

SPLIT-DOOR CRITERION: IDENTIFICATION OF CAUSAL EFFECTS THROUGH AUXILIARY OUTCOMES

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We present a method for estimating causal effects in time series data when fine-grained information about the outcome of interest is available. Specifically, we examine what we call the *split-door* setting, where the outcome variable can be split into two parts: one that is potentially affected by the cause being studied and another that is independent of it, with both parts sharing the same (unobserved) confounders. We show that under these conditions, the problem of identification reduces to that of testing for independence among observed variables, and propose a method that uses this approach to automatically find subsets of the data that are causally identified. We demonstrate the method by estimating the causal impact of Amazon’s recommender system on traffic to product pages, finding thousands of examples within the dataset that satisfy the split-door criterion. Unlike past studies based on natural experiments that were limited to a single product category, our method applies to a large and representative sample of products viewed on the site. In line with previous work, we find that the widely-used click-through rate (CTR) metric overestimates the causal impact of recommender systems; depending on the product category, we estimate that 50–80% of the traffic attributed to recommender systems would have happened even without any recommendations. We conclude with guidelines for using the split-door criterion as well as a discussion of other contexts where the method can be applied.

1. Introduction. The recent growth of digital platforms has generated an avalanche of highly granular and often longitudinal data regarding individual and collective behavior in a variety of domains of interest to researchers, including in e-commerce, healthcare, and social media consumption. Because the vast majority of this data is generated in nonexperimental settings, researchers typically must deal with the possibility that any causal effects of interest are complicated by a number of potential confounds. For example, even effects as conceptually simple as the causal impact of recommendations on customer purchases are likely confounded by selection effects [Lewis, Rao and Reiley (2011)], correlated demand [Sharma, Hofman and Watts (2015)], or other shared causes of both exposure and purchase. Figure 1(a) shows this canonical class of causal inference problems in the form of a causal graphical model [Pearl (2009)], where X is the cause and Y is its effect. Together U and W refer to all of the common causes of X and Y that may confound estimation of the causal effect, where critically some of these confounders

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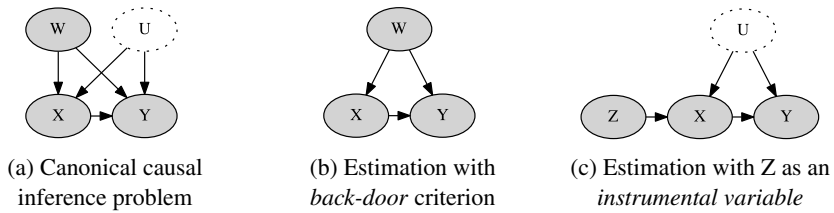


FIG. 1. *Left: Graphical model for the canonical problem in causal inference. We wish to estimate the effect of X on Y . W represents observed common causes of X and Y ; U represents other unobserved (and unknown) common causes that confound observational estimates. Middle: The causal model under the selection on observables assumption, where there are no known unobserved confounders U . Right: The canonical causal model for an instrumental variable Z that systematically shifts the distribution of the cause X independently of confounders U .*

(labeled W) may be observed, while others (U) are unobserved or even unknown. Ideally one would answer such questions by running randomized experiments on these platforms, but in practice such tests are possible only for the owners of the platform in question, and even then are often beset with implementation difficulties or ethical concerns [Fiske and Hauser (2014)]. As a result researchers are left with two main strategies for making causal estimates from large-scale observational data, each with its own assumptions and limitations: either conditioning on observables or exploiting natural experiments.

1.1. *Background: Back-door criterion and natural experiments.* The first and by far the more common approach is to assume that the effect of unobserved confounders (U) is negligible after conditioning on the observed variables (W). Under such a *selection on observables* assumption [Imbens and Rubin (2015)], one conditions on W to estimate the effect of X on Y when these confounders are held constant. In the language of graphical models, this strategy is referred to as the *back-door criterion* [Pearl (2009)] on the grounds that the “back-door pathway” from X to Y (via W) is blocked by conditioning on W [see Figure 1(b)] and can be implemented by a variety of methods, including regression, stratification, and matching [Rubin (2006), Stuart (2010)]. Unfortunately for most practical problems it is difficult to establish that all of the important confounders have been observed. For example, consider the problem of estimating the causal impact of a recommender system on traffic to e-commerce websites such as Amazon.com, where X corresponds to the number of visits to a product’s webpage, and Y the visits to a recommended product shown on that webpage. One could compute the observed click-through rate after conditioning on all available user and product attributes (e.g., user demographics, product categories and popularities, etc.), assuming that these features constitute a proxy for latent demand. Unfortunately, there are also many potentially unobserved confounders (e.g., advertising, media coverage, seasonality, etc.) that impact both a product and its recommendations, which if excluded would render the back-door criterion invalid.

Motivated by the limitations of the back-door strategy, a second main approach is to identify an external event that affects the treatment X in a way that is arguably random with respect to potential confounds. The hope is that such variation, known as a *natural experiment* [Dunning (2012)], can serve as a substitute for an actual randomized experiment. Continuing with the problem of estimating the causal impact of recommendations, one might look for a natural experiment in which some products experience large and sudden changes in traffic, for instance when a book is featured on Oprah’s book club [Carmi, Oestreicher-Singer and Sundararajan (2012)]. Assuming that the increase in traffic for the book is independent of demand for its recommendations, one can estimate the causal effect of the recommender by measuring the change in sales to the recommended products before and after the book was featured, arguing that these sales would not have happened in the absence of the recommender. Such events provide *instrumental variables* that identify the effect of interest by shifting the distribution of the cause X independently of unobserved confounds U [Angrist, Imbens and Rubin (1996)]. Figure 1(c) depicts this in a graphical model, where the additional observed variable Z denotes the instrumental variable.

These two main approaches trade off critical goals of identification and generalization in causal inference. The estimate for back-door conditioning is typically derived using all available data, but provides no identification guarantees in the presence of unobserved confounders. Instrumental variables, in contrast, provide identification guarantees even in the presence of unobserved confounders, but these guarantees apply only for local subsets of the available data—the relatively rare instances for which a valid instrument that exogenously varies the cause X is known (e.g., lotteries [Angrist, Imbens and Rubin (1996)], variation in weather [Phan and Airolidi (2015)], or sudden, large events [Dunning (2012), Rosenzweig and Wolpin (2000)]).

1.2. *The “split-door” criterion.* In this paper we introduce a causal identification strategy that incorporates elements of both the back-door and natural experiment approaches, but that applies in a different setting. Rather than conditioning on observable confounds W or exploiting sources of independent variation in the cause X , we instead look to *auxiliary outcomes* [Mealli and Pacini (2013)] to identify subsets of the data that are causally identified. Specifically, our strategy applies when the outcome variable Y can be effectively “split” into two constituents: one that is caused by X and another that is independent of it. Figure 2(a) shows the corresponding causal graphical model, where Y_R denotes the “referred” outcome of interest affected by X and Y_D indicates the “direct” constituent of Y that does not directly depend on X . Returning to the recommender system example, Y_R corresponds to recommendation click-throughs on a product whereas Y_D would be all other traffic to that product that comes through channels such as direct search or browsing. Whenever such fine-grained data on Y is available, we show that it is possible to reduce causal identification to an independence test between the cause

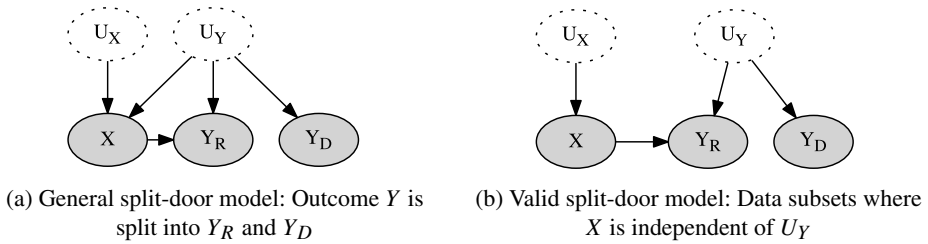


FIG. 2. Panel (a) illustrates the canonical causal inference problem when outcome Y can be split up into two components. For clarity, unobserved confounders U are broken into U_Y that affects both X and Y , and U_X that affects only X . The split-door criterion finds subsets of the data where the cause X is independent of U_Y by testing independence of X and Y_D , leading to the unconfounded causal model shown in Panel (b).

X and the auxiliary outcome Y_D . Because this strategy depends on the availability of a split set of variables for Y , we call it the *split-door* criterion for causal identification, by analogy with the more familiar back-door criterion.

Although we make no assumptions about the functional form of relationships between variables, a crucial assumption underlying the split-door criterion is *connectedness*; that is, that the auxiliary outcome Y_D must be affected (possibly differently) by *all* causes that also affect Y_R . As we discuss in more detail in Section 5, this assumption is plausible in scenarios such as online recommender systems, where recommended products are reachable through multiple channels (e.g., search or direct navigation) and it is unlikely that demand for a product manifests itself exclusively through only one of these channels. More generally, the connectedness assumption is expected to hold in scenarios where direct and referred outcomes incur similar cost, which makes it unlikely that something that causes the outcome does so only when referred through X , but never directly.

Under the above assumption, the split-door criterion seeks to identify subsets of the data where causal identification is possible. In this sense, the method resembles a natural experiment, except that instead of looking for an instrument that creates variation in X , we look for variations in X directly. As in a natural experiment, however, it is important that any such variation in X is independent of potential confounds. For instance in the example above, it is important that a sudden burst of interest in a particular book is not correlated with changes in latent demand for its recommendations. To verify this requirement, the split-door criterion relies on a statistical test to select for cases where there are no confounds (observed or otherwise) between X and Y_R . Specifically, we show that given a suitable auxiliary outcome Y_D , and a test to establish if X and Y_D are independent, the causal effect between X and Y_R can be identified. Furthermore, since this test involves two observed quantities (X and Y_D), we can systematically search for subsets of the data that satisfy the required condition, potentially discovering a large number of cases in which we can identify the causal effect of X on Y_R .

