Split-door criterion for causal identification:
Automatic search for natural experiments

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Abstract

Unobserved or unknown confounders complicate even the simplest attempts to estimate the effect of one variable on another using observational data. When cause and effect are both affected by unobserved confounders, methods based on identifying natural experiments have been proposed to eliminate confounds. However, their validity is hard to verify because they depend on assumptions about the independence of variables, that by definition, cannot be measured. In this paper we investigate a particular scenario in time series data that permits causal identification in the presence of unobserved confounders and present an algorithm to automatically find such scenarios. Specifically, we examine what we call the split-door setting, when the effect variable can be split up into two parts: one that is potentially affected by the cause, and another that is independent of it. We show that when both of these variables are caused by the same (unobserved) confounders, the problem of identification reduces to that of testing for independence among observed variables. We discuss various situations in which split-door variables are commonly recorded in both online and offline settings, and demonstrate the method by estimating the causal impact of Amazon’s recommender system, obtaining more than 23,000 natural experiments that provide similar—but more precise—estimates than past studies.

Keywords: causal inference, graphical model, recommender system, back-door criterion, data mining

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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 non-experimental settings, however, 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 et al. 2011), correlated demand (Sharma et al. 2015), or other shared causes of both exposure and purchase. Figure 1a 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 (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 & 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.

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 compared to the observed variables (W). Under such a selection on observables assumption (Imbens & Rubin 2015), one can condition 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 1b) and can be implemented by a variety of methods, including regression,
Motivated by the limitations of the back-door strategy, a second main approach is to look for naturally occurring variation in the data 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 proxy 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 such a book is featured on Oprah’s book club (Carmi et al. 2012). Assuming that the increase in traffic for a 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 shock, arguing that these sales would not have happened in the absence of the recommender. Such shocks are known as **instrumental variables** that identify the effect of interest by shifting the distribution of the cause $X$ independently of unobserved confounds $U$ (Angrist et al. 1996). Figure 1c depicts this in a graphical model, where the additional observed variable $Z$ denotes the instrumental variable.

Natural experiments are often preferred to back-door conditioning because, under the right assumptions, they provide valid causal estimates even in the presence of unobserved confounders. Unfortunately, however, these assumptions are often difficult or impossible to test empirically, as they involve independence statements about variables $U$ that, by definition, cannot be measured. For instance in the example above, one cannot directly verify that a sudden burst of interest in a particular book says nothing about the latent demand for its recommendations. Furthermore, because natural experiments are by design restricted to subsets of the data, the resulting estimates are necessarily localized, and hence are subject to the criticism that they fail to generalize to the entire population of interest (Robins & Greenland 1996). Researchers typically use domain knowledge to argue for these assumptions, but such qualitative justifications are far from satisfactory, especially since the validity of causal estimates hinges on them. All of these qualifications make it quite difficult to identify and use valid instruments in practice, and limit the generalizability of results derived from them (Sekhon & Titiunik 2012). In practice, most instrumental variable analyses rely on just a handful of sources for random variation, such as lotteries, the weather or a sudden, large event (Rosenzweig & Wolpin 2000, Dunning 2012), which are applicable in a relatively limited set of domains.

### 1.2 The “split-door” criterion

In this paper we introduce a novel data-driven causal identification strategy that incorporates advantages of both the back-door and instrumental variable approaches: unlike the back-door criterion it applies in the presence of unobserved confounders, and in contrast to instrumental variables its key requirement involves a *testable* independence assumption
Figure 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 $X$. 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).

between observed variables. As a result, its untestable assumptions are weaker than those required by current methods for identifying natural experiments.

The key element that drives this shift in assumptions is the availability of auxiliary outcome data that facilitates causal identification (Mealli & Pacini 2013). Specifically, our strategy applies when the outcome variable $Y$ can be effectively “split” into two constituents: one that is caused by $X$ and the other that is independent of it. Figure 2a 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 automatically identify natural experiments over different subsets of the data and provide a simple, scalable algorithm to estimate the causal effect in each of them. 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.

Intuitively, the split-door criterion works by generalizing the notion of large and sudden variations in $X$ that instrumental variable approaches exploit to break correlations between the cause $X$ and the effect $Y$ due to unobserved confounds $U$. The key advantage it has
over instrumental variable approaches is that instead of having to argue for this condition rhetorically, we can use the auxiliary outcome $Y_D$ to directly select for cases where there are no confounds (observed or otherwise) between $X$ and $Y_R$. Specifically, we show that given a statistical test to establish if $X$ and $Y_D$ are independent, the causal effect between $X$ and $Y_R$ can be identified as long as there is some common cause driving $Y_R$ and $Y_D$. 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 and therefore automatically discover a large number of natural experiments.

