Abstract

Drawing causal conclusions from observational data requires making assumptions about the true data-generating process. Causal inference research typically considers low-dimensional data, such as categorical or numerical fields in structured medical records. High-dimensional and unstructured data such as natural language complicates the evaluation of causal inference methods; such evaluations rely on synthetic datasets with known causal effects. Models for natural language generation have been widely studied and perform well empirically. However, existing methods not immediately applicable to producing synthetic datasets for causal evaluations, as they do not allow for quantifying a causal effect on the text itself. In this work, we develop a framework for adapting existing generation models to produce synthetic text datasets with known causal effects. We use this framework to perform an empirical comparison of four recently-proposed methods for estimating causal effects from text data. We release our code and synthetic datasets.\footnote{https://github.com/zachwooddoughty/causal_text_dgps}

1. Introduction

Causal understanding is necessary for reasoning about hypothetical interventions (Pearl and Mackenzie, 2018). As machine learning (ML) methods demonstrate predictive success in complex domains, there is considerable interest in relying on ML to make real-world decisions. However, predictive models cannot be relied upon for decision-making without considering how confounding or selection biases may affect the models’ predictions (Char et al., 2018; Chen and Asch. 2017; Liu et al., 2019; Subbaswamy et al., 2019). Real-world interventions require causal reasoning, but causal reasoning requires evaluations that go beyond traditional ML metrics such as test set accuracy. In particular, causal methods rely on untestable assumptions about the data-generating process (DGP) that produced the data. Violations of the methods’ assumptions may lead to biased predictions or estimates.

Researchers need complete knowledge of a DGP to test the assumptions of a causal method, but such knowledge is often impossible for real-world datasets. Thus while synthetic data has its limitations (Jensen et al. 2019; Gentzel et al., 2019), it plays a crucial
role in understanding how a causal method performs when its assumptions are met or violated. Recently, causal inference evaluations have tested proposed methods against held-out synthetic DGPs (Hahn et al., 2019; Dorie et al., 2019; Shimoni et al., 2018). These synthetic datasets are designed to test different empirical properties of the methods, such as the coverage of confidence intervals or the finite-sample behavior variance of an estimator.

Synthetic datasets are rarely used for predictive tasks when empirical data is widely available. The enormous quantities of text and image data have been curated to produce widely-used datasets for ML and natural language processing (NLP) research (Deng et al., 2009; Brown et al., 2020). However, synthetic datasets have been used in predictive tasks to explore how models handle edge cases or low-resource settings (Elman, 1990; Patki et al., 2016; Khayrallah and Koehn, 2018; Wang and Eisner, 2018; Kim and O’Neill-Brown, 2019; Winata et al., 2019). This is especially true in domains where data is not as widely available, such as clinical settings (Boag et al., 2016; Belinkov and Bisk, 2018; Melamud and Shivade, 2019).

Causal methods have only recently been applied to natural language datasets. Keith et al. (2020) provides a comprehensive overview of recent work, focusing specifically on cases where text data can be used to adjust for (otherwise unobserved) confounding. Text data provides a particularly difficult domain for evaluating causal methods because it requires modeling causal relationships between structured variables and text: “what caused the author to write the text this way?” While there is plentiful text data for training predictive models, we cannot directly measure the underlying processes that humans use to produce or adapt their language in complex domains. Synthetic DGPs need to balance ‘realism and control’ (Wendling et al., 2018): the goal of producing realistic text data against the competing goal of completely specifying the causal effects that produce the text. Past methods evaluated on synthetic data have only satisfied one such goal, either by producing particularly unrealistic text with known effects (Yao et al., 2019; Wood-Doughty et al., 2018; Johansson et al., 2016) or using real-world text without a fully-specified DGP (Veitch et al., 2020; Mozer et al., 2018; Weld et al., 2020).

We introduce a synthetic framework for evaluating causal methods that incorporate text data, exploring desiderata of synthetic text DGPs and tradeoffs between competing goals. We introduce two nontrivial synthetic DGPs, one which samples a bag-of-words from an Latent Dirichlet Allocation (LDA) topic model, and another which samples full sentences from GPT-2 (Blei et al., 2003; Radford et al., 2019). These two underlying generative models allow us to test how causal methods perform when their assumptions are violated (e.g. whether word order matters) (Wallach, 2006). We use our framework to compare four causal methods that rely on text, addressing a known gap in empirical evaluation of such methods (Keith et al., 2020). We explore how existing methods’ empirical performance depends on their assumptions and show that when the causal estimator depends on a text classifier model, better classification accuracy of that classifier does not necessarily imply better causal estimates. We release our code and synthetic datasets to facilitate further development and evaluation of causal methods for language data.
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![Causal DAG](image)

Figure 1: The causal DAG we consider. $A$ is our treatment, $Y$ is our outcome, $C$ and $U$ are confounders, and $T$ is the raw text which is influenced by $U$. The counterfactual $p(Y(a))$ cannot be non-parametrically identified from $p(C,A,Y)$ alone due to unobserved confounding from $U$. Methods may make parametric assumptions on the relationship between $T$ and $U$ in order to estimate the causal effect, or assume knowledge of $p(U|T)$. We parameterize $p(T|U)$ with text generation models in § 4. We discuss the limitations of this DAG model and extensions to other models in § 9.1.

2. Clinical Notes: A Motivating Example

We begin by motivating causal inference for text data through an example. Free text notes in medical records contain information about patients’ histories, possible diagnoses, or patient-doctor relationships (Rajkomar et al., 2018; McVeigh et al., 2016). Importantly, such information often does not appear anywhere else in a patient’s medical record, and thus is inaccessible to retrospective causal analyses that do not use the free text data (Wu et al., 2013; Rosenbloom et al., 2011; Zheng et al., 2011).

In this domain, assumptions about the DGP correspond to assumptions about how clinical notes are written. Unless we have the requisite domain expertise to precisely model the style, vocabulary, and semantics in the true DGP, we must be particularly conservative about the assumptions we make. Synthetic DGPs allow us to test how a method performs when its assumptions are violated, which is essential to understanding whether to trust a real-world application. While empirical success on synthetic data does not guarantee similar performance on real data, any proposed method to draw causal inferences from medical notes should first be validated on synthetic datasets that can capture at least some of the complexity of human language. The goal of this work is the development of synthetic DGPs for language data which make it possible to evaluate causal inference methods.

3. Overview of Causal Assumptions

While randomized control trials are the gold standard for determining causal effects, they are often unethical, impossible, or prohibitively expensive. Causal methods use non-randomized, observational data and assumptions about the DGP to draw conclusions about hypothetical interventions. The ability to make causal conclusions from observational data is transformative, but comes at a cost. The methods require assumptions about the underlying DGP, and violation of these assumptions can invalidate the model’s conclusions. These assumptions are often represented by a directed acyclic graph (DAG; Pearl, 2009) like Figure 1.

