants who were simply breezing through the experiment to get their payment. We also excluded 6 participants who got sufficient practice questions correct but then took more than five minutes to answer a single question under the assumption that they got distracted while taking the experiment. Table 2 describes the total number of participants that remained in each experiment out of the original 150 participants.

Number of Participants

Experiment	Recipe	Clinical
Explanation Size (V1)	59	69
New Cognitive Chunks (V2)	62	55
Variable Repetition (V3)	70	52

Table 2: Number of participants who met our inclusion criteria for each experiment.

Most participants were from the US or Canada and were less than 50 years old. A majority had a Bachelor's degree. There were somewhat more male participants than female. We note that US and Canadian participants with moderate to high education dominate this survey, and results may be different for people from different cultures and backgrounds. Table 3 summarizes the demographics of all subjects included in the analysis across the experiments.

Participant Demographics

Feature	Categories and Proportion					
Age	18-34	61.1%	35-50	33.5%	51-69	5.4%
Gender	Male	62.7%	Female	37.0%	Other	0.3%
Education	High School	33.2%	Bachelors	53.4%	Masters and Beyond	9.1%
Region	US/Canada	92.8%	Asia	4.8%	South America	1.6%

Table 3: Participant Demographics. There were no participants over 69 years old. 4.3% of participants reported "other" for their education level. The rates of participants from Australia, Europe and Latin America were all less than 0.5%. (All participants were included in the analyses, but we do not list specific proportions for them for brevity.)

4 Results

We report response time, accuracy and subjective satisfaction (whether participants thought the task was easy to complete or not) across all six experiments in Figures 3, 5 and 7, respectively. Response time is shown for subjects who correctly answered the questions. Response time and subjective satisfaction were normalized across participants by subtracting the participant-specific mean.

We evaluated the statistical significance of the trends in these figures using a linear regression for the continuous outputs (response time, subjective score) and a logistic regression for binary output (accuracy). For each outcome, one regression was performed for each of the experiments V1, V2, and V3. If an experiment had more than one independent variable—e.g., number of lines and terms in output—we performed one regression with both variables. We included whether the task was a verification or a counterfactual question as 2 distinct binary variables that should be interpreted with respect to the simulation task. Regressions were performed with the statsmodels library [Seabold and Perktold, 2010] and included an intercept term. In Table 4, Table 6 and Table 8, we report the results of these regressions with p-values that are significant at $\alpha = 0.05$ after a Bonferroni multiple comparisons correction across all tests of all experiments highlighted in bold.

We next describe our main findings. We find that greater complexity results in longer response times, but the magnitude of the effect varies across the different kinds of explanation variation in sometimes unexpected ways. These results are consistent across domains, tasks, and metrics.

Greater complexity results in longer response times, but the magnitude of the effect varies by the type of complexity. Unsurprisingly, adding complexity generally increases response times. In Figure 3, we see that increasing the number of lines, the number of terms within a line, adding new cognitive chunks, and repeating variables all show trends towards increasing response time. Table 4 reports which of these response time trends are statistically significant: the number of cognitive chunks and whether these are implicitly embedded in the explanation or explicitly defined had a statistically significant effect on response time in both domains, the number of lines and the number of output terms had a statistically significant effect on response time only in the recipe domain, and the number of repeated variables did not have a statistically significant effect in either domain.

