Confounding is a Pervasive Problem in Real World Recommender Systems

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Abstract

Unobserved confounding arises when an unmeasured feature influences both the treatment and the outcome, leading to biased causal effect estimates. This issue undermines observational studies in fields like economics, medicine, ecology or epidemiology. Recommender systems leveraging fully observed data seem not to be vulnerable to this problem. However many standard practices in recommender systems result in observed features being ignored, resulting in effectively the same problem. This paper will show that numerous common practices such as feature engineering, A/B testing and modularization can in fact introduce confounding into recommendation systems and hamper their performance. Several illustrations of the phenomena are provided, supported by simulation studies with practical suggestions about how practitioners may reduce or avoid the affects of confounding in real systems.

1 Introduction

In a recommender system all information that leads to a recommendation is known, which in principle should make unobserved confounding impossible. However this paper shows that many common practices for training click models in production results in available covariates or features being ignored. These practices are pervasive in real systems and likely result in many systems behaving sub-optimally.

Researchers in recommender systems are probably somewhat familiar with the way confounding can lead to scenarios where correlation does not equal causation, but to set concepts and terminology Figure 1 Left provides a simple illustration of Simpson's famous paradox. The diagram serves to illustrate how three variables interact, life expectancy, income and Champagne consumption. The diagram shows that life expectancy increases with Champagne consumption, but it also illustrates that once income is adjusted for, life expectancy decreases with Champagne consumption (which is perhaps a more plausible causal relationship). Simpson's paradox refers to the fact that there can be different causal interpretations between three or more variables as in this example. A confounder is a covariate that must be incorporated into the model in order for the correct causal inference to be arrived at (in this case income is a confounder), in natural experiments it is possible (indeed common) for the confounder to be unobserved rendering a correct causal inference impossible. If a click model is used to produce personalized recommendations then in principle everything is observed and confounding will not occur. Many 'best practices' in production systems can result in observed information being ignored resulting in confounding and reduced performance.

Let a click be denoted c, the recommendation or action a, and the covariate used for personalization currently is x_1, a second covariate that may also be used to personalize is denoted x_2, The terms covariate and feature are used interchangeably. The causal graph is shown in Figure 1 Right, the click c is caused by all three of the action a and the covariates x_1 and x_2. In the current setup the

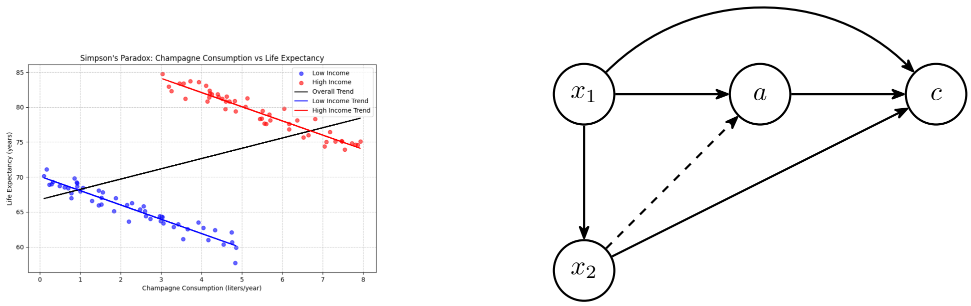


Figure 1: Left: Simpson's paradox, Right Causal DAG.

recommendation is personalized only by x_1 only indicated by the arrow from $x_1 \to a$, the dashed arrow from $x_2 \to a$ is absent (for now). The joint distribution on the covariates is denoted by the arrow $x_1 \to x_2$, this choice of direction simplifies the proofs. The recommender system is trained on a log of observations of a, x_1, x_2, c by regressing c on x_1, a i.e. a logistic regression model is fit:

$$\hat{\beta} = \ \text{argmax}_\beta \sum_{i=1}^{n} c_i \log \sigma((x_{1_i} \otimes a_i)^T \beta) + (1 - c_i) \log(1 - \sigma((x_{1_i} \otimes a_i)^T \beta))$$

where $\sigma(\cdot)$ is the logistic sigmoid and $\otimes$ is the Kronecker product. It is assumed that training on recent history (the previous day) mitigates any non-stationarity. This model can then be used in order to produce a new epsilon greedy recommendation policy, where the exploration parameter is given by ϵ and the number of actions is A.

$$\pi(a|x_1) = (1 - \epsilon)\mathbf{1}\{a = \text{argmax}_{\text{a}'} (x_1 \otimes a')^T \hat{\beta}\} + \frac{\epsilon}{A}.$$

