Combining Observational and Experimental Data Using First-stage Covariates

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Abstract

Randomized controlled trials generate experimental variation that can credibly identify causal effects, but often suffer from limited scale, while observational datasets are large, but often violate desired identification assumptions. To improve estimation efficiency, I propose a method that combines experimental and observational datasets when 1) units from these two datasets are similar and 2) some characteristics of these units are observed. I show that if these characteristics can partially explain treatment assignment in the observational data, they can be used to derive moment restrictions that, in combination with the experimental data, improve estimation efficiency. I outline three estimators (weighting, shrinkage, or GMM) for implementing this strategy, and show that my methods can reduce variance by up to 50% in typical experimental designs; therefore, only half of the experimental sample is required to attain the same statistical precision. If researchers are allowed to design experiments differently, I show that they can further improve the precision by directly leveraging this correlation between characteristics and assignment. I apply my method to a search listing dataset from Expedia that studies the causal effect of search rankings, and show that the method can substantially improve the precision.

1 Introduction

Measuring the causal effect of different business practices is increasingly important for firms that have shifted their businesses online. Firms are interested in understanding

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how marketing variables such as prices, ads, and recommendations affect outcomes such as purchases and clicks. Firms cannot optimize their algorithms to customize their marketing strategies without understanding these causal effects. One popular solution to measure causal effects is using observational methods to train a predictive model of consumer outcomes. Another solution is to conduct experiments that compare the outcomes of similar individuals randomly exposed to different treatments.

Unfortunately, both solutions have limitations. Although observational methods benefit from the large amount of data that is relatively inexpensive to obtain, they often severely bias estimates of causal effects due to endogeneity (Lewis et al. (2011), Blake et al. (2015), Ursu (2018), Gordon et al. (2019)). Due to this bias, firms often proceed to run an experiment instead and make their future decisions based on the experiment results. However, because experimentation could hurt user experience and profit, experiments often have limited scale, conducted only within a subset of traffic, markets, or weeks. For example, Google (Wang et al. (2016)), Microsoft (Li et al. (2015)), and Expedia (Ursu (2018)) have all conducted experiments that randomizes rankings for a small subset of traffic. Uber (Chen et al. (2019)) has conducted experiments that randomized incentives within one county for three weeks. Advertisers sometimes randomize ad exposure for 14 days to measure advertising ROI (Lewis and Rao (2015)). These experiments often produce imprecise estimates due to their limited scale. For example, Lewis and Rao (2015) examine 25 advertising field experiments and document that the confidence interval on advertising ROI is more than 100% wide, implying that advertisers cannot distinguish a campaign with a low 0% ROI from a campaign with a highly profitable 50% ROI. Therefore, it is particularly important to develop efficient methods to improve the use of observational and experimental data.

This paper highlights important aspects of observational data in the digital environment that can be used to improve estimation efficiency: 1) the abundance of variation in the treatment and 2) the abundance of relevant first-stage covariates that can explain the treatment variation. Firms often have access to a large observational dataset from non-experimental traffic, markets, or weeks. In this observational setting, users may experience different treatments because these treatments are often affected by many covariates. For example, hotel recommendations/rankings are affected by the search criteria and the characteristics of different hotels, Uber prices are affected by local supply and demand, and ad exposure is affected by bids of competing advertisers and targeting criteria. In fact, these treatments are sometimes directly generated by an algorithm that takes these observed covariates as inputs. Users experience different treatments because they are associated with different covariates.

I propose a method that leverages these first-stage covariates to combine observational and experimental data to improve estimation efficiency. A key challenge of incorporating observational data into the experimental data is that the observational estimate is often biased. I show that if the observational estimate is derived by incorrectly treating these
first-stage covariates as instrumental variables, then this bias can be corrected using the experimental data. More importantly, I show that this bias-corrected estimate is uncorrelated with the experiment-only benchmark estimate. Using either weighting, shrinkage, or GMM, these two estimates can be combined into a more precise unbiased estimate.

When compared to the estimator that only uses the experimental data, I prove that my method can improve the efficiency by up to 50% in terms of mean squared error (MSE). I also explore the selection of experimental units from an experimental design perspective. I show that the estimation efficiency further increases if the experimental design incorporates how the treatment X is endogenously generated. I apply the method to datasets in Ursu (2018) that measure how online hotel rankings affect clicks. My method increases efficiency by 25% given a common experimental design and by 40% given a novel experimental design.

The remainder of the paper is organized as follows: Section 2 reviews the literature. Section 3 presents the setup and key assumptions. Section 4 introduces the estimation method. Section 5 discusses the theoretical efficiency gain and its implications for researchers and firms. Section 6 extends the method from linear to nonlinear models. Section 7 provides methods that are robust when the observational model is misspecified. Section 8 uses simulation to illustrate the efficiency gain in different situations. Section 9 presents my results that measure effects of hotel rankings on clicks using datasets in Ursu (2018). Section 10 discusses additional applications. Section 11 offers my conclusions.

2 Literature Review

My paper is related to several contemporary studies that combine experimental and observational data. This strand of literature typically has two objectives: 1) identifying effects that cannot be identified using only experimental data, 2) improving the precision of estimates although experiment is sufficient for identification. Several papers focus on achieving the objective of identification. For example, Athey et al. (2020) consider a case when the long-term treatment effect cannot be identified based only on the experimental data, because the long term outcome in the experimental data is missing. Kallus et al. (2018) consider a setting when the treatment effect for a certain population cannot be identified because the experiment does not cover this subpopulation.

My paper focuses on improving efficiency instead of achieving identification. Even though experiments are often sufficient for identifying causal effects of interests, the estimate may be imprecise when large-scale experiments are costly or unrealistic. Given the limited experiment size, it is important to develop methods to improve the efficiency. Rosenman et al. (2020) consider a case when the population is partitioned into several strata, and researchers are interested in improving average precision across strata. They show that the mean-squared-error can be improved by shrinking unbiased experimental estimates to additional biased observational estimates. However, when the number of
strata is small or the bias is large, the efficiency gain could be small. Peysakhovich and Lada (2016) consider a case where researchers observe a panel of individuals that appear many times. They estimate individual-level observational estimates and use them as features to differentiate and sort individuals.\textsuperscript{1} Another popular method by Deng et al. (2013) incorporates pre-experimental variables as additional covariates into the experimental analysis. Both Deng et al. (2013) and Peysakhovich and Lada (2016) require observing individuals before and during the experiment, and is conceptually different from my method that is applicable even if individuals are only observed once. My method also has different determinants for efficiency gains. Method of Deng et al. (2013) and Peysakhovich and Lada (2016) are most effective when the pre-experimental data contain variables that are highly predictive of the experimental outcome. My method is most effective when the pre-experimental data contain variables that are highly predictive of the endogenous variable. My method is complementary because it uses new assumptions that leverage a new source of information, allowing us to improve efficiency even when the number of strata is small or each individual is only observed once in the data. These methods are also not exclusive: they can be applied together if the data requirements and assumptions are met.

My estimation strategy is closely related to that of Imbens and Lancaster (1994), who combine macro and micro data to improve estimation efficiency. My method is similar in that one dataset is sufficient for identifying the parameter of interest, and the moment conditions from the additional dataset improve efficiency. However, Imbens and Lancaster (1994) consider a setting in which all variables in both datasets are generated from the same distribution. In contrast, I consider a different setting in which the variable of interest is endogenously determined in the observational data but randomized in the experimental data.

My model for observational data is related to the literature by Conley et al. (2010) and Kippersluis and Rietveld (2018) that study plausibly exogenous models. In their model, some covariates are considered as "plausible IV" because they satisfy the relevance condition but slightly violate the exclusion restriction. Conley et al. (2010) assume that there is a prior on how much the first-stage covariate violates the exclusion restriction. My method also depends on the availability of first-stage covariates that satisfy the relevance condition. However, my model allows the exclusion restriction to be strongly violated, and do not require a prior on the degree of violation, because this violation can be informed by the experimental data.

\textsuperscript{1}The method also requires a monotonic relationship between observational estimates and true causal effects.
3 Setup

For illustrative purposes, consider a linear causal model

\[ Y_i = \beta_1 X_i + \beta_2 Z_i + U_i, \]  

(1)

where \( Y_i \) is the outcome, \( X_i \) is the focal variable of interest, \( Z_i \) is the observed covariate, and \( U_i \) is the unobserved covariate. \( \beta_1 \) and \( \beta_2 \) are respectively the causal effect of \( X_i \) and \( Z_i \) on \( Y_i \). \( \beta_1 \) is the primary parameter of interest, and \( \beta_2 \) is a nuisance parameter that may or may not be 0.

The main difference between the observational and the experimental data is how \( X_i \) is generated. Let \( G_i \in \{ E, O \} \) be the indicator for the data a unit \( i \) is drawn from, where \( E \) denotes experimental data and \( O \) denotes observational data. Assume the first stage equations for \( X_i \) to be:

\[
X_i = \begin{cases} 
\gamma Z_i + V_i & \text{if } G_i = O \\
\text{randomized} & \text{if } G_i = E.
\end{cases}
\]  

(2)

\( \gamma \) is a non-zero first stage coefficient for the covariate \( Z_i \) in the observational data and \( V_i \) is the corresponding unobserved error term\(^2\). Assume \( X_i \) in the observational data is correlated with the unobserved covariate \( U_i \) so that the observational data has an endogeneity problem:

\[
\text{Cov}(X_i, U_i|G_i = O) \neq 0.
\]

I also assume \( Z_i \) violates the exclusion restriction so that it is not an instrumental variable. This violation can be caused by a direct effect on \( Y_i \) or a correlation with the unobserved covariate:

\[
\beta_2 \neq 0 \text{ or } \text{Cov}(Z_i, U_i|G_i = O) \neq 0.
\]

Compared to the observational data, the experimental data can identify \( \beta_1 \) because \( X_i \) is randomized and thus independent of \( U_i \):

\[
X_i \perp U_i|G_i = E.
\]

To summarize the basic setup, the observational data alone cannot identify \( \beta_1 \) because \( X_i \) is endogenous and \( Z_i \) is not an instrumental variable. The experimental data are sufficient to identify \( \beta_1 \), but the accuracy is limited by the sample size. \( Z_i \) is a covariate that satisfies the first-stage relevance condition in the observational data. The goal of

\(^2\)The first-stage equation does not have to be linear nor causal. \( Z_i \) can be thought of as a composite score of consumer characteristics after applying nonlinear rules on multiple covariates, and this nonlinear rule can be estimated using any flexible predictive models. Following the two-stage-least-square method, the coefficient \( \gamma \) can also be understood as the best linear predictor for \( X \) given \( Z \).
this paper is to leverage this first-stage covariate $Z_i$ to improve estimation efficiency.

Worth discussing is the linearity and additive separability assumption I made about the model. This assumption allows me to better articulate the intuition of my method as well as analytically quantifying the magnitude of the efficiency gain. The linearity assumption is not necessary for my method to improve efficiency. I discuss how the method can be extended to other classes of nonlinear models in Section 6 and Appendix G.

3.1 Assumptions

To improve efficiency, a key assumption is that the joint distributions of $(Z_i,U_i)$ are similar between experimental and observational datasets, such that information learned in one dataset can be transferred to another dataset. The strongest condition is that the distribution is identical between experimental and observational data:

**Assumption 1 (Identical Distribution of Covariates)**

$$G_i \perp (Z_i, U_i),$$

Assumption 1 holds if a unit $i$ with attributes $(Z_i, U_i)$ is first randomly sampled from the population and then randomly assigned into the experimental or observational group, independent of $(Z_i, U_i)$. Figure 1 illustrates an experimental procedure in the digital environment that satisfies this assumption:

Figure 1: An experimental design example that randomly assigns users to the experimental/observational group

This experimental procedure is commonly used for experiments that shuffle the rankings of search results or ads displayed to users. Google (Wang et al. (2016))$^3$ and Microsoft (Li et al. (2015)) have conducted experiments that first randomly select a small fraction of users, and then randomize the search results displayed to those users. Expedia (Ursu

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$^3$As described in the randomization section of the paper: given a ranked result list of n documents returned for some query, instead of showing the original list, we permute the results uniformly at random and present the shuffled list to a small fraction of end users


(2018)) has also run such an experiment that shuffles the ranks of hotels displayed to a subset of customers. *JD.com* (Carrion et al. (2021)) has conducted experiments that randomly shuffle the orders of ads for a random subset of users. In these scenarios, $Z$ can be the relevance score of a website, $X$ is the rank of a website, and $Y$ is the outcome of interest such as a click. By conducting these experiments, firms can answer counterfactual questions such as how likely a customer is going to click a certain webpage when it has a ranking of $X$, a crucial input for firms to improve ranking and user engagement.