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 and give precise conditions under which the split-door criterion holds. For clarity we provide proofs for causal identification both in terms of the causal graphical model above and also in terms of structural equations. In section 2.3 we propose a simple, scalable algorithm for identifying causal effects using the split-door criterion. Then in section 2.4 we explain more formally how the split-door criterion differs from the instrumental variables and back-door methods mentioned above. Next, in section 3 we illustrate the utility of the split-door with a detailed example in which we estimate the causal impact of Amazon.com’s recommendation system using historical web browsing data. Consistent with previous work that employs a shock-based instrumental variables approach (Sharma et al. 2015), we find that observational estimates of recommendation click-through rates (CTRs) overstate the actual effect by at least two-fold. The split-door criterion identifies roughly three times as many natural experiments (approximately 20,000) in the same data as the shock-based method, improving both the precision and generalizability of estimates, and also provides a more principled mechanism for determining which natural experiments to include in the analysis. In section 4 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. Finally, in section 5
we conclude with a prediction that as the size and granularity of available datasets, and the number of variables in them, increase at an ever faster rate, data-driven approaches to identifying natural experiments will become commonplace.

2 The Split-door Identification Criterion

The split-door criterion can be used whenever the observed data is generated from the model shown in Figure 2a. Here $X$ represents the cause of interest, $Y_R$ denotes the “referred” portion of the 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 $X$, thus 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.) The unobserved variables $U_Y$ make it difficult to estimate the causal effect of $X$ on $Y$; that is, they create “back-door pathways” that confound the relationship of interest, resulting in biased estimates of the causal effect.

One way to remove this confound is to experimentally manipulate $X$ and record the corresponding change in $Y$; this would correspond to the interventional causal model shown in Figure 2b. However, in some cases, there can also be a natural variation in the observed data that changes the causal model to Figure 2b. Such a natural, local variation provides a natural experiment that can be used to estimate the same causal effect. Using the auxiliary outcome $Y_D$, the split-door criterion allows a systematic search for natural experiments in the observed data. As we show below, if we can identify scenarios—subsets of the full dataset—in which $X$ is independent of $Y_D$, then we can establish that $U_Y$ is either constant or in fact has no causal effect on $X$, removing the potential confound and leading to an unbiased estimate of the effect of $X$ on $Y$. In other words, we can conclude that these subsets of the data were generated from the unconfounded causal model shown in Figure 2b and therefore the causal effect of $X$ on $Y$ can be estimated directly from observed data.
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 $Y_R$ also causes $Y_D$. Therefore, causal effect of such $U_Y$ on $Y_D$ is non-zero.*

Note that this assumption also applies to all sub-components of $U_Y$. That is, we assume that any unobserved sub-component $U_{Y_i}$ of $U_Y$ that causes $Y_R$ also causes $Y_D$ and the causal effect of such $U_{Y_i}$ on $Y_D$ cannot be zero.

While this may seem as a strong assumption in general, whenever $Y_D$ and $Y_R$ are components of the same variable, it is reasonable to expect them to share their causes as well. Further, we only require that the causal effect of $U_Y$ on $Y_D$ be non-zero, we make no other requirements on the size of the effect. It is instructive to compare this assumption to strict independence assumptions involving unobserved confounders required by prior methods for identifying natural experiments such as instrumental variables (Angrist et al. 1996). By allowing any non-zero dependence, in general the Connectedness assumption is weaker than an independence assumption.

In addition, we make the standard assumption connecting statistical and causal independence between observed variables (related to *Faithfulness* (Spirtes et al. 2000) or *Stability* (Pearl 2009)). This assumption serves to rule out an event where incidental equality of parameters or certain data distributions render two variables statistically independent even though they are causally related. This is a general requirement for causal discovery from observational data for this and many other methods.

**Assumption 2** (Independence). *If any two observed variables are statistically independent, then they are also causally independent in the graphical model of Figure 2a.*

Specifically, for the model shown in Figure 2a, the above assumption rules out the possibility of an (unlikely) event where the effect of the unobserved confounders $U_Y$ cancels out exactly in a way such that $X$ and $Y_D$ become statistically independent.
While the above two assumptions are conceptually simple and are sufficient for proving the validity of the split-door criterion, there are in fact weaker versions of each that remain sufficient for establishing causal identification.