We verify that this standard practice is in fact sound from a causal inference point of view. Applying the do calculus gives the following expression:

$$P(c|\text{do}(a), x_1) = \sum_{x_2} P(c|a, x_1, x_2) P(x_2|x_1) \tag{1}$$

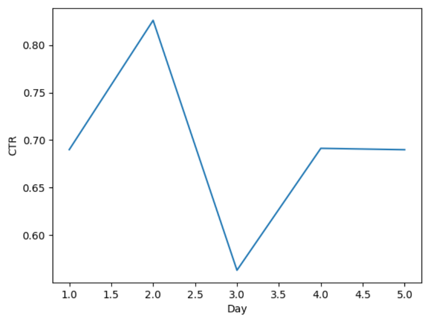
however the absent of the edge $x_2 \to a$, by the backdoor criterion shows that x_2 is ignorable. This means that the logistic regression model in Equation 1, directly estimates the causal quantity in Equation 2.

What if the system then changes such that personalization now uses x_2 i.e. the edge $x_2 \to a$ is added, but the goal remains to propose a new system that *does not use* x_2. Applying the do calculus still gives the same formula for the causal effect (Equation 1), however now ignorability of x_2 no longer applies and this formula must be applied (which is rather unpalatable to practitioners as it involves fitting a larger model to both $P(c|a, x_1, x2)$ and $P(x_2|x_1)$ and performing a sum or integral over x_2).

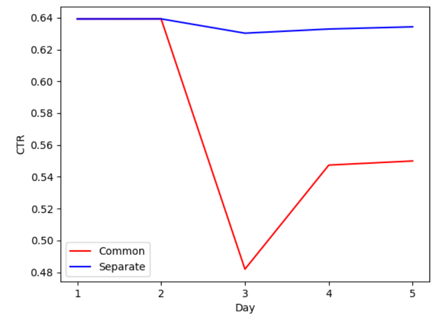
In a recommender system x_1 is the feature currently used for personalization and x_2 is an additional feature that may be used in the future. This paper makes two main points: If the recommender system ignores some feature x_2 i.e. there is no arrow $x_2 \to a$, then ignorability applies even if $x_2 \to c$. Researchers do not need to worry about confounding or causal theory in this case. Numerous common practices result in the link between some feature $x_2 \to a$ existing, but the model training procedure ignoring this link and erroneously assume x_2 to be ignorable.

The main mathematical results that this paper relies on is that Equation 1 is the appropriate formula for both causal graphs in Figure 1 Right (both with and without the dashed line), as can be shown using the do calculus[1]. Secondly that ignorability of x_2 applies if and only if the graph in Figure

[1]By application of the do calculus' Rule 2 and Rule 3, resulting in a rule that slightly generalized the backdoor rule Pearl (1995).



(a) The standard practice of feature engineering causes confounding on Day 3. Exactly the same training procedure is used on Day 1, 4 and 5 as Day 3, but on the log for Day 3 (Day 2) the policy implements $x_2 \to a$ causing x_2 to not be ignorable. The same procedure that worked on Day 1 no longer works on Day 3.



(b) Demonstrates how A/B testing with common training can entrench confounding effects The A/B test starts on Day 1, where A uses only x_1 and B uses x_1, x_2. Both A and B are trained on a combined log resulting in A suffering from confounding that persists as long as the A/B test runs. The plot shows two approaches to A/B testing with separate training data (blue) and combined (red), only population A suffers confounding and only population A is shown.

Figure 2: Confounding caused by dropping x_2.

1 has no arrow $x_2 \to a$, as can be shown by applying the backdoor criterion or the Rubin Causal Model[2]. There are two main cases where a link might be introduced into $x_2 \to a$, yet is ignored in subsequent training. The first is due to feature engineering, the second is due to modularization (both good practices from other points of view).

2 Confounding due to Feature Engineering and A/B Testing

Feature engineering is a standard optimization of machine learning models in recommendation teams. Consider the following story.

Day 0: the recommender system is doing pure random exploration of the action a independent of all features. That is the policy is $\pi_0(a|x_1, x_2) = \pi_0(a) = \frac{1}{A}$. Data is collected on Day 0 i.e. $\mathcal{D}_0 = \{c^{(i)}, x_1^{(i)}, x_2^{(i)}, a^{(i)}\}_{i=1}^{L_0}$.