Assumption 1 can be relaxed by focusing on the key moments of the distributions required for estimating the model. In linear models as in Equation 1, it is sufficient to assume that the covariances are similar:

**Assumption 1.a (Similarity in Covariance)**

\[
\frac{\text{Cov}(Z_i, U_i | G_i = O)}{\text{Var}(Z_i | G_i = O)} = \frac{\text{Cov}(Z_i, U_i | G_i = E)}{\text{Var}(Z_i | G_i = E)}
\]

In a more general setting where the model is non-linear and the experimental data are not a random sample, Assumption 1 can be relaxed to assume that the distributions are identical after adjusting for observed covariate $Z_i$:

**Assumption 1.b (Conditional Similarity in Unobservables)**

\[
G_i \perp U_i | Z_i
\]

This conditional independence condition is common in the related literature (e.g., Athey et al. (2020), Kallus et al. (2018)) to ensure effects learned in one dataset can be transferred to another dataset.

Compared to Assumption 1, Assumption 1.a and 1.b are plausible in a more general setting when the sampling of experimental unit is not random and instead follows arbitrary rules. For example, the observational data may include all customers that arrive in the previous month, and the experimental data include all customers that arrive today. For firms that are willing to conduct experiments, it is plausible to assume that past customers are similar to current customers: if past customers are drastically different from future customers, then experiments conducted on past customers should have limited value to guide strategies for future customers.

For illustrative purposes, I focus on the estimation strategy for cases when the distribution of $(Z_i, U_i)$ are identical based on Assumption 1. The estimation strategy remains the same under Assumption 1.a. Section 6 discusses how the estimation strategy can be extended to Assumption 1.b.
4 Estimation Strategies

4.1 Intuition

To develop intuition, consider when $Z_i$ is incorrectly used as an IV to estimate $\beta_1$ in observational data. Define the probability limit of this estimator as $b_{IV}^1$:

$$b_{IV}^1 \equiv \frac{\text{Cov}(Y_i, Z_i|G_i = O)}{\text{Cov}(X_i, Z_i|G_i = O)}$$ (3)

This estimator is biased because $Z_i$ violates the exclusion restriction, either due to a direct effect on $Y_i$ or an unobserved correlation with $U_i$:

$$b_{IV}^1 = \frac{\text{Cov}(Y_i, Z_i|G_i = O)}{\text{Cov}(X_i, Z_i|G_i = O)} = \beta_1 + \frac{\beta_2 \text{Var}(Z_i|G_i = O) + \text{Cov}(U_i, Z_i|G_i = O)}{\text{Cov}(X_i, Z_i|G_i = O)}$$ (4)

Let $b_2$ denote the violation of such exclusion restriction:

$$b_2 \equiv \beta_2 + \frac{\text{Cov}(U_i, Z_i|G_i = O)}{\text{Var}(Z_i|G_i = O)}$$ (5)

A direct implication of Equation 4 and 5 is:

**Lemma 1** The bias of the incorrect IV estimator is $\frac{b_2}{\gamma}$.

When using observational data alone, this bias typically cannot be corrected because the violation of exclusion restriction $b_2$ is unknown and cannot be estimated. However, approximating this violation becomes possible with experimental data by regressing $Y_i$ on $Z_i$. Define the probability limit of this estimated coefficient as $b_{E}^2$:

$$b_{E}^2 \equiv \frac{\text{Cov}(Y_i, Z_i|G_i = E)}{\text{Var}(Z_i|G_i = E)}$$

**Lemma 2** Under Assumption 1, the violation of exclusion restriction $b_2$ is identified by estimating $b_{E}^2$ using experimental data.
Proof:

\[ b_2^E = \frac{Cov(Y_i, Z_i|G_i = E)}{Var(Z_i|G_i = E)} \]

= \frac{Cov(\beta_1 X_i + \beta_2 Z_i + U_i, Z_i|G_i = E)}{Var(Z_i|G_i = E)}

= 0 + \beta_2 + \frac{Cov(U_i, Z_i|G_i = E)}{Var(Z_i|G_i = E)}

= \beta_2 + \frac{Cov(U_i, Z_i|G_i = O)}{Var(Z_i|G_i = O)}

= b_2

To summarize the intuition, the incorrect IV estimator in the observational data is more useful when its bias can be quantified, and the experimental data help quantify such bias. Next I propose three different estimation procedures: 1) weighting, 2) regularized regression, and 3) GMM. These procedures are qualitatively the same and yield similar estimates, but give different intuitions to help understand the source of efficiency gain.

4.2 Weighting

One procedure is to derive two unbiased estimators of \( \beta_1 \) that are uncorrelated and then combine them through weighting. This procedure involves multiple steps visualized in Figure 2:

1. Regress \( Y_i \) on \((X_i, Z_i)\) in experimental data to obtain \( (\hat{\beta}_1^E, \hat{b}_2^E) \).

2. Incorrectly use \( Z_i \) as an IV in observational data to obtain \( \hat{b}_1^{IV} \).

3. Regress \( X_i \) on \( Z_i \) in observational data to obtain \( \hat{\gamma}^O \).

4. Use Lemma 1 to correct the bias of the incorrect IV estimator: \( \hat{\beta}_1^O = \hat{b}_1^{IV} - (\hat{b}_2^E / \hat{\gamma}^O) \).

5. Take a weighted average of the two estimates of \( \hat{\beta}_1^{weighted} = w_O\hat{\beta}_1^O + w_E\hat{\beta}_1^E \) where the weight \( (w_O, w_E) \) are hyperparameters chosen to minimize \( Var(\hat{\beta}_1^{weighted}) \).

\footnotetext{Note that even though experimental data are insufficient to identify \( \beta_2 \) due to the potential correlation between \( U \) and \( Z \), \( b_2 \) can be identified. So the method focuses on \( (\hat{\beta}_1^E, \hat{b}_2^E) \) instead of \( (\hat{\beta}_1^E, \hat{\beta}_2^E) \).}
Lemma 3 $\hat{\beta}_1^O$ and $\hat{\beta}_1^E$ are uncorrelated.

Lemma 4 Under Lemma 3 the weighting minimizes the variance of the combined estimator, if each estimate is weighted in inverse proportion to its variance:

$$\frac{w_{\text{O}}^*}{w_{\text{E}}^*} = \frac{\text{Var}(\hat{\beta}_1^E)}{\text{Var}(\hat{\beta}_1^O)}$$

I prove Lemma 3 and Lemma 4 in Appendix A.1.

This weighting method helps explain the determinants of efficiency gain, which depends on the accuracy of the additional estimator $\hat{\beta}_1^O$. Since $\hat{\beta}_1^O = \hat{b}_1^{IV} - (\hat{b}_2^E / \hat{\gamma}^O)$, the efficiency gain is large if 1) the observational dataset is large such that $\hat{b}_1^{IV}$ and $\hat{\gamma}^O$ are accurate, 2) $\hat{b}_2^E$ is accurate, and 3) $\gamma$ is large such that $\text{Var}(\hat{b}_2^E / \gamma)$ is small. If the observational dataset is extremely large such that $b_1^{IV}$ and $\gamma$ are precisely measured, the only remaining uncertainty comes from $\text{Var}(\hat{b}_2^E / \gamma)$.

4.3 Regularized or Constraint Regression

While the weighting method helps illustrate the determinants for efficiency gain given the weights ($w_{\text{O}}, w_{\text{E}}$), it may not be immediately clear what values of ($w_{\text{O}}, w_{\text{E}}$) strictly increase the efficiency. The other procedure is to use experimental data for estimation, while incorporating information from observational data as an additional constraint. With only experimental data, the estimator $(\hat{\beta}_1^E, \hat{b}_2^E)$ minimizes the objective function:

$$\min_{\hat{\beta}_1, \hat{b}_2} \sum_{i: G_i = E} (Y_i - \hat{\beta}_1 X_i - \hat{b}_2 Z_i)^2$$

The key information from observational data can be summarized using $(\hat{b}_1^{IV}, \hat{\gamma}^O)$ that are estimated in the weighting procedure in Section 4.2. Lemma 1 suggests that if the
estimates \((\hat{\beta}^E_1, \hat{b}^E_2, \hat{\beta}^{IV}_1, \hat{\gamma}^O)\) are sufficiently precise, they must satisfy the constraint:

\[
\hat{b}^{IV}_1 - \hat{\beta}^E_1 - \frac{\hat{b}^E_2}{\hat{\gamma}^O} \to 0,
\]

If \((\hat{b}^{IV}_1, \hat{\gamma}^O)\) are accurate, any deviation from this constraint should be penalized because it implies that \((\hat{\beta}^E_1, \hat{b}^E_2)\) are inaccurate. This penalization can be incorporated by adding an additional regularization term into the objective function:

\[
\min_{\beta, b} \sum_{i \in G_i = E} (Y_i - \hat{\beta}^E_i X_i - \hat{b}^{IV}_2 Z_i)^2 + \lambda (\hat{b}^{IV}_1 - \hat{\beta}^E_1 - \frac{\hat{b}^E_2}{\hat{\gamma}^O})^2
\]

where \(\lambda\) is a non-negative hyperparameter that determines how much to penalize the deviation from the constraint. Intuitively, \(\lambda\) depends on how much researchers believe the violation of such constraint can be attributed to the inaccuracy of \((\hat{\beta}_1, \hat{b}_2)\).

When \(\lambda = 0\), it implies that researchers ignore information from observational data, and the optimization problem is equivalent to the one using only experimental data. A small value of \(\lambda\) can be rationalized if the observational dataset is small, such that a violation of the constraint can be explained by the inaccuracy of \((\hat{b}^{IV}_1, \hat{\gamma}^O)\) rather than the inaccuracy of \((\hat{\beta}_1, \hat{b}_2)\).

When \(\lambda = \infty\), it implies that researchers heavily rely on the constraint. A large \(\lambda\) can be justified if the observational dataset is large such that the precision of \((\hat{b}^{IV}_1, \hat{\gamma}^O)\) is high. Then any violation of the constraint must be attributed to the inaccuracy of \((\hat{\beta}_1, \hat{b}_2)\). When \(\lambda = \infty\), the problem then becomes a constraint optimization:

\[
\min_{\beta, b} \sum_{i \in G_i = E} (Y_i - \hat{\beta}^E_i X_i - \hat{b}^{IV}_2 Z_i)^2
\]

subject to \(b^{IV} - \hat{\beta}^E_1 - \frac{\hat{b}^E_2}{\hat{\gamma}} = 0\)

When researchers add this additional constraint into the original experiment-only optimization procedure, the method strictly improves efficiency when this constraint is correctly specified.

### 4.4 GMM

I also consider a GMM approach that can easily calculate standard errors using existing framework. Equation 1 is not suitable for direct GMM estimation however, because one of the parameters \(\beta_2\) cannot be identified when \(Cov(U_i, Z_i) \neq 0\). To derive relevant moment conditions, it is convenient to work with a residual \(\epsilon_i\) that is determined by identifiable parameters \((\beta_1, b_2)\):

\[
\epsilon_i \equiv Y_i - \beta_1 X_i - b_2 Z_i
\]
Lemma 5 If Assumption 1 is satisfied, then

$$\epsilon_i = U_i - \frac{Cov(U_i, Z_i)}{Var(Z_i)} Z_i.$$ 

such that $\epsilon_i$ and $Z_i$ are not correlated in both the observational and experimental data:

$$Cov(\epsilon_i, Z_i|G_i = g) = 0 \text{ for } g \in \{O, E\}$$

I prove Lemma 5 in Appendix A.2. Note $\epsilon_i$ is different from $U_i$ and can be understood as a transformation of $U_i$ that removes its correlation with the observed covariate $Z_i$. Based on Lemma 5, two moment conditions can be derived from the experimental data

$$E[(Y_i - \beta_1 X_i - b_2 Z_i)X_i|G_i = E] = 0$$
$$E[(Y_i - \beta_1 X_i - b_2 Z_i)Z_i|G_i = E] = 0$$

and one extra moment condition from the observational data:

$$E[(Y_i - \beta_1 X_i - b_2 Z_i)Z_i|G_i = O] = 0$$

Denote the vector of moment functions as $g_i$:

$$g_i(\beta_1, b_2) = \begin{bmatrix} (Y_i - \beta_1 X_i - b_2 Z_i)X_i I(G_i = E) \\ (Y_i - \beta_1 X_i - b_2 Z_i)Z_i I(G_i = E) \\ (Y_i - \beta_1 X_i - b_2 Z_i)Z_i I(G_i = O) \end{bmatrix}$$

and the GMM estimator can be written as

$$\left(\hat{\beta}_1^{GMM}, \hat{b}_2^{GMM}\right) = \arg \min \left[ \frac{1}{N} \sum_{i=1}^{N} g_i(\hat{\beta}_1, \hat{b}_2)^T \cdot W \cdot \left[ \frac{1}{N} \sum_{i=1}^{N} g_i(\hat{\beta}_1, \hat{b}_2) \right] \right]$$

where $W$ is a non-negative definite weighting matrix. The optimal $W^*$ depends on the covariance matrix of the moment conditions, which can be estimated by running the standard feasible GMM. I discuss the optimal weighting matrix when $\epsilon_i$ satisfies homoscedasticity and non-autocorrelation in Section 5.