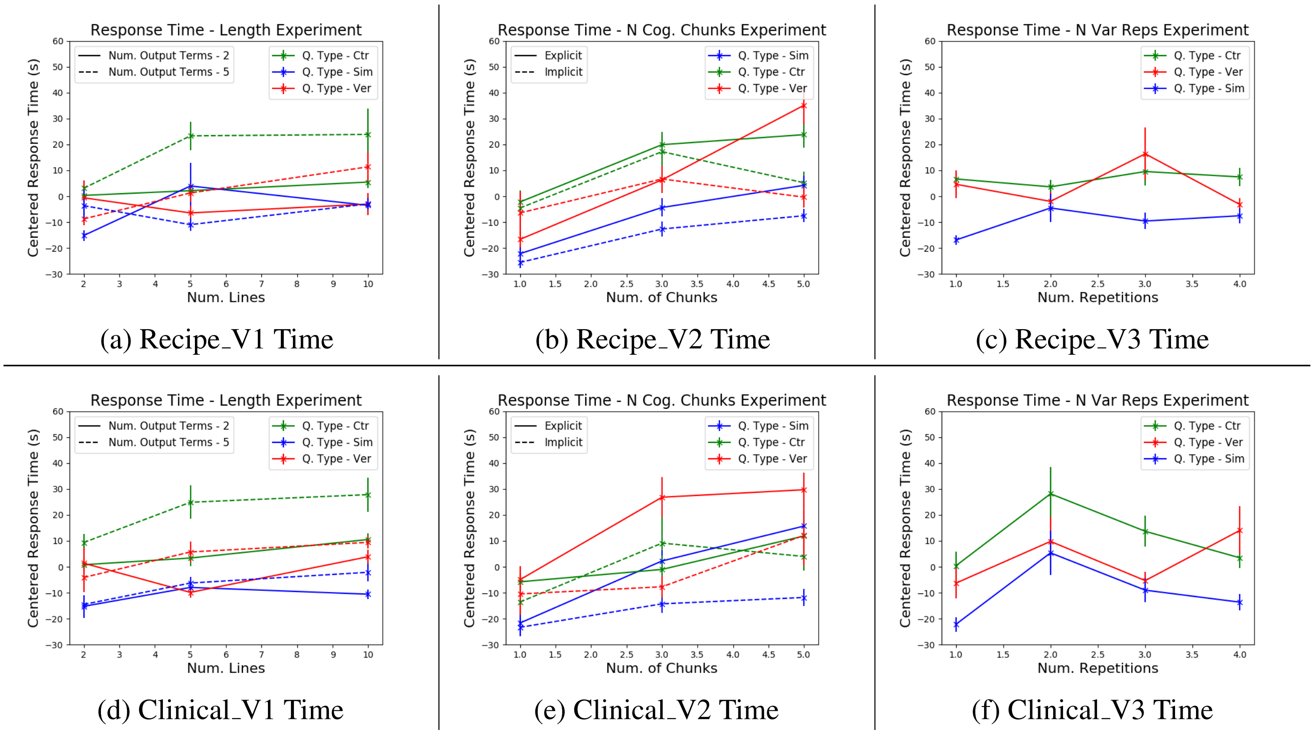
More broadly, the magnitude of the increase in response time varies across these factors. (Note the the y-axes in Figure 3 all have the same scale for easy comparison.) Introducing new cognitive chunks can result in overall increases in response time on the order of 20 seconds, whereas increases in length has increases on the order of 10 seconds. Increases in variable repetition does not always have a consistent effect, and even when it does, the trend does not appear to be on the order of more than a few seconds. These relationships have implications for designers of explanation systems: variable repetitions seem to have significantly less burden than new concepts.

Another interesting finding was that participants took significantly longer to answer when new cognitive chunks were made explicit rather than implicitly embedded in a line. That is, participants were faster when they had to process fewer, longer lines (with an implicit concept) rather than when they had to process more, simpler lines. One might have expected the opposite—that is, it is better to break complex reasoning into smaller chunks—and it would be interesting to unpack this effect in future experiments. For example, it could be that explicitly instantiating new concepts made the explanation harder to scan, and perhaps highlighting the locations of input terms (to make them easier to find) would negate this effect.

Consistency across domains: Magnitudes of effects change, but trends stay the same. In all experiments, the general trends are consistent across both the recipe and clinical domains. Sometimes an effect is weaker or unclear, but never is an effect clearly reversed. There were 21 cases of factors that had a statistically significant effect on a dependent variable in at least 1 of the 2 domains. For 19 of those, the 95% confidence interval of both domains had the same sign (i.e., the entire 95% confidence interval was positive for both domains or negative for both domains). For the other 2 (the effect of verification questions on accuracy and response time for experiment V1), one of the domains (clinical) was inconclusive (interval overlaps zero). The consistency of the signs of the effects bodes well for there being a set of general principles for guiding explanation design, just as there exist design principles for user interfaces and human-computer interaction.