**Assumption 1a.** 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 $Y_D$ cannot be zero.

That is, it is not required that $Y_D$ be affected by all $U_Y$ that cause $Y_R$, only those sub-components of $U_Y$ that also cause $X$.

**Assumption 2a.** If $X$ and $Y_D$ variables are statistically independent, then they are also causally independent in the graphical model of Figure 2a.

That is, we simply require the Independence assumption to hold for $X$ and $Y_D$.

Using Assumptions 1a and 2a, we now show that statistical independence of $X$ and $Y_D$ ensures that $X$ is not confounded by $U_Y$.

**Theorem 1 (The Split-door Criterion).** If $X$, $Y_R$ and $Y_D$ are three random variables generated by the process shown in Figure 2a, and the Connectedness (1a) and Independence (2a) assumptions hold, then $X \perp \perp Y_D$ implies that the effect of $X$ on $Y_R$ is not confounded by $U_Y$.

**Proof.** The proof can be completed directly from Figure 2a 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 2a, 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 $U_Y$ is constant (and thus blocks the path), or one of the edges exists trivially (does not have a causal effect).

(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) Using Assumption 1a, $U_Y$ has a non-zero effect on $Y_D$. Then, the only alternative is that $X \leftarrow U_Y$ edge does not exist, leading to the unconfounded causal model in Figure 2b.

In both (i) and (ii) scenarios, there are no back-door (confounding) paths from $X$ to $Y_R$ and thus the effect of $X$ on $Y_R$ is unconfounded.
Without the confounding due to \( U_Y \), the observational estimate is also the causal estimate. Formally, using Pearl’s do-calculus (Pearl 2009), whenever the split-door criterion is satisfied \( (X \perp \perp Y_D) \), we have the following estimation result:

\[
P(Y|do(X = x)) \equiv P(Y_R|do(X = x)) = P(Y_R|X = x)
\]

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

### 2.2 The split-door criterion through structural equations

Although we have motivated the split-door criterion in terms of the causal model in Figure 2a, for expositional clarity we note that it is also possible to express it in terms of structural equations. Specifically, we can write three structural equations:

\[
\begin{align*}
x &= g(u_x, u_y, \varepsilon_x) \\
y_r &= f(x, u_y, \varepsilon_{yr}) \\
y_d &= h(u_y, \varepsilon_{yd}),
\end{align*}
\]

where \( \varepsilon_x, \varepsilon_{yr}, \) and \( \varepsilon_{yd} \) are mutually independent, zero-mean random variables that capture modeling error and statistical variability. As in Assumption 1a, we assume that \( U_Y \) affects both \( Y_D \) and \( Y_R \). Clearly 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:

\[
\begin{align*}
x &= \eta u_x + \gamma_1 u_y + \varepsilon_x \\
y_r &= \rho x + \gamma_2 u_y + \varepsilon_{yr} \\
y_d &= \gamma_3 u_y + \varepsilon_{yd},
\end{align*}
\]

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

$$0 = Cov(X, Y_D) = E[XY_D] - E[X]E[Y_D]$$

$$= E[(\eta u_x + \gamma_1 u_y + \epsilon_x)(\gamma_3 u_y + \epsilon_yd)] - E[\eta u_x + \gamma_1 u_y + \epsilon_x]E[\gamma_3 u_y + \epsilon_yd]$$

$$= \gamma_1\gamma_3 E[U_Y.U_Y] - \gamma_1\gamma_3 E[U_Y]E[U_Y] = \gamma_1\gamma_3 \text{Var}(U_Y)$$

Assuming that $Y_D$ is affected by $U_Y$ (and therefore $\gamma_3$ 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 $Y_R$:

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

(3)

where $\epsilon^{'}_{yr}$ is now independent, zero-mean noise.

### 2.3 Algorithm for Finding Natural Experiments

The above results point to an algorithm for automatically identifying natural experiments in 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, our algorithm for identifying natural experiments works as follows. First, divide the data into equally-spaced time-intervals $\tau$ such that each interval has enough data points to reliably estimate the joint probability distribution $P(X, Y_D)$. Then, for each time interval $\tau$,

- Determine whether $X$ and $Y_D$ are independent using an empirical independence test.