4.5 Method Comparison

These three methods are qualitatively similar but offer different intuitions. All of these methods involve selecting a hyperparameter: the weight in the weighting method, the regularization parameter $\lambda$ in the regularization regression method, and the weighting matrix $W$ in the GMM method. In practice, if researchers believe the model is correctly specified, I recommend researchers to use the GMM approach, because the weighting matrix and standard errors can be easily determined using existing frameworks such as
two-step feasible GMM. If the model is plausibly misspecified, I recommend researchers to use a regularization approach that makes use of cross-validation for hyperparameter tuning.\(^5\) Appendix B discusses this cross-validation procedure.

## 5 Efficiency Gain

In this section I illustrate the efficiency gain using the GMM approach. It is convenient to first formulate the estimation problem in matrix notation:

\[
Y = \beta_1 X + b_2 Z + \epsilon \tag{7}
\]

where each row is

\[
Y_i = \beta_1 X_i + b_2 Z_i + \epsilon_i,
\]

and the unit is ordered by group \(g \in \{E, O\}\) such that

\[
[Y, X, Z, \epsilon] = [Y_E, X_E, Z_E, \epsilon_E] \begin{bmatrix}
Y_O & X_O & Z_O & \epsilon_O
\end{bmatrix}
\]

Since the exact efficiency gain depends on the distribution of \((X_i, Z_i, \epsilon_i)\), I focus on the simple case of homoscedasticity and nonautocorrelation\(^6\)

**Assumption 2 (Homoscedasticity and Nonautocorrelation):**

\[
E[\epsilon \epsilon' | X, Z, G] = \Sigma = \sigma^2 I,
\]

This assumption is not essential but helps illustrate the determinants of efficiency gain.

**Theorem 1** If Assumptions 1 and 2 are satisfied, an optimal weighting matrix \(W\) satisfies \(W = \Omega^{-1}\) where,

\[
\Omega = \begin{bmatrix}
X_E'X_E & 0 & 0 \\
0 & Z_E'Z_E & 0 \\
0 & 0 & Z_O'Z_O
\end{bmatrix},
\]

and the GMM estimator using this weighting matrix has an asymptotic variance of:

\[
V(\hat{\beta}_1^{GMM}) = \sigma^2 \left\{ \frac{1}{n_E} [Var(X_i|G_i = E) + \pi_O Var(\gamma Z_i)] \right\}^{-1}
\]

\(^5\)Alternatively, researchers can use a GMM approach with a weighting matrix that is robust to model misspecification as in Cheng et al. (2019).

\(^6\)This assumption is satisfied if \(\{(Z_i, U_i)\}_{i=1}^n\) are independent draws from a bi-variate normal distribution such that \((Z_i, \epsilon_i)\) as the transformation of \((Z_i, U_i)\) also follows a normal distribution.
In comparison, the asymptotic variance of the experiment-only estimator is:

\[ V(\hat{\beta}^E_1) = \sigma^2 \frac{1}{n_E} [\text{Var}(X_i|G_i = E)]^{-1} \]

A comparison of these two variances suggests that the efficiency gain is determined by 1) the proportion of observational data, 2) the relevance of \( Z_i \) as a first-stage covariate, and 3) the relative variance of \( X_E \) and \( X_O \):

**Lemma 6** The relative variance of the two estimators can be written as:

\[
\frac{V(\hat{\beta}^E_1)}{V(\hat{\beta}^{GMM}_1)} = 1 + \frac{\pi_O}{\text{Propotion of Observational Data}} \times \frac{\text{Var}(\gamma Z_i)}{\text{Var}(X_i|G_i = O)} \times \frac{\text{Var}(X_i|G_i = O)}{\text{Var}(X_i|G_i = E)}
\]

Worth noting is that Assumption 1 implies that \( Z_i \perp G_i \), which ensures that \( \text{Var}(Z_i|G_i = O) = \text{Var}(Z_i|G_i = E) \) and simplifies the asymptotic variance. The proof for this theorem is provided in Appendix A.3.

### 5.1 Implications For Firms Conducting Experiments

To understand the magnitude of the efficiency gain, I make two additional auxiliary assumptions that are not uncommon within firms. These two assumptions are not essential to the method but help illustrate the order of magnitude of its efficiency gain.

**Assumption 3** (Perfect Knowledge of First-Stage)

\[
\frac{\text{Var}(\gamma Z_i|G_i = O)}{\text{Var}(X_i|G_i = O)} = 1
\]

This assumption can be rationalized if firms have perfect knowledge over how \( X_i \) is endogenously generated, and \( Z_i \) perfectly explains how \( X_i \) is generated in the observational data such that \( X_i = \gamma Z_i \) for all \( G_i = O \). With perfect knowledge, there are no unobservables that affect \( X_i \) other than covariates represented by \( Z_i \). For example, Google and other search engines or recommendation engines know the algorithms that generate the rankings. Advertising platforms know how the ad exposure is determined by the bids and qualities scores in an online ad auction.\(^7\)

**Assumption 4** Identical Variance of \( X \)

\[
\text{Var}(X_i|G_i = O) = \text{Var}(X_i|G_i = E)
\]

---

\(^7\)The identification challenge using the observational data is no longer endogeneity with perfect knowledge of the first stage, because there is no unobservables that are changing \( X_i \). The issue is collinearity: there is no variation in \( X_i \) conditional on \( Z_i \). Therefore, experimental data is still needed.
This assumption is easily satisfied in ranking experiments in online settings, where positions of ads or hotels listed by a website are randomly shuffled. It is also satisfied if the experimental $X$ is sampled from the marginal distribution of the observational $X$.

**Lemma 7** If Assumptions 1-4 are satisfied, the efficiency gain is bounded by the proportion of the observational data

$$\frac{\text{Var}(\hat{\beta}_E^{\text{F}})}{\text{Var}(\hat{\beta}_1^{\text{GMM}})} = 1 + \pi_O \in [1, 2)$$

Given the above assumptions, firms can reduce the variance of the estimator by up to 50%, which implies that half of the experimental data is required to achieve the same accuracy when observational data is incorporated.

### 5.2 Implications for Experimental Designs

To examine the implications for experimental designs, I consider the efficiency gain in a more general case when Assumption 1 in Theorem 1 is violated due to alternative experimental designs. Recall that Assumption 1 ensures that $Z_i \perp G_i$ and thus $\text{Var}(Z_i|G_i = O) = \text{Var}(Z_i|G_i = E)$. When the sampling of experimental units is non-random and depends on $Z_i$ such that $\text{Var}(Z_i|G_i = O) \neq \text{Var}(Z_i|G_i = E)$, it is possible that Assumption 1 is violated but Assumption 1.a still holds.

**Lemma 8** If Assumption 1.a and 2 are satisfied, the efficiency gain of the GMM method under an optimal weighting matrix satisfies:

$$\frac{\text{Var}(\hat{\beta}_E^{\text{F}})}{\text{Var}(\hat{\beta}_1^{\text{GMM}})} = 1 + \frac{\pi_O}{\text{Propotion of Observational Data}} \times \frac{\text{Var}(Z_i|G_i = E)}{\text{Var}(Z_i)} \times \frac{\text{Var}(\gamma Z_i|G_i = O)}{\text{Var}(X_i|G_i = O)} \times \frac{\text{Var}(X_i|G_i = O)}{\text{Var}(X_i|G_i = E)}$$

The proof of this lemma is provided in Appendix A.4. To understand the magnitude of the efficiency gain in this general case, consider when the proportion of observational data approaches 1:

**Lemma 9** If Assumption 1.a and 2 are satisfied, and $\pi_O \to 1$:

$$\frac{\text{Var}(\hat{\beta}_E^{\text{F}})}{\text{Var}(\hat{\beta}_1^{\text{GMM}})} \to 1 + \frac{\text{Var}(\gamma Z_i|G_i = E)}{\text{Var}(X_i|G_i = E)}$$

The efficiency gain of incorporating the observational data increases if the variation of $Z_i$ in the experimental data increases. Intuitively, this additional efficiency gain comes from a more accurate measurement of $b_2$, the violation of the exclusion restriction. When $\text{Var}(Z_i|G_i = E)$ is large such that $b_2$ is precisely measured, the observational data can be better leveraged.
Var$(Z_i|G_i = E)$ is a moment that can be adjusted by experimenters when they choose an assignment rule that increases the variation of $Z_i$. After sampling a unit $i$ with observed covariate $Z_i$ from the population, experimenters can randomly assign this unit into the experimental group based on a probability that depends on $Z_i$. For example, if $Z_i$ is binary with $p(Z_i = 1) = p_1 \neq 0.5$, the experimenter can design a balanced sampling procedure to increase $Var(Z_i|G_i = E)$ by making sure that $P(Z_i = 1|G_i = E) = P(Z_i = 0|G_i = E) = 0.5$.

6 Extension to Nonlinear Models

For illustrative purposes, the paper so far has mostly focused on random selection of experimental units in Assumption 1 as well as linear models specified in Equation 1. This section describes how the method can be applied to the relaxed Assumption 1.b and to nonlinear models with additive unobservables:

$$Y_i = f(X_i, Z_i) + U_i$$

where $f(X, Z)$ is a flexible non-linear function that allows interactions between $X$ and $Z$.

Analogous to the linear case, $f$ may not be directly identified by the experimental data when $Cov(U_i, Z_i) \neq 0$. It is thus convenient to work with a transformed function that incorporates this covariance:

$$h(X_i, Z_i) \equiv f(X_i, Z_i) + E[U_i|Z_i, G_i = E]$$

This function $h(X, Z)$ can be understood as the conditional average potential outcome for units with attributes $Z$ that receives treatment $X$. Accurately estimating this function is valuable because the function can be used to derive the average treatment effect (ATE), the conditional average treatment effect (CATE), as well as the marginal effect. For example, when $X$ is binary, $h(1, Z) - h(0, Z)$ is CATE conditional on a specific $Z$, and $E[h(1, Z_i) - h(0, Z_i)|G_i = E]$ is the ATE for the experimental population. When $X$ is continuous, $\frac{\partial h(X, Z)}{\partial X}$ is the marginal effect. Given experiments that randomly change $X$, it is not surprising these treatment effects can be identified. When researchers are interested in effects such as CATE and marginal effects instead of ATE, it becomes even more important to improve precision because the variances of CATE and marginal effects are typically larger than the variance of ATE. I show that my method can be extended from linear to these nonlinear cases.

Lemma 10 Given Assumption 1.b, Equation 8 and Equation 9, two moment conditions
can be derived from experimental data:

\[
E[(Y_i - h(X_i, Z_i))X_i | G_i = E] = 0
\]
\[
E[(Y_i - h(X_i, Z_i))Z_i | G_i = E] = 0
\]

and one extra moment condition can be derived from the observational data:

\[
E[(Y_i - h(X_i, Z_i))Z_i | G_i = O] = 0
\]

Denote the vector of moment functions as \( g \):

\[
g_i(h(\cdot)) = \begin{bmatrix}
(Y_i - h(X_i, Z_i))X_i I(G_i = E) \\
(Y_i - h(X_i, Z_i))Z_i I(G_i = E) \\
(Y_i - h(X_i, Z_i))Z_i I(G_i = O)
\end{bmatrix}
\]

and the GMM estimator can be written as:

\[
(\hat{h}(\cdot)^{GMM}) = \arg\min \left[ \frac{1}{N} \sum_{i=1}^{N} g_i(h(\cdot)^{GMM}) \right]' \cdot W \cdot \left[ \frac{1}{N} \sum_{i=1}^{N} g_i(h(\cdot)^{GMM}) \right]
\]

The proof of this lemma is provided in Appendix A.6. The exact efficiency gain of incorporating this additional moment depends on the functional format of \( h \), what types of variables are available, and what the objectives are. Providing an analytical formula for this more general case is beyond the scope of this paper, but Appendix F offers some intuition. The key insight is that the local moment conditions of these nonlinear cases mimic the moment conditions in the linear cases, such that similar efficiency gain could be achieved.

One limitation of this class of non-linear model is that the unobserved \( U_i \) is additively separable from the observed \((X_i, Z_i)\). This additive separability is common in nonlinear GMM to ensure the construction of moment conditions that mimic the linear ones. When \( U_i \) is not additively separable, it is well known that the endogeneity is difficult to handle and has only been extensively studied in a few special cases. Appendix G discusses how a similar method can still be applied in binary probit model, of which the error term is not directly separable.

7 Robustness to Model Misspecification

My method can be generalized to settings when the experimental data is still sufficient for identification, but the observational model is misspecified. Recall that Equation 1 assumes a model with additive separability that rules out any interactions. Equation 8 assumes a more flexible nonlinear model with additive unobservables. Assumption 1.b assumes that distributions of unobservables are similar between observational and ex-
experimental units after conditioning on covariates. To alleviate the concerns that these assumptions may be violated in practice, I discuss strategies that help researchers determine: 1) whether they should incorporate the observational data and 2) how to tune the hyperparameter when the model may be misspecified. The method is still valuable if the goal is to minimize mean-squared error (MSE), which does not only include the bias but also the variance.

One strategy is to use cross validation, a popular procedure for tuning hyperparameters in the regularized regression. The key is to cross validate on out-of-sample experimental data. Intuitively, a better causal estimator should more accurately predict \( Y \) given a randomized \( X \). Researchers can therefore test the usefulness of a combined estimator \( \hat{\beta}_1^{\text{combine}} \) relative to the experiment-only estimator \( \hat{\beta}_1^{E} \) using out-of-sample experimental data. Appendix B gives a detailed discussion of this approach.