Consistency across tasks: Relative trends stay the same, different absolute values. The effects of different kinds of complexity on response time were also consistent across tasks. That said, actual response times varied significantly between tasks. In Figure 3, we see that the response times for simulation questions are consistently low, and the response times for counterfactual questions are consistently high (statistically significant across all experiments except V2 in the Recipe domain). Response times for verification questions are generally in between, and often statistically significantly higher than the easiest setting of simulation. For designers of explanation, this consistency of the relative effects of different explanation variations bodes well for general design principles. For designers of tasks, the differences in absolute response times suggests that the framing of the task does matter for the user.

Response Time



(a) Recipe_V1 Time (b) Recipe_V2 Time (c) Recipe_V3 Time

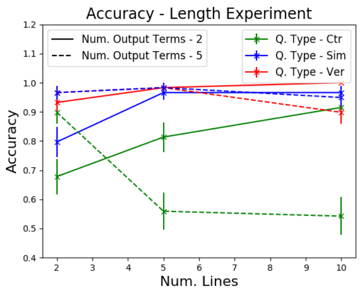
(d) Clinical_V1 Time (e) Clinical_V2 Time (f) Clinical_V3 Time

Figure 3: Response times across the six experiments. Responses were normalized by subtracting out the subject-specific mean to create centered values, and only response times for those subjects who got the question right are included. Vertical lines indicate standard errors.

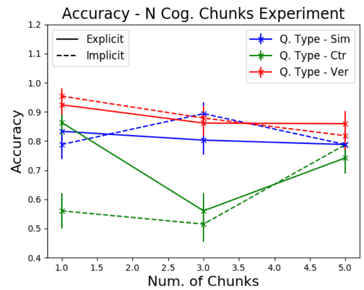
	Clinical		Recipe	
Factor	Weight	P-Value	Weight	P-Value
Number of Lines (V1)	1.17	**1.41E-05**	1.01	0.00317
Number of Output Terms (V1)	2.35	**7.37E-05**	1.57	0.0378
Verification (V1)	10.5	**3.98E-07**	4.11	0.121
Counterfactual (V1)	21	**1.58E-19**	13.7	**1.79E-06**
Number of Cognitive Chunks (V2)	6.04	**8.22E-11**	5.88	**4.45E-17**
Implicit Cognitive Chunks (V2)	-13	**2.16E-05**	-7.93	**0.000489**
Verification (V2)	16.3	**6.91E-06**	15.4	**1.27E-08**
Counterfactual (V2)	8.56	0.0265	19.9	**6.65E-12**
Number of Variable Repetitions (V3)	1.9	0.247	0.884	0.463
Verification (V3)	13	0.00348	13.7	**3.03E-05**
Counterfactual (V3)	20.3	**2.41E-05**	16.6	**1.91E-06**

Figure 4: Significance tests for each factor for normalized response time. A single linear regression was computed for each of V1, V2, and V3. Coefficients for verification and counterfactual tasks should be interpreted with respect to the simulation task. Highlighted p-values are significant at $\alpha = 0.05$ with a Bonferroni multiple comparisons correction across all tests of all experiments.