Imagine we want to study whether maternal vitamin D deficiency is a risk factor for the pregnancy complication preeclampsia (Bodnar et al., 2014; Silva et al., 2008). In Figure 1.
the treatment \( A \) is a binary measure of vitamin D deficiency and the outcome \( Y \) is the onset of preeclampsia. \( C \) and \( U \), age above 35 years and socioeconomic status (SES), are confounders that influence both \( A \) and \( Y \). Suppose SES is not directly recorded in structured (i.e. tabular) records, but can be inferred from physician’s text notes about the patient. While for simplicity we will assume \( A, C, U, \) and \( Y \) are binary variables, we let \( T \) denote the raw text of the clinical notes. The edge from \( U \) to \( T \) assumes that the clinician’s note-taking is influenced by the underlying \( U \) value; the lack of edges between \( \{ A, C, Y \} \) and \( T \) reflects a simplifying assumption. The relationship between \( U \) and \( T \) is complex and essential to the methods we will consider.

In this setting, the target of interest is the average treatment effect; how much more likely, on average, would patients suffer preeclampsia if they were to have a vitamin D deficiency. We write this as
\[
E[Y(1)] - E[Y(0)]
\]
where \( Y(1) \) is a counterfactual random variable representing “preeclampsia status if a patient, possibly contrary to fact, had a vitamin D deficiency.” This counterfactual variable’s distribution can be identified as:
\[
p(Y(a)) = \sum_{C,U} p(Y|A=a,C,U) p(C,U) \tag{1}
\]
All confounders (common causes) must be included in Eq. (1) to draw valid causal inferences (Pearl, 2009). If we have no information on \( U \) and only observe \( p(C,A,Y) = \sum_U p(Y,A,C,U) \), it is generally impossible to write \( p(Y(a)) \) as a function of the observed data (Pearl, 2009). In this case, we say \( p(Y(a)) \) is not identified; it is impossible to derive a consistent estimator for the causal effect. In real-world applications, an estimator for an unidentified effect may return arbitrarily bad estimates. For a known DAG model, we can use the \( \text{ID} \) algorithm to determine whether a causal effect is identified given which variables are observed (Shpitser and Pearl, 2006).

In Figure 1, we need nontrivial assumptions to identify \( p(Y(a)) \) from \( p(Y,A,C,T) \). The joint \( p(U,T) \) determines whether identification is possible. If \( U \perp T \) (\( T \) provides no information on \( U \)), the causal effect is not identified and no method will succeed; if \( T \) is an exact copy of \( U \), then it should be trivial to recover the causal effect by replacing \( U \) with \( T \) in Eq. (1). When \( T \) is not an exact copy of \( U \), we may be able to treat it as a noisy, high-dimensional proxy for the unobserved confounder \( U \). Depending on the empirical relationship between the text and the structured variables, methods that observe \( T \) instead of \( U \) may be biased.

For real-world data, we cannot validate assumptions about the DGP. Therefore, while applying a causal method to the data will produce conclusions given our assumptions, it cannot validate the efficacy of the method itself. This is the role of the synthetic DGP; we can compare the method’s assumptions to a known ground truth to explore how causal methods succeed or fail as the relationship between text and structured data varies.

4. Causal Effects in Text Generation

Recent work in natural language generation has introduced language models with enormous empirical gains in perplexity and according to human judgments (Radford et al., 2019; Hashimoto et al., 2019; Brown et al., 2020). Language models generate text sequences token by token, where token \( i \) is sampled conditional on the previous \( i-1 \) tokens and the
first token is often sampled conditional on some initial context. These existing methods, however, do not produce datasets with known causal effects on text itself; we must first produce a formal definition for the causal effect of a structured variable on the text generation process. In our clinical example, such an effect represents how a doctor’s notes would have changed had a patient, counterfactually, been of high SES. By controlling the effect of \( U \) on \( T \) in Figure 1, we can evaluate how causal methods perform when their assumptions are met or violated.

We want our marginal \( p(T) \) to conform to a language model that generates text according to a learned distribution, but want to parameterize \( p(T|U) \) such that we can force the generation to smoothly diverge from its learned distribution to depend on \( U \). We want a causal effect of \( U \) on \( T \) to make some words or topics more likely and others less so. That is, texts generated when \( U = 1 \) should be quantitatively and qualitatively different from texts when \( U = 0 \). We will introduce \( \tau \) and \( \delta \) as hyperparameters that control our causal effects. Intuitively, \( \tau \) controls rankings over the vocabulary; the larger \( \tau \) is, the more the ranked preference for \( U = 0 \) differs from that of \( U = 1 \). We can conceptualize \( \delta \) as controlling how much the model indulges its preference; the larger \( \delta \) is, the more likely \( p(T|U = u) \)
Table 1: \( p(U|T) \) classifier accuracy for the nine examples of our trivial DGP. When both \( \tau \) and \( \delta \) are small, accuracy is near random chance. If one of the two parameters is large, accuracy improves; if both are large, accuracy nears 100%. Samples according to these ranked preferences rather than from the pre-trained language model distribution.

To formalize this, let \( V = \{x_1, \ldots, x_N\} \) be a vocabulary of \( N \) words. The learned language model provides an initial distribution \( p(V) \) and uses it to generate the sequences that comprise \( p(T) \). Let \( \tilde{V} \) be an ordering over \( V \). For a binary \( U \), we choose two orderings, \( \tilde{V}_{u=1} \) and \( \tilde{V}_{u=0} \). Our \( \tau \) parameter controls the correlation between those two orderings. When \( \tau = 0 \), the orderings are the same; when \( \tau = 1 \), they are exact reversals of each other. For a given \( \tau \), we sample these orderings such that their Kendall Tau correlation is approximately \( 1 - 2\tau \).

For a given \( \tilde{V} \) and our choice of \( \delta \), we will construct a new distribution over the vocabulary. Define \( f_{\tilde{V}}(x_i) \) as a mapping from a vocabulary item \( x_i \) to the position of that item in the ordering. If \( x_{42} \) is the first item in the \( \tilde{V} \) ordering, then \( f_{\tilde{V}}(x_{42}) = 1 \). Now define a ‘modified Zipfian distribution’ as \( p(x_i) \propto f_{\tilde{V}}(x_i)^{-\delta/(1-\delta)} \). When \( \delta = 0 \), this is simply a uniform distribution over the vocabulary; when \( \delta = 1 \), it is a point mass on the first item in its preference.\(^2\)

Now, given our language model’s learned \( p(V) \), we construct a new distribution:

\[
p'(x_i; \tilde{V}, \delta) \propto p(x_i) \otimes f_{\tilde{V}}(x_i)^{-(\delta/1+\delta)}
\]  

where \( \otimes \) indicates element-wise multiplication. The distribution \( p' \) represents an average between the initial \( p(V) \) and the modified Zipfian defined by \( \tilde{V} \) and \( \delta \). We define a function \( h \) which takes in an initial text generation distribution \( p(V) \), and values for \( \tau \) and \( \delta \) and returns new distributions \( p'_u \) following Eq. (2). We write this as:

\[
h: (p(V), \tau, \delta) \rightarrow \{p'_0(V; \tilde{V}_0, \delta), p'_1(V; \tilde{V}_1, \delta)\}
\]  

Both \( \tau \) and \( \delta \) live in the [0,1] domain. We can conceptualize \( \tau \) as controlling the ‘preference’ over words in the vocabulary and \( \delta \) as controlling the ‘strength’ of that preference. If either hyperparameter is 0, the structured variable \( U \) has no effect on the text generation. If \( \tau = 0 \) then \( \tilde{V}_1 = \tilde{V}_0 \); while \( \delta \) will change the word probabilities, it will change them equally for either value of \( U \). Similarly, if \( \delta \) is 0, then no matter how different \( \tilde{V}_1 \) is from \( \tilde{V}_0 \), \( h(p(V), \tau, 0) \) ignores those preferences and returns the language model’s learned \( p(x_i) \).