Another strategy is to average multiple GMM estimators given the recent development in the literature. The key is to recognize that two estimators can be generated when both observational and experimental estimates are available\(^8\): 1) an valid experiment-only estimate \( \hat{\beta}_1^{E} \) that does not require the additional assumptions, and 2) a more efficient estimator \( \hat{\beta}_1^{\text{combine}} \) that is potentially biased due to model misspecification. This setting fits into the classical Hausman specification test (Hausman (1978)), where researchers can decide to use the more efficient combined estimator if it is not significantly different from the less efficient one that only uses the experimental data. This setting also fits into the more recent framework developed by Cheng et al. (2019) that averages two GMM estimators, where one is always consistent, and the other one could be inconsistent due to misspecifications of moment conditions. The framework guarantees that given certain weights, the mean-squared-error can be improved. Following their framework, one can combine the two estimators:

\[
\hat{\beta}_1^{\text{combine,robust}} = w\hat{\beta}_1^{\text{combine}} + (1 - w)\hat{\beta}_1^{E}
\]

where the optimal weight \( w \) is determined by the relative variance and bias differences.

\[
w^* = \frac{\text{ImprovedVariance}}{\text{Bias}^2 + \text{ImprovedVariance}} = \frac{\text{V} (\hat{\beta}_1^{E}) - \text{V} (\hat{\beta}_1^{\text{combine}})}{(E[\hat{\beta}_1^{E}] - E[\hat{\beta}_1^{\text{combine}}])^2 + \text{V} (\hat{\beta}_1^{E}) - \text{V} (\hat{\beta}_1^{\text{combine}})}
\]

Therefore, when the combined estimator is unbiased, the optimal weight is \( w = 1 \), suggesting that one can safely use the combined estimate. When the combined estimator has large bias relative to the variance improvement, \( w \) approaches 0, suggesting that one should avoid using the combined estimate. When the bias is small, researchers can still use the combined method to improve the overall MSE.

\(^8\)I thank the anonymous associate editor for this valuable insight.
8 Simulation

This section uses simulation to illustrate the effectiveness of the method and how different factors affect the efficiency gain. I start with a linear causal model

\[ Y_i = 0.2X_i + 0.1Z_i + U_i \]  

(10)

where \( X_i \) is endogenously determined by \((Z_i, V_i)\) in the observational group but randomly assigned in the experimental group:

\[
X_i = \begin{cases} 
\gamma Z_i + V_i & \text{if } G_i = O \\
\text{drawn from } N(0, 1) \text{ randomly} & \text{if } G_i = E 
\end{cases}
\]

Unit \( i \) with characteristics \((Z_i, U_i, V_i)\) is drawn from a distribution unknown to researchers:

\[
(Z_i, U_i, V_i) \sim N \left( \begin{bmatrix} 1 & 0.4 & 0 \\ 0.4 & 1 & 0.4\sigma_v \\ 0 & 0.4\sigma_v & \sigma_v^2 \end{bmatrix} \right)
\]

\( V_i \) is the residual term in the observational first stage and therefore by definition uncorrelated with \( Z_i \). I set \( \sigma_v^2 = 1 - \gamma^2 \) such that \( V(X_i|G_i = O) = 1 = V(X_i|G_i = E) \) and \( \gamma^2 \) is the first-stage \( R^2 \). In addition, I assume a unit is randomly selected into the experimental group with probability \( 1 - \pi_O \) and randomly selected into the observational group with probability \( \pi_O \).

I draw 10000 samples from the above distribution, where each sample contains \( n_E = 100 \) experimental units. Within each sample \( s \), I calculate the experiment-only estimator \( \hat{\beta}_1^{E,s} \) and the combined GMM estimator \( \hat{\beta}_1^{Combine,s} \). Table 1 gives an example of the regression results estimated using one such sample, where the combined GMM estimator detects that the effect is statistically significant from 0.

Table 1: A sample where combined GMM estimator improves statistical significance

| Dependent variable: | \( Y \) |
|---------------------|--------|
| Experiment-Only     | 0.101  |
| Combined            | 0.182**|
| (0.115)             | (0.088)|

|                      | Experiment Data | Observational Data | Ground Truth |
|----------------------|-----------------|--------------------|--------------|
|                      | 100             | 1900               | 0.2          |

Note: *\( p < 0.1 \); **\( p < 0.05 \); ***\( p < 0.01 \)
After performing the estimation for each sample, I then summarize the bias, variance, and MSE across all samples. Figure 3 compares the distributions of these two estimators when first-stage $R^2 = 0.95$ and $\pi_O = 0.95$. It shows that the combined GMM estimator is unbiased and on average more accurate.

Figure 3: Distribution of Estimates Across 10,000 Samples

The vertical line represents the true value of $\beta_1$

To investigate the sensitivity of the efficiency gain I compare the MSE for each method under different values of $\pi_O$ and $R^2$. Figure 4a fixes the first-stage $R^2$ to be 0.95 and shows the relative MSE for different values of $\pi_O$. Figure 4b fixes the proportion of observational data at $\pi_O = 0.95$ and shows the relative MSE for different values of first-stage $R^2$. This result illustrates the prediction of Lemma 6, that the $\text{MSE} (\hat{\beta}_1^{\text{Combine}})$ is small if the proportion of observational data is large and $Z$ is a relevant variable.

Figure 4: Relative MSE $\frac{\text{MSE}(\hat{\beta}_1^{\text{Combine}})}{\text{MSE}(\hat{\beta}_1^{\text{Experiment Only}})}$ for different values of $\pi_O$ and first-stage $R^2$.

(a) Fix first-stage $R^2 = 0.95$, change $\pi_O$  
(b) Fix $\pi_O = 0.95$, change first-stage $R^2$

For what follows, I fix first-stage $R^2 = 0.95$ and $\pi_O = 0.95$ such that the relevance of $Z$ and the proportion of observational data are both high. I then compare the performance
of the combined method with other standard methods.

### 8.1 Comparison with Other Methods

Table 2 compares the combined GMM estimator with OLS and IV estimators that only use one dataset.

1. Experimental \( (\hat{\beta}_1^E): \) Regress \( Y_E = \beta_1 X_E + b_2 Z_E + \epsilon_E \)
2. OLS(Obs) \( (\hat{\beta}_1^{OLS}): \) Regress \( Y_O = \beta_1 X_O + b_2 Z_O + \epsilon_O \)
3. IV(Obs): \( \hat{\beta}_1^{IV} = (X_O'Z_O)^{-1}(Y_O'Z_O) \)

The last column of Table 2 documents the MSE of these estimators relative to that of the experiment-only estimator. The combined GMM estimator reduces the MSE by almost 50%. Methods based on observational data have high MSE due to high bias: OLS is biased because \( X \) is highly endogenous, and IV is biased because \( Z \) is not an instrumental variable.

Table 2: Summary statistics for the performance of estimators over 10,000 samples

| Method       | Bias  | Variance | MSE     | Relative MSE |
|--------------|-------|----------|---------|--------------|
| Experiment Only | -0.0007 | 0.0185 | 0.0185  | 1.0000       |
| Combined GMM  | -0.0006 | 0.0092 | 0.0092  | 0.4950       |
| OLS (Obs)     | 1.7899  | 0.0141 | 3.2177  | 173.9579     |
| IV (Obs)      | 0.5128  | 0.0009 | 0.2638  | 14.2626      |

To illustrate how often the improved efficiency changes inferences, Table 3 compares how often the estimated coefficient has the correct sign (positive), and how often it is statistically significant. Both experimental and combined estimators generate the correct signs for most of the samples. However, the estimate is only significantly positive for 32% of the time for experiment-only method, but 55% for the combined method.

Table 3: Summary of statistical significance \( (p < 0.05) \) over 10,000 samples

|               | Positive (Correct Sign) | Significantly Positive |
|---------------|-------------------------|------------------------|
| Experiment Only | 93.10%                  | 32.14%                 |
| Combined GMM   | 98.19%                  | 55.58%                 |

### 8.2 Estimation with Improved Experimental Design

Following Lemma 9, I demonstrate how to design experiments differently to further increase the efficiency gain of incorporating observational data. Instead of randomly assigning a unit into the experimental or observational group, experimenters can select an
experimental group that has large variation in the experimental $Z_i$ to more accurately estimate the violation of the exclusion restriction $b_2$. One method is to order the sampled units by $Z_i$, and assign a unit into the experimental group with non-zero probability only if the unit is in the top or bottom $Q$ quantiles.\footnote{To ensure at least $1 - \pi_O$ proportion of units are assigned into the experiment group, I require $Q > \frac{\pi_O}{2}$.} Let $\{q_i\}_{i=1}^N$ denote the quantiles of units ordered by $Z_i$, the assignment rule is:

$$p(G_i = E) = \begin{cases} 
\frac{1 - \pi_O}{2Q} & \text{if } q_i < Q \\
0 & \text{if } q_i \in [Q, 1 - Q] \\
\frac{1 - \pi_O}{2Q} & \text{if } q_i > 1 - Q 
\end{cases}$$

where $\frac{1 - \pi_O}{2Q}$ is an assignment probability that ensures $1 - \pi_O$ proportion of units are assigned into the experimental group. Figure 5 shows the relative MSE when I hold $1 - \pi_O = 0.05$ and sample experimental units from the top/bottom $Q$ quantiles for different values of $Q$. When $Q = 0.5$, the sampling procedure is equivalent to random sampling, and the efficiency gain is close to 50% as predicted by Lemma 7. When $Q = \frac{1 - \pi_O}{2}$, the sampling procedure is equivalent to assigning all the top and bottom $\frac{1 - \pi_O}{2}$ quantiles of units into the experimental groups and maximizes $\text{Var}(Z_i|G_i = E)$. The relative MSE is around 20%, indicating that only 20% of the experimental data is needed to achieve the same level of accuracy.

Figure 5: Relative MSE $\frac{\text{MSE}(\hat{\beta}_1^{\text{Combine}})}{\text{MSE}(\hat{\beta}_1^E)}$ when sampling experimental units from top/bottom $Q$ quantiles for different values of $Q$

9 Empirical Application: Online Hotel Ranking

I apply my method to measure the effect of website positions on clicks using the Expedia ranking dataset in Ursu (2018). The dataset consists of consumers’ search queries for
hotels on Expedia, with each search query containing a ranked list of hotels. For each hotel in a query, I observe its rank, characteristics, and click outcome. There are 4.5 million such observations: two thirds of the data are observational in which the hotel rankings are ordered by relevance, and one third of the data are experimental in which the hotel rankings are randomized.

Assume a linear causal model:

\[ \text{Click}_i = \beta_1 \text{Position}_i + \beta_2 f(\text{HotelCharacteristics}_i) + \epsilon_i \]  

(11)

where each observation corresponds to a queried-hotel \( i \). The position is the numerical rank of the hotel in that query, and I assume it to be correlated with other hotel characteristics in the observational data:

\[
\text{Position}_i = \begin{cases} 
 f(\text{HotelCharacteristics}_i) + V_i & G_i = O \\
 \text{Randomized} & G_i = E 
\end{cases}
\]

One challenge is to derive a relevant first-stage covariate \( Z_i \) to predict positions that may depend on locations and other characteristics. I select locations that have more than 10,000 observations and for each location apply a random forest procedure that uses the hotel characteristics to predict the hotel position in the observational data to estimate \( f \):

\[ Z_i \equiv f(\text{HotelCharacteristics}_i) \]

The experimental and observational datasets respectively contain \( N_E = 452,974 \) and \( N_O = 833,798 \) observations in these selected locations. Table 4 shows the observational first-stage regression in which \( Z \) is a relevant covariate that explains around 57% of variation in terms of \( R^2 \).

| Dependent variable: |  |
|--------------------|---|
| Position           |  |
| \( f(\text{HotelCharacteristics}) \) | 1.786*** (0.002) |
| Constant           | -2.285*** (0.021) |
| Observations       | 833,798 |
| \( R^2 \)          | 0.574 |
| Adjusted \( R^2 \) | 0.574 |
| Residual Std. Error| 7.033 (df = 833796) |
| F Statistic        | 1,123,260.000*** (df = 1; 833796) |

Note: *p<0.1; **p<0.05; ***p<0.01
9.1 Efficiency Gain Using a Random Experimental Sample

I examine the effectiveness of my method when units are randomly assigned into the experimental and observational groups. In order to evaluate the performance of different estimators, I approximate the true causal effect $\beta_1$ by estimating it using the original experimental dataset in these selected locations. I focus on a case when the observational group is large with $n_O = 20,000$ units and the experimental group is small with $n_E = 1,000$ units. This sample can be simulated by randomly drawing $n_O$ units from the observational data and $n_E$ units from the experimental data. Table 5 gives an example of the regression results estimated using one such sample, where the combined GMM estimator detects that the effect is statistically significant from 0.