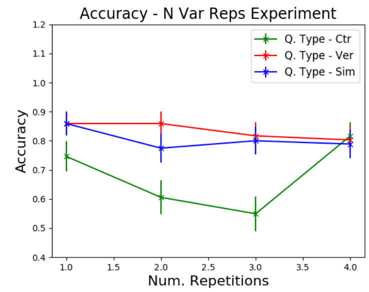
Accuracy



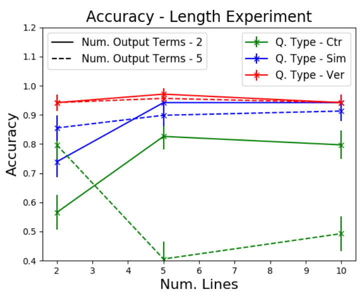
(a) Recipe_V1 Accuracy



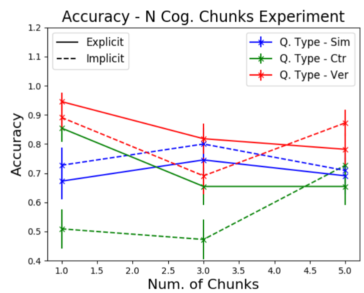
(b) Recipe_V2 Accuracy



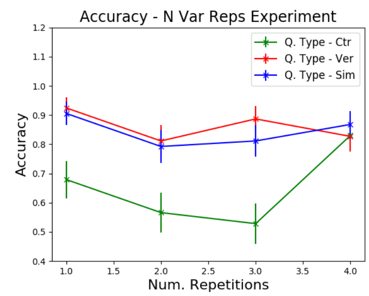
(c) Recipe_V3 Accuracy



(d) Clinical_V1 Accuracy



(e) Clinical_V2 Accuracy



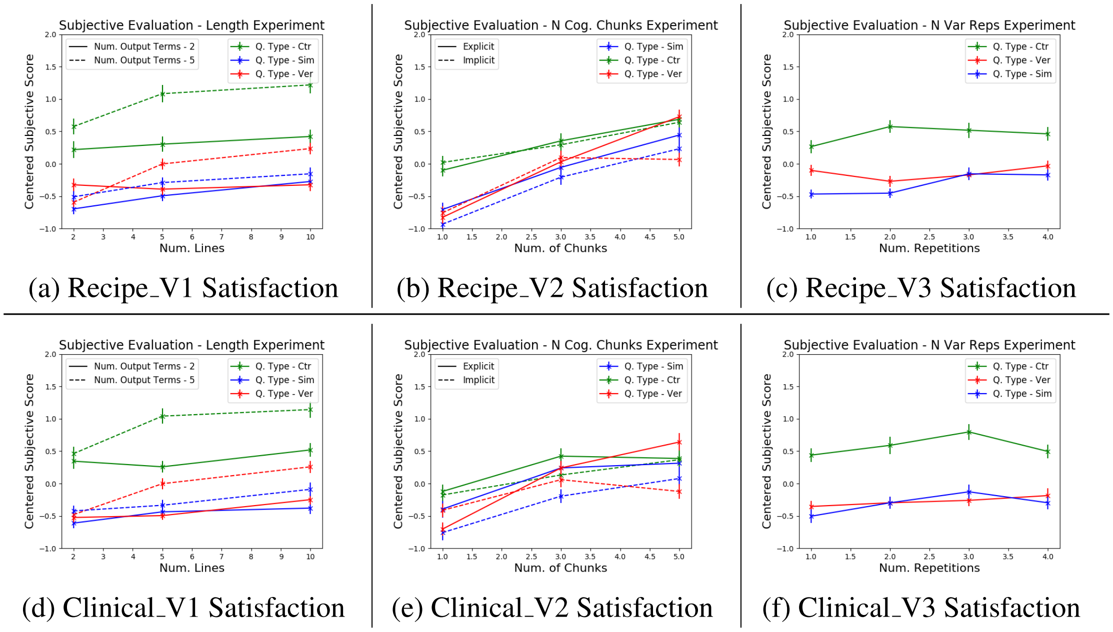
(f) Clinical_V3 Accuracy

Figure 5: Accuracy across the six experiments. Vertical lines indicate standard errors.

	Clinical		Recipe	
Factor	Weight	P-Value	Weight	P-Value
Number of Lines (V1)	0.029	0.236	0.00598	0.842
Number of Output Terms (V1)	-0.136	0.011	-0.117	0.0771
Verification (V1)	0.925	**0.000652**	0.476	0.174
Counterfactual (V1)	-1.41	**1.92E-14**	-1.7	**1.24E-11**
Number of Cognitive Chunks (V2)	-0.0362	0.42	-0.0364	0.416
Implicit Cognitive Chunks (V2)	-0.246	0.093	-0.179	0.222
Verification (V2)	0.646	0.0008	0.532	0.00904
Counterfactual (V2)	-0.368	0.0294	-0.773	**4.36E-06**
Number of Variable Repetitions (V3)	0.0221	0.804	-0.0473	0.524
Verification (V3)	0.146	0.596	0.196	0.371
Counterfactual (V3)	-1.07	**7.25E-06**	-0.67	**0.00066**

Figure 6: Significance tests for each factor for accuracy. A single logistic regression was computed for each of V1, V2, and V3. Coefficients for verification and counterfactual tasks should be interpreted with respect to the simulation task. Highlighted p-values are significant at $\alpha = 0.05$ with a Bonferroni multiple comparisons correction across all tests of all experiments.