\(^2\) For any value of \( \delta \), we will normalize \( p \) to be a distribution with probabilities in \([1e^{-10}, 1 - 1e^{-10}]\).
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Table 2: $p(U|T)$ classification accuracy for LDA text. Increasing $\tau$ and $\delta$ values lead to increased classification accuracy, with exceptions when $\delta$ increases but $\tau$ decreases. If either the topic or word effects are particularly large, classification accuracy exceeds 90%; when both are large, it quickly approaches 100%.

Figure 2 shows how $\delta$ and $\tau$ control a trivial text generation model. We sample nine datasets of 10k sequences of 16 tokens. Our initial $p(V)$ distribution is simply uniform over the vocabulary of 16 tokens. Each cell in the figure shows how the Trivial $p(T|U)$ distributions change as we vary $\delta$ and $\tau$. When $\delta$ is large but $\tau$ is small, some words are much more likely than others, but the two distributions only differ on a single word. When $\tau$ is large but $\delta$ is small, the distributions differ by a small amount on many words.

If we want to explore how causal methods perform in Figure 1, we can control the $p(T|U)$ distribution with $\delta$ and $\tau$. As we turn to more complicated $p(V)$ distributions, we want a better way to interpret the text generated with a given choice of these hyperparameters.

Our approach differs from past (semi-)synthetic text datasets for causal evaluation. Wood-Doughty et al. (2018) sampled synthetic ‘texts’ in a bag-of-words manner similar to our Trivial distribution above, except without the ability to control the strength of the $p(T|U)$ relationship. Veitch et al. (2020) used real text from Reddit or academic papers and sampled synthetic outcomes conditional on metadata related to each text, but without the ability to measure or specify the causal relationship between the text and its metadata. Weld et al. (2020) generate semi-synthetic data by inserting template-based posts into the actual post history of a social media user. These synthetic interventions are discrete, however; there is no way to specify a real-valued causal effect and manipulate it arbitrarily. The flexibility of our approach allows us to explore how methods perform as we vary the causal effect on the text.

4.1 Classification Accuracy and $\delta, \tau$

The $\delta$ and $\tau$ hyperparameters completely control the effect of the structured variables on the text, but are not particularly interpretable. How do we know if particular $\delta$ or $\tau$ values are realistic? What values best mimic a real clinical notes DGP?
Table 3: \( p(U|T) \) classification accuracy for GPT-2 text. Accuracy is much lower than on trivial or LDA data. Increasing \( \tau \) and \( \delta \) values generally leads to increased classification accuracy, but this is not monotonic. When increasing \((\delta_{\text{word}}, \tau_{\text{word}})\) from \((0.5, 0.15)\) to \((0.7, 0.15)\) and reducing \((\delta_{\text{template}}, \tau_{\text{template}})\) from \((0.9, 0.15)\) to \((0.7, 0.15)\) we see a notable decrease in accuracy even when \((\delta_{\text{template}}, \tau_{\text{template}})\) returns to \((0.9, 0.15)\). This is because GPT-2 word and template effects can conflict; because the language model tries to maintain grammatical structure, certain templates make it unlikely to sample certain words.

Rather than adapt our hyperparameters to a specific natural language domain, we will use text classification accuracy as a lens that can be equally applied to both synthetic and real-world text. Given a synthetic dataset, we will train a classifier with \( T \) as the features and \( U \) as the labels. Considering the accuracy of such a classifier will let us compare a synthetic dataset to a real dataset; past work has extensively considered the task of classifying clinical concepts from unstructured text (Liu et al., 2018; Meystre et al., 2008; Afzal et al. 2018; Savova et al., 2010). A synthetic dataset in which a text classifier achieves 99% accuracy is unrealistic, implying \( \delta \) and \( \tau \) are too large. Similarly, if \( \delta \) and \( \tau \) are too small, a \( p(U|T) \) classifier will be no better than chance.

Table 1 shows binary classification accuracy of a simple bag-of-words model trained on the datasets from Figure 2. We use a train/dev/test split of 8k/1k/1k sequences for this and all subsequent text classification experiments. Accuracy improves above random chance as either \( \delta \) or \( \tau \) increase, and quickly maxes out when both are large. Classification accuracy on this task provides a useful way to abstract away the underlying DGP as we introduce more complicated synthetic datasets.

### 4.2 LDA with Causal Effects

For a slightly more complicated synthetic DGP, we consider Latent Dirichlet Analysis (LDA), one of the most widely-used models of text (Blei et al., 2003). It provides a generative model of text that clusters the distribution over the vocabulary into a distribution over topics. While the LDA model ignores word order, so each sampled word drawn from the trained model is independent. This results in generated texts that have no grammatical structure. We train an LDA model on a set of 250,000 documents which was released as part of the training data for GPT-2 (Radford et al., 2019).

To define a \( p(T|U) \) distribution that uses LDA, we will define causal effects for both the words and the topics. Let \( V_{\text{word}} \) be the word vocabulary and \( V_{\text{topic}} \) be the set of learned topics. Then \( p_{\text{LDA}}(V_{\text{topic}}) \) is LDA’s learned baseline distribution over the topics, and \( p_{\text{LDA}}(V_{\text{word}} \mid t \in V_{\text{topic}}) \) is the learned distribution over words for topic \( t \).
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| \( \delta_w \) | The child was known for . . . |
|-----------|--------------------------------|
| 0.0       | his role in the very real Peter Pan film that skyrocketed |
| 0.1       | his role in the flamboyant sleuth Jackie Turner’s hit |
| 0.15      | his German business, and her books were sold in Bavaria |
| 0.25      | her ability to play, run and shoot gags involving giant |
| 0.4       | her ability to see. She began training one more spring |
| 0.45      | her ability to see in one eye; her ability conquer magic |
| 0.5       | her ability to disown her magic ability and her identify |
| 0.6       | her ability one ability her magic ability her magic |

Figure 3: DistilGPT-2 generation when we fix the random seed, template, and \( \tilde{V}_{\text{word}} \) but vary \( \delta_{\text{word}} \). We construct \( \tilde{V}_{\text{word}} \) so the most-preferred words are \textit{her}, \textit{magic}, and \textit{ability}. The model switches from \textit{his} to \textit{her} pronouns as \( \delta \) increases. As \( \delta \) further increases, sentence fluency decreases.