We illustrate this method with a detailed example in which we estimate the causal impact of Amazon.com’s recommendation system using historical web browsing data. Under the above assumptions on the dependence between referred and direct visits to a product’s webpage, we show how the criterion provides a principled mechanism for determining which subsets of the data to include in the analysis. The split-door criterion identifies thousands of such instances in a nine-month period, comparable in magnitude to a manually tuned approach using the same data [Sharma, Hofman and Watts (2015)], and an order of magnitude more than traditional approaches [Carmi, Oestreicher-Singer and Sundararajan (2012)]. Further, the products included in our analysis are representative of the overall product distribution over product categories on Amazon.com, thereby improving both the precision and generalizability of estimates. Consistent with previous work [Sharma, Hofman and Watts (2015)], we find that observational estimates of recommendation click-through rates (CTRs) overstate the actual effect by anywhere from 50% to 80%, calling into question the validity of popular CTR metrics for assessing the impact of recommendation systems. For applications to other online and offline scenarios, we provide an R package¹ that implements the split-door criterion.

1.3. *Outline of paper.* The remainder of this paper proceeds as follows. In Section 2 we start with a formal definition of the split-door criterion and give precise conditions under which the criterion holds. For clarity we provide proofs for causal identification both in terms of the causal graphical model from Figure 2(a) and also in terms of structural equations. In Section 3 we propose a simple, scalable algorithm for identifying causal effects using the split-door criterion. Then in Section 4, we explain more formally how the split-door criterion differs from the instrumental variables and back-door methods mentioned above. Section 5 presents details about the Amazon.com data and an application of the split-door criterion to estimate the causal impact of its recommendation system. In Section 6 we then discuss limitations of the split-door criterion as well as other settings in which the criterion applies, arguing that many existing datasets across a variety of domains have the structure that outcomes of interest can be decomposed into their “direct” and “referred” constituents. We conclude with a prediction that as the size and granularity of available datasets, along with the number of variables in them, increase at an ever faster rate, data-driven approaches to causal identification will become commonplace.

2. The split-door identification criterion. The split-door criterion can be used whenever observed data is generated from the model shown in Figure 2(a). Here X represents the cause of interest, Y_R denotes the “referred” portion of the

¹URL: <https://www.github.com/amit-sharma/splitdoor-causal-criterion>.

outcome affected by it, and Y_D indicates the “direct” part of the outcome which does not directly depend on X . We denote the overall outcome by $Y = Y_R + Y_D$. We let U_Y represent all unobserved causes of Y , some of which may also be common causes of X , hence the arrow from U_Y to X . Additional latent factors that affect only X are captured by U_X . Both U_X and U_Y can be a combination of many variables, some observed and some unobserved. (For full generality, the analysis presented here assumes that all confounds are unobserved.) As noted earlier, the unobserved variables U_Y create “back-door pathways” that confound the causal effect of X on Y , resulting in biased estimates. The central idea behind the split-door criterion is that we can use an independence test between the auxiliary outcome Y_D and X to systematically search for subsets of the data that are free of these confounds and do not contain back-door pathways between X and Y_R . In other words, we can conclude that such subsets of the data were generated from the unconfounded causal model shown in Figure 2(b), and therefore the causal effect of X on Y can be estimated directly from these data. Importantly, identification of the causal effect rests on the assumption that no part of U_Y causes one part of Y and not the other.

2.1. *The split-door criterion through a graphical model.* Here we formalize the intuition above in the causal graphical model framework. To identify the causal effect, we make the following two assumptions. The first pertains to connectedness of the causal model.

ASSUMPTION 1 (Connectedness). Any unobserved confounder U_Y that causes both X and Y_R also causes Y_D and the causal effect of such U_Y on Y_D is nonzero.

Note that Assumption 1 requires only that the causal effect of U_Y on Y_D be nonzero, without any requirements on the size of the effect(s) involved. That said, it is a strong requirement in general, as it applies to all sub-components of U_Y and thus involves assumptions about potentially high-dimensional, unobserved variables. Whenever Y_D and Y_R are components of the same variable it is plausible that they share causes, but one still must establish that this condition holds to ensure causal identification. It is instructive to compare this assumption to the strict independence assumptions involving unobserved confounders required by methods such as instrumental variables [Angrist, Imbens and Rubin (1996)].

The second assumption, which relates statistical and causal independence between observed variables, is standard for many methods of causal discovery from observational data.

ASSUMPTION 2 (Independence). If X and Y_D are statistically independent, then they are also causally independent in the graphical model of Figure 2(a).

Here *causal* independence between two variables means that they share no common causes and no directed path in the causal graphical model leads from one to another. More formally, the two variables are “*d-separated*” [Pearl (2009)] from each other. Thus, Assumption 2 is a variant of the Faithfulness or Stability assumptions in causal graphs with latent unobserved variables [Pearl (2009), Spirtes, Glymour and Scheines (2000)]. In the causal model shown in Figure 2(a), for instance, this assumption rules out the possibility of an event where the observed variables X and Y_D are found to be statistically independent, but U_Y still affects both of them and the observed independence in the data results from U_Y 's effect canceling out exactly over the path $X-U_Y-Y_D$. In other words, this assumption serves to rule out an (unlikely) event where incidental equality of parameters or certain data distributions render two variables statistically independent even though they are causally related.

Under Assumptions 1 and 2, we can show that statistical independence of X and Y_D ensures that X is not confounded by U_Y . First, we provide a result about the resulting causal graph structure when $X \perp\!\!\!\perp Y_D$.

LEMMA 1. *Let X , Y_R , and Y_D be three observed variables corresponding to the causal model in Figure 2(a), where U_Y refers to unobserved causes of Y_R . If the connectedness (1) and independence (2) assumptions hold, then $X \perp\!\!\!\perp Y_D$ implies that the edge $U_Y \rightarrow X$ does not exist or that U_Y is constant.*

PROOF (ARGUMENT). The proof can be completed directly from Figure 2(a) and properties of a causal graphical model.

$X \perp\!\!\!\perp Y_D$ implies that the causal effect of U_Y on Y_D and X somehow cancels out on the path $X \leftarrow U_Y \rightarrow Y_D$. By Assumption 2, this cancellation is not due to incidental equality of parameters or a particular data distribution, but rather a property of the causal graphical model. Therefore, this can only happen if:

- (i) U_Y is constant (and thus *blocks* the path), or
- (ii) One of the edges exists trivially (does not have a causal effect). Using Assumption 1, U_Y has a nonzero effect on Y_D . Then, the only alternative is that the $X \leftarrow U_Y$ edge does not exist, leading to the unconfounded causal model in Figure 2(b).

PROOF. We provide a proof by contradiction using the principle of *d-separation* [Pearl (2009)] in a causal graphical model.

Let us suppose $X \perp\!\!\!\perp Y_D$, and that the $U_Y \rightarrow X$ edge exists and U_Y is not constant.

Using the rules of *d-separation* on the causal model in Figure 2(a), the path $X-U_Y-Y_D$ corresponds to

$$(2.1) \quad (X \perp\!\!\!\perp Y_D | U_Y)_G,$$

$$(2.2) \quad (X \not\perp\!\!\!\perp Y_D)_G,$$

where the notation $(\cdot)_G$ refers to *d-separation* under a causal model G . In our case, G corresponds to the causal model in Figure 2(a).

However, using Assumption 2, statistical independence of X and Y_D implies causal independence, and thus, d-separation of X and Y_D .

$$(2.3) \quad (X \perp\!\!\!\perp Y_D)_G.$$

Equations (2.2) and (2.3) result in a contradiction. To resolve:

- (i) Either U_Y is constant and thus (2.1) implies $(X \perp\!\!\!\perp Y_D)_G$ holds, or
- (ii) The path $X-U_Y-Y_D$ does not exist. Using Assumption 1 of dependence of Y_D on U_Y , the only possibility is that the $X \leftarrow U_Y$ edge does not exist. \square

We now show that Lemma 1 removes confounding due to U_Y and that the observational estimate $P(Y_R|X = x)$ is also the causal estimate.

THEOREM 2.1 (Split-door Criterion). *Under the assumptions of Lemma 1, the causal effect of X on Y_R is not confounded by U_Y and is given by:*

$$P(Y_R|\text{do}(X = x)) = P(Y_R|X = x),$$

where $\text{do}(X = x)$ refers to experimental manipulation of X and $Y_R|X = x$ refers to the observed conditional distribution.

PROOF (ARGUMENT). Lemma 1 leads to two cases:

- (i) By the back-door criterion [Pearl (2009)], if U_Y is constant, then X and Y_R are unconfounded, because the only back-door path between X and Y_R contains U_Y on it.
- (ii) Similarly, if the $U_Y \rightarrow X$ edge does not exist, then X and Y_R are unconfounded because absence of the $U_Y \rightarrow X$ edge removes the back-door path between X and Y_R .

In both cases, unconfoundedness implies that the effect of X on Y_R can be estimated using the observational distribution. \square

PROOF. The proof follows from an application of the second rule of do-calculus [Pearl (2009)]:

$$(2.4) \quad P(\mathcal{Y}|\text{do}(\mathcal{Z} = z), \mathcal{W}) = P(\mathcal{Y}|\mathcal{Z} = z, \mathcal{W}) \quad \text{if } (\mathcal{Y} \perp\!\!\!\perp \mathcal{Z}|\mathcal{W})_{G_{\underline{\mathcal{Z}}}},$$

where $G_{\underline{\mathcal{Z}}}$ refers to the underlying causal graphical model with all outgoing edges from \mathcal{Z} removed.

Substituting $\mathcal{Y} = Y_R$, $\mathcal{Z} = X$, $G_{\underline{X}}$ corresponds to the causal model from Figure 2(a) without the $X \rightarrow Y_R$ edge. Using Lemma 1, two cases exist:

- (i) U_Y is constant.

Let $\mathcal{W} = U_Y$. Under the modified causal model $G_{\underline{X}}$ without the $X \rightarrow Y_R$ edge, the path $X-U_Y-Y_R$ is the only path connecting X and Y_R , which leads to the following *d-separation* result:

$$(2.5) \quad (Y_R \perp\!\!\!\perp X|U_Y)_{G_{\underline{X}}}.$$

Combining Rule (2.4) and the above *d-separation* result, we obtain

$$P(Y_R|\text{do}(X = x), U_Y) = P(Y_R|X = x, U_Y) = P(Y_R|X = x),$$

where the last equality holds because U_Y is constant throughout.

(ii) The edge $U_Y \rightarrow X$ does not exist.