Table 5: An Expedia sample where combined GMM estimator improves statistical significance

| Dependent variable: | Click |
|---------------------|-------|
|                     | Position | Experiment-Only | Combined |
|                     | Std Error | (0.00059) | (0.00048) |
|                     | Experimental Data (N) | 1,000 | 1,000 |
|                     | Observational Data (N) | 20,000 | |
|                     | Ground Truth | −0.0015 | −0.0015 |

Note: *p<0.1; **p<0.05; ***p<0.01

I repeatedly draw $M = 10,000$ such samples and perform estimation on each sample. Table 6 reports the bias, variance, and MSE of different estimators discussed in Section 8.1 across 10,000 replications. To characterize the efficiency gain, I compare the MSE of these methods with a benchmark that uses only a random sample of experimental data for estimation. The OLS using observational data has large bias due to endogeneity. The incorrect IV approach also has large bias, because the variable $Z_i$ violates the exclusion restriction and is not a valid instrumental variable. In comparison, the combined GMM method has a efficiency gain of 24.2% when using a first-stage covariate that explains 57% of variation in ranking. For the two unbiased estimators, Table 7 reports how often the increased efficiency changes the statistical significance at the 95% level. Both estimators have the correct sign 99% of the time. However, the estimate is only significantly negative for 72% of the time under the experiment-only method, but 82.5% of time under the combined GMM method.

---

10 This approximation is sufficient for approximating MSE because the size of the original experimental dataset is more than 100 times the size of the experimental sample.
Table 6: Summary for different estimators when units are randomly assigned into experimental and observational groups

| Estimator       | Bias$^2$ | Variance | MSE   | Relative MSE | Efficiency Gain |
|-----------------|----------|----------|-------|--------------|-----------------|
| Experiment Only | 0.000    | 0.384    | 0.384 | 1.000        | 0.000           |
| Combined GMM    | 0.001    | 0.286    | 0.287 | 0.746        | 0.254           |
| OLS (Obs)       | 0.608    | 0.042    | 0.650 | 1.691        | -0.691          |
| IV (Obs)        | 2.225    | 0.039    | 2.264 | 5.892        | -4.892          |

Notes: Bias$^2$, Variance, and MSE are all multiplied by $10^6$ for readability.

Table 7: Summary of statistical significance ($p < 0.05$) over 10,000 Expedia samples

| Method          | Negative (Correct Sign) | Significantly Negative |
|-----------------|-------------------------|------------------------|
| Experiment Only | 99.28%                  | 72.10%                 |
| Combined GMM    | 99.50%                  | 82.50%                 |

9.2 Efficiency Gain Given Improved Experimental Design

In the previous section I study an experimental procedure that randomly assigns units into the experimental group. In this section I examine alternative experimental designs that adjust the sampling procedure of experimental units. This adjustment can be achieved by assigning a unit to the experimental group with a probability that depends on $Z_i$ as discussed in section 8.2. The experimental group can be simulated by ordering the experimental population by $Z_i$, and drawing $nE$ units from the top and bottom $Q$ quantiles. The observational group can still be approximated by randomly drawing $nO$ units from the observational data.

To illustrate this alternative experimental design, Figure 6 shows the distribution of 1) all experimental $Z_i$, 2) $Z_i$ randomly sampled from the original experimental data, and 3) $Z_i$ sampled from the top/bottom 20% quantiles of the experimental data.
Figure 7 summarizes the distribution of estimates under the two experimental designs. The first figure summarizes a benchmark in which a random experimental sample is used for the estimation. The second figure summarizes the GMM estimators that combine the random experimental sample with a large observational sample. The third figure summarizes GMM estimators that combine a large observational sample with experimental units sampled from the top and bottom 20% quantiles ordered by $Z_i$. A comparison of the first and second figures shows that incorporating observational data into the experimental analysis can improve the efficiency. A comparison of the second and third figure shows that researchers should not only consider incorporating the observational data into the experimental analysis but also into the experimental design, which has even larger efficiency gain.

Figure 7: Distribution of estimates under different experimental designs and methods
Table 8 reports the bias, variance, and MSE of estimators when experiments are designed to sample units from the top and bottom $Q$ quantiles ordered by $Z_i$ for different values of $Q$. All of these alternative experimental designs have lower MSE than the design that randomly assigns units into the experimental group, but the bias and variance differ. Under a moderate adjustment in which an experimental unit is sampled from the top/bottom 30% or 20% quantiles, the efficiency gain is higher than 35% and the bias is negligible relative to the variance. The bias becomes larger given more extreme experimental designs that sample experimental units from the top/bottom 10% or 5% quantiles, but the overall efficiency gain is still higher than 40%. This exercise suggests that it is valuable to design an sampling procedure that increases $\text{Var}(Z_i|G_i = E)$ for a relevant first-stage covariate $Z_i$. A moderate adjustment of the experimental design can improve the $MSE$ by further reducing the variance at the cost of only slightly increasing the bias. But researchers should be cautious with a more extreme adjustment of the experimental design, which could lead to a bias increase that cannot be offset by the variance reduction.

Table 8: Summary for different estimators when units are randomly assigned into experimental and observational groups

| Sampling Method | Bias\(^2\) | Variance | MSE  | Relative MSE | Efficiency Gain |
|-----------------|------------|----------|------|--------------|-----------------|
| **Benchmark: Only Use Experimental Data** | | | | | |
| Random          | 0.000      | 0.384    | 0.384| 1.000        | 0.000           |
| **Combined GMM**| | | | | |
| Random          | 0.001      | 0.286    | 0.287| 0.746        | 0.254           |
| Top/Bottom 30%  | 0.000      | 0.248    | 0.249| 0.647        | 0.353           |
| Top/Bottom 20%  | 0.005      | 0.219    | 0.224| 0.583        | 0.417           |
| Top/Bottom 10%  | 0.020      | 0.192    | 0.211| 0.550        | 0.450           |
| Top/Bottom 05%  | 0.015      | 0.169    | 0.184| 0.479        | 0.521           |

Notes: $\text{Bias}^2$, Variance, and MSE are all multiplied by $10^6$ for readability.

### 9.3 Limitations of the Expedia Application

This application has several limitations. As discussed in Ursu (2018), the dataset has some selection bias and therefore the distribution of queried hotels in the experimental dataset is different from that in the observational dataset.\(^{11}\) However, this selection bias only supports the robustness of the combined method: even though Assumption 1 is violated because the observational and experimental units are drawn from slightly different distributions, incorporating the observational sample can still improve the precision of the experimental estimates. In addition to the distributions being different, the reduced form model in Equation 11 may be misspecified in several ways. First, the position effect can

\(^{11}\)Appendix D uses Hausman specification test to examine whether the similarity assumption is violated.
be non-linear. For example, changing from the second place to the first may be different from changing from the third place to the second. Second, I assume a linear probability model of click. Third, I assume that each observation is i.i.d but the positions and clicks may be negatively correlated within the same query. Alleviating these concerns requires developing a rich structural model of consumer search and estimating it using a full information maximum likelihood approach. Since dealing with model misspecification and developing structural model is not the primary goal of this paper, I leave this to future extension.

Despite these limitations, this dataset has several advantages for evaluating the effectiveness of my methods. First, the dataset contains both observational and experimental data, eliminating the need for simulation. Second, it contains covariates that satisfy the first-stage relevance condition. Variables, such as rating and price, are likely to affect hotel rankings. Third, the dataset is relatively large, allowing us to approximate the true causal parameter using the entire experimental data. An accurate approximation of the underlying truth allows us to evaluate parameters estimated using only a small subsample of the experimental data. I therefore believe this is still a useful empirical dataset for demonstrating the usefulness of the method.

10 Additional Empirical Applications

This section discusses additional examples under which my method is useful. A common reason for firms to analyze data in the past is to improve decisions and profit in the future. Firms are only interested in conducting such analyses if past customers are not too different from future customers, otherwise insights gained from the past are not useful for the future. For this type of firms, if the observational data include all customers that arrive in the past, and the experimental data include all customers that arrive in the near future, it is plausible to assume that these two populations are similar, and Assumption 1.a and 1.b hold. To improve future decisions, firms need to determine the size of the experiment as well as what to do after they obtain the experimental results.

My method can improve the profit by lowering the size of the experiment needed to achieve certain statistical accuracy. According to Feit and Berman (2019), the profit depends on the size of the experimental sample because larger experiments imply that more customers receive suboptimal treatments under randomization, leading to lower profit. Because my method can reduce the variance by up to 50%, only half of the experimental sample is required to attain the same statistical precision, thus improving the profit.

My method can also improve the profit when the size of the experiment is predetermined. As highlighted by Feit and Berman (2019), a common practice is that firms will pursue a strategy if its impact is statistically significant. Table 3 and 7 show that my method is more likely to detect statistical significance given the same experimental data.
10.1 Credit Loan

In general, it is difficult to find a dataset where both experimental and observational data are available in other contexts. To best demonstrate the efficiency gain of my method in another context, I simulated a semi-realistic setting based on a direct mail field experiment (Bertrand et al. (2010)) implemented by a consumer lender that randomizes creative content and interest rates. One parameter of interest in the experiment is how the offered interest rate affects the customer loan amount, an important managerial input that firms can use to optimize future offering of interest rates. Bertrand et al. (2010) assume a linear causal model to answer this question:

\[ \text{LoanAmount}_i = \beta_1 \text{InterestRate}_i + \beta_2 \text{ConsumerCharacteristics}_i + \epsilon_i \quad (12) \]

where consumer characteristics include variables such as credit risk as well as months-since-last-loan, \( \beta_2 \) is the vector of corresponding coefficient, and \( \beta_1 \) is the parameter of interest. The experiment is sufficient for identifying the impacts of interest rate on loan because the interest rate is randomly drawn from a distribution after stratifying on credit risk. However, in practice the interest rate is directly affected by many consumer characteristics. For example, the firm may give a lower interest rate to churned customers who have not made a loan for a few months. If researchers also have access to the large amount of business-as-usual direct mail data before the experiment, then my method can incorporate this observational data to improve the efficiency of the experiment estimate. Because Bertrand et al. (2010) does not include such observational data, additional assumptions are needed to simulate such a semi-realistic setting. I first follow Equation 12 to estimate the model using all the experimental sample and take the estimated parameters and unobservables as ground truth. I then assume that the interest rate is determined by a weighted average between the months-since-last-loan and unobservables, where months-since-last-loan receive 95% of the weight. Appendix E gives more detail of the simulation exercise. Figure 8 shows that the interest rate is random in the experimental data, but is negatively correlated with the months-since-last-order in the simulated observational data.

\(^{12}\)The OLS specification in Table (3) Column 4 of the paper.

\(^{13}\)For illustrative purposes, I focus on the customers in the high-risk stratum, whose interest rate is randomly drawn from the same distribution because the experiment is stratified. My method can be easily extended to other strata by combining experimental and observational data within those strata.

\(^{14}\)In practice, many other features are used for determining the interest rate offer, such as a continuous credit score. All these variables can be incorporated as well.
Similar to the Expedia application, I examine the effectiveness of my method when units are randomly assigned into the experimental and observational groups. I focus on a case when the observational group is large with 20,000 units and the experimental group is small with 1,000 units. Table 9 gives an example of the regression results estimated using one such sample, where the combined GMM estimator detects that the effect is statistically significant from 0.

Table 9: A sample where combined GMM estimator improves statistical significance

| Dependent variable: |  |
|---------------------|---------------|
| Click               |  |
| **Position**        | Experiment-Only: $-5.915$ | Combined: $-5.537^{**}$ |
| **Std Error**       | (3.558)       | (2.802) |
| Experimental Data (N) | 1,000        | 1,000   |
| Observational Data (N) | 20,000       |
| Ground Truth        | $-3.408$      | $-3.408$ |

*Note:* $^{*}p<0.1; ~^{**}p<0.05; ~^{***}p<0.01$

I repeatedly draw $M = 10,000$ such samples and perform estimation on each sample. Table 10 reports the bias, variance, and MSE of different estimators discussed in Section 8.1 across 10,000 replications. To characterize the efficiency gain, I compare the MSE of these methods with a benchmark that uses only a random sample of experimental data for estimation. The $OLS$ using observational data has large bias due to endogeneity. The incorrect IV approach also has large bias, because the variable $Z_i$ violates the exclusion restriction and is not a valid instrumental variable. In comparison, the combined GMM method has a efficiency gain of $45.7\%$. 

---

Figure 8: Distribution of interest rate vs months since last order
Table 10: Summary for different estimators when units are randomly assigned into experimental and observational groups

| Estimator       | Bias\(^2\) | Variance | MSE     | Relative MSE | Efficiency Gain |
|-----------------|------------|----------|---------|--------------|-----------------|
| Experiment Only | 0.007      | 16.796   | 16.803  | 1.000        | 0.000           |
| Combined GMM    | 0.002      | 9.127    | 9.129   | 0.543        | 0.457           |
| OLS (Obs)       | 12107.454  | 56.715   | 12164.169 | 723.928      | -722.928        |
| IV (Obs)        | 105.028    | 0.524    | 105.552 | 6.282        | -5.282          |

For the two unbiased estimators, Table 11 reports how often the increased efficiency changes the statistical significance at the 95% level. The combined GMM approach is more likely to detect that the effect of interest rate on loan is negative, and also more likely to detect that this effect is statistically significant.