We introduce causal effects with \( h \) from (3). To sample a word from our modified LDA model when \( U = u \), we first sample a topic \( t \) from \( h(p_{\text{LDA}}(V_{\text{topic}}), \tau_{\text{topic}}, \delta_{\text{topic}}) \). Then, instead of sampling from the original LDA distribution, \( p_{\text{LDA}}(V_{\text{word}} | t) \), we sample from \( h(p_{\text{LDA}}(V_{\text{word}} | t), \tau_{\text{word}}, \delta_{\text{word}}) \).

How do these \( \tau \) and \( \delta \) hyperparameters control the generated text? Table 2 shows text classification results. We see that in general, larger \( \tau \) and \( \delta \) lead to higher accuracy, yet there are exceptions. Within a given row or column, when \( \delta \) increases but \( \tau \) decreases, we see a brief drop in accuracy. We can conceptualize this with the plots in Figure 2: as \( \delta \) increases the effect of \( U \) on \( T \) grows and the word distribution changes from its learned distribution, but as \( \tau \) decreases it decreases the difference between the \( U = 0 \) and \( U = 1 \) ‘preference’ distributions. If we plot \( \tau_{\text{word}} \) against \( \delta_{\text{word}} \) and hold topic effects constant, we would see that accuracy monotonically increases as either word effect hyperparameter increases.

4.3 GPT-2 with Causal Effects

One of the primary drawbacks of LDA is that it only models topic, and has no sense of word order or syntax. Therefore, we consider a more complex DGP by extending our synthetic data framework to more complicated neural models that are widely used for text generation.

GPT-2 is a large neural language model that has improved the state-of-the-art on several benchmark evaluations (Radford et al., 2019). It uses 1.5-billion parameters to encode a context sentence into an internal representation and then uses that representation to predict a distribution over the next word in the sentence. Once a word has been sampled from that distribution, it is fed back into the model as additional context, and the sampling process continues. Word-order is thus intrinsic to the sentences generated by GPT-2. To save computation time, we use a smaller 82M parameter DistilGPT-2 model (Sanh et al., 2019). We discuss extensions to more recent neural language models in § 9.2.

While the model can take as input an arbitrary context sentence or phrase, we follow Sheng et al. (2019) and use a set of simple templates to seed the generation of the GPT-2 model. The templates are a combination of a subject (e.g. ‘the person’) and the beginning
of a verb phrase (e.g. ‘was known for’). Our \(V_{\text{template}}\) has 60 templates. We treat GPT-2 as a black-box which inputs a distribution over these 60 templates and outputs a distribution over the words in the vocabulary. As with our LDA model, we will introduce causal effects which influence these inputs and outputs, but otherwise leave the model untouched.

We start with an initial uniform distribution over the 60 templates. From an initially uniform \(p_{\text{GPT-2}}(V_{\text{template}})\), we sample a template \(t\) from \(h(p_{\text{GPT-2}}(V_{\text{template}}), \tau_{\text{template}}, \delta_{\text{template}})\). Then, we feed that template into the GPT-2 model as context, and it produces a distribution over words: \(p_{\text{GPT-2}}(V_{\text{word}} \mid t)\). We then sample the first word from \(h(p_{\text{GPT-2}}(V_{\text{word}} \mid t), \tau_{\text{word}}, \delta_{\text{word}})\). We then feed that sampled word, \(w_1\), back into the GPT-2 model and sample the next word, conditioning on both the template and the first sampled word, from \(h(p_{\text{GPT-2}}(V_{\text{word}} \mid w_1, t), \tau_{\text{word}}, \delta_{\text{word}})\).

Table 3 shows how text classification accuracy changes as our \(\tau\) and \(\delta\) parameters change. As in Table 2, larger \(\tau\) and \(\delta\) values lead to better classification accuracy, with some exceptions. Every \(p(U \mid T)\) accuracy drop on LDA data in Table 2 co-occurred with a drop in a \(\tau\) or \(\delta\) effect. With GPT-2, we see one case where causal effects strictly increase but text classification accuracy decreases. When \(\tau_{\text{word}} = 0.15\) and \(\delta_{\text{template}} = 0.7\) and \(\delta_{\text{word}}\) increases from 0.5 to 0.7 and \(\tau_{\text{template}}\) increases from 0.05 to 0.15, text classification accuracy drops from 85\% to 78\%. A likely explanation for this is that the GPT-2 templates do not affect individual word probabilities, but provide context that affects the entire sequence. The template fragment ‘worked as a’ likely increases occupation-related words, where the fragment ‘was known for’ may not. These non-monotonic effects may complicate the ability of our simple bag-of-words model to differentiate the two distributions.

We also see that while the formal definitions of \(\tau\) and \(\delta\) are the same between LDA and GPT-2, the values must be much larger for the classifier to reach 90\% test set accuracy. This reflects the mismatch between the bag-of-words assumption of our text classifier and the more complex text sequences of GPT-2.

As GPT-2 produces more fluent text than LDA, we can also visualize the effect of \(\delta_{\text{word}}\) by slightly varying its value while repeatedly sampling from the model. Figure 3 shows how the generation changes when we fix the template and GPT-2’s random seeds, and increase \(\delta_{\text{word}}\) for a given \(V_{\text{word}}\) preference.

5. Causal Methods with Text

We have introduced a framework for producing datasets where we can provide fine-grained control over how structured variables influence the text. We can use this framework to evaluate existing methods for estimating causal effects with text data. We will first provide an overview of four such approaches, and then use our framework to conduct a range of simulation studies that explore how well these methods perform as we vary the \(p(T \mid U)\) relationship.

Each method relies on sample-splitting for robust inference (Chernozhukov et al. 2016; Anderson and Magruder 2017). In particular, we will split dataset in half, use one split to train and validate a simple bag-of-words logistic regression model, and then use the other split to estimate our causal effect. Then we will flip the splits to get a second effect estimate on the first split, and then report the average of the two. As we only use simple models for these evaluations, we leave full implementation and training details to our released code.
5.1 Matching with Text

Matching is a popular causal method (Stuart, 2010), which has been recently applied to text datasets (Roberts et al., 2018; Mozer et al., 2018; Yao et al., 2019; Wang and Culotta, 2019). Matching adjusts for confounding by estimating the causal effect among patients who are similar, where similarity can be defined by confounders or by their propensity to have received the treatment. We consider two types of text matching: propensity score matching and representation matching.