The reco team decides that features x_1 are more interesting, and they will ignore x_2 for now, they fit the model with maximum likelihood i.e.

$$\hat{\beta}_0 = \text{argmax}_\beta \sum_{i=1}^{L_0} \log P(c^{(i)}|x_1^{(i)}, a^{(i)}, \beta).$$

The model might be a logistic regression $P(c = 1|x_1, a, \beta) = \sigma((x_1 \otimes a)^T \beta)$. From a confounding point of view, everything is fine, it doesn't matter that x_2 is ignored. In practice this means that if the estimate $\hat{\beta}_1$ is good it means that the 'true' CTR for any given x_1 and action a is indeed given by $P(c = 1|x_1, a, \hat{\beta}_1)$. Similarly, the model can be used to find the optimal a for any given x_1 which is achieved using an epsilon greedy policy starting on Day 1.

[2]When the arrow $x_2 \to a$ is present in the causal graph ($x_1 \to a, x_2 \to a, x_1 \to c, x_2 \to c, a \to c, x_1 \to x_2$), x_2 is not ignorable for estimating $P(c \mid x_1, \text{do}(a))$, as it confounds the $a \to c$ relationship via the backdoor path $a \leftarrow x_2 \to c$, requiring adjustment with $\sum_{x_2} P(c \mid x_1, x_2, a)P(x_2 \mid x_1)$. When the arrow $x_2 \to a$ is absent ($x_1 \to a, x_1 \to c, x_2 \to c, a \to c, x_1 \to x_2$), x_2 is ignorable, as all backdoor paths ($a \leftarrow x_1 \to c$, $a \leftarrow x_1 \to x_2 \to c$) are blocked by conditioning on x_1, yielding $P(c \mid x_1, \text{do}(a)) = P(c \mid x_1, a)$. , for ignorability in the Rubin Causal Model see Rubin (1974); Rosenbaum and Rubin (1983).

Day 1: the reco team then deploys on Day 1 the following policy:

$$\pi_1(a|x_1) = (1-\epsilon)\mathbf{1}\{a = \text{argmax}_{a'} P(c=1|x_1, a', \hat{\beta}_0)\} + \frac{\epsilon}{A}$$

where ϵ controls an epsilon greedy policy. This new policy produces an uplift and everyone is happy. The reco team also collect some new data for Day 1 $\mathcal{D}_1 = \{c^{(i)}, x_1^{(i)}, x_2^{(i)}, a^{(i)}\}_{i=L_0+1}^{L_1}$. The reco team then decides that maybe they could also incorporate the features x_2 into the model, so at the end of Day 1 they then fit:

$$\hat{\beta}_1 = \text{argmax}_{\beta} \sum_{i=L_0+1}^{L_1} \log P(c^{(i)}|x_1^{(i)}, x_2^{(i)}, a^{(i)}, \beta).$$

The model again might be a logistic regression $P(c=1|x_1, a, \beta) = \sigma((x_1 \otimes x_2 \otimes a)^T \beta)$. There is no confounding, and indeed there is better personalization because now x_2 is used. Again, this means that the 'true' CTR for a user arriving with features x_1, x_2 and action a is estimated by $P(c|x_1, x_2, a, \hat{\beta}_1)$.

Day 2 the reco team then deploy the following policy:

$$\pi_2(a|x_1, x_2) = (1-\epsilon)\mathbf{1}\{a = \text{argmax}_{a'} P(c=1|x_1, x_2, a', \hat{\beta}_1)\} + \frac{\epsilon}{A}.$$

The new policy is more personalized (as it now uses both x_1 and x_2) and produces an uplift, but for technical reasons it is decided that incorporating the extra complexity of using x_2 is not worth the extra engineering cost. So, the reco team decide that in the future the model will return to only use x_1 i.e. they re-apply the methodology they applied at the end of Day 0. It seems like the same methodology should work again, but will it? The data collected is $\mathcal{D}_2 = \{c^{(i)}, x_1^{(i)}, x_2^{(i)}, a^{(i)}\}_{i=L_1+1}^{L_2}$. They use this data to estimate:

$$\hat{\beta}_2 = \text{argmax}_{\beta} \sum_{i=L_1+1}^{L_2} \log P(c^{(i)}|x_1^{(i)}, a^{(i)}, \beta)$$

Unfortunately, the model $P(c=1|x_1, a, \hat{\beta}_2)$ is now confounded, this means that the true CTR for a given x_1 and action a is *not* given by $P(c=1|x_1, a, \hat{\beta}_2)$, similarly selecting the a that maximizes the click probability will *not* give the best policy.