Let $\mathcal{W} = \emptyset$. Under the modified causal model $G_{\underline{X}}$ without the $X \rightarrow Y_R$ edge, X and Y_R are trivially *d-separated* because no path connects them without the edge $U_Y \rightarrow X$.

$$(2.6) \quad (Y_R \perp\!\!\!\perp X)_{G_{\underline{X}}}.$$

From Rule (2.4) and the above *d-separation* result, we obtain

$$P(Y_R|\text{do}(X = x)) = P(Y_R|X = x). \quad \square$$

2.2. *The split-door criterion through structural equations.* Although we have already analyzed the split-door criterion in terms of the causal graphical model in Figure 2(a), for expositional clarity we note that it is also possible to do the same using structural equations. Specifically, we can write three structural equations:

$$(2.7) \quad x = g(u_x, u_y, \epsilon_x), \quad y_r = f(x, u_y, \epsilon_{yr}), \quad y_d = h(u_y, \epsilon_{yd}),$$

where $\epsilon_x, \epsilon_{yr}$, and ϵ_{yd} are mutually independent, zero-mean random variables that capture modeling error and statistical variability. As in Assumption 1, we assume that U_Y affects both Y_D and Y_R . In general, the causal effects among variables may not be linear; however, for the purpose of building intuition we rewrite the above equations in linear parametric form:

$$(2.8) \quad x = \eta u_x + \gamma_1 u_y + \epsilon_x, \quad y_r = \rho x + \gamma_2 u_y + \epsilon_{yr}, \quad y_d = \gamma_3 u_y + \epsilon_{yd},$$

where ρ is the causal parameter of interest, and $\epsilon_x, \epsilon_{yr}, \epsilon_{yd}$ are independent errors in the regression equations. The split-door criterion requires independence of X and Y_D , which in turn implies that $\text{Cov}(X, Y_D) = 0$:

$$\begin{aligned} 0 &= \text{Cov}(X, Y_D) \\ &= E[XY_D] - E[X]E[Y_D] \\ &= E[(\eta u_x + \gamma_1 u_y + \epsilon_x)(\gamma_3 u_y + \epsilon_{yd})] - E[\eta u_x + \gamma_1 u_y + \epsilon_x]E[\gamma_3 u_y + \epsilon_{yd}] \\ &= \gamma_1 \gamma_3 E[U_Y \cdot U_Y] - \gamma_1 \gamma_3 E[U_Y]E[U_Y] \\ &= \gamma_1 \gamma_3 \text{Var}(U_Y). \end{aligned}$$

Assuming that Y_D is affected by U_Y (and therefore γ_3 is not 0), the above can be zero only if $\gamma_1 = 0$, or if U_Y is constant ($\text{Var}[U_Y] = 0$). In both cases, X becomes independent of U_Y and the following regression can be used as an unbiased estimator for the effect of X on Y_R :

$$(2.9) \quad y_r = \rho x + \epsilon'_{y_r},$$

where ϵ'_{y_r} denotes an independent error.

3. Applying the split-door criterion. The results of the previous section motivate an algorithm for applying the split-door criterion to observational data. Specifically, given an empirical test for independence between the cause X and the auxiliary outcome Y_D , we can select instances in our data that pass this test and satisfy the split-door criterion. In this section we develop such a test for time series data, resulting in a simple, scalable identification algorithm.

At a high level, the algorithm works as follows. First, divide the data into equally-spaced time periods τ such that each period has enough data points to reliably estimate the joint probability distribution $P(X, Y_D)$. Then, for each time period τ :

1. Determine whether X and Y_D are independent using an empirical independence test.
2. If X and Y_D are determined to be independent, then the current time period τ corresponds to a valid split-door *instance*. Use the observed conditional probability $P(Y_R|X = x)$ to estimate the causal effect in the time period τ . Otherwise, exclude the current time period from the analysis.
3. Average over all time periods where $X \perp\!\!\!\perp Y_D$ to obtain the mean causal effect of X on Y_R .

Implementing the algorithm requires making suitable choices for an independence test and also its significance level, taking into account multiple comparisons. In the following sections, we discuss these choices in detail, as well as sensitivity of the method to violations in our assumptions.

3.1. Choosing an independence test. Each X – Y_D pair in Step 1 provides two vectors of length τ with observed values for X and Y_D . The key decision is whether these vectors are independent of each other. In theory any empirical test that reliably establishes independence between X and Y_D is sufficient to identify instances where the split-door criterion applies. For instance, assuming we have enough data, we could test for independence by comparing the empirical mutual information to zero [Pethel and Hahs (2014), Steuer et al. (2002)]. In practice, however, because we consider subsets of the data over relatively small time periods τ , there may be substantial limits to the statistical power we have in testing for independence. For example, it is well known that in small sample sizes, testing for independence via mutual information estimation can be heavily biased [Paninski (2003)].

Thus, when working with small time periods τ we recommend the use of exact independence tests and randomization inference [Agresti (1992, 2001), Lydersen et al. (2007)].² In general, this approach involves repeatedly sampling randomized versions of the empirical data to simulate the null hypothesis and then comparing a test statistic on the observed data to the same on the null distribution. Specifically, for each X – Y_D pair, we simulate the null hypothesis of independence between X and Y_D by replacing the observed X vector with a randomly sampled vector from the overall empirical distribution of X values. From this simulated X – Y_D instance, we compute a test statistic that captures statistical dependence, such as the distance correlation, which can detect both nonlinear and linear dependence [de Siqueira Santos et al. (2014), Székely, Rizzo and Bakirov (2007)]. We then repeat this procedure many times to obtain a null distribution for the test statistic of this X – Y_D pair. Finally, we compute the probability p of obtaining a test statistic as extreme as the observed statistic under the null distribution, and select instances in which the probability p is above a pre-chosen significance level α .

3.2. Choosing a significance level. In contrast to standard hypothesis testing where one is looking to reject the null hypothesis that two variables are independent and therefore thresholds on a small p -value, here we are looking for independent X – Y_D pairs that are highly probable under the null and thus want a large p -value. In other words, we are interested in a low type II error (or false negatives), in contrast to standard null hypothesis testing, where the focus is on type I errors (false positives) and hence significance levels are set low. Therefore, one way to choose a significance level would be to choose α as close as possible to 1 to minimize type II errors when X and Y_D are dependent. At the same time, we need to ensure that the test yields adequate power for finding independent X – Y_D pairs. Unlike a conventional hypothesis test for dependent pairs, power for our test is $1 - \alpha$, the probability that the test declares an X – Y_D pair to be independent when it is actually independent. As we increase α , type II errors decrease, but power also decreases.

Complicating matters, the combination of low power and a large number of hypothesis tests raises concerns about falsely accepting pairs that are actually dependent. As an extreme example, even when all X – Y_D pairs in a given dataset are dependent, some of them will pass the independence test simply due to random chance. Therefore, a more principled approach to selecting α comes through estimating the expected fraction of erroneous split-door instances returned by the procedure, which we refer to as ϕ . As described in Appendix A, we apply techniques from the multiple comparisons literature [Farcomeni (2008), Liang and Nettleton (2012), Storey (2002)] to estimate this fraction ϕ for any given significance level.

²When X and Y_D are discrete variables, methods such as Fisher's exact test are appropriate. If, however, X and Y_D are continuous—as is this case for the example we study in Section 5—we recommend the use of resampling-based randomization inference for establishing independence.

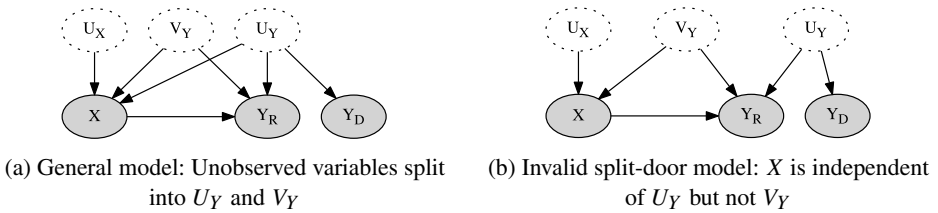


FIG. 3. Violation of the connectedness assumption. Causes for X and Y_R consist of two components, U_Y and V_Y , where V_Y does not affect Y_D and hence is undetectable by the split-door criterion. In the general causal model shown in Panel (a), $X \rightarrow Y_R$ is confounded by both U_Y and V_Y . In the causal model corresponding to a split-door instance in Panel (b), $X \rightarrow Y_R$ is still confounded by the common cause V_Y .

3.3. *Sensitivity to identifying assumptions.* The above algorithm yields a causal estimate only if the identifying assumptions of *connectedness* and *independence* are satisfied. Independence is based on the standard faithfulness assumption in causal discovery [Spirtes, Glymour and Scheines (2000)]. Connectedness, on the other hand, requires justification based on domain knowledge. Even when the connectedness assumption seems plausible, we recommend a sensitivity analysis to assess the effects of potential violations to this assumption.

From Assumption 1, violation of connectedness implies that there exist some unobserved variables that affect X and Y_R but not Y_D . Figure 3(a) shows this scenario, which is identical to the model in Figure 2(a) with the addition of an unobserved variable V_Y that affects X and Y_R , but not Y_D . Applying the split-door criterion in this setting ensures that there is no effect of U_Y on X , but does not alleviate possible confounds from V_Y , as shown in Figure 3(b). Note that this is analogous to the situation in back-door-based methods when one fails to condition on unobserved variables that affect both the treatment and outcome. Correspondingly, sensitivity analyses designed for back-door-based methods [Carnegie, Harada and Hill (2016), Harding (2009), Rosenbaum (2010), VanderWeele and Arah (2011)] can be readily adapted to analyzing split-door instances. In addition, noting that split-door estimates represent averages over all discovered split-door instances, we introduce an additional sensitivity parameter κ that denotes the fraction of instances for which connectedness is violated. In Appendix B we provide a derivation showing that sensitivity for the split-door estimate reduces to sensitivity for back-door methods and conduct this analysis for the application presented in Section 5.

4. **Connections to other methods.** The split-door criterion is an example of methods that use empirical independence tests to identify causal effects under certain assumptions [Cattaneo, Frandsen and Titiunik (2015), Grosse-Wentrup et al. (2016), Jensen et al. (2008), Sharma, Hofman and Watts (2015)]. By searching for

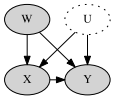
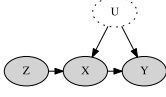
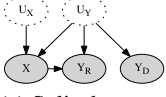
Graphical model	Description	Untestable assumptions	Limitations	Recommendations example
 <p>(a) Back-door criterion</p>	Condition on observed confounders W to isolate the treatment effect.	$X \perp\!\!\!\perp U$ or $Y \perp\!\!\!\perp U$	Unlikely that there are no unobserved confounders U .	Regress click-throughs on product attributes and direct visits to recommended product.
 <p>(b) Instrumental variable</p>	Analyze subset of data that has independent variation in the treatment.	$Z \perp\!\!\!\perp U$ and $Z \perp\!\!\!\perp Y X, U$	Difficult to find a source of exogenous variation in the treatment.	Measure marginal click-throughs on products that experience large, sudden shocks in traffic.
 <p>(c) Split-door criterion</p>	Analyze subset of data where the auxiliary outcome Y_D is independent of the treatment.	$Y_D \not\perp\!\!\!\perp U_Y$	Requires dependency between an auxiliary outcome and all confounders.	Measure marginal click-throughs on all pairs of products that have uncorrelated direct traffic.