If $U$ were observed in Figure 1, valid propensity score matching would proceed by learning a model for $p(A|C, U)$ and matching patients based on the estimated propensity. With $U$ unobserved, we will instead match on a propensity score modeled as $p(A|C, T)$. This method will be biased in general because matching requires the true propensity score. However, if there exists a function that maps our estimated $p(A|C, T)$ to the true propensity $p(A|C, U)$, this approach can be unbiased. To implement this method, we model the propensity $p(A|C, T)$ with a bag-of-words classifier. We then match on the estimated propensity using full matching as implemented in the R package optmatch, following Mozer et al. (2018).

Representation matching attempts to adjust for confounding by matching patients on their covariates $(C, U)$ and then taking $p(Y|A)$ within each matched group as an unbiased estimate of $p(Y(a))$. As $U$ is unobserved, we can instead match on both $C$ and a learned representation of $T$. The intuition is that if two patients have similar $T$ representations, they are likely to have the same value of $U$. However, this method will be biased in general if two values $U$ can produce the same $T$ representation. For our experiments, we use an LDA topic model representation of $T$ and perform full matching using cosine similarity, following (Mozer et al., 2018).

5.2 Conditioning on Text

Rather than matching on the propensity score, we can directly use it in an inverse propensity weighting (IPW) model (Rosenbaum and Rubin, 1983). This approach reweighs the observed data by the inverse of the true propensity model; if the true propensity $p(A|C, U)$ is used, this is a consistent estimator for Eq. (1). When we replace $p(A|C, U)$ with $p(A|C, T)$, our estimates are no longer guaranteed to converge to the ground truth. Instead, we must assume that if the effect of $U$ on $T$ is strong, then the learned propensity score will suffice to reweigh the examples. This approach is similar to the bag-of-words method used by Veitch et al. (2020). Initial experiments, we found that more powerful neural models performed poorly on our datasets of only 10k examples. This method follows other work in controlling for high-dimensional confounders (Hill et al., 2011; McCaffrey et al., 2004; Low et al., 2016).

Our implementation again models $p(A|C, T)$ as a bag-of-words classifier. We truncate propensity weights and report the mean of 100 bootstrap estimates (Lee et al., 2011).

5.3 Imputing with Text

Our fourth causal method assumes access to a text classifier model $p(U|T)$ that can impute $U^*$, a noisy proxy for the true $U$. The method uses the classifier and an estimate of the error
Table 4: Causal estimation error for the four estimation methods on our trivial DGP. All methods approach zero error as $\delta$ and $\tau$ values increase.

| $\tau_{\text{word}}$ | Representation | Propensity | IPW | Measurement |
|----------------------|---------------|------------|-----|-------------|
| 0.1                  | 0.1 0.52 0.84 | 0.1 0.52 0.84 | 0.1 0.52 0.84 | 0.1 0.52 0.84 |
| $\delta_{\text{word}}$ | 0.1 0.19 0.19 | 0.17 0.16 0.16 | 0.19 0.18 0.18 | 0.11 0.03 0.03 |
| 0.4                  | 0.18 0.03 0.02 | 0.14 0.05 0.05 | 0.17 0.06 0.04 | 0.03 0.01 0.00 |
| 0.7                  | 0.16 0.01 0.01 | 0.12 0.05 0.05 | 0.14 0.01 0.01 | 0.01 0.00 0.00 |

rate of the classifier to correct for the bias induced by the imperfect classifications (Pearl, 2010). Importantly, this approach requires more information than text matching or IPW, as we must have access to either a pre-trained classifier with known error rate or enough labeled data $p(U, T)$ to train a classifier. In many cases, such labeled data may be difficult or impossible to collect. We train a logistic regression classifier for $p(U|T)$, using half the training split to train the classifier, and the other half to estimate the classifier’s error rates. Our implementation uses code released by Wood-Doughty et al. (2018).

6. Evaluating Causal Methods with Text

Our framework for producing synthetic text datasets and discussed four past methods that have been proposed for estimating causal effects from text datasets. We will now apply each of these four methods – text propensity score matching (Prop), text representation matching (Rep.), IPW, and measurement error (ME) – to the synthetic datasets we have introduced. Our released code reproduces these experiments.

6.1 Structured Variable Distribution

In §4, we introduced hyperparameters that control the causal effect of a structured variable on a text generation model. To build our datasets, we first define $p(Y, A, C, U)$ and then define the text distribution $p(T|U)$. We limit ourselves to the DAG in Figure 1 and only consider binary structured variables.

We choose the parameters of $p(Y, A, C, U)$ randomly, subject to three constraints. First, we ensure that the true distribution-level causal effect (1) is equal to 0.1; given $C$ and $U$, the treatment increases the likelihood of the outcome by 0.1. Second, we ensure that our dataset exhibits Simpson’s paradox: if we estimate (1) without conditioning on $U$, the causal effect should appear to be $-0.1$. This setup ensures that methods that completely ignore $U$ and $T$ will fail to estimate the causal effect. Finally, we ensure that $p(U = 1) = 0.5$, which makes a majority-guess strategy for inferring $U$ maximally uninformative. These constraints allow for consistency across experimental evaluations; each structured distribution should be comparable.
Table 5: Estimation error for each causal method on LDA synthetic data, averaged over the combination of four structured distributions and four text distributions for each cell. All methods reduce estimation error as the $\delta$ and $\tau$ effects increase in strength, but only measurement error achieves near-zero error for any effect strength.

### 6.2 Reproducibility of Experiments

Because we have a complex method for producing our text distribution $p(T|U)$ and we enforce non-trivial constraints on $p(Y,A,C,U)$, we carefully seed the random number generation required to produce these synthetic distributions. In particular, our sampling of text distributions and structured distributions are orthogonal. We consider four separate structured distributions that meet our above constraints, which we reuse in our evaluations across all three text distribution settings: the trivial 16-word vocabulary, the LDA model, and the GPT-2 model.

All results in Tables 4, 5, and 6 show the absolute-value divergence of the methods’ estimates from an oracle with access to the full structured distribution $p(Y,A,C,U)$. The causal estimate errors for a given $(\tau,\delta)$ pair are averaged over the 16 synthetic distributions that combine our four structured distributions and four text distributions.

### 6.3 Evaluation with Trivial Text

Table 4 shows how the four causal methods perform on the trivial 16-word vocabulary dataset we introduced in § 4. We see that when the $p(T|U)$ relationship is very weak ($\delta_w = 0.1, \tau_w = 0.1$), all four methods perform about as poorly as they would if they had ignored the text entirely. As the $p(T|U)$ relationship becomes stronger, all four methods
Table 6: Causal estimation error for each method on the GPT-2 synthetic data. The measurement error method estimates approach zero only for the largest values of $\delta$ and $\tau$. Neither Propensity nor IPW correct more than half the confounding of a naive estimator, and Representation barely reduces the confounding bias at all.

improve. The text matching and measurement error methods are able to perfectly estimate the true causal effect when the effect of $U$ on $T$ becomes overwhelmingly strong. The IPW method does worse, but does correct for the $U$ confounding as the $p(T|U)$ relationship strengthens. It is not surprising that the measurement error approach works here, as Table 1 and Figure 2 showed us that $p(U|T)$ classification can achieve perfect accuracy on this trivial dataset. The success of the text matching approach highlights that even though $p(A|C,T)$ is not the true propensity score, the relationship between $U$ and $T$ is strong enough to allow for the method to correct for the confounding.