Table 11: Summary of statistical significance (\(p < 0.05\)) over 10,000 Expedia samples

| Method          | Negative (Correct Sign) | Significantly Negative |
|-----------------|-------------------------|------------------------|
| Experiment Only | 80.38%                  | 13.99%                 |
| Combined GMM    | 87.61%                  | 21.18%                 |

10.2 Advertising

Consider an advertiser that runs advertising campaigns on a platform such as Google or Facebook that serves ads through auctions. After certain observational periods, the advertiser becomes interested in measuring the impacts and ROI of the campaign. The advertiser realizes that estimates obtained using past observational data may be biased due to endogeneity. They decide to collaborate with the advertising platform to run an experiment, and make future advertising decisions based on the ROI estimated using this experiment. If the ROI estimate is not significantly higher than a certain threshold, the advertiser may choose to stop advertising in the future. Hence, whether the ROI can be accurately estimated is crucial for advertiser’s future decision. Unfortunately, when the advertising effect is small, it is difficult to precisely estimate advertising ROI. For example, Lewis and Rao (2015) conducted several large-scale experiments with major U.S. retailers and document that the median standard error on ROI is 26.1%, implying a confidence interval over 100 percentage points wide. This wide confidence interval implies the advertiser may refuse to run a campaign that has high ROI of 50% because this ROI measure is not statistically significant from 0.

Because the observational data has variation in ad exposure and the advertising platform observes factors that may affect this variation, my method can be used to improve precision in this scenario. For example, because the ad is served through auction, and the focal advertiser has to be the highest bidder to show ads to customers, the bids of
competing advertisers are first-stage covariates that may generate variation in ad exposure. The competing bid is usually not used as an instrumental variable because it may be correlated with customer type and lead to biased estimates. Because my method can account for this bias, researchers can use competing bids and other variables that determine ad exposure as first-stage covariates to generate an additional bias-corrected IV estimate. This biased-corrected estimate can then be combined with the experiment-only estimate to improve the precision of advertising ROI.

Following Lemma 7, my method can reduce the variance by up to 50%, thus reducing the standard error on $ROI$ from 26.1% to 18.5% for a median experiment in Lewis and Rao (2015). This improved precision will change the advertiser’s decision for a campaign with a highly profitable 50% ROI: with the experiment-only method, the advertiser would stop running the campaign in the future because the ROI is not statistically significant; but with the combined method, the advertiser detects that the effect is significant and continue to run the profitable campaign.

10.3 Incentives

Ridershare platforms such as Uber and Lyft are often interested in how workers respond to wages and incentives. Causally measuring the elasticity of labor supply is crucial for satisfying consumer demand. The experimental approach is to randomly change the wages. However, because randomizing wages is controversial and may lead to public backlash, firms are sometimes only willing to conduct a small-scale experiment to minimize the risk. For example, Chen et al. (2019) mentioned that Uber has conducted some randomized wage experiments on a limited basis in several cities. One such experiment is conducted in Orange County, California that covers approximately 3,000 Uber drivers during April 2016. In the experiment, a random set of drivers receive an email indicating that the driver would receive a 10 percent increase in wages for 3 weeks. Chen et al. (2019) use this small-scale experiment to show that the results obtained using a larger observational dataset that covers 200,000 drivers are valid.  

In this setting, the experimental dataset is small, containing one market for three experimental weeks, and the observational dataset is large, containing all other non-experimental markets and weeks. In the experimental data, the wage is set randomly; in the observational data, the wage is affected by many factors observed by firms, such as local consumer demand and worker characteristics. Because workers’ willingness to work may be correlated with these factors, observational methods that incorrectly use these factors as IV may generate biased results. Because my method can account for this bias, researchers can derive an additional bias-corrected estimate that is uncorrelated with the experiment-only estimate to improve the efficiency.

\footnote{An implicit assumption behind this exercise is that observational and experimental population are similar, satisfying my Assumption 1}
11 Conclusion

In this paper, I develop a new method that combines experimental and observational data to improve estimation efficiency. I focus on a setting in which the experimental data are sufficient for identification but limited in size, and the observational data are large but suffer from endogeneity. The focal variable (e.g., price, ad exposure, and ranking) experiences random variation in the experimental data, but experiences endogenous variation in the observational data. I show that if the focal variable is affected by some observed covariates $Z$, additional moment conditions can be derived from observational data. This additional moment condition can be used to improved the accuracy of the experimental estimate.

I illustrate the magnitude of the efficiency gain in the case of a linear model, and I show that the method reduces the variance by up to 50% without increasing the bias. The efficiency gain is affected by the size of the observational data and the relevance of the first-stage covariates $Z$. Intuitively, an incorrect IV estimate obtained using the observational data is more useful when its bias is quantified and corrected. This bias, if the assumptions are satisfied, can be quantified by the experimental data. Using similar intuition, I discuss how the method can be generalized to nonlinear models with additive errors as well as probit models with non-additive errors.

I also demonstrate alternative experimental designs that can further reduce the variance at the potential cost of slightly increasing the bias. This improvement is achieved by assigning a unit $i$ into the experimental group with a probability that depends on $Z_i$. Intuitively, this alternative experimental design can more accurately estimate the bias of the incorrect IV estimate, allowing us to better leverage the observational data. I apply this combined method to the Expedia experiment and show that the method reduces the MSE by about 25% when experimental units are randomly selected and by about 40% when the selection of experimental units depends on the first-stage covariate $Z$.

My method is not without limitations. It is most useful when the unobserved error enters the outcome model additively, and when its distribution is similar in the experimental and observational samples after adjusting for covariates. These two conditions allow additional moment restrictions to be derived from the observational data. When these assumptions are satisfied, the combined estimator derived from my method strictly decreases the variance without increasing the bias. When these assumptions are moderately violated, the combined estimator can be thought of as a more efficiency estimator that may be biased due to misspecified moment conditions. Although my method may introduce additional bias in this scenario, my method can still improve the overall MSE by reducing the variance.
A Proof

A.1 Proof of Lemma 3 and 4

Lemma 3: \( \hat{\beta}_1^O \) and \( \hat{\beta}_1^E \) are uncorrelated.

**Proof**: Since \((\hat{\beta}_1^O, \hat{\gamma}^O)\) are estimated using observational data while \((\hat{\beta}_1^E, \hat{\beta}_1^E)\) are estimated using experimental data, these pairs of estimates are independent:

\[
(\hat{\beta}_1^O, \hat{\gamma}^O) \perp (\hat{\beta}_1^E, \hat{\beta}_1^E) \tag{13}
\]

Because \(X_i\) and \(Z_i\) in the experimental data are independent and are used to estimate \((\hat{\beta}_1^E, \hat{\beta}_1^E)\):

\[
\text{Cov}(\hat{\beta}_1^E, \hat{\beta}_1^E) = 0. \tag{14}
\]

Plugging Equations 13 and 14 into the covariance:

\[
\text{Cov}(\hat{\beta}_1^O, \hat{\beta}_1^E) = \text{Cov}(\hat{\beta}_1^{IV} - (\hat{\beta}_1^E / \hat{\gamma}^O), \hat{\beta}_1^E) = \text{Cov}(\hat{\beta}_1^{IV}, \hat{\beta}_1^E) - \text{Cov}(\hat{\beta}_1^E, \hat{\beta}_1^E) = 0
\]

Lemma 4: Under Lemma 3 the weighting is optimal if each estimate is weighted in inverse proportion to its variance:

\[
\frac{w_*^O}{w_*^E} = \frac{\text{Var}(\hat{\beta}_1^E)}{\text{Var}(\hat{\beta}_1^O)}
\]

**Proof**: 

\[
\text{Var}(w_0\hat{\beta}_1^O + w_E\hat{\beta}_1^E) = w_0^2\text{Var}(\hat{\beta}_1^O) + w_E^2\text{Var}(\hat{\beta}_1^E) + 2w_0w_E\text{Cov}(\hat{\beta}_1^O, \hat{\beta}_1^E) = w_0^2\text{Var}(\hat{\beta}_1^O) + w_E^2\text{Var}(\hat{\beta}_1^E) \tag{15}
\]

Taking the first order condition subject to the constraint that \(w_0 + w_E = 1\):

\[
w_0 = \frac{\text{Var}(\hat{\beta}_1^E)}{\text{Var}(\hat{\beta}_1^E) + \text{Var}(\hat{\beta}_1^O)}
\]

\[
w_E = \frac{\text{Var}(\hat{\beta}_1^O)}{\text{Var}(\hat{\beta}_1^E) + \text{Var}(\hat{\beta}_1^O)}
\]
A.2 Proof of Lemma 5

Recall that $b_2$ is defined in equation 5 as

$$b_2 \equiv \frac{Cov(Y_i - \beta_1X_i, Z_i| G_i = O)}{Var(Z_i| G_i = O)} = \beta_2 + \frac{Cov(U_i, Z_i| G_i = O)}{Var(Z_i| G_i = O)}$$

Then

$$\epsilon_i = Y_i - \beta_1X_i - b_2Z_i$$

$$= Y_i - \beta_1X_i - (\beta_2 + \frac{Cov(U_i, Z_i| G_i = O)}{Var(Z_i| G_i = O)})Z_i$$

$$= (Y_i - \beta_1X_i - \beta_2Z_i) - \frac{Cov(U_i, Z_i| G_i = O)}{Var(Z_i| G_i = O)}Z_i$$

$$= U_i - \frac{Cov(U_i, Z_i| G_i = O)}{Var(Z_i| G_i = O)}Z_i$$

In the observational data:

$$Cov(\epsilon_i, Z_i| G_i = O) = Cov \left( U_i - \frac{Cov(U_i, Z_i| G_i = O)}{Var(Z_i| G_i = O)}Z_i, Z_i| G_i = O \right)$$

$$= Cov(U_i, Z_i| G_i = O) - \frac{Cov(U_i, Z_i| G_i = O)}{Var(Z_i| G_i = O)}Cov(Z_i, Z_i| G_i = O)$$

$$= Cov(U_i, Z_i| G_i = O) - Cov(U_i, Z_i| G_i = O)$$

$$= 0$$

Given Assumption 1 or Assumption 1.b,

$$\epsilon_i = U_i - \frac{Cov(U_i, Z_i| G_i = O)}{Var(Z_i| G_i = O)} = U_i - \frac{Cov(U_i, Z_i| G_i = E)}{Var(Z_i| G_i = E)}$$

so the same relationship holds for the experimental data: $Cov(\epsilon_i, Z_i| G_i = E) = 0$.

A.3 Proof of Theorem 1

I provide a general proof for the multivariate case and later simplifies the results to the univariate case. Recall that the moment function is

$$g_i(\beta_1, b_2) = \begin{bmatrix} (Y_i - X_i\beta_1 - Z_ib_2)X_iI(G_i = E) \\ (Y_i - X_i\beta_1 - Z_ib_2)Z_iI(G_i = E) \\ (Y_i - X_i\beta_1 - Z_ib_2)Z_iI(G_i = O) \end{bmatrix}$$

with $\pi_E$ and $\pi_O$ proportion of units in the experimental and observational groups.
The inverse of the covariance matrix of the moment function $\Omega$ is:

$$
\Omega = E \left[ g_i(\beta_1, b_2) g_i(\beta_1, b_2)^T \right]
= \begin{bmatrix}
\pi E[\epsilon_i^2 X_i'X_i | G_i = E] & \pi E[\epsilon_i^2 X_i'Z_i | G_i = E] & 0 \\
0 & 0 & \pi O E[\epsilon_i^2 Z_i'Z_i | G_i = O] \\
\pi E[\epsilon_i^2 X_i'X_i | G_i = E] & 0 & 0 \\
0 & \pi E[\epsilon_i^2 Z_i'Z_i | G_i = E] & 0 \\
0 & 0 & \pi O E[\epsilon_i^2 Z_i'Z_i | G_i = O]
\end{bmatrix}
$$

which can be simplified under Assumption 2:

$$
\Omega = \begin{bmatrix}
\pi E[X_i'X_i | G_i = E] & 0 & 0 \\
0 & \pi E[Z_i'Z_i | G_i = E] & 0 \\
0 & 0 & \pi O E[Z_i'Z_i | G_i = O]
\end{bmatrix} \sigma^2
$$

Let $\Gamma$ be matrix of the derivatives of the moment functions with respect to $(\beta_1, b_2)$:

$$
\Gamma = \begin{bmatrix}
\Sigma_{g_i \beta_1} & \Sigma_{g_i b_2} \\
\Sigma_{g_i \beta_1} & \Sigma_{g_i b_2} \\
\Sigma_{g_i \beta_1} & \Sigma_{g_i b_2}
\end{bmatrix} = \begin{bmatrix}
X'_E X_E & 0 \\
0 & Z'_E Z_E \\
Z'_O X_O & Z'_O Z_O
\end{bmatrix}
$$