- If $X \perp\!\!\!\!\!\!\perp Y_D$, then use the observed conditional probability $P(Y_R|X = x)$ to estimate the causal effect in the interval $\tau$. Otherwise exclude the interval $\tau$ from the analysis.

- Average over all time-intervals where $X \perp\!\!\!\!\!\!\perp Y_D$ to obtain the mean causal effect of $X$ on $Y_R$. 
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 (Steuer et al. 2002, Pethel & Hahs 2014). In practice, however, because we consider subsets of the data over each time interval \(\tau\), 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 \(\tau\) we recommend the use of exact independence tests (Agresti 1992, 2001, Lydersen et al. 2007), specifically a variant of Fisher’s exact test. The idea behind this test is simple: we compute the likelihood of obtaining the observed counts \(X\) and \(Y_D\) assuming that they were generated independently, and proceed to select only those subsets whose likelihood is greater than a pre-specified threshold. Note that since our goal is to identify the subsets where \(X\) and \(Y_D\) are independent, rather than refute their independence, we are interested in a higher likelihood under the \(X-Y_D\) independence hypothesis, and therefore higher \(p\) values are desirable.

Specifically, for each data subset, we bin counts for \(X\) and \(Y_D\) into \(K\) discrete levels, thus creating a \(K\times2\) contingency table. Then we compute the likelihood \(p\) of the observed data assuming that the discrete \(X\) and \(Y_D\) counts were generated independently, and we select instances in which the likelihood \(p\) is above a pre-chosen significance level \(\alpha\). To alleviate concerns about the accuracy of the false positive rate—due to inherent conservative nature of the Fisher’s test and the fact that timeseries data may be autocorrelated—we test independence at multiple significance levels for robustness (Upton 1992). For instance, in the results presented in the next section we vary the significance level—likelihood of the observed \(X-Y_D\) counts given the independence hypothesis—from 0.8 to 0.999 to check robustness against bias in false positive rates. Finally, the choice of \(K\) depends on a tradeoff between expressiveness of the discrete levels and the chances of obtaining non-

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1We are interested in a lower Type II error, therefore a higher threshold value is desirable. Our formulation of the independence test is a departure from standard null hypothesis testing, where the focus is on Type I error and hence significance levels are low.
| Graphical model | Description | Untestable assumptions | Limitations | Recommendations example |
|----------------|-------------|------------------------|-------------|--------------------------|
| ![Diagram](https://via.placeholder.com/150) | Condition on observed founders $W$ to isolate 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. |
| ![Diagram](https://via.placeholder.com/150) | Analyze subset of data having independent variation in the treatment. $Z \perp \perp U$ and $Z \perp \perp Y \mid X, U$ | Hard to find a source of independent variation in treatment. | Measure marginal click-throughs on subset of products that experience large, sudden shocks in traffic. |
| ![Diagram](https://via.placeholder.com/150) | Analyze subset of data where auxiliary outcome $Y_D$ is independent of treatment. $Y_D \not\perp\not\perp U_Y$ | Requires dependency between an auxiliary outcome and confounders. | Measure marginal click-throughs on all pairs of products that have uncorrelated direct traffic. |

Figure 3: Comparison of methods for the canonical problem in causal inference: estimating the effect of treatment $X$ on outcome $Y$. $U$ represents all the unobserved (and unknown) confounders that commonly cause both $X$ and $Y$.

trivial counts for each discrete level. To ensure non-trivial frequency for each discretized level, we suggest $K = \tau/4$ levels.

### 2.4 Connections to other methods

The split-door criterion is an example of methods that algorithmically discover natural experiments from observational data (Jensen et al. 2008, Sharma et al. 2015, Grosse-Wentrup et al. 2016). By searching for 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; table provides a summary for easy comparison.
2.4.1 Instrumental Variables

Both the split-door criterion and instrumental variable (IV) methods exploit naturally occurring variation in observational data to identify causal effects. Importantly, however, they make different assumptions. In IV methods, one identifies an auxiliary variable $Z$, called an instrument, 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 unobserved 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, in contrast, exploits an auxiliary variable $Y_D$ that serves as a proxy for unobserved common causes $U_Y$, and therefore depends on different assumptions. The first 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 2a), equivalent to the assumption of faithfulness (Spirtes et al. 2000). This simply rules out unlikely cases where we obtain a particular distribution where statistical independence happens by chance. The second assumption asserts some common cause(s) between the outcome of interest $Y_R$ and the auxiliary outcome $Y_D$ via unobserved confounders $U_Y$. As with IV methods, this second requirement involves an unobserved variable and, as a result, cannot be tested. That said, it is an arguably weaker assumption about dependence between the unobserved confounds $U_Y$ and the auxiliary outcome $Y_D$. Therefore the key difference between the two causal identification criteria is that in instrumental variables we assume independence of random variables, while in the split-door criterion we assume dependence between them.