Day 3: the reco team deploy:

$$\pi_3(a|x_1) = (1-\epsilon)\mathbf{1}\{a = \text{argmax}_{a'} P(c=1|x_1, a', \hat{\beta}_2)\} + \frac{\epsilon}{A}$$

The click model is confounded and consequently the wrong preferred action is selected for some contexts resulting in a lower average click through rate. On subsequent days, (Day 4, Day 5), the confounding disappears again, because the model is only trained on the previous day and from Day 3 onwards, the recommendations are determined only by x_1. Figure 2a shows a simulated demonstration of this behavior where confounding impacts the Day 3 CTR but it returns on Day 4 and 5. This suggests that one solution to mitigate confounding is to simply wait, unfortunately this solution is not generally applicable. Consider the same scenario but now the feature engineering performance is being measured using A/B testing and both A and B are trained on a common log as shown by the red line in Figure 2b, the practice of having each policy training on its own log as shown by the blue line remedies the confounding problem but reduces the sample size.

The experiments in Figure 2a and Figure 2b have a similar setup. Both x_1 and x_2 are categorical variables with 5 states and a is a categorical with 10 states. The simulations use 400 000 samples. In Figure 2b the A/B test starts on Day 2, causing confounding starting on Day 3. Population A is the model that only has access to x_1, only population A suffers from confounding as such only the CTR of population A is shown. Code for both experiments is available here.

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A Confounding due to Feature Engineering and Modularization

Modularization is an important engineering practice, but when combined with feature engineering it can also lead to confounding. Consider the situation where there is a separate sale and click model. Let c be a click, s be a sale, a be an action and x be the context, the goal is to maximize post click sales. This can be achieved by solving

$$\begin{aligned} a^* &= \mathrm{argmax}_a P(c=1, s=1|a,x) \\ &= \mathrm{argmax}_a P(s=1|a,x,c=1)P(c=1|a,x) \end{aligned}$$

Now, consider that a different team build the click and the sale model and they both do feature engineering and produce different feature sets x', and x'', so instead the delivered action is:

$$a^* = \mathrm{argmax}_a P(s=1|a,x',c=1)P(c=1|a,x'')$$

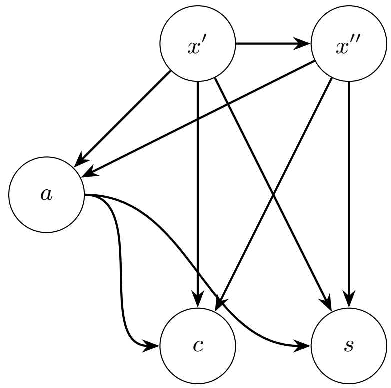

Figure 3: Causal graph for the post click sale models. If the sale model only is able to see x' and the click model is only able to see x'' then the two models confound each other.

This procedure looks sound from the point of view of the individual models, but when the individual models are estimated on real data the above procedure produces sub-optimal actions due to each model being confounded. The causal graph is shown in Figure 3, training the click model and sales model individually results in either x' or x'' being an unobserved (or more accurately ignored) confounder. This highlights a dilemma. As the post click sales model is trained on a log where clicks have already occurred it is a much smaller dataset and hence there is a risk of higher variance in the estimation. Conventional machine learning wisdom suggests that x' should be simpler than x'' as a way to reduce this variance in the sales model, but having x' and x'' different leads to confounding.

A similar situation can occur when there are two decisions, let's say d is the decision to display a recommendation and a is the recommendation and our problem is to maximize clicks c, then a correct solution (neglecting exploration) is:

$$\hat{\beta} = \text{argmax}_{\beta} \sum_{i=1}^{N} \log P(c_i|a_i, d_i, x_i, \beta) \tag{2}$$

$$\pi(a, d|x) = \mathbf{1}\{a, d = \text{argmax}_{a',d'} P(c = 1|a', d', x, \hat{\beta})\}$$

However perhaps the decision of a and d must be made separately and without knowledge of the other part of the system. A common practice is to fit two models, $P(c|a, x')$ and $P(c|d, x'')$, and let $\pi(a, d|x) = \pi(a|x')\pi(d|x'')$, where $\pi(a|x') = \mathbf{1}\{a = \text{argmax}_{a'} P(c = 1|a, x')\}$, and $\pi(d|x') = \mathbf{1}\{a = \text{argmax}_{d'} P(c = 1|d, x'')\}$. Similar to the above example each of these models confound each other.

Avoiding confounding requires fitting the model in Equation 3, and then if engineering limits require the policy to be simplified to $\pi(a, d|x) = \pi_{\Xi}(a|x')\pi_{\Gamma}(d|x'')$, where Ξ and Γ parameterize the implementable policies. Policy learning can be used to find:

$$\text{argmax}_{\Xi,\Gamma} E_{a\sim\pi_{\Xi}(a|x')d\sim\pi_{\Gamma}(d|x'')x,x',x''\sim P(x,x',x'')} P(c = 1|a, d, x, \hat{\beta}).$$

Algorithms such as REINFORCE Williams (1992) can be used to target this type of objective.