The sandwich formula gives:

$$
\Gamma' \Omega^{-1} \Gamma = \begin{bmatrix}
X'_E X_E + X'_O Z_O (Z'_O Z_O)^{-1} Z'_O X_O & X'_O Z_O \\
X'_O Z_O & Z'_E Z_E + Z'_O Z_O
\end{bmatrix}
$$

and the variance of the combined GMM estimator using $S = \Omega^{-1}$ as the weighting matrix is therefore

$$
V(\hat{\beta}_1^{GMM}, \hat{b}_2) = \sigma^2 \begin{bmatrix}
X'_E X_E + X'_O Z_O (Z'_O Z_O)^{-1} Z'_O X_O & X'_O Z_O \\
X'_O Z_O & Z'_E Z_E + Z'_O Z_O
\end{bmatrix}^{-1}
$$
With block inversion, the variance of \( \hat{\beta}_{1}^{GMM} \) can be written as

\[
V(\hat{\beta}_{1}^{GMM}) = \sigma^2[X'_{E}X_{E} + X'_{O}Z_{O}(Z'_{O}Z_{O})^{-1}Z'_{O}X_{O} - X'_{O}Z_{O}(Z'_{E}Z_{E} + Z'_{O}Z_{O})^{-1}Z'_{O}X_{O}]^{-1}
\]

\[
= \sigma^2[X'_{E}X_{E} + X'_{O}Z_{O}((Z'_{O}Z_{O})^{-1} - (Z'_{E}Z_{E} + Z'_{O}Z_{O})^{-1})Z'_{O}X_{O}]^{-1}
\]

When both \( X_{i} \) and \( Z_{i} \) are univariate random variables with 0 means, the variance can be simplified to

\[
V(\hat{\beta}_{1}^{GMM}) = \sigma^2 \left[ n_{E} \text{Var}(X_{i}|G_{i} = E) + \frac{n_{O}^2 \text{Cov}(X_{i}, Z_{i}|G_{i} = O))}{(n_{E} + n_{O})\text{Var}(Z_{i})\text{Var}(Z_{i}|G_{i} = O)} \right]^{-1}
\]

\[
= \sigma^2 \left[ n_{E} \text{Var}(X_{i}|G_{i} = E) + \frac{n_{O}^2 \text{Var}(Z_{i}|G_{i} = O))}{(n_{E} + n_{O})\text{Var}(Z_{i})\text{Var}(Z_{i}|G_{i} = O)} \right]^{-1}
\]

\[
= \frac{\sigma^2}{n_{E}} \left[ \text{Var}(X_{i}|G_{i} = E) + \pi_{O} \frac{\text{Var}(\gamma Z_{i}|G_{i} = O)\text{Var}(Z_{i}|G_{i} = E)}{\text{Var}(Z_{i})} \right]^{-1}
\]

The proof so far only makes use of Assumption 1.a. If Assumption 1 is satisfied such that \( \text{Var}(Z_{i}|G_{i} = E) = \text{Var}(Z_{i}|G_{i} = O) = \text{Var}(Z_{i}) \). The variance can be further simplified to

\[
V(\hat{\beta}_{1}^{GMM}) = \frac{\sigma^2}{n_{E}} \left[ \text{Var}(X_{i}|G_{i} = E) + \pi_{O} \frac{\text{Var}(\gamma Z_{i}|G_{i} = O)\text{Var}(Z_{i}|G_{i} = E)}{\text{Var}(Z_{i})} \right]^{-1}
\]

This completes the proof of Theorem 1.

### A.4 Proof of Lemma 8

The proof of Theorem 1 shows that:

\[
V(\hat{\beta}_{1}^{GMM}) = \frac{\sigma^2}{n_{E}} \left[ \text{Var}(X_{i}|G_{i} = E) + \pi_{O} \frac{\text{Var}(\gamma Z_{i}|G_{i} = O)\text{Var}(Z_{i}|G_{i} = E)}{\text{Var}(Z_{i})} \right]^{-1}
\]

The relative variance can be written as:

\[
\frac{V(\hat{\beta}^{E})}{V(\hat{\beta}_{1}^{GMM})} = 1 + \pi_{O} \frac{\text{Var}(\gamma Z_{i}|G_{i} = O)\text{Var}(Z_{i}|G_{i} = E)}{\text{Var}(Z_{i})\text{Var}(X_{i}|G_{i} = E)}
\]

\[
= 1 + \pi_{O} \frac{\text{Var}(Z_{i}|G_{i} = E)\text{Var}(\gamma Z_{i}|G_{i} = O)}{\text{Var}(Z_{i})\text{Var}(X_{i}|G_{i} = E)}
\]

\[
= 1 + \pi_{O} \frac{\text{Var}(Z_{i}|G_{i} = E)\text{Var}(\gamma Z_{i}|G_{i} = O)\text{Var}(X_{i}|G_{i} = O)}{\text{Var}(Z_{i})\text{Var}(X_{i}|G_{i} = O)\text{Var}(X_{i}|G_{i} = E)}
\]

This completes the proof for Lemma 8.

Note \( \frac{\text{Var}(Z_{i}|G_{i} = E)}{\text{Var}(Z_{i})} \) can be understood as the additional term that characterizes the relative variance of \( Z \) between observational data and experimental data because it can
be written as:

\[
\frac{\text{Var}(Z_i|G_i = E)}{\text{Var}(Z_i)} = \frac{\text{Var}(Z_i|G_i = E)}{\pi_O \text{Var}(Z_i|G_i = O) + \pi_E \text{Var}(Z_i|G_i = E)}
\]

\[
= \frac{1}{\pi_O \text{Var}(Z_i|G_i = O) + \pi_E}
\]

A.5 Combined GMM Estimator

The analytical solution to the GMM can be derived using the standard procedure. It is convenient to re-write the moment conditions and the minimization problem into matrix notation. Let

\[
A = \begin{bmatrix} X & Z \end{bmatrix}
\]

denote the matrix of independent variables. Let

\[
B = \begin{bmatrix} X_E & Z_E & 0 \\ 0 & 0 & Z_O \end{bmatrix}
\]

be the matrix that corresponds to the two conditions in the experimental data and one condition in the observational data, and

\[
\theta = \begin{bmatrix} \beta_1 \\ b_2 \end{bmatrix}
\]

be the vector parameter of interest. Then

\[
\frac{1}{N} \sum_{i=1}^{N} g_i(\hat{\beta}_1, \hat{b}_2) = \frac{1}{N} (B'(Y - A\theta))
\]

Under the optimal weighting matrix, \(W = (B'B)^{-1}\), GMM minimizes

\[(Y - A\theta)'B(B'B)^{-1}B'(Y - A\theta)\]

Let \(P \equiv B(B'B)^{-1}B'\) be the projection matrix. After taking the first order conditions with respect to \(\theta\), one can get:

\[
\hat{\theta} = (A'PA)^{-1}(A'PY)
\]
\( \hat{\theta} \) is unbiased because

\[
E[\hat{\theta}] = E[(A'PA)^{-1}(A'P(A\theta + \epsilon))]
= E[(A'PA)^{-1}(A'P(A\theta + \epsilon))]
= \theta + E[(A'PA)^{-1}(A'P\epsilon)]
= \theta + (A'PA)^{-1}A'B(B'B)^{-1}E[(B'\epsilon)]
= \theta
\]

where the moment conditions imply that

\[
E[(B'\epsilon)] = E \left[ \sum_i X_i \epsilon_i I(G_i = E) \right] \sum_i Z_i \epsilon_i I(G_i = E) \sum_i Z_i \epsilon_i I(G_i = O) = 0
\]

### A.6 Proof of Lemma 10

In the experimental data, by definition of Equation 9

\[
Y_i - h(X_i, Z_i) = Y_i - f(X_i, Z_i) - E[U_i|Z_i, G_i = E] = U_i - E[U_i|Z_i, G_i = E]
\]

Because \( X_i \) is randomized:

\[
E[(Y_i - h(X_i, Z_i))X_i|G_i = E] = E[(U_i - E[U_i|Z_i, G_i = E])X_i|G_i = E] = 0
\]

By law of iterated expectation:

\[
E[E[U_i|Z_i, G_i = g]Z_i|G_i = g] = E[U_iZ_i|G_i = g]
\]

\[
E[(Y_i - h(X_i, Z_i))Z_i|G_i = E] = E[(U_i - E[U_i|Z_i, G_i = E])Z_i|G_i = E] = 0
= E[U_iZ_i|G_i = E] - E[E[U_i|Z_i, G_i = E]Z_i|G_i = E]
= E[U_iZ_i|G_i = E] - E[U_iZ_i|G_i = E]
= 0
\]

Assumption 1.b implies that \( E[U_i|Z_i, G_i = E] = E[U_i|Z_i, G_i = O] \), such that

\[
E[(Y_i - h(X_i, Z_i))Z_i|G_i = O] = E[(U_i - E[U_i|Z_i, G_i = E])Z_i|G_i = O]
= E[(U_i - E[U_i|Z_i, G_i = O])Z_i|G_i = O]
= 0
\]
This section discusses cases when the observational model is plausibly misspecified. One popular strategy to make sure that the model is robust is cross validation. Since we are interested in the causal effect, the key is to cross validate on out-of-sample experimental data. Intuitively, a better causal estimator should more accurately predict $Y$ given a randomized $X$. Researchers can therefore test the usefulness of a combined estimator $\hat{\beta}_1^{\text{combine}}$ relative to the experiment-only estimator $\hat{\beta}_1^E$ using out-of-sample experimental data. It is possible that the experiment-only estimator is unbiased but the combined estimator has low MSE due to a reduction in variance. This validation strategy is different from a classical cross validation method that randomly splits the entire sample. In contrast, this strategy only splits a subset of datasets in which researchers believe the models are correctly specified, leveraging the assumption that the experimental data alone is sufficient for causal identification.

One implementation is to use a leave-one-out cross-validation (LOOCV) strategy that measures the cross-validation error:

$$CV = \frac{1}{n_E} \sum_{i,G_i=E} (Y_i - \hat{\beta}_1^{(-i)} X_i - \hat{b}_2^{(-i)} Z_i)^2$$

where $(\hat{\beta}_1^{(-i)}, \hat{b}_2^{(-i)})$ are estimated by omitting the $i$th observation in the experimental data. Researchers can calculate the LOOCV error for the experiment-only estimator $\hat{\beta}_1^{E,(-i)}$ and the combined GMM estimator $\hat{\beta}_1^{\text{combine},(-i)}$ to compare their average performance. This comparison helps researchers determine if incorporating observational data can reduce the MSE despite possible model misspecification.

Since this cross validation strategy can be used to compare estimators, it can help tune hyperparameters of a class of estimators, even when the observational model is misspecified. Next I demonstrate how to tune hyperparameters and improve efficiency using 1) the weighting method and 2) the regularization method.

### B.1 Weighting Method with Cross Validation

Recall that the weighting method combines two estimators $\hat{\beta}_1^E$ and $\hat{\beta}_1^O$, where $\hat{\beta}_1^E$ is an estimator that only depends on the experimental data. When the experimental model is correctly specified but the experimental dataset is small, $\hat{\beta}_1^E$ is an unbiased estimator with large variance. When the observational model is misspecified but the observational dataset is large, $\hat{\beta}_1^O$ is a biased estimator with small variance. Given the trade-off between the bias and the variance, $(w_E, w_O)$ are hyperparameters that require tuning. Researchers can pick the $(w_E, w_O)$ that has the lowest LOOCV to achieve the best out-of-sample performance.

This approach makes use of the ensemble intuition that combines multiple estimators
or models, where the weights are tuned using cross validation. The key difference is that only experimental data are used for validation.

### B.2 Regularized Regression with Cross Validation

Recall that the objective function in the regularized regression approach is:

$$
\min_{\hat{\beta}_1, \hat{\beta}_2, G_i \in E} \sum_i (Y_i - \hat{\beta}_1 X_i - \hat{\beta}_2 Z_i)^2 + \lambda (\hat{b}_1^{IV} - \bar{\beta}_1 - \frac{\hat{b}_2}{\hat{\gamma}_O})^2
$$

where $(\hat{b}_1^{IV}, \hat{\gamma}_O)$ are estimated using observational data and $\lambda$ is a hyperparameter that can be tuned. Intuitively, $\lambda$ should be large if the violation of this constraint can be mostly attributed to the inaccuracy of $(\hat{\beta}_1, \hat{\gamma}_O)$. Therefore $\lambda$ should be large if the observational dataset is large and the model is correctly specified. If the model is moderately misspecified such that the term $(\hat{b}_1^{IV} - \bar{\beta}_1 - \frac{\hat{b}_2}{\hat{\gamma}_O})$ is non-zero but small, then $\lambda$ should be smaller but still non-zero because it can reduce the variance.

This method is similar to other shrinkage methods in which the shrinkage hyperparameter is tuned using cross validation. The key difference is that instead of shrinking the estimates to 0, as in Lasso and Ridge Regression, the method shrinks estimates toward moments implied by the observational data and model assumptions.

### C Simulation: Robustness Check

This section uses simulation to illustrate the robustness of the method when the experimental data are sufficient for identification but the observational model is incorrectly specified.