This is not to say that the split-door criterion is superior to IVs, but rather that the two methods require different assumptions and apply in different settings. In particular, since the independence assumptions for IVs cannot be empirically tested and must be argued for

\[ \text{This might be a concern in an adversarial setting (e.g. when Nature or system designers have incentive to invalidate the method), however, this is not the case in most observational data.} \]
rhetorically, it is up to the researcher to manually discover and justify instruments. If a valid instrument exists, 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 \textit{a priori}. Automatic discovery of natural experiments is possible with the split-door criterion because of its testable independence assumption involving the auxiliary variable \( Y_D \). In this way, the split-door method allows us to discover a potentially broader set of natural experiments than IV approaches when such an auxiliary variable is present. This can lead to more generalizable conclusions because we are able to estimate causal effects over a larger sample of the population. It is also a general method that can be applied to any time series data so long as the outcome \( Y \) is split-up into two observed variables (\( Y_R \) and \( Y_D \)), thereby mitigating the substantial creative burden of identifying and positing valid instruments.

At a fundamental level, however, both methods are trying to obtain a variation in the cause \( X \) that does not affect the outcome \( Y \) directly. Recent work on data-driven search for instrumental variables bridges gap between the two methods by searching for a specific kind of instrument: a large and sudden shock to time series data (Sharma et al. 2015). In this paper, we show that the shock intuition can be extended to identify causal effect in any time-series data with an observed auxiliary outcome.

2.4.2 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 \( X \). Such an approach may appear to be nothing more than a variant of the back-door criterion (Figure 1b) 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 \( Y_D \) is simply affected by \( U_Y \), because any difference (measurement error) between \( Y_D \) and \( U_Y \) can invalidate the
back-door criterion (Spirtes et al. 2000). Further, the assumption about $Y_D$ being affected by $U_Y$ becomes more plausible when $Y_D$ is simply an additive component of $Y$. 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 valid natural experiments and simplifies effect estimation, whereas methods based on back-door criterion such as regression, matching, and stratification process the whole dataset and extract estimates by sophisticated statistical models (Morgan & 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

$$y_r = \rho''x + \beta y_d + \epsilon''_y,$$

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

$$y_r = \rho x + \epsilon'_y,$$

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

3 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 et al. 2011). Figure 4 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 number of 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 (Mulpuru 2006, Grau 2009). But this ignores complications that arise due to correlated demand: users might have found their way to some of these recommended products anyway, via direct search or browsing. For instance, users who are interested in the book “Purity” might be fans of Franzen in general, and so might have directly searched on Amazon.com for some of 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 correlated demand between a focal product and its recommendations. For instance, it might happen to be the case that people searching for “Purity” might not
independently seek out the third recommendation shown, “City on Fire”, in the absence of the recommender.

In this section we show how the split-door criterion can be used to eliminate this issue of correlated demand by automatically identifying and analyzing examples like this, where demand for a product and one (or more) of its recommendations are independent over some time interval \( \tau \). 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 over 20,000 natural experiments covering nearly three times the number of products as previous studies (Sharma et al. 2015). 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.

3.1 Building the causal model

The above discussion suggests 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. We formalize this in the graphical model shown in Figure 5a with the following variables defined and aggregated for each day:

- \( X \) denotes the number of visits to the focal product \( i \)'s webpage.

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

- \( Y_D \) is the number of direct 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 a direct visit to \( j \)'s webpage URL.

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This was quite likely at some point in time, as this is Hallberg’s debut novel, making him relatively unknown to most readers until the book later received critical acclaim.
Figure 5: Equivalent causal graphical models for estimating the impact of recommendations on Amazon.com.

- $U_Y$ denotes the overall unobserved demand for product $j$, which manifests itself as unobserved demand $U_{Yr}$ for recommendation click-throughs for product $j$ and unobserved demand $U_{Yd}$ for direct visits to product $j$.