Subjective Satisfaction



(a) Recipe_V1 Satisfaction (b) Recipe_V2 Satisfaction (c) Recipe_V3 Satisfaction

(d) Clinical_V1 Satisfaction (e) Clinical_V2 Satisfaction (f) Clinical_V3 Satisfaction

Figure 7: Subjective satisfaction across the six experiments. Participants were asked to rate how difficult it was to use each explanation to complete the task on a scale from 1 (very easy) to 5 (very hard). Responses were normalized by subtracting out the subject-specific mean to create centered values. Vertical lines indicate standard errors.

	Clinical		Recipe	
Factor	Weight	P-Value	Weight	P-Value
Number of Lines (V1)	0.0495	**8.5E-13**	0.0491	**5.57E-11**
Number of Output Terms (V1)	0.116	**2.28E-14**	0.116	**2.54E-12**
Verification (V1)	0.13	0.0187	0.169	0.00475
Counterfactual (V1)	1.01	**1.16E-65**	1.04	**1.5E-59**
Number of Cognitive Chunks (V2)	0.177	**3.96E-24**	0.254	**3.76E-54**
Implicit Cognitive Chunks (V2)	-0.228	**4.1E-05**	-0.121	0.0171
Verification (V2)	0.0697	0.305	0.092	0.14
Counterfactual (V2)	0.288	**2.42E-05**	0.52	**1.61E-16**
Number of Variable Repetitions (V3)	0.057	0.0373	0.0676	0.00411
Verification (V3)	0.0326	0.664	0.169	0.00899
Counterfactual (V3)	0.887	**1.93E-29**	0.767	**2.41E-30**

Figure 8: Significance tests for each factor for normalized subjective satisfaction. A single linear regression was computed for each of V1, V2, and V3. Coefficients for verification and counterfactual tasks should be interpreted with respect to the simulation task. Highlighted p-values are significant at $\alpha = 0.05$ with a Bonferroni multiple comparisons correction across all tests of all experiments.

Consistency across metrics: Subjective satisfaction follows response time, less clear trends in accuracy. So far, we have focused our discussion on trends with respect to response time. In Table 8, we see that subjective satisfaction largely replicates the findings of response time. We see a statistically significant preference for simulation questions over counterfactuals. We also see a statistically significant effect of the number of cognitive chunks, explanation length, and number of output terms. The finding that people prefer implicit cognitive chunks to explicit cognitive chunks appeared only in the recipe domain. These results suggest that, in general, peoples' subjective preferences may reflect objective measures of interpretability like response time. However, we must also keep in mind that especially for a Turk study, subjective satisfaction may match response times because faster task completion corresponds to a higher rate of pay.

Unlike response time and subjective satisfaction, where the trends were significant and consistent, the effect of explanation variation on accuracy was less clear. None of the effects due to explanation variation were statistically significant, and there are no clear trends. We believe that this was because in our experiments, we asked participants to be fast but accurate, effectively pushing any effects into response time. That said, even when participants were coached to be accurate, some tasks proved harder than others: counterfactual tasks had significantly lower accuracies than simulation tasks.

5 Discussion

Identifying how different factors affect a human's ability to utilize explanation is an essential piece for creating interpretable machine learning systems—we need to know what to optimize. What factors have the largest effect, and what kind of effect? What factors have relatively little effect? Such knowledge can help us expand to faithfulness of the explanation to what it is describing with minimal sacrifices in human ability to process the explanation.

Consistent patterns provide guidance for design of explanation systems. In this work, we found consistent patterns across metrics, tasks, and domains for the effect of different kinds of explanation variation. These patterns suggest that, for decision sets, the introduction of new cognitive chunks or abstractions had the greatest effect on response time, then explanation size (overall length or length of line), and finally there was relatively little effect due to variable repetition. These patterns are interesting because machine learning researchers have focused on making decision set lines orthogonal (e.g., [Lakkaraju et al., 2016]), which is equivalent to minimizing variable repetitions, but perhaps, based on these results, efforts should be more focused on explanation length and if and how new concepts are introduced.