#### C.1 Robustness to Interactions

I examine whether the method is still effective when the model is misspecified due to interactions between $X$ and $U$. Assume the researcher wrongly believes that the causal model follows Equation 10 but the actual causal model has an additional interaction term:

$$
Y_i = 0.2X_i + 0.1Z_i + \theta X_i U_i + U_i
$$

Figure 9 shows the relative MSE under different values of $\theta$. Overall the efficiency gain is still large, with a relative MSE of around 60% when $|\theta| = 2$. This is because the combined estimator happens to remain unbiased when $(Z_i, U_i, V_i)$ are normally distributed. Under this normality, incorporating observational data can still strictly improve the MSE by reducing the variance without increasing the bias. The efficiency gain decreases slightly as $|\theta|$ increases, because homoscedasticity in Assumption 2 no longer holds.
C.2 Robustness to Misspecified Covariance

Another form of model misspecification is that the distributions of \((Z_i, U_i, V_i)\) are actually different between the observational and experimental samples but researchers wrongly believe the distributions are identical. For example, the observational sample includes all users who arrive at the website in the past, and the experimental sample represents all users who arrive this week. When the two datasets are sampled at different points in time, they may have different distributions. The covariance \(\text{Cov}(Z_i, U_i|G_i = g)\) may therefore depend on \(g \in \{E, O\}\):

\[
(Z_i, U_i, V_i|G_i = g) \sim N\left(0, \begin{bmatrix}
1 & \rho_{zu}^g & 0 \\
\rho_{zu}^g & 1 & 0.4\sigma_v \\
0 & 0.4\sigma_v & \sigma_v^2
\end{bmatrix}\right),
\]

which violates Assumption 1 when \(\rho_{zu}^O \neq \rho_{zu}^E\). I fix \(\rho_{zu}^E\) at 0.4 and examine how different values of \(\rho_{zu}^O\) affect the efficiency gain. Figure 10 shows the relative MSE under different values of \((\rho_{zu}^E - \rho_{zu}^O) \in [0, 0.4]\). The combined GMM approach using the standard weights is more efficient than the experiment-only estimator if the difference in correlation is smaller than 0.2; however, the combined approach under the standard weight no longer improves the efficiency when the difference in correlation continues to increase. I also test the combined GMM approach with robust weighting discussed in Section 7. Although this robust approach is not as effective as the standard-weight GMM approach when the model is correctly specified, it is more robust to model misspecification. The cross-validation method still has non-negative efficiency gain when the correlation difference is around 0.3. Furthermore, when the correlation difference is as large as 0.4, the cross-validation approach only performs slightly worse than the experiment-only estimator.
This simulation suggests that researchers can still use the combined GMM approach when the experimental and observational data are slightly different. However, if researchers believe that the correlation between the observed attribute $Z_i$ and unobserved attribute $U_i$ are very different across the two datasets, they should either discard the observational data or use the robust weighting approach discussed in Section 7.

**D Robustness Check for Empirical Applications: Hausman Specification Test**

This section tests whether the observational model in the hotel ranking application is misspecified by comparing the combined estimator with the experiment-only estimator. To maximize the power of the test, instead of using a small sample of experimental data of size 1,000, I use all the experimental and observational data. Table 12 shows that the null hypothesis is not rejected at a significance of 0.05.

**E Simulation Detail for Credit Loan Application**

This section discusses how the observational data is simulated from the experimental data in the credit loan application. The exercise uses data from Bertrand et al. (2010) and focuses on the stratum of customers with high credit risk. Table 13 shows the experimental estimates for customers in this stratum, which will be used as the ground truth for simulating outcomes in the observational data.

I consider a scenario where researchers also have access to the large amount of business-as-usual direct mail data before the experiment. In this observational data, the interest rate is determined by a score calculated by a weighted average between months-since-last-loan and unobservables, such that months-since-last-loan has an $R^2$ of 95%.
Table 12: Hausman specification test for Expedia application

|                      | Experiment-Only ($\hat{\beta}^E_1$) | Combined ($\hat{\beta}^{Combined}_1$) |
|----------------------|--------------------------------------|---------------------------------------|
| Position ($\times 10^3$) | -1.4786                               | -1.4566                               |
| Std Error ($\times 10^3$)  | (0.02725)                             | (0.02412)                             |
| Experiment Data       | 452,974                               | 452,974                               |
| Observational Data    | 833,798                               |                                       |

Null hypothesis: Difference in coefficients not systematic.

\[
\text{chi}^2(1) = (\hat{\beta}^E_1 - \hat{\beta}^{GMM}_1)'[(V^E - V^{GMM})^{-1}] (\hat{\beta}^E_1 - \hat{\beta}^{GMM}_1) = 3.026
\]

Prob > chi2 = 0.0819

Table 13: Effects of interest rate on loan amount

|                      | Loan Amount |
|----------------------|-------------|
| Interest rate        | -3.409***   |
| (0.643)              |             |
| Months since last loan | -4.016***  |
| (0.258)              |             |
| Medium risk          | 136.770***  |
| (6.517)              |             |
| Observations         | 40,507      |
| R^2                  | 0.007       |
| Adjusted R^2         | 0.007       |
| Residual Std. Error  | 313.743 (df = 40504) |
| F Statistic          | 135.517*** (df = 2; 40504) |

*Note: *p<0.1; **p<0.05; ***p<0.01
ensure that the unconditional distribution of interest rate is realistic and similar to the experimental data, I draw the interest rate from the experimental data, and then assign the interest rate to individuals based on the score, such that individuals with higher score will be assigned with higher interest rate.

In practice, many other features are used for determining the interest rate offer, including a continuous credit score. If these features are observed, I can incorporate them into the simulation as well.

F Efficiency Gain for Nonlinear Models

This section discusses intuition over why the method can be used to improve efficiency in nonlinear cases in Equation 8. For illustrative purposes, consider the case when $Z$ is a continuous covariate observed by researcher, $X$ is a continuous variable, and the function is smooth in the continuous $X$ and $Z$. This assumption allows the partial derivatives of the function to be well defined.\footnote{Additional parametric assumptions may be needed for binary $Z_i$ and $X_i$.}

Consider the neighborhood of any $(X_0, Z_0)$. Let $b_2 \equiv \frac{\partial h(X_0, Z_0)}{\partial Z}$, and $\beta_1 \equiv \frac{\partial h(X_0, Z_0)}{\partial X}$. Then around the neighborhood of $(X_0, Z_0)$:

\[
h(X_i, Z_i) \approx h(X_0, Z_0) + \frac{\partial h(X_0, Z_0)}{\partial X}(X_i - X_0) + \frac{\partial h(X_0, Z_0)}{\partial Z}(Z_i - Z_0)
\]

\[
= h(X_0, Z_0) + \beta_1(X_i - X_0) + b_2(Z_i - Z_0)
\]

Let $\alpha_0 = h(X_0, Z_0) - \beta_1 \cdot X_0 - b_2 \cdot Z_0$ be the local intercept. Then the local moment conditions in these nonlinear models can be written as

\[
E[(Y_i - \alpha_0 - \beta_1 X_i - b_2 Z_i) X_i | G_i = E] \approx 0
\]

\[
E[(Y_i - \alpha_0 - \beta_1 X_i - b_2 Z_i) Z_i | G_i = E] \approx 0
\]

as well as an additional moment conditions from the observational data

\[
E[(Y_i - \alpha_0 - \beta_1 X_i - b_2 Z_i) Z_i | G_i = O] \approx 0
\]

Because these local moment conditions are similar to the moment conditions in the linear case, the accuracy of $\beta_1$ can also be improved based on Theorem 1, where the determinants of efficiency gain is the local variance and first-stage relevance of $Z_i$, as well as the proportion of observational data.
G Extension to Binary Probit

The paper mainly focuses on additively separable models and does not formally consider the case of discrete choice model. It is well known that the endogeneity in discrete choice models is difficult to handle and has only been extensively studied in a few special cases. In this section I illustrate how the observational data can be incorporated into the case of binary probit with endogenous and continuous regressors following a textbook example in Wooldridge (2010, Section 15.7.2), where:

\[ Y_i = \begin{cases} 
1 & \beta_1 X_i + \beta_2 Z_i + U_i > 0 \\
0 & \text{otherwise}
\end{cases} \]  

(17)

I hold the first stage-equation to be the same as in Equation 2:

\[ X_i = \begin{cases} 
\gamma Z_i + V_i & \text{if } G_i = O \\
\text{randomized} & \text{if } G_i = E
\end{cases} \]

The standard probit assumes that \((U_i, V_i)\) follows a standard normal distribution that is independent of the instrumental variable \(Z_i\). To make the case more general, I allow \(Z_i\) to be correlated with \(U_i\):

\[
(U_i, V_i | Z_i) \sim N \left( \begin{bmatrix} \rho_{zu} \times Z_i \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho_{uv} \sigma_v \\ \rho_{uv} \sigma_v & \sigma_v^2 \end{bmatrix} \right)
\]

\(Z_i\) is therefore not an instrumental variable because it either directly affects \(Y_i^*\), or correlated with unobservables \(U_i\).

As in my linear case, not all parameters can be separately identified in this setup and I need to aggregate some parameters for estimation convenience. I define \(b_2\) as

\[ b_2 \equiv \beta_2 + \rho_{zu} \]

which captures both the direct effect of \(Z\) on \(Y^*\) and an indirect correlation with the unobservables \(U\). Let \(\Theta\) denote the set of unknown parameters:

\[ \Theta = \{ \beta_1, b_2, \rho_{uv}, \gamma, \sigma_v \} \]

A maximum likelihood estimator that only uses the experimental data chooses \((\beta_1, b_2)\) to

---

17I maintain the assumption that \(\text{Cov}(Z_i, V_i | G_i = O) = 0\) since \(\gamma\) is a linear projection parameter. Since I assume \(X_i\) is determined by \((Z_i, V_i)\) and I do not consider simultaneous equation, I do not allow the distribution to depend on \(X_i\).
maximizes:

\[
\ln \mathcal{L}_E(\Theta) = \ln \prod_{i:G_i=E} P(Y_i|X_i, Z_i, \beta_1, b_2)
\]

The log-likelihood of the observational data can be written as

\[
\ln \mathcal{L}_O(\Theta) = \ln \prod_{i:G_i=O} P(Y_i, X_i|Z_i, \Theta)
= \ln \prod_{i:G_i=O} P(Y_i|Z_i, X_i, \Theta) P(X_i|Z_i, \Theta)
\]

The data can be combined by optimizing the sum of these two log-likelihoods:

\[
\max_{\Theta} \ln \mathcal{L}_E(\Theta) + \ln \mathcal{L}_O(\Theta)
\]

To understand why incorporating the observational data can help improve efficiency in this probit case, consider when the size of the observational data is infinite. \((\gamma, \sigma_v)\) are identified using the first-stage equation, where

\[
P(X_i|Z_i, G_i = O, \Theta) = \phi\left(\frac{X_i - \gamma Z_i}{\sigma_v}\right)
\]

Following Wooldridge, under normality, the second stage can be written as:

\[
P(Y_i = 1|X_i, Z_i, G_i = O, \Theta) = \Phi\left[\frac{\beta_1 X_i + b_2 Z_i + \frac{\rho_{uv}}{\sigma_v} (X_i - \gamma Z_i)}{\sqrt{1 - \rho_{uv}^2}}\right]
= \Phi\left[\frac{1}{\sqrt{1 - \rho_{uv}^2}} (\beta_1 + \frac{\rho_{uv}}{\sigma_v}) X_i + \frac{1}{\sqrt{1 - \rho_{uv}^2}} (b_2 - \frac{\rho_{uv}}{\sigma_v} \gamma) Z_i\right]
\]

A probit of \(Y_i\) on \((X_i, Z_i)\) on observational data can then consistently identify two constraints:

\[
C_1 = \frac{1}{\sqrt{1 - \rho_{uv}^2}} (\beta_1 + \frac{\rho_{uv}}{\sigma_v})
\]
\[
C_2 = \frac{1}{\sqrt{1 - \rho_{uv}^2}} (b_2 - \frac{\rho_{uv}}{\sigma_v} \gamma)
\]

Since \((\gamma, \sigma_v)\) are already identified in the first-stage, \((b_2, \beta_1, \rho_{uv})\) are the three unknowns left. If \(b_2 = 0\), the problem is reduced to an endogenous binary probit with \(Z_i\) being an instrumental variable; then \((\beta_1, \rho_{uv})\) are just identified under \(C_1\) and \(C_2\). However, because \(b_2\) is unknown, the observational data alone are not sufficient for identification.
Although the observational data are not sufficient for identification, it can help improve efficiency of the experimental data by imposing additional constraints onto the maximum likelihood problem:

$$\max_{\beta_1, b_2, \rho_{uv}} \ln L_E(\beta_1, b_2)$$
subject to
$$C_1 = \frac{1}{\sqrt{1 - \rho_{uv}^2}} (\beta_1 + \frac{\rho_{uv}}{\sigma_v})$$
$$C_2 = \frac{1}{\sqrt{1 - \rho_{uv}^2}} (b_2 - \frac{\rho_{uv}}{\sigma_v})$$

Intuitively, when the two constraints $C_1$ and $C_2$ are precisely measured, any deviations from these constraints must be attributed to the inaccuracy of $(\beta_1, b_2)$. Incorporating the observational data can therefore help improving the efficiency.

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