- $U_X$ represents the part of unobserved demand for product $i$ that is independent of $U_{Yr}$.

We wish to estimate the causal effect of $X$ on $Y$. As in the graphical model for the split-door we introduced in Figure 2a, $Y$ is split up into two constituents: $Y_R$ and $Y_D$, and $X$ directly causes only one of them.

At first glance, Figure 5a appears to differ from the split-door model, because $U_Y$ is separated into two components, $U_{Yr}$ and $U_{Yd}$. This is because customers who visit product $j$ through recommendation from product $i$ are not the same as those who visit $j$ directly. In other words, we expect a different population of users to visit product $j$ directly. However, we can make a transformation on the causal graphical model to produce an equivalent model amenable to the split-door criterion. Specifically, we collapse components of $U_Y$, $U_{Yr}$ and $U_{Yd}$ to generate Figure 5b. Overall unobserved demand $U_Y$ for product $j$ now affects both $Y_R$ and $Y_D$. Intuitively, the causal model in Figure 5b assumes that the unobserved demand $U_{Yr}$ that confounds $X$’s effect on $Y_R$ also partly causes direct visits $Y_D$ to product $j$, since

Since we are interested in estimating causal exposure from the recommender system, we only consider a customer’s first visit to a product as the source of exposure. Therefore, the same user cannot discover a product $j$ both through click-through on a recommendation and searching directly.
both $Y_R$ and $Y_D$ correspond to visits to the same product $j$. Note that as in the structural equations in Section 2, each of the observed variables in the causal graphical model also has an independent error term associated with it, which we do not show in the graph.

To apply the split-door criterion, we must investigate the plausibility of the Connectedness and Independence assumptions from Section 2.1. As mentioned above, $Y_R$ and $Y_D$ are additive components of visits to the same product $j$, so the Connectedness assumption is expected to hold. We cannot rule out coincidental cancellation of effects that result in $X \perp\!\!\!\!\!\!\perp Y_D$ and violate the Independence assumption, however, we expect such events to be unlikely over a large number of product pairs. Further, 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 $U_{Y_R}$ cannot be canceled out on the path $X \leftarrow U_{Y_R} \rightarrow Y_D$ 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 allows us to derive $X \perp\!\!\!\!\!\!\perp Y_D$ as a valid causal identification criterion.

### 3.2 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 have installed the Bing Toolbar and have consented to provide their 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 non-secure 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 views by 2.1 million users to 1.3 million unique products. Of these products, we examine those that receive a minimum of 10 page views on at least one day in this time period,
resulting in roughly 22,000 focal products of interest.

To apply the split-door criterion, we need to identify focal product and recommended product pairs from the log data and to separate out traffic for recommended products into direct ($Y_D$) and recommended ($Y_R$) visits. Fortunately it happens to be the case that Amazon explicitly embeds information in their URLs that makes this possible. Specifically, given a URL for an Amazon.com page view, we can use the ref, or referrer, parameter to determine if a user arrived at a page by clicking on a recommendation or by other means. Amazon shows many different kinds of recommendations on its site. We limit our attention here to the “Customers who bought this also bought” recommendations depicted in Figure 4 as these recommendations are the most common and are shown on product pages from all product categories. We then use the sequence of page views in a session to identify focal and recommended product pairs by looking for focal product views that precede recommendation views. Further details about the toolbar dataset and construction of focal and recommended product pairs can be found in past work (Sharma et al. 2015).

### 3.3 Estimating the causal effect

Having argued for 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 search for natural experiments where a product and its recommendation have uncorrelated demand. Recalling Section 2.3, this amounts to employing an empirical test to identify instances where the direct traffic to a product and one (or more) of its recommendations is statistically independent. As recommended there, we employ Fisher’s exact test and search for 15 day time periods that fail to reject the null hypothesis that these two time series were independently generated. The choice of 15 days is guided by empirical considerations: we require a time period over which we will have enough data for estimation, but also during which Amazon’s recommendations are expected to stay constant.

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 $Y^{(ij)}_R$, the
number of click-throughs to each recommended product \( j \). Also record the total direct visits \( Y_{D}^{(j)} \) to each recommended product \( j \).

2. For each recommended product \( j \), use Fisher’s exact test to determine if \( X^{(i)} \) is independent of \( Y_{D}^{(j)} \) at a pre-specified significance level.