6.4 Evaluation with LDA Text

Table 5 shows how the four causal methods perform on synthetic datasets using the LDA text generation we introduce in § 4.2. These results are less encouraging. Our text generated from LDA is word-order independent, so simple bag-of-words models $p(A|C,T)$ should be powerful enough to capture the text’s complexity. Even so, the matching methods struggle to correct for $U$’s confounding, though they slightly improve as $\tau$ and $\delta$ increase. Compared to the trivial setting, in LDA there is much less direct relationship between $U$ and the sampled text. Thus Representation matching is more likely to match two texts with different $U$ values, and in Propensity the estimated $p(A|C,T)$ diverges from the true propensity. That Propensity outperforms Representation when it did not for Trivial text suggests that the propensity matching may be more effective as it in a single dimension (Roberts et al., 2018). The IPW method, on the other hand, does extremely poorly when the effects of $U$ on $T$ are small. Because a naïve estimator that ignores the text can achieve a causal error of 0.20, the IPW estimator actually worsens the confounding bias. The measurement error approach is effective when $\tau$ and $\delta$ are large enough.

6.5 Evaluation with GPT-2 Text

Table 5 shows how the four causal methods perform on synthetic datasets using the GPT-2 text generation we introduce in § 4.3. Here we see that neither the matching nor IPW
Table 7: Pearson correlation between absolute causal estimation error and the test accuracy of the text classifier that the estimation method relies on. On the Trivial text data, all methods have a negative correlation: increased test accuracy implies lower estimation error. As the text DGP increases in complexity to LDA and GPT-2, this correlation dwindles and then reverses for the Propensity and IPW methods, but remains stable for the measurement method.

Methods ever noticeably improve. The measurement error method is still effective, but only when the effect of $U$ on $T$ is strongest.

While GPT-2 clearly does not produce language at the complexity of real-world datasets, we can better understand the assumptions made by these causal models by exploring how they perform as the underlying text generation become more complex. On this data, simple bag-of-words models we consider are not flexible enough to fully capture the complexity of the text. Even though Table 3 shows us that a bag-of-words classifier can effectively learn this more complicated $p(U|T)$ when the word and template effects are large enough, the $p(A|C,T)$ model learned for the IPW and matching methods does not capture information on the true propensity. The measurement error method and its $p(U|T)$ classifier can provide unbiased estimates, but only when $\delta$ and $\tau$ effects are strongest.

7. Text Classification Accuracy and Estimation Error

Our propensity score matching, IPW, and measurement error methods all rely in part upon a text classifier to estimate the causal effect. However, better performance (as measured by classification accuracy) of this classifier does not necessarily translate into lower causal estimation error. For both propensity score matching and IPW, the text classifier models $p(A|C,T)$. For the measurement error estimator, the text classifier models $p(U|T)$. For the binary $A$ and $U$ we consider, we can easily characterize these models in terms of their classification accuracy. The density plots in Figure 4 shows the relationship between text classifier accuracy and the causal estimation error.

Across all three DGPs, we see that when the $p(U|T)$ classifier has accuracy greater than 80%, our estimate of the causal effect is within 0.05 of the truth. If we could achieve 100% classifier accuracy for the measurement method, it would imply that we had access to the true $p(A,Y,C,U)$, and can trivially estimate the causal effect.

However, for propensity and IPW methods, better classification accuracy does not imply lower estimation error. In fact, better classification accuracy of $p(A|C,T)$ is orthogonal to our goals of low causal estimation error. Instead, we need $p(A|C,T)$ to converge to the true $p(A|C,U)$, which is untestable without observing $U$. 
Figure 4: Joint and marginal density plots of text classifier accuracy and mean absolute causal estimation error for each DGP and each estimation method that relies on a text classifier. Each dot represents one experiment. Figure 5 shows a zoomed-out plot for LDA+IPW; all other plots contain all data. Colors indicate the four structured variable random seeds used to create the true data-generating distributions. For the IPW and Prop methods, the visible clusters show that the relationship between classifier accuracy and causal error is highly dependent on the random seed for structured variables. Thus, for a real-world analysis with an unknown DGP, better classifier accuracy does not imply lower causal error. For the ME method, classifier accuracy and causal error are not clustered by the underlying DGP.
Table 7 shows that as we increase the complexity of our DGP from the Trivial text to LDA and then to GPT-2, we can also empirically see that the correlation between classifier accuracy and estimation error degrades for the Propensity and IPW methods. For the Propensity and IPW methods on GPT-2 data, classifier accuracy is positively correlated with estimation error, suggesting that the \( p(A|C,T) \) classifier has overfit and diverged from the true \( p(A|C,U) \) propensity.

8. Availability and Use of Labeled \( U \) Data

Our empirical results have demonstrated that the measurement error estimator performs the best on our synthetic datasets. However, this method relies upon access to labeled \( p(U|T) \) data. This finding raises two questions: how much labeled data does the measurement error method require, and could other methods perform as well or better if given access to such labeled \( p(U,T) \) data?

We run additional experiments where we limit the amount of labeled data that our estimator has access to. Of a dataset of 10,000 total examples, we use \( n \) of them to train and validate a classifier \( p(U|T) \) and use \((10,000 - n)\) to compute our estimate of the causal effect. Our previous experiments have considered \( n = 5,000 \); in Table 8 we plot estimation error as we vary \( n \) from 50 to 5,000. For the DGPs with the strongest causal effects, the mean absolute error remains small even as we substantially reduce the number of examples. Estimation error on DGPs with weaker causal effects are more sensitive to the number of examples.