B Past Work on Confounding for Recommender Systems

Recommender systems by construction have access to all covariates that lead to past recommendations, hence confounding can in principle be avoided simply by adjusting for all non-ignorable covariates.

The study in Xu et al. (2023) develops personalization algorithms for cases where confounders are truly unobserved using an instrumental variable approach. As past recommendations are always a function of observed information this does not apply to recommendation. Computational advertising can handle market conditions (price of inventory) by making the action the bid, or by using intention to treat, which reformulates the problem without any unobserved confounders.

The study in Jadidinejad et al. (2021) does not use click models to build a recommender system but rather applies to standard collaborative filtering datasets. It is rather difficult to map this work to the practices in a real world recommender system.

The study in Jeunen and London (2023) considers estimating the reward of a policy using inverse propensity score based estimators when both the propensity and covariates that determine the propensity are missing.

Propensity score methods are relevant to confounding, there are two broad approaches. Balancing scores Rosenbaum and Rubin (1983) use the balancing score as a (usually) low dimensional substitute for a covariate in order to estimate an unconfounded model. In contrast, inverse propensity score estimators (IPS) avoid using a model and instead estimate the expected utility of a new policy Joachims et al. (2018). Variants of this estimator typically make trade-offs in terms of bias and variance of the expected utility, common variants include clipping, self-normalized importance sampling and doubly robust. Both balancing scores for models propensity scores for policy estimators avoid problems of confounding. The trade-offs in using estimators directly of the policy utility, rather than a likelihood based approach for estimating a model are rather complicated (see Sims (2006)) and beyond scope. Broadly, both methods do something that might be considered unnecessary; model based approaches must estimate the reward of every action, IPS based methods must estiamte the expected utility (which is not needed to select an action). IPS based estimators can have very high variance and violate the likeihood principle Berger and Wolpert (1988), but they are much more practical in multi-turn problems Sakhi et al. (2025).

C Are These Practices Really ‘Pervasive’?

Real recommender systems are built from many sub-models. Although the details of real systems are not publicly known, it is not a secret that many systems use at least two stages Borisyuk et al. (2016); Yi et al. (2019) and separate the reward into components such as clicks and sales Vasile et al. (2017). To avoid confounding requires a lot of discipline, either using the same features in all sub-models or another more sophisticated approach. Given that no paper describes this practice, it is reasonable speculation that most tech companies do not implement a strategy to avoid confounding. Moreover, there are real costs in making avoiding confounding a priority. Working legacy systems will need significant modification. Parameter estimation accuracy may suffer in some sub-models (that have their feature dimension increased). Also note that many tech companies prioritize building highly personalized recommendation engines using many features perhaps based on deep learning models. Often the pursuit of this goal will be at the expense of making sure that all sub-models are absolutely free from confounding.

Similarly, the temporal confounding effects of adding or removing a feature into a sub-model are not documented in the literature. While surely some practitioners are aware of these concerns, the lack of a systematic discussion, strongly suggests that it is neglected by most tech companies.

It is hard to know the A/B testing practices used at different tech companies, and again, there is no systematic treatment of the concept applied to recommendation in the literature (where models are re-trained). Our reasonably informed speculation is that some A/B tests involve models training on a common log producing another source of confounding.

D Strategies for Avoiding Confounding

Some strategies for avoiding confounding remain research questions, but the following ideas should be implemented where possible:

- A/B Testing should involve each candidate recommender system training exclusively on its own log.
- A feature can be added to (all models) within a system without any concern about confounding.

- The removal of a feature from (all models) within a system will cause confounding. Simply, waiting for the confounded log to move outside the training window is one possible approach. Another approach is to use the backdoor rule, but this is likely very unappealing to practitioners.
- Adding a feature into a sub-model will improve the performance of that sub-model, but confound all other models. This should give pause to thought to improving only one sub-model.
- Putting the same features in all sub-models might well be unacceptable from a model fitting point of view (some models may estimate poorly if the feature dimension becomes larger). One approach is to use the balancing scores (see Rosenbaum and Rubin (1983)) which is a score $b(x_2)$ such that $x_2 \rightarrow a$ can be replaced with $b(x_2) \rightarrow a$, using $b()$ as a feature can help modularize, reduce variance and avoid confounding. However, the first step is for practitioners to simply recognize that confounding is a potentially serious problem in real recommender systems.