- If \( X^{(i)} \) is found to be independent of \( Y_{D}^{(j)} \), compute the observed click-through rate (CTR) as \( \hat{\rho}_{ij\tau} = (\sum_{t=1}^{\tau} Y_{R}^{(ij)})/(\sum_{t=1}^{\tau} X^{(i)}) \) during this period 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, \( \hat{\rho}_{i\tau} \).

Averaging the causal CTR estimate over all time periods provides the mean causal effect \( \hat{\rho}_{i} \) for each focal product, as some products may appear during more than one time period. Finally, we average this estimate over all products to obtain \( \hat{\rho} \), the overall mean causal effect of the recommender.

Using this algorithm with a significance level—equivalently, likelihood of the data given \( X-Y_{D} \) independence hypothesis—of \( \alpha = 0.999 \) we obtain more than 20,000 natural experiments that satisfy the split-door criterion. Figure 6a shows examples of product pairs that are accepted by Fisher’s test. The first panel shows a focal product that receives a large a

\[ \hat{\rho}_{ij\tau} = \frac{\sum_{t=1}^{\tau} Y_{R}^{(ij)}}{\sum_{t=1}^{\tau} X^{(i)}} \]

Figure 6: Examples of visit time series for focal and recommended products that are accepted or rejected by the split-door criterion for \( \alpha = 0.95 \).
Figure 7: Left subplot shows the number of natural experiments obtained as the likelihood threshold for independence is increased. Right subplot shows the corresponding causal CTR estimate ($\hat{\rho}$); error bars correspond to one standard error of the estimate.

sudden shock in traffic, while direct traffic to its recommended product remains relatively flat. This is reminiscent of the examples analyzed in Carmi et al. (2012) and Sharma et al. (2015). The panel on the right, however, shows the more general patterns that are accepted under the split-door criterion but not considered by these previous approaches: although direct traffic 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 6b. As is visually apparent, each of the focal and recommended traffic patterns are highly correlated, and therefore not useful in our analysis.

The number of natural experiments increases as we decrease the minimum accepted likelihood value, leading to over 23,000 natural experiments for $\alpha = 0.95$, covering nearly 12,000 unique focal products (Figure 7a). Figure 7b shows the causal CTR ($\hat{\rho}$) as a function of the likelihood threshold $\alpha$. We find that as we increase this threshold, the value of average causal CTR tends to stabilize. At the highest likelihood threshold (0.999), the causal CTR estimate is 3.4%, whereas at the other extreme, a threshold of 0.8, the estimate is 3.8%. It is instructive to compare this to the naive observational estimate taken by computing the click-through rate across all focal and recommended product pairs, which produces an estimate of 9.6%. As in past work, this clearly shows that correlated demand
results in an upwards bias of the causal effect \cite{Sharma2015}.

Furthermore, we can break these estimates down by the different product categories present on Amazon.com. Figure 8 shows the variation of $\hat{\rho}$ with the most popular categories, at a likelihood threshold of $\alpha = 0.95$. We see substantial variation in the naive estimate, ranging from 14% on e-Books to 6% on Personal Computer and Movies. 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 the naive observational estimate overstates the causal impact by anywhere from two- to four-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 \cite{Carmi2012} or restricting our attention to mining only shocks in observational data \cite{Sharma2015}. On the same dataset, the shock-based method in \cite{Sharma2015} identified natural experiments on 4,000 products, while the split-door criterion finds natural experiments for over 12,000 products. Second, the split-door criterion yields a more interpretable tuning parameter—the p-value for independence between traffic to a product and its recommendations—compared to the parameters used
in previous work.

That said, as with any natural experiment analysis, the results rely on certain assumptions and so, of course, come with qualifications. 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 the split-door time-intervals are not selected at random, thus violating the \textit{as-if-random} (Angrist et al. 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, in this instance the split-door criterion covers a broad range of the observed data. For example, on Amazon.com, products with at least one valid split-door time-interval spanned multiple product categories and covered nearly half of all focal products in the dataset. As an additional robustness check, we looked at the distribution over product categories for products identified by the split-door criterion and compared this to the same distribution for all products in the population. For comparison, we apply the same popularity filter—at least 10 page views on at least one day—to the dataset with all products. Figure 9a shows that the distribution of products analyzed by the split-door criterion across different product categories is nearly identical to the overall set of products,
while Figure 9b shows a similar result for number of page visits across different product groups. Although these results do not necessarily imply that the as-if-random assumption is satisfied (indeed it very likely not satisfied) they do indicate that the split-door criterion at least allows us to study a representative sample of popular product categories, which is a clear improvement over past work (Carmi et al. 2012, Sharma et al. 2015).