We then compare these evaluations on limited labeled data against a baseline that assumes access to an equal amount of data on the full \( p(U,C,A,Y) \) distribution. Suppose we can pay clinicians to annotate \( n \) patient records for the unobserved confounder \( U \);
Table 8: Measurement error method’s mean absolute estimation error on GPT-2 data as we vary the amount of labeled data used. Train and validation data is split evenly; we train the \( p(U|T) \) classifier with half and estimate its error rate on the other half. The last column is equivalent to the last row of Table 6. The \( p(U,C,A,Y) \) baseline ignores the text and simply computes the causal effect using Equation 1.

| \( \delta_w \) | \( \tau_w \) | \( \delta_t \) | \( \tau_t \) | 50  | 100 | 200 | 300 | 400 | 500 | 1000 | 1500 | 2000 | 2500 | 5000 |
|----------------|--------|--------|--------|-----|-----|-----|-----|-----|-----|------|------|------|------|------|
| 0.0            | 0.00   | 0.7    | 0.45   | 0.19| 0.16| 0.14| 0.11| 0.10| 0.08| 0.08  | 0.07  | 0.11  | 0.10  |
| 0.2            | 0.03   | 0.7    | 0.15   | 0.19| 0.18| 0.16| 0.17| 0.16| 0.16| 0.14  | 0.10  | 0.10  | 0.10  | 0.10  |
| 0.2            | 0.03   | 0.7    | 0.45   | 0.18| 0.17| 0.13| 0.09| 0.11| 0.10| 0.10  | 0.08  | 0.07  | 0.06  | 0.04  |
| 0.2            | 0.15   | 0.7    | 0.15   | 0.19| 0.17| 0.13| 0.13| 0.13| 0.09| 0.06  | 0.04  | 0.03  | 0.03  | 0.03  |
| 0.5            | 0.05   | 0.5    | 0.05   | 0.17| 0.16| 0.13| 0.11| 0.12| 0.09| 0.04  | 0.03  | 0.04  | 0.03  | 0.03  |
| 0.5            | 0.05   | 0.7    | 0.05   | 0.18| 0.16| 0.14| 0.13| 0.12| 0.06| 0.03  | 0.03  | 0.04  | 0.04  | 0.04  |
| 0.5            | 0.15   | 0.7    | 0.05   | 0.10| 0.05| 0.06| 0.04| 0.04| 0.02| 0.02  | 0.01  | 0.01  | 0.01  | 0.01  |
| 0.5            | 0.15   | 0.9    | 0.15   | 0.11| 0.07| 0.06| 0.05| 0.04| 0.03| 0.03  | 0.02  | 0.02  | 0.02  | 0.02  |
| 0.7            | 0.15   | 0.7    | 0.15   | 0.10| 0.08| 0.05| 0.04| 0.04| 0.03| 0.02  | 0.02  | 0.02  | 0.02  | 0.01  |
| 0.7            | 0.15   | 0.9    | 0.15   | 0.10| 0.07| 0.04| 0.04| 0.04| 0.03| 0.02  | 0.02  | 0.02  | 0.02  | 0.01  |

\( p(U,C,A,Y) \) Baseline

should we use those examples to use the measurement error method, or should we just directly compute the causal effect using Equation (1), ignoring the text entirely? The \( p(U,C,A,Y) \) baseline in Table 8 suggests that as soon as we have at least 200 examples, this baseline is as good on average as the measurement error method, even on DGPs with the strongest \( U \rightarrow T \) causal effects. Figure 6 shows in more detail this baseline compared against two of the DGPs in Table 8. In particular, this figure shows the 95% confidence interval for the three methods. For the DGP with large causal effects, the measurement error method is quite comparable to the baseline as \( n \geq 500 \), but has somewhat smaller confidence intervals at lower-data settings. On the DGP with small \( U \rightarrow T \) causal effects, the measurement error method is strictly worse than the baseline.

The measurement error method is the only approach that achieves success on our GPT-2 DGPs, but requires access to \( p(U|T) \) labels. If this method can be matched by a baseline that ignores the text entirely, it may seem that incorporating NLP methods into causal inference is not worth the effort. But our results are not entirely pessimistic and the flaws they do reveal point to many opportunities for future work. The \( p(U,C,A,Y) \) baseline importantly requires access to the full joint, whereas the measurement error method only requires data on the \( p(U|T) \) conditional. This has many practical implications. For example, if researchers at a hospital cannot collect \( U \) annotations for their data due to patient privacy restrictions, they still may be able to apply a \( p(U|T) \) classifier to that data. Thus if we can leverage existing anonymized clinical datasets as the \( p(U,T) \) data, we can produce analysis that would otherwise be impossible.
Figure 6: A closer look at three rows from Table 8. Solid line plots mean (not mean absolute) causal error; shaded regions show 95% confidence interval from 100 bootstrap samples. Measurement error results are averaged over four structured variables distributions and four text distributions. The baseline ignores the text and is averaged over four structured distributions. Even for text data with the strongest causal effects we consider, the measurement error approach is not noticeably better than the $p(U, C, A, Y)$ baseline once we have at least 200 labeled examples.

There are also many opportunities to develop new approaches that outperform the four methods we evaluated. We should expect that some access to labeled data should make it possible to learn a propensity score or text representation that provides for lower estimation error when primarily using data without labeled $U$. An unsupervised text representation such as LDA could be augmented with labeled $p(U, T)$ so that learned topics are more discriminative of the underlying $U$ (Blei and McAuliffe, 2007). Similarly, if we were given access to some labeled $p(U, T, C)$ data, we could train a propensity score model such that predicted propensities must be roughly equal for examples with the same $U$. We can also explore approaches that combine these four methods to produce new multiply-robust methods. Many causal estimators use multiple models and are provably unbiased if at least one or more of those models are correctly-specified (Bang and Robins, 2005; Vansteelandt et al., 2008). Can we develop a new matching method that are unbiased if either the propensity model or representation model are unbiased? Can we effectively combine all four methods we considered into a single multiply-robust estimator?

9. Limitations and Extensions

Our evaluation framework and experimental results provide new insights into how existing estimators perform on synthetic text datasets. In generating our synthetic datasets and evaluating these methods, we have made simplifying assumptions. Many of these assumptions
may limit the efficacy of our work to certain applications, yet most such assumptions can be relaxed by extending our work.

9.1 Other DAG Models

We only sample datasets from synthetic DGPs corresponding to the DAG model in Figure 1. There are of course infinitely many DAG models that could be considered, but we point out a few important generalizations that would complicate our methods for sampling data or evaluating methods.

Figure 7a extends Figure 1 by adding causal effects from all structured variables to the text data. Such a DAG complicates our approach for sampling text from a language model conditional on the structured variables. In § 4 we parameterized \( p(T|U) \) with our two types of hyperparameters: \( \delta \) and \( \tau \). Figure 7a requires sampling from \( p(T|U,C,A,Y) \), which may require a different hyperparameter formulation. Our implementation assumes \( U \) is binary, the immediate extension to a continuous-valued \( U \) simply requires replacing the two orderings (\( \tilde{V}_{u=1} \) and \( \tilde{V}_{u=0} \)) with a continuous function of \( U \) that outputs an ordering \( \tilde{V}_{u} \). If \( T \) is sampled conditional on multiple structured variables, then we need a function that maps from those variables to an ordering over the vocabulary. In such a setting, we need one or more \( \tau \) hyperparameters that control how sensitive this function is to changes in one or more structured variables.

The DAG in Figure 7a also changes the assumptions for the causal methods we consider. The Propensity and Representation methods, like any matching estimator, requires matching only on pre-treatment covariates; variables that are non-descendants of the treatment \( A \). Matching on post-treatment variables can introduce significant bias (Rosenbaum, 1984; Stuart, 2010). If the text data is influenced by both \( U \) and \( A \), it cannot be easily used for matching. Similarly, for the IPW model (or an outcome model), if the text is a collider (descendant of both \( A \) and \( Y \)), conditioning on it may introduce bias (Greenland, 2003).