FIG. 4. Comparison of methods for estimating the effect of a treatment X on an outcome Y . W and U represent all observed and unobserved confounders, respectively, that commonly cause both X and Y .

subsets of the data where desired independence holds, it also shares some properties with natural experiment methods such as instrumental variables and conditioning methods such as regression. We discuss these connections below; Figure 4 provides a summary for easy comparison.

4.1. *Instrumental variables.* Both the split-door criterion and instrumental variable (IV) methods can be used to exploit naturally occurring variation in subsets of observational data to identify causal effects. Importantly, however, they make different assumptions. In IV methods, one uses an auxiliary variable Z , called an instrument, that is assumed to be exogenous and that systematically shifts the distribution of the cause X . The validity of an instrument relies on two additional assumptions: first that it is effectively random with regard to potential confounders ($Z \perp\!\!\!\perp U$), and second that the instrument affects the outcome Y only through the cause X ($Z \perp\!\!\!\perp Y|X, U$). Both of these conditions involve independence claims between observed and unobserved variables, making them impossible to test in practice [Dunning (2012)].

The split-door criterion also relies on an auxiliary variable, but one that relates to the outcome instead of the treatment. Specifically, it exploits an auxiliary outcome Y_D that serves as a proxy for unobserved common causes U_Y under three important assumptions. The first is that the cause X does not affect Y_D directly. The second assumption requires that all unobserved confounders (between the cause

and outcome) that affect Y_R also affect Y_D . As with IV methods above, these two assumptions involve knowledge of an unobserved variable and, as a result, cannot be tested. The third assumption requires independence between the cause X and the auxiliary outcome Y_D . Since both of these variables are observed, this assumption can be tested empirically so long as we are in the standard setting where statistical independence implies causal independence (Assumption 2), equivalent to the assumption of *faithfulness* [Spirtes, Glymour and Scheines (2000)].

It is difficult to compare these two sets of assumptions in general, but in different scenarios, one of these methods may be more suitable than the other. If a valid instrument is known to exist, for instance through changes in weather or as a result of a lottery, the variation it produces can and should be exploited to identify causal effects of interest. The split-door criterion, in contrast, is most useful when one suspects there is random variation in the data, but cannot identify its source a priori. In particular, it is well suited for large-scale data where the first two assumptions mentioned above are plausible, such as in digital or online systems.

4.2. *Back-door criterion.* Alternatively, the split-door criterion can be interpreted as using Y_D as a proxy for all confounders U_Y , and estimating the causal effect whenever Y_D (and hence U_Y) is independent of X . Viewed this way, the split-door approach may appear to be nothing more than a variant of the back-door criterion where one conditions on Y_D instead of U_Y , however there are two key differences between the two methods.

First, substituting Y_D for U_Y in the back-door criterion assumes that Y_D is a perfect proxy for U_Y . This is a much stronger assumption than requiring that Y_D be simply affected by U_Y , because any difference (e.g., measurement error) between Y_D and U_Y can invalidate the back-door criterion [Spirtes, Glymour and Scheines (2000)]. Second, the two methods differ in their approach to identification. The split-door criterion *controls* for the effect of unobserved confounders by finding subsets of data where X is not affected by U_Y , whereas the back-door criterion *conditions* on a proxy for U_Y to nullify the effect of unobserved confounders. Therefore, by directly controlling at the time of data selection, the split-door criterion focuses on admitting a subset of the data for analysis and simplifies effect estimation, whereas methods based on back-door criterion such as regression, matching, and stratification process the whole dataset and extract estimates via statistical models [Morgan and Winship (2014)].

To illustrate these differences, we compare mathematical forms of the split-door and back-door criteria in terms of regression equations. Conditioning on Y_D using regression will lead to the following equation

$$y_r = \rho''x + \beta y_d + \epsilon''_{y_r},$$

applied to the entire dataset. In contrast the split-door criterion leads to the simpler equation (as shown earlier in Section 2.2)

$$y_r = \rho x + \epsilon'_{y_r},$$

applied only to subsets of data where X and Y_D are independent.

4.3. *Methods based on empirical independence tests.* Finally, the split-door criterion is similar to recent work that proposes a data-driven method for determining the appropriate window size in regression discontinuity designs [Cattaneo, Frandsen and Titiunik (2015), Cattaneo, Titiunik and Vazquez-Bare (2017)]. In regression discontinuities, treatment (e.g., acceptance into a program) is assigned based on whether an observed variable (e.g., a test score) is above or below a pre-determined cutoff. The assumption is that one can compare outcomes for those just above and just below the cutoff to estimate causal effects, but the central problem is how far from the cutoff this assumption holds. The authors present a data-driven method for selecting a window by testing for independence between the treatment and pre-determined covariates that are uncoupled to the outcome of interest. This approach resembles the split-door criterion in that both use independence tests to determine which subsets of the data to include when making a causal estimate. As a result, both methods are subject to concerns around multiple hypothesis testing, although the regression discontinuity setting typically involves many fewer comparisons than the split-door criterion (dozens instead of the thousands we analyze here) and occurs over nested windows. For these reasons we treat multiple comparisons differently, estimating the error rate in identifying independent instances instead of adjusting nominal thresholds to try to eliminate errors.

5. Application: Impact of a recommender system. We now apply the split-door criterion to the problem of estimating the causal impact of Amazon.com's recommender system. Recommender systems have become ubiquitous in online settings, providing suggestions for what to buy, watch, read, or do next [Ricci, Rokach and Shapira (2011)]. Figure 5 shows an example of one of the millions of product pages on Amazon.com, where the main item listed on the page, or *focal product*, is the book "Purity" by Jonathan Franzen. Listed alongside this item are a few *recommended products*—two written by Franzen and one by another author—suggested by Amazon as potentially of interest to a user looking for "Purity." Generating and maintaining these recommendations takes considerable resources, and so a natural question one might ask is how exactly exposure to these recommended products changes consumer activity.

While simple to state, this question is difficult to answer because it requires an estimate of the counterfactual of what would have happened had someone visited a focal product but had not been exposed to any recommendations. Specifically, we would like to know how much traffic recommender systems *cause*, over and above what would have happened in their absence. Naively one could assume that users would not have viewed these other products without the recommender system, and as a result simply compute the observed click-through rate on recommendations [Grau (2009), Mulpuru (2006)]. As discussed earlier, however, this assumption ignores correlated demand: users might have found their way to some of these recommended products anyway via direct search or browsing, which we collectively refer to as "direct traffic." For instance, some users who are interested



FIG. 5. Screenshot of a focal product, the book “Purity,” and its recommendations on Amazon.com.

in the book “Purity” might be fans of Franzen in general, and so might have directly searched on Amazon.com for his other works such as “Freedom” or “The Corrections,” even if they had not been shown recommendations linking to them. The key to properly estimating the causal impact of the recommender, then, lies in accounting for this correlated demand between a focal product and its recommendations.

In this section we show how the split-door criterion can be used to eliminate the issue of correlated demand by automatically identifying and analyzing instances where demand for a product and one (or more) of its recommendations are independent over some time period τ . We do so by first formalizing this problem through a causal graphical model of recommender system traffic, revealing a structure amenable to the split-door criterion. Then we apply the criterion to a large-scale dataset of web browsing activity on Amazon.com to discover thousands of instances satisfying the criterion. Our results show that a naive observational estimate of the impact of this recommender system overstates the causal impact on the products analyzed by a factor of at least two. We conclude with a number of robustness checks and comments on the validity and generalizability of our results.

5.1. *Building the causal model.* The above discussion highlights that unobserved common demand for both a focal product and its recommendations can introduce bias in naive estimates of the causal click-through rate (CTR) on recommendations. Referring back to Figure 2(a), we formalize the problem as follows, with variables aggregated for each day:

- X denotes the number of visits to the focal product i 's webpage.
- Y_R denotes recommendation visits, the number of visits to the recommended product j through clicks on the recommendation for product j on product i 's webpage.

- Y_D denotes direct visits, the number of visits to product j that did not occur through clicking on a recommendation. These could be visits to j from Amazon's search page or through direct visits to j 's webpage.
- U_Y denotes unobserved demand for product j , including both recommendation click-throughs and direct visits.
- U_X represents the part of unobserved demand for product i that is independent of U_Y .

To apply the split-door criterion, we must investigate the plausibility of the *connectedness* and *independence* assumptions from Section 2.1. First, the connectedness assumption states that both Y_R and Y_D are affected (possibly differently) by the same components of demand U_Y for the product j . As mentioned above, connectedness is especially plausible in the context of online recommender systems where products are easily reachable through multiple channels (e.g., search, direct navigation, or recommendation click-through) and it is unlikely that demand for a product manifests itself exclusively through only one of these channels. Specifically, it is unlikely that there exists a component of demand for a product that manifests itself only through indirect recommendation click-throughs, but not through direct visits. Put another way, for connectedness not to hold, it would have to be the case that users would have demand for a product *only* if they arrived via a recommendation link, but not through other means. To the best of our knowledge no path-specific feature of this sort exists on Amazon; thus, we expect the connectedness assumption to hold.

Second, with respect to the independence assumption, although we cannot rule out coincidental cancellation of effects that result in $X \perp\!\!\!\perp Y_D$ and violate the assumption, we expect such events to be unlikely over a large number of product pairs. Furthermore, for complementary product recommendations (which are the focus of this paper), we can logically rule out violation of the independence assumption because the demand for two complementary products are expected to be positively correlated with each other. Therefore, it is reasonable to assume that the unobserved demand U_Y (and all its sub-components) affect both X and Y_D in the same direction. For instance, let the effect of U_Y be increasing for both X and Y_D . Then the independence assumption is satisfied because the effect of U_Y cannot be canceled out on the path $X \leftarrow U_Y \rightarrow Y_D$ if the effects of U_Y (and any of its sub-components) on X and Y_D are all positive. Given the above assumptions, the same reasoning from Section 2.1 allows us to establish that $X \perp\!\!\!\perp Y_D$ is a sufficient condition for causal identification.