4 Discussion

In this paper we have presented a novel 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 discuss some of the advantages and also limitations of the split-door criterion with respect to validity and generalizability of the estimate. We suggest guidelines to ensure proper use of the criterion and discuss other applications for which it might be used.

4.1 Guidelines for using the criterion

As with any observational method for causal inference, the split-door criterion rests on certain assumptions, in particular: independence (of \( X \) and \( Y_D \)), connectedness (i.e. non-zero casual effect of \( U_Y \) on \( Y_D \)), and unconfoundedness (of \( X \) and \( Y_R \)). Here we give guidelines for reasoning about these assumptions and checking the robustness and sensitivity of estimates.

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 might seem more restrictive, but is justified in domains where the measurements $Y_R$ and $Y_D$ are additive components of the same outcome $Y$. That said, it 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, $U_{Y_R}$ may not be connected to $Y_D$ at all. In a recommender system this can happen when the population of customers who browse product $j$ directly is completely independent from users who visit product $i$. In such a scenario, the split-door criterion will be invalid. We note, however, that this situation can arise only in the (unlikely) event that there is no relationship between demand for a product from users who visit the same product on the same website, just through different means. When there is even a small overlap between the demands of the two populations, the split-door criterion will again be valid and depending on the precision of the statistical independence condition, can also be applied empirically. Acknowledging that it is usually easier to reason about dependence than independence between variables, we put these considerations in contrast with prevailing methods for finding natural experiments that almost always depend on untestable assumptions about (conditional) independence between variables that are usually connected otherwise through causal links.

A key advantage of the split-door criterion is that once these two assumptions are met, it provides a statistical test for verifying its last and most critical assumption: unconfoundedness of $X$ and $Y_R$. This differentiates our approach from most research in natural experiments, where the assumptions required for causal identification cannot be tested from observational data (Dunning 2012). By including a statistical test for some of its assumptions, the split-door criterion is able to reduce its reliance on untestable assumptions. Effectively, this approach allows for weaker untestable assumptions by testing for the more complex assumptions upstream. For instance, instrumental variables require two assumptions about independence on observed and unobserved variables, whereas the split-door only requires a weaker assumption about dependence between observed and unobserved variables. There-
fore, compared to current methods (Dunning 2012, Angrist & Pischke 2008) for justifying validity of natural experiments—justifications based on untestable assumptions—the split-door criterion offers a more precise specification of its validity. At the same time, statistical independence tests used in applying the split-door criterion often have their own free parameters. Any such parameters should be varied to check the sensitivity of estimates to these choices, as in Figures 7a and 7b.

Finally, after identifying and estimating the effect of interest, we need to consider how useful it 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 experiments discovered by the method, the more likely the estimate is to be of general use. Additionally, we recommend that researchers perform checks to compare the distribution of any available covariates in found natural experiments to the general population, as shown in Figure 9.

### 4.2 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 particular
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 effect of ads 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 interest recently \cite{Flaxman2013} on 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. Most websites record the source of each page visit, so obtaining two components for the outcome—visits to an article through social media and through other means—should be straightforward. 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. 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. Suppose, for example, that a brick and mortar store routine sends discount coupons for one of its products to some of its customers. The split-door criterion could easily be used to estimate the causal effect of giving away discounts on product purchases: $X$ would be the number of customers that are
sent a discounted offer; $Y_R$ would be the customers among them who used the discount to purchase the product; and $Y_D$ would be the number of customers who bought the product through other channels (i.e. without a discount). $U_Y$ represents common demand for the product that affects both $Y_R$ and $Y_D$, as well as whether it was chosen as a discounted product ($X$). $U_X$ represents all the other factors, independent of product demand $U_Y$, that may have gone into the selection of products to give away discounts for. More generally, the split-door criterion could be used to estimate the impact of the effect of targeted advertising on product sales, or in any context where demand for a given product can be differentiated into more than one channel.

5 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 mining approach to the problem of identifying natural experiments (Jensen et al. 2008, Grosse-Wentrup et al. 2016, Sharma et al. 2015). As we have noted, data-driven methods have important advantages over traditional methods for exploiting natural experiments—allowing inference to be performed on much larger and more representative samples, and also providing testable assumptions that can be verified from observational data—while also being far 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.

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