Within the context of the measurement error estimator, Figure 7a violates our previous assumption of non-differential measurement error (Carroll et al., 2006; Wood-Doughty et al., 2018). Thus, rather than estimating two (assuming \( U \) is binary) marginal error rates \( p(U^* = 1|U = 0) \) and \( p(U^* = 0|U = 1) \), we must estimate several conditional error rates of the form \( p(U^* = u'|U = u, A = a, C = c, Y = y) \). Estimating such error rates requires data on the full joint \( p(U,C,A,Y,T) \) which, as discussed in § 8, reduces the efficacy of these methods compared to simpler approaches that ignore the text data entirely.
In the DAG in Figure 7b, the text $T$ can be seen as a treatment or an outcome; $p(T(a))$ is the counterfactual distribution over $T$ if we intervene on $A$, and $p(Y(t))$ is the counterfactual distribution over $Y$ if we intervene on $T$. Because our framework currently does not support sampling structured variables conditional on the text, we cannot sample from $p(Y|T, U, C)$. The causal estimators we consider do not make the necessary assumptions to estimate the high-dimensional effects of $A$ on $T$ or of $T$ on $Y$ (Nabi et al., 2017; Egami et al., 2018).

### 9.2 Other Language Models

Recent years have seen an explosion in both the frequency and size of neural language models (Bender et al., 2021). While the only such model we have considered is a compressed version of GPT-2 (Sanh et al. 2019; Radford et al. 2019), our framework for adding causal effects can be easily extended to new language models such as GPT-3 or Switch-C (Brown et al., 2020; Fedus et al., 2021). All our approach assumes is that the model takes as input an initial context and then, for each word, outputs a distribution over the vocabulary. Our causal effects simply adjust the distribution over context inputs and the distribution over the word logits.

Other work on language modeling has focused on controllable text generation which can produce sentences that follow a specified style (Xu et al., 2020; Keskar et al., 2019; Kedzie and McKeown, 2020). For example, the approach from Dathathri et al. (2019) specifies topic (e.g. politics) and a sentiment (e.g. negative) which guides the text generation. Such an approach could help generate synthetic datasets which are more domain-specific (see § 9.4). In any future work analyzing synthetic text generated from large-scale language models, researchers should be careful to examine how such models learn and reproduce societal biases encoded in the training data (Sheng et al., 2019; Bender et al., 2021).

### 9.3 Better Estimators

We have mentioned in § 8 that future work should consider multiply-robust estimators with better asymptotic properties. Our evaluations could also be extended by implementing more flexible (e.g. neural) nuisance models that capture relationship between the structured variables and the text. Veitch et al. (2020) proposed causal methods that leverage existing text embeddings which have been widely successful in many predictive tasks. Such neural models may require new assumptions – such as with respect to smoothness (Farrell et al., 2021) – but have demonstrated empirical performance greatly surpassing that of the bag-of-words logistic regression models we have considered (Rajpurkar et al., 2016). Such neural models often require large datasets for training or pre-training, and in our initial experiments, such models did not outperform logistic regression on our small datasets. Future work could combine pre-training on large datasets (Lee et al. 2020) with fine-tuning on our small datasets (Jin et al. 2019). We could also compare against stronger baselines that ignore the text but leverage all available data, such as the estimator of Yang and Ding (2020) which combines both a small dataset that includes the unobserved confounder and a large dataset that does not. Such an estimator should outperform the $p(U, C, A, Y)$ baseline we considered in § 8 by leveraging the additional data that does not contain $U$. 
9.4 More realistic DGPs

Our synthetic DGPs enable new evaluations for causal methods for text, but synthetic data in general is not without its inherent limitations. One barrier that prevents generalizability of results on synthetic data to real-world data is that often synthetic DGPs are explicitly designed to demonstrate the utility of a proposed method, and thus other assumptions that could expose the method’s flaws may be ignored by the creator (Gentzel et al., 2019). While our framework addresses some of these concerns by making it easy to randomize the DGP parameterization and enabling extensions to new language models, there is more that can be done. Gentzel et al. (2019) suggests semi-synthetic datasets that, for example, use \( p(U, C) \) data from a real-world study and then sample \( p(A, Y|U, C) \) synthetically so the causal effects are known (Dorie et al., 2019; Shimoni et al., 2018). While our framework could adopt this approach and use empirical \( p(U, C) \) data, if we use empirical text data we lose any knowledge of the causal relationships between text and structured variables.

Within the synthetic framework we have proposed, there are many ways to make our synthetic DGPs more realistic for applications to specific domain areas. We have used EHR data and clinical notes as a motivating example throughout, but our DGPs are unrelated to such applications. Suppose we have an EHR dataset with physiological measurements and clinical notes. If we want to conduct a retrospective causal analysis using text, we might first develop a synthetic DGP that tries to approximate the empirical dataset (Neal et al., 2020). To adapt the synthetic DGPs from this work to this application, we might consider using a language model fine-tuned on clinical notes (Lee et al., 2020) or adapted to the complex vocabulary and style of the domain (Ruch et al., 2003; Melamud and Shivade, 2019; Boag et al., 2016; Choi et al., 2017). If our clinical data has a structured variable \( U \) that we believe influences the text \( T \), we might incorporate controllable generation techniques to parameterize \( p(T|U) \) more realistically, for example by choosing a vocabulary preference \( \tilde{V}_u \) that reflect which words are more commonly used when describing patients with different values of \( U \). Such adaptations could make inferences drawn from synthetic data more robust or make evaluations more interpretable to domain experts.

10. Conclusions

Our experiments demonstrate the importance of accurate assumptions in a causal analysis. All four causal methods can control for unobserved confounding in a trivial text generation setting, but as our generative \( p(T|U) \) increases in complexity, the implicit assumptions of the matching and IPW methods render them biased. Although the matching and IPW methods use the same \( p(A|C, T) \) propensity score model, the matching approaches work are superior in the trivial and LDA settings. Even though the trained models are identical, the underlying assumptions are different. Because it requires additional data, the measurement error approach is able to make fewer assumptions, remaining effective as long as its \( p(U|T) \) classifier is accurate. These results do not imply that text matching and IPW methods cannot control for unobserved confounding, but rather that we should be cautious and clear about what assumptions we make about our models and the underlying DGP. Evaluating on synthetic data can help clarify these assumptions.

As NLP research furthers the state-of-the-art in predictive modeling, such tools offer the potential to influence human decision-making and guide our understanding of the
world. Such models rely on assumptions that may be irrelevant for a supervised learning benchmark and yet essential to any real-world application. Explicitly adopting a causal inference perspective on natural language datasets can help enable inferences that are robust to confounding or other biases. We hope our evaluation framework and released code will support further research in these directions.
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