5.2. Browsing data. Estimating the causal impact of Amazon.com's recommender system requires fine-grained data detailing activity on the site. To obtain such information, we turn to anonymized browsing logs from users who installed the Bing Toolbar and consented to provide their anonymized browsing data through it. These logs cover a period of nine months from September 2013 to May

2014 and contain a session identifier, an anonymous user identifier, and a time-stamped sequence of all nonsecure URLs that the user visited in that session. We restrict our attention to browsing sessions on Amazon.com, which leaves us with 23.4 million page visits by 2.1 million users spanning 1.3 million unique products. Of these products, we examine those that receive a minimum of 10 page visits on at least one day in this time period, resulting in roughly 22,000 focal products of interest.

Amazon shows many kinds of recommendations on its site. We limit our analysis to the “Customers who bought this also bought” recommendations depicted in Figure 5, as these recommendations are the most common and are shown on product pages from all product categories. To apply the split-door criterion, we need to identify focal product and recommended product pairs from the log data and separate out traffic for recommended products into direct (Y_D) and recommended (Y_R) visits. Fortunately it happens to be the case that Amazon makes this identification possible by explicitly embedding this information in their URLs. Specifically, given a URL for an Amazon.com page visit, we can use the `ref`, or referrer, parameter in the URL to determine if a user arrived at a page by clicking on a recommendation or by other means. We then use the sequence of page visits in a session to identify focal and recommended product pairs by looking for focal product visits that precede recommendation visits. Further details about the toolbar dataset and construction of focal and recommended product pairs can be found in past work [Sharma, Hofman and Watts (2015)].

5.3. Applying the split-door criterion. Having argued for the assumptions underlying the split-door criterion and extracted the relevant data from browsing logs, the final step in estimating the causal effect of Amazon.com’s recommendation system is to use the criterion to search for instances where a product and its recommendation have uncorrelated demand.

Recalling Section 3, we employ a randomization test to search for 15-day time periods that fail to reject the null hypothesis that direct visits to a product and one (or more) of its recommended products are independent. The choice of 15 days represents a trade-off between two requirements: first, a time period large enough to yield reliable estimates; and second, a time period short enough that Amazon’s recommendations for any given product are unlikely to have changed within that window.

The full application of the split-door criterion is as follows. For each focal product i and each $\tau = 15$ day time period:

1. Compute $X^{(i)}$, the number of visits to the focal product on each day, and $Y_R^{(ij)}$, the number of click-throughs to each recommended product j . Also record the total direct visits $Y_D^{(j)}$ to each recommended product j .

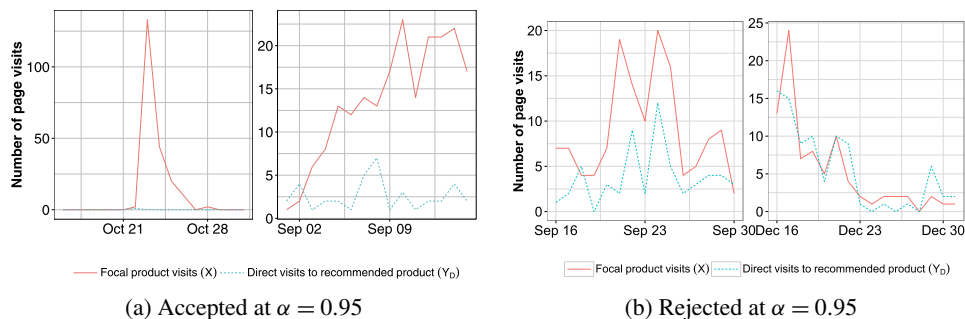


FIG. 6. Examples of time series for focal and recommended products that are (a) accepted or (b) rejected by the split-door criterion at a significance level of $\alpha = 0.95$ for the independence test.

2. For each recommended product j , use the randomization test from Section 3.1 to determine if $X^{(i)}$ is independent of $Y_D^{(j)}$ at a pre-specified significance level.³
 - If $X^{(i)}$ is found to be independent of $Y_D^{(j)}$, compute the observed click-through rate (CTR), $\hat{\rho}_{ij\tau} = (\sum_{t=1}^{\tau} Y_R^{(ij)}) / (\sum_{t=1}^{\tau} X^{(i)})$, as the causal estimate of the CTR. Otherwise ignore this product pair.
3. Aggregate the causal CTR estimate over all recommended products to compute the total causal CTR per focal product, $\hat{\rho}_{i\tau}$.

Finally, average the causal CTR estimate over all time periods and focal products to arrive at the mean causal effect, $\hat{\rho}$, and compute the rate of erroneous split-door instances ϕ to estimate error in this estimate, as detailed in Appendices A and C.

5.4. *Results.* Applying the above algorithm results in over 114,000 potential split-door instances, where each instance consists of a pair of focal and recommended product over a 15-day time period. At a significance level of $\alpha = 0.95$, we obtain more than 7000 instances that satisfy the split-door criterion. Consistent with previous work [Sharma, Hofman and Watts (2015)], the corresponding causal CTR estimate $\hat{\rho}$ is 2.6% (with the error bars spanning 2.0% to 2.7%), roughly one quarter of the naive observational estimate of 9.6% arrived at by computing the click-through rate across all focal and recommended product pairs. Put another way, these results imply that nearly 75% of page visits generated via recommendation click-throughs would likely occur in the absence of recommendations.

Figure 6(a) shows examples of product pairs that are accepted by the test at $\alpha = 0.95$. The example on the left shows a focal product that receives a large and sudden shock in page visits, while direct visits to its recommended product remains

³Here we filter out any time periods where Y_D is exactly constant (because that will satisfy empirical independence conditions trivially).

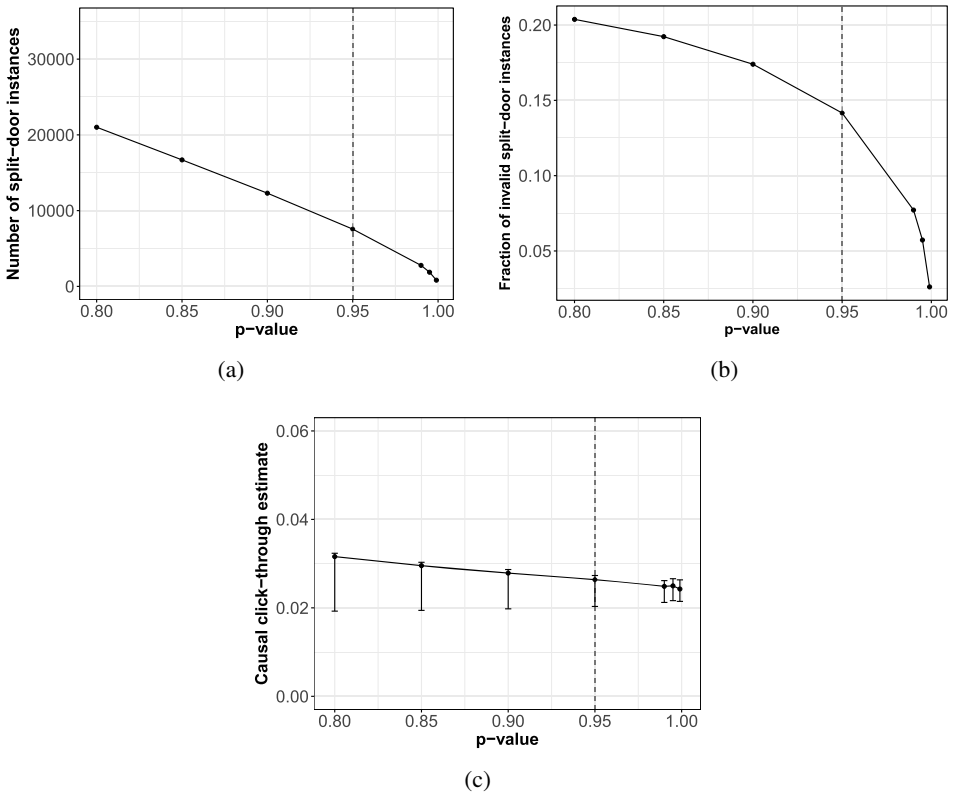


FIG. 7. Subplot (a) shows the number of valid split-door instances obtained as the p -value threshold (α) is increased. Subplot (b) shows the expected fraction of erroneous instances (ϕ) returned by the method for those values of α . The corresponding estimate for causal CTR is shown in Subplot (c); error bars account for both bias due to ϕ and natural variance in the mean estimate.

relatively flat. This is reminiscent of the examples analyzed in Carmi, Oestreicher-Singer and Sundararajan (2012) and Sharma, Hofman and Watts (2015). The example on the right, however, shows more general patterns that are accepted under the split-door criterion but not considered by these previous approaches: although direct visits to both the focal and recommended products vary substantially, they do so independently, and so are still useful in our estimate of the recommender's effect. Conversely, two example product pairs that are rejected by the test are shown in Figure 6(b). As is visually apparent, visit patterns for each of the focal and recommended product pairs are highly correlated, and therefore not useful in our analysis.

Changing the nominal p -value threshold used in the independence test allows us to explore a tradeoff between coverage across products in our dataset and the precision of our causal estimate. As detailed in Appendix A, a lower threshold results in more discovered instances, but with a higher likelihood of these instances being invalid. For instance, Figures 7(a) and (b) show that decreasing the threshold to

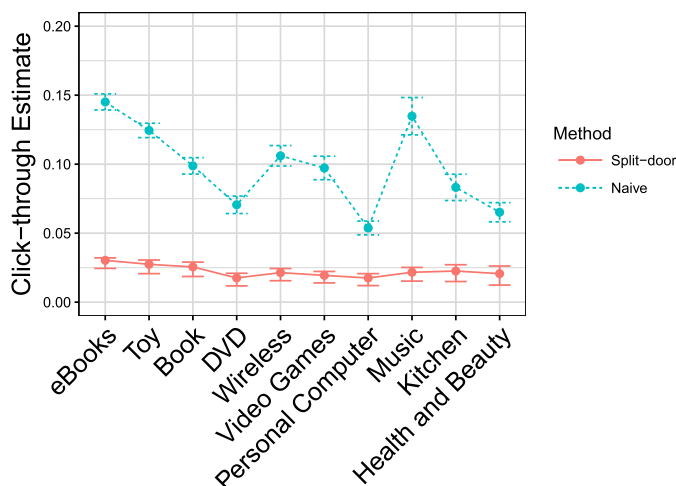


FIG. 8. Comparison of the causal CTR with the naive observational CTR for products that satisfy the split-door criterion. Categories are ordered by the number of products found by the split-door criterion in each category, with eBooks containing the most and Health and Beauty the least.