We also find consistent patterns across explanation forms for the effect of certain tasks. Simulation was the fastest, followed by verification and then counterfactual reasoning. The counterfactual reasoning task also had the lowest accuracies. This suggests that participants doing the verification and counterfactual reasoning tasks were likely first simulating through the explanation and then doing the verification or counterfactual reasoning. (We note that while our results focus on response time, if participants were time-limited, we would expect effects in response time to turn into effects in accuracy.) While these observations are less relevant to designers of explanation systems, they may be valuable for those considering how to design tasks.

There exist many more directions to unpack. While we found overall consistent and sensible trends, there are definitely elements from this study that warrent further investigation. Particularly unexpected was that participants had faster response times when new cognitive chunks were implicit rather than explicit. It would be interesting to unpack whether that effect occurred simply because it meant one could resolve the answer in one long line, rather than two (one to introduce the concept, one to use it), and whether the familiarity of the new concept has an effect. More broadly, as there are large ML efforts around representation learning, understanding how humans understand intermediate concepts is an important direction.

Other interesting experimental directions include the metrics and the interface. Regarding the metrics, now that we know what kinds of explanations can be processed the fastest, it would be interesting to see if subjective satisfaction correlates to those variables in the absence of a task. That is, without any time pressure or time incentive, do people still prefer the same properties purely subjectively? Regarding the interface, we chose ours based on several rounds of pilot studies and then fixed it. However, one can imagine many ways to optimize the display of information, such as highlighting relevant lines. Ultimately, the choice of the display will have to reflect the needs of the downstream task.

More broadly, there are many interesting directions regarding what kinds of explanation are best in what contexts. Are there universals that make for interpretable procedures, whether they be cast as decision sets, decision trees, or more general pseudocode; whether the task is verification, forward simulation, or counterfactual reasoning? Do these universals also carry over to regression settings? Or does each scenario have its own set of requirements? When the dimensionality of an input gets very large, do trade-offs for defining new intermediate concepts change? A better understanding of these questions is critical to design systems that can provide effective explanation to human users.

Finally, future work will need to connect performance on these basic tasks to finding errors, deciding whether to trust the model, and other real-world tasks. These tasks are more difficult to do in controlled settings because each user must have a similar level of grounding in the experimental domain to determine whether an action might be, for example, safe. Our work is just one part of a process of building our understanding of how humans use explanation.

6 Conclusion

In this work, we investigated how the ability of humans to perform a set of simple tasks—simulation of the response, verification of a suggested response, and determining whether the correctness of a suggested response changes under a change to the inputs—varies as a function of explanation size, new types of cognitive chunks and repeated terms in the explanation. We found consistent effects across tasks, metrics, and domains, suggesting that there may exist some common design principles for explanation systems.

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Description of Pilot Studies

We conducted several pilot studies in the design of these experiments. Our pilot studies showed that asking subjects to respond quickly or within a time limit resulted in much lower accuracies; subjects would prefer to answer as time was running out rather than risk not answering the question. That said, there are clearly avenues of adjusting the way in which subjects are coached to place them in fast or careful thinking modes, to better identify which explanations are best in each case.

The experiment interface design also played an important role. We experimented with different placements of various blocks, the coloring of the text, whether the explanation was presented as rules or as narrative paragraphs, and also, within rules, whether the input was placed before or after the conclusion (that is, ‘if A: B” vs. “B if A”). All these affected response time and accuracy, and we picked the configuration that had the highest accuracy and user satisfaction.

Finally, in these initial trials, we also varied more factors: number of lines, input conjunctions, input disjunctions, output conjunctions, output disjunctions and global variables. Upon running preliminary regressions, we found that there was no significant difference in effect between disjunctions and conjunctions, though the number of lines, global variables, and general length of output clause—regardless of whether that length came from conjunctions or disjunctions—did have an effect on the response time. Thus, we chose to run our experiments based on these factors.

Interface

Figure 9: Screenshot of our interface for the simulation task in the Recipe domain. Participants must give a valid recommendation that will satisfy the alien given the observations and preferences.

Figure 10: Screenshot of our interface for the counterfactual task in the Recipe domain. Participants must determine whether the alien's satisfaction with the recommendation changes under the change to the observations described in the magenta box given the observations and the alien's preferences.