$\alpha = 0.80$ results in over 20,000 split-door instances covering nearly 11,000 unique focal products, but does so at the expense of increasing the expected fraction of invalid instances to 0.21, indicating that approximately one in five of the returned split-door instances may be invalid. The result, summarized in Figure 7(c), is that the error bars on our estimate of ρ increase as we decrease α . These error bars, calculated using equation (C.4) from Appendix C, account for both bias due to erroneous split-door instances and the natural variance in the mean estimate due to sampling.⁴ As α decreases, erroneous instances due to ϕ contribute to most of the magnitude of the error bars shown in Figure 7(c). We observe that $\alpha = 0.95$ offers a good compromise: error bounds are within 1 percentage point and we obtain more than 7000 split-door instances.

Furthermore, we can break these estimates down by the different product categories present on Amazon.com. Figure 8 shows the variation of $\hat{\rho}$ across the most popular categories, at a nominal significance level of $\alpha = 0.95$. For the set of focal products that satisfy the split-door criterion, we also compute the naive observational CTR. We see substantial variation in the naive estimate, ranging from 14% on *e-Books* to 5% on *Personal Computer*. However, when we use the split-door criterion to compute estimates, we find that the causal CTR for all product categories lies below 5%. These results indicate that naive observational estimates

⁴Note that the error bars are asymmetric; we expect erroneous split-door instances to drive the causal estimate up from its true value, under the assumption that demand for the two products are positively correlated with each other, as argued in Section 5.1.

overstate the causal impact by anywhere from two- to five-fold across different product categories.

There are two clear advantages to the split-door criterion compared to past approaches for estimating the causal impact of recommender systems. First, we are able to study a larger fraction of products compared to instrumental variable approaches that depend on single-source variations [Carmi, Oestreicher-Singer and Sundararajan (2012)] or restricting our attention to mining only shocks in observational data [Sharma, Hofman and Watts (2015)]. On the same dataset, the shock-based method in Sharma, Hofman and Watts (2015) identified valid instances on 4000 unique focal products, while the split-door criterion finds instances for over 5000 unique focal products at $\alpha = 0.95$, and over 11,000 at $\alpha = 0.80$. Second, the split-door criterion provides a principled method to select valid instances for analysis by tuning α , the desired significance level, while also allowing for an estimate of the fraction of falsely accepted instances, ϕ .

5.5. Threats to validity. As with any observational analysis, our results rely on certain assumptions that may be violated in practice. Furthermore, results obtained on a subset of data may not be representative of the broader dataset of interest. Here we conduct additional analyses to assess both the internal and external validity of our estimate of the causal effect of Amazon's recommendations.

5.5.1. Internal validity: Sensitivity to the connectedness assumption. As described in Section 3.3, connectedness is the key identifying assumption for the split-door criterion. Here we describe a test for sensitivity of the obtained estimate ($\hat{\rho}$) to violations of the connectedness assumption.

Referring to the causal model in Figure 3(a), violation of the connectedness assumption implies that there exist components of unobserved demand V_Y that affect both focal product visits X and recommendation click-throughs Y_R , but not direct visits to the recommended product Y_D . For simplicity, let us assume that V_Y is univariate normal and affects both X and Y_R linearly. We can write the corresponding structural equations for the causal model in Figure 3(b) for each split-door instance as

$$(5.1) \quad x = c_1 v_y + \epsilon_1,$$

$$(5.2) \quad y_r = f(x) + c_2 v_y + \epsilon_2,$$

where f is an unknown function, and ϵ_1 and ϵ_2 are independent from all variables mentioned above and are also mutually independent. Note that ϵ_1 includes the effect of U_X and ϵ_2 includes the effect of U_Y . For any split-door instance, the estimator from Section 5.3 estimates the causal effect assuming that either c_1 or c_2 is zero.

To test the sensitivity of our estimate to the connectedness assumption, we take our actual data and introduce an artificial confound V_Y by simulation, adding $c_1 V_Y$

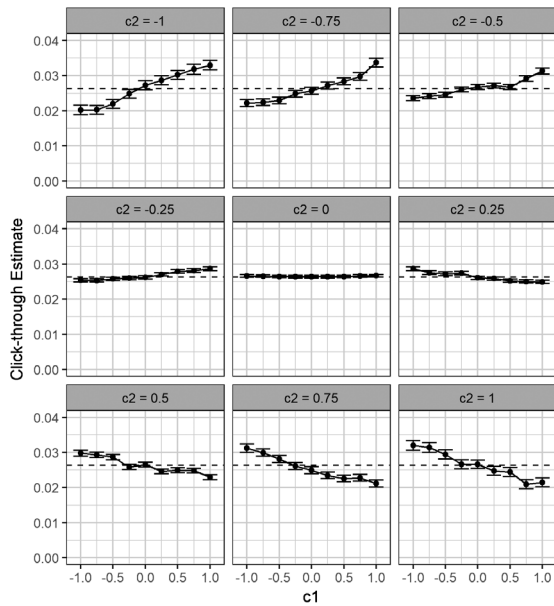
to X and $c_2 V_Y$ to Y_R , respectively, for a range of different c_1 and c_2 values. We simulate V_Y as a standard normal and vary c_1 and c_2 between $[-1, 1]$, and compare these artificially confounded estimates to our actual estimate of $\hat{\rho} = 2.6\%$ for $\alpha = 0.95$. Figure 9(a) shows the deviation between estimates using the actual and simulated data as c_1 and c_2 vary. The difference is maximized when both c_1 and c_2 are high in magnitude and is negligible when either of c_1 or c_2 are zero. These simulation results suggest a bilinear sensitivity to c_1 and c_2 , a result we confirm theoretically in the case of a linear causal model in Appendix B.

This analysis assumes that *all* split-door instances violate the connectedness assumption. Recognizing that this need not be the case, and that only some instances may be invalid, we introduce a third sensitivity parameter κ , which corresponds to the fraction of split-door instances that violate connectedness. For instance, we can test sensitivity of the estimate when at least half of the split-door instances satisfy connectedness, as done by Kang et al. (2016) for inference under multiple possibly invalid instrumental variables. As shown in Figure 9(b), when $\kappa = 0.5$ deviations from the obtained split-door estimate are nearly halved, resulting in more robust estimates.

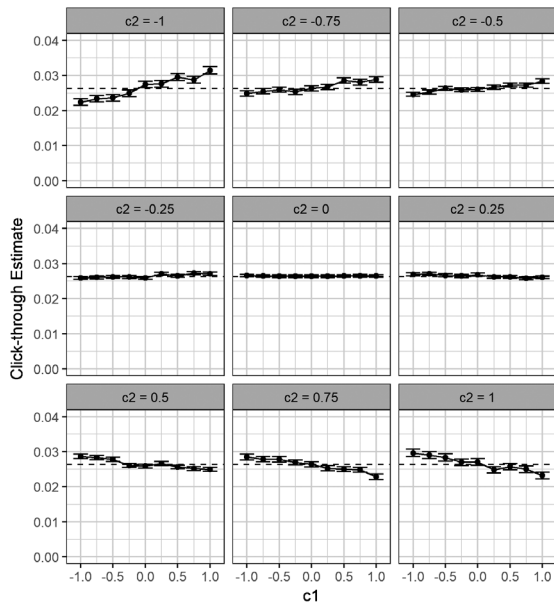
5.5.2. External validity: Generalizability. Although the split-door criterion yields valid estimates of the causal impact of recommendations for the time periods where product pairs are found to be statistically independent, it is important to emphasize that products in the split-door sample may not be selected at random, thus violating the *as-if-random* [Angrist, Imbens and Rubin (1996)] assumption powering generalizability for natural experiments. As a result, care must be taken to extrapolate these estimates to all products on Amazon.com.

Fortunately, as shown in Figure 10, the distribution of products and page visits in our sample closely matches the inventory and activity on Amazon.com. Products with at least one valid split-door time period span many product categories and cover nearly a quarter of all focal products in the dataset at $\alpha = 0.95$. Figure 10(a) shows that the distribution of products analyzed by the split-door criterion across different product categories is almost identical to the overall set of products. Figure 10(b) shows a similar result for the number of page visits of these products across different product categories, except for eBooks which are over-represented in valid split-door instances. For comparison, we apply the same popularity filter that we used for the split-door criterion—at least 10 page visits on at least one day—to the dataset with all products.

Although these results do not necessarily imply that the *as-if-random* assumption is satisfied (indeed it is very likely not satisfied) they do indicate that the split-door criterion at least allows us to estimate causal effects over a diverse sample of popular product categories, which is a clear improvement over past work [Carmi, Oestreicher-Singer and Sundararajan (2012), Sharma, Hofman and Watts (2015)].



(a)



(b)

FIG. 9. Sensitivity analysis of the obtained click-through estimate. Panel (a) shows the scenario where all split-door instances may be invalid. Panel (b) assumes that at most half of the instances are invalid. The deviation of the true estimate from the obtained estimate increases as the magnitude of the confounding from V_Y to $X(c_1)$ or $Y(c_2)$ increases; however, this deviation is lower in Panel (b). In both figures, dotted line shows the obtained CTR estimate.

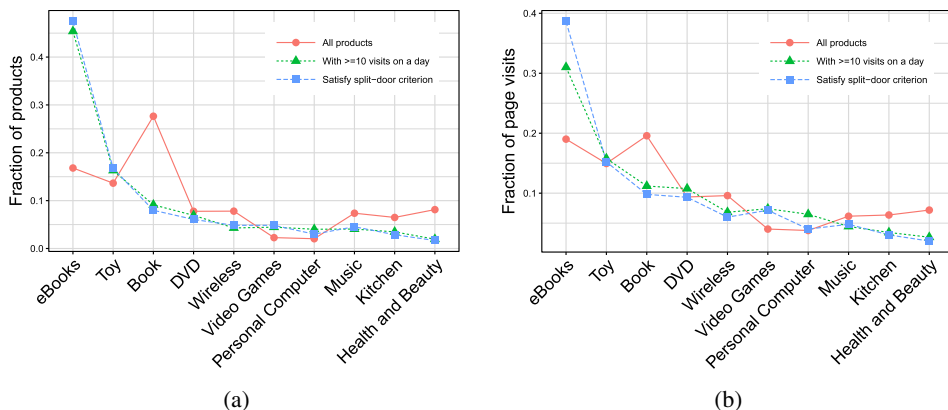


FIG. 10. The distribution of products and total visits over product categories. Among products with at least 10 page visits on at least one day, the subset of focal products that satisfy the split-door criterion are nearly identical to the set of all products. Fraction of page visits to those focal products show more variation, but the overall distributions are similar.

6. Discussion. In this paper we have presented a method for computing the causal effect of a variable X on another variable Y_R whenever we have an additional variable Y_D which follows some testable conditions, and have shown its application in estimating the causal impact of a recommender system. We now suggest guidelines to ensure proper use of the criterion and discuss other applications for which it might be used.

6.1. *Guidelines for using the criterion.* As with any nonexperimental method for causal inference, the split-door criterion rests on various untestable assumptions and requires making certain modeling choices. We encourage researchers to reason carefully about these assumptions, explore sensitivity to modeling choices, and examine threats to the validity of their results.

6.1.1. *Reason about assumptions.* The split-door criterion relies on two untestable assumptions: *independence* (of X and Y_D), and *connectedness* (i.e., nonzero causal effect of U_Y on Y_D). The independence assumption is a standard assumption for observational causal inference. Barring coincidental equality of parameters such that the effect of unobserved confounders on X and Y_D cancel out, the independence assumption is likely to be satisfied. Nonetheless we encourage researchers to think carefully about this assumption in applying the criterion in other domains. Depending on the application it may be possible to rule out such cancellations. For example, in our recommendation system study we expect demand for the focal and recommended product to be correlated. Therefore, the causal effect of demand on both products is expected to be directionally identical, and hence cancellation becomes impossible.

The connectedness assumption is potentially more restrictive. In general, it is plausible whenever measurements Y_R and Y_D are additive components of the same tangible outcome Y that can be reached by similar means. That said, connectedness remains an untestable assumption where, once again, domain knowledge should be used to assess its plausibility. For instance, even when Y_R and Y_D are additive components, in some isolated cases, U_Y may not be connected to Y_D at all. In a recommender system this can happen when customers with pre-existing interest in a product somehow visit it only through recommendation click-throughs from other products. In such a scenario, the split-door criterion would be invalid. We note, however, that this situation can arise only in the (unlikely) event that no such user found the product directly. When there is even a small number of users that visit the product directly, the split-door criterion will again be valid and, depending on the precision of the statistical independence condition, can be applied.

6.1.2. *Explore sensitivity to test parameters.* A key advantage of the split-door criterion is that once these two assumptions are met, it reduces the problem of causal identification to that of implementing a test for statistical independence. At the same time, this requires choosing a suitable statistical test and deciding on any free parameters the test may have. For instance, in the case of the randomization test used here, there is a significance level α used to determine when to accept or reject focal and recommended product pairs as statistically independent. Any such parameters should be varied to check the sensitivity of estimates to these choices, as in Figures 7(b) and (c).

6.1.3. *Examine threats to validity.* After identifying and estimating the effect of interest, one should examine both the internal and external validity of the resulting estimate. In terms of internal validity, we recommend conducting a sensitivity analysis to assess how results change when the assumptions required for identification are violated. In the case of the recommender system example, we simulated violations of the connectedness assumption by artificially adding correlated noise to X and Y_R (but not Y_D) and re-ran the split-door method to look at variation in results, as shown in Figure 9.

Finally, after establishing internal validity, one needs to consider how useful the resulting estimate is for practical applications. As remarked earlier and demonstrated in our recommender system application, the split-door criterion is capable of capturing the local average causal effect for a large sample of the dataset that satisfies the required independence assumption ($X \perp\!\!\!\perp Y_D$). The argument has been made that such local estimates are indeed useful in themselves [Imbens (2010)]. That said, the sample may not be representative of the entire population, and so one must always be careful to qualify an extension of the split-door estimate to the general population. Naturally, the more instances discovered by the method, the more likely the estimate is to be of general use. Additionally, we recommend

that researchers perform checks similar to those in Figure 10 to compare the distribution of any available covariates to check for differences between the general population and instances that pass the split-door criterion.

6.2. Potential applications of the split-door criterion. The key requirement of the split-door criterion is that the outcome variable must comprise two distinct components: one that is potentially affected by the cause, and another that is not directly affected by it. In addition, we should have sufficient reason to believe that the two outcome components share common causes (i.e., the connectedness assumption must be satisfied), and that one of outcome variables can be shown to be independent of the cause variable (i.e., the independence assumption must be satisfied). These might seem like overly restrictive assumptions that limit applicability of the criterion, but in this section we argue that there are in fact many interesting cases where the split-door criterion can be employed.

As we have already noted, recommendation systems such as Amazon's are especially well suited to these conditions, in large part because Y_D has a natural interpretation of "direct traffic," or any traffic that is not caused by a particular recommendation. Likewise the criterion can be easily applied to other online systems that automatically log user visits, such as in estimating the causal effect of advertisements on search engines or websites. Somewhat more broadly, time series data in general may be amenable to the split-door criterion, in part because different components of the outcome occurring at the same time are more likely to be correlated than components that share other characteristics, and in part because time series naturally generate many observations on the input and output variables, which permits convenient testing for independence.

For example, consider the problem of estimating the effect of social media on news consumption. There has been recent interest [Flaxman, Goel and Rao (2016)] in how social media websites such as Facebook impact the news that people read, especially through algorithmic recommendations such as those for "Trending news." Given time series data for user activity on a social media website and article visits from news website logs, we can use the split-door criterion to estimate the effect of social media on news reading. Here Y_R would correspond to the visits that are referred from social media, and Y_D would be all other direct visits to the news article. Most websites record the source of each page visit, so obtaining these two components for the outcome—visits to an article through social media and through other means—should be straightforward. Whenever people's social media usage is not correlated with direct visits to a news article, we can identify the causal effect of social media on news consumption. Similar analysis can be applied to problems such as estimating the effect of online popularity of politicians on campaign financing or the effect of television advertisements on purchases.

Finally, although we have focused on online settings for which highly granular time series data is often collected by default, we note that there is nothing intrinsic

to the split-door criterion that prevents it from being applied offline. For example, many retailers routinely send direct mail advertisements to existing customers whom they identify through loyalty programs. The split-door criterion could easily be used to estimate the causal effect of these advertisements on product purchases: X would be the number of customers that are sent an advertisement; Y_R would be the customers among them who purchased the product; and Y_D would be the number of customers who bought the product without receiving the mailer. More generally, the split-door criterion could be used in any context where the outcome of interest can be differentiated into more than one channel.

7. Conclusion. In closing we note that the split-door criterion is just one example of a more general class of methods that adopt a data-driven approach to causal discovery [Cattaneo, Frandsen and Titiunik (2015), Grosse-Wentrup et al. (2016), Jensen et al. (2008), Sharma, Hofman and Watts (2015)]. As we have discussed, data-driven methods have important advantages over traditional methods for exploiting natural variation—allowing inference to be performed on much larger and more representative samples—while also being less susceptible to unobserved confounders than back-door identification strategies. As the volume and variety of fine-grained data continues to grow, we expect these methods to increase in popularity and to raise numerous questions regarding their theoretical foundations and practical applicability.

APPENDIX A: ESTIMATING THE FRACTION OF ERRONEOUS SPLIT-DOOR INSTANCES

Let the expected fraction of erroneous X - Y_D pairs—split-door instances—returned by the method be ϕ . In the terminology of multiple testing, ϕ refers to the *False Nondiscovery Rate (FNDR)* [Delongchamp et al. (2004)]. This is different from the more commonly used False Discovery Rate (FDR) [Farcomeni (2008)], since we deviate from standard hypothesis testing by looking for split-door instances that have a p -value higher than a predetermined threshold. Given m hypothesis tests and a significance level of α , we show that the false nondiscovery rate ϕ for the split-door criterion can be characterized as

$$(A.1) \quad \phi_\alpha \leq \frac{(1 - \alpha)\pi_{\text{dep}}m}{W_\alpha},$$

where π_{dep} is the fraction of actually dependent X - Y_D instances in the dataset and W_α is the observed number of X - Y_D instances returned by the method at level α .

The above estimate can be derived using the framework proposed by Storey (2002) under two assumptions. The first is that the distribution of p -values under the null hypothesis is uniform, and the second is that the distribution of p -values under the alternative hypothesis is stochastic smaller than the uniform distribution.

Let the number of invalid instances found using the split-door criterion be T . Then, by definition, the false nondiscovery rate can be written as

$$\phi_\alpha = E\left[\frac{T}{W} \mid W > 0\right].$$

Since the alternative distribution is stochastically smaller than uniform, we can arrive at an upper bound by replacing T by the expected number of split-door instances if the alternative distribution were uniform, $(1 - \alpha) * m_{\text{dependent}} = (1 - \alpha) * \pi_{\text{dep}} * m$, giving

$$(A.2) \quad \phi_\alpha \leq \frac{(1 - \alpha) * \pi_{\text{dep}} m}{W_\alpha}.$$

Here π_{dep} is unknown, so it needs to be estimated. A common approach is to estimate the fraction of actually independent instances or null hypotheses π_{indep} and then use $\pi_{\text{dep}} = 1 - \pi_{\text{indep}}$ [DeLongchamp et al. (2004)]. For robustness, we suggest using multiple procedures to estimate π_{indep} and verify sensitivity of results to the choice of π_{indep} . In this paper, we use two different estimates, derived from Storey and Tibshirani (2003), Storey (2002) (Storey's estimate); and Nettleton et al. (2006), Liang and Nettleton (2012) (Nettleton's estimate).

Storey's estimate is defined as

$$(A.3) \quad \hat{\pi}_{\text{indep}} = \frac{W_\lambda}{m(1 - \lambda)},$$

where $\lambda \in [0, 1)$ is a tunable parameter—similar in interpretation to α —and W_λ is the number of hypothesis tests having a p -value higher than λ . The choice of λ involves a bias-variance tradeoff, with $\lambda = 0.5$ being a common choice, as in the SAM software developed by Storey and Tibshirani (2003).

Nettleton's estimate, on the other hand, chooses the effective value of λ adaptively, based on the observed p -value distribution. First, the p -value distribution is summarized in a histogram containing B bins. Then, a threshold λ is chosen as the index (I) corresponding to the left-most bin whose count fails to exceed the average count of the bins to its right. This results in the following estimate, where $\lambda = (I - 1)/B$:

$$(A.4) \quad \hat{\pi}_{\text{indep}} = \frac{W_\lambda}{m(1 - \lambda)} = \frac{W_\lambda}{m(1 - \frac{I-1}{B})}.$$

Applying each of these to the $m = 114,469$ focal and recommended product pairs analyzed in Section 5 allows us to estimate the true number of dependent X - Y_D pairs in the dataset, π_{dep} . At $\alpha = 0.95$, both methods give very similar results ($\pi_{\text{dep,Storey}} = 0.184$, $\pi_{\text{dep,Nettleton}} = 0.187$); we use $\pi_{\text{dep}} = 0.187$ in our analysis.

APPENDIX B: SENSITIVITY ANALYSIS FOR THE CONNECTEDNESS ASSUMPTION

In this section we analyze the sensitivity of an estimate obtained using the split-door criterion to violations of the connectedness assumption. As Figure 3(a) shows, violation implies that there exist variables V_Y that affect only X and Y_R but not Y_D . We use the structural equation model from Section 2.2 to illustrate sensitivity analysis.

Given that the unobserved confounders can be broken down into two components U_Y and V_Y , we can rewrite the linear structural equations from equation (2.8) as

$$(B.1) \quad x = \eta u_x + \gamma_1 u_y + c_1 v_y + \epsilon_x,$$

$$(B.2) \quad y_r = \rho x + \gamma_2 u_y + c_2 v_y + \epsilon_{y_r},$$

$$(B.3) \quad y_d = \gamma_3 u_y + \epsilon_{y_d},$$

with two additional parameters c_1 and c_2 denoting the effect of the unobserved variable V_Y on X and Y_R , respectively. Applying the split-door criterion $X \perp\!\!\!\perp Y_D$, we write the following equations for each obtained split-door instance:

$$(B.4) \quad x = \eta u_x + c_1 v_y + \epsilon'_x,$$

$$(B.5) \quad y_r = \rho x + c_2 v_y + \epsilon'_{y_r}.$$

Here V_Y is unobserved and hence the causal effect is not identified. Using (B.5) as an estimating equation will lead to a biased estimate of the causal effect due to the confounding effect of the unobserved common cause V_Y . Note that this structure is identical to the omitted variable bias problem in back-door and conditioning-based methods [Harding (2009)]. Consequently, we obtain a similar bilinear dependence of the split-door estimate to sensitivity parameters c_1 and c_2 .

Specifically, the split-door method regresses Y_R on X to obtain an estimate $\hat{\rho}$ for each obtained instance. When connectedness is violated, the bias of this estimate can be characterized as

$$\begin{aligned} \hat{\rho} &= (X^T X)^{-1} X^T Y_R = \frac{\sum_i x_i y_{r_i}}{\sum_j x_j^2} \\ &= \frac{\sum_i x_i (\rho x_i + c_2 v_{y_i} + \epsilon'_{y_{r_i}})}{\sum_j x_j^2} \\ &= \rho + c_2 \frac{\sum_i v_{y_i} x_i}{\sum_j x_j^2} + \frac{\sum_i \epsilon'_{y_{r_i}} x_i}{\sum_j x_j^2}, \end{aligned}$$

where we use (B.5) to expand y_{r_i} . As in Section 3, let τ denote the sample size for each split-door instance. When X and V_Y are both standardized to have zero mean

and unit variance, and taking expectation on both sides, we obtain

$$\begin{aligned}
 \text{E}[\hat{\rho}] &= \rho + c_2 \text{E}\left[\frac{1}{\tau} \sum_i v_{y_i} x_i\right] + \text{E}\left[\frac{1}{\tau} \sum_i \epsilon'_{y r_i} x_i\right] \\
 &= \rho + c_2 \text{E}\left[\frac{1}{\tau} \sum_i v_{y_i} (\eta u_{x_i} + c_1 v_{y_i} + \epsilon'_{x_i})\right] \\
 \text{(B.6)} \quad &= \rho + c_1 c_2 \text{E}\left[\frac{1}{\tau} \sum_i v_{y_i} v_{y_i}\right] + \eta \text{E}\left[\frac{1}{\tau} \sum_i v_{y_i} u_{x_i}\right] + \text{E}\left[\frac{1}{\tau} \sum_i v_{y_i} \epsilon'_{x_i}\right], \\
 \text{E}[\hat{\rho}] &= \rho + c_1 c_2,
 \end{aligned}$$

where we use the independence of error terms and that $U_X \perp\!\!\!\perp V_Y$.

In addition, note that the split-door method averages the estimate $\hat{\rho}$ obtained from each instance. Not all instances may violate the connectedness assumption, therefore we introduce an additional sensitivity parameter κ that denotes the fraction of invalid split-door instances. Bias in the final split-door estimate is then given by the following equation in the three sensitivity parameters:

$$\text{(B.7)} \quad \text{E}[\hat{\rho}] = \rho + \kappa c_1 c_2.$$

For expositional clarity, the above analysis assumed a linear structural model and demonstrated similarities with sensitivity of conditioning-based methods to unobserved common causes. However, in practice, the structural model may not be linear. In the recommendation example discussed in Section 5, we do not assume a linear model and instead use an aggregate ratio estimator. As shown in Figure 9, simulations show that sensitivity of this estimator follows a similar bilinear dependence on c_1 and c_2 .

APPENDIX C: CHARACTERIZING ERROR IN THE SPLIT-DOOR ESTIMATE FOR A RECOMMENDATION SYSTEM

In Section 5.3, the split-door causal estimate is defined as the mean of CTR estimates over all time periods and focal products with valid split-door instances. Here we characterize the error in this estimate. The key idea is that the error comes from two components: the first due to some erroneously identified split-door instances, and the second due to natural variance in estimating the mean. For a significance level α of the independence test, let W be the number of obtained split-door instances and N be the number of aggregated CTR estimates $\hat{\rho}_{i\tau}$ computed from these instances. Then the mean estimate can be written as

$$\text{(C.1)} \quad \hat{\rho} = \frac{\sum_{i\tau} \hat{\rho}_{i\tau}}{N},$$

where i refers to a focal product and τ refers to a split-door time period. As in Appendix A, let ϕ denote the expected fraction of erroneous split-door instances

obtained. That is, for an expected number of ϕW instances, the method may have erroneously concluded that the focal and recommended products are independent. Correspondingly, an expected $\phi W = \phi' N$ number of $\hat{\rho}_{i\tau}$ estimates will be invalid.⁵ These invalid estimates can be expanded as

$$(C.2) \quad \hat{\rho}_{i\tau} = \rho_{i\tau}^{\text{causal}} + \eta_{i\tau},$$

where η refers to the click-through rate due to correlated demand between the focal and recommended products. Thus, the overall mean estimate can be written as

$$\begin{aligned} \hat{\rho} &= \frac{\sum_{i\tau \in A} \rho_{i\tau}^{\text{causal}} + \sum_{i\tau \in B} (\rho_{i\tau}^{\text{causal}} + \eta_{i\tau})}{N} \\ &= \frac{\sum_{i\tau} \rho_{i\tau}^{\text{causal}}}{N} + \frac{\sum_{i\tau \in B} \eta_{i\tau}}{N}, \end{aligned}$$

where A and B refer to (i, τ) pairs with valid and erroneous split-door estimates respectively [$|A| = (1 - \phi')N$, $|B| = \phi'N$].

Comparing this to the true ρ^{causal} , we obtain

$$(C.3) \quad \rho^{\text{causal}} - \hat{\rho} = (\rho^{\text{causal}} - \bar{\rho}^{\text{causal}}) - \frac{\sum_{i\tau \in B} \eta_{i\tau}}{N}.$$

The first term of the RHS corresponds to error due to sampling variance, and the second term corresponds to error due to correlated demand (ϕ). We estimate these terms below.

Error due to ϕ . Based on the argument for justifying the independence assumption in Section 5.1, let us assume that the total effect of U_Y on Y_R is positive (without stipulating it for each individual instance). This means that the term due to correlated demand is positive, $\sum_{i\tau \in B} \eta_{i\tau} \geq 0$. Further, the maximum value of $\eta_{i\tau}$ is attained when all the observed click-throughs are due to correlated demand ($\eta_{i\tau} = \hat{\rho}_{i\tau}$). Under this assumption,

$$0 \leq \frac{\sum_{i\tau \in B} \eta_{i\tau}}{N} \leq \frac{\rho_{\text{maxsum}}}{N},$$

where ρ_{maxsum} corresponds to the maximum sum of any subset of $\phi'N \hat{\rho}_{i\tau}$ values. An approximate estimate can be derived using $\hat{\rho}$ —the empirical mean over all N values of $\rho_{i\tau}$ —leading to $\rho_{\text{maxsum}} \approx \phi'N \hat{\rho}$.

Error due to natural variance. We characterize this error by the 99% confidence interval for the mean estimate, given by $2.58 * \frac{\hat{\sigma}}{\sqrt{N}}$, where $\hat{\sigma}$ is the empirical standard deviation.

⁵In general, the expected number of invalid $\hat{\rho}_{i\tau}$ estimates may be less than or equal to ϕW , since a focal product may have more than one recommended product that corresponds to an invalid split-door instance.

Combining these two, the resultant interval for the split-door estimate is

$$(C.4) \quad \left(\hat{\rho} - \frac{\rho_{\max\text{sum}}}{N} - 2.58 \frac{\hat{\sigma}}{\sqrt{N}}, \hat{\rho} + 2.58 \frac{\hat{\sigma}}{\sqrt{N}} \right).$$

The above interval demonstrates the bias-variance tradeoff in choosing a nominal significance level for the independence test and the corresponding ϕ . At high nominal significance level α , bias due to ϕ is expected to be low but variance of the estimate may be high due to low N . Conversely, at low values of α , variance will be lower but ϕ is expected to be higher because we accept many more split-door instances.

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SUPPLEMENTARY MATERIAL

Code for split-door criterion (DOI: [10.1214/18-AOAS1179SUPP](https://doi.org/10.1214/18-AOAS1179SUPP); .zip). We provide an R package that implements the split-door criterion, along with code samples for applying the criterion to new applications.

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