Identification of Partial Effects with Endogenous Controls

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Abstract

While control variables are often assumed to be exogenous, it is common to encounter endogenous controls in practice. This creates a dilemma: without controls, the treatment of interest may be endogenous; with controls, the endogeneity of the controls may contaminate identification. The problem may not be solved with an instrumental variable when it is only conditionally valid. For these scenarios, we provide identification of partial effects of treatment under a rank condition called measurable separability. For parametric models, our approach amounts to employing a nonparametric estimator of a nesting model. For nonparametric models, our results show that endogenous controls are generally innocuous under the usual identifying conditions, except in the case of "bad controls." We further propose a test for endogenous controls. Simulation results and an empirical application demonstrate this prevalent issue and provide practical implications of our methods.

Keywords: endogenous control, average partial effects, local average response, nonseparable model.

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1 Introduction

We study a critical and prevalent, yet often overlooked, issue in empirical research: the endogeneity of control variables. In models with endogenous treatments, researchers often leverage conditional independence or instrumental variables (IV) to identify treatment effects, while rather casually assuming the control variables are exogenous.

In applications, these additional control variables are often included either because they are relevant for both the treatment variable and the outcome, or because the exogeneity condition of the IV is more likely to hold when these controls are included. However, empirical researchers often end up with control variables that are subject to additional endogeneity concerns, while finding instruments for every endogenous control can be challenging or impossible.

In this paper, we demonstrate how endogeneity in control variables affects the identification of partial effects of treatment and, when it does, how the issue can be addressed in certain scenarios.

1.1 Illustration in the Simplest Case

To illustrate the problem, let's consider the following linear model:

$$Y = \tau W + \xi X + \varepsilon, \ E[\varepsilon|W, X] = E[\varepsilon|X], \tag{1}$$

where W is a treatment variable of interest, the control variable X is a determinant of the outcome Y and likely of W, too, and ε is an unobserved determinant. While it is assumed that ε is mean independent of W conditional on X, ε may not be mean independent of X. For example, if the functional form of X is misspecified and some nonlinear effects of X end up being part of ε, then the treatment is indeed conditionally independent while ε is dependent on X. Generally, this is more than a specification issue, although, as we will see in a moment, in many cases this can be mitigated by a flexible functional form.

There are two consequences of the model above. First, without considering X, the dependence between W and X can cause bias in the OLS estimation. Second, with the presence of X, the dependence between X and ε can also pollute the identification of the average partial effect (APE) τ even if W is conditionally independent of ε. In either case, τ is not identified by the linear projection parameters, so the OLS estimator is inconsistent and biased. The bias in the first case can be interpreted as an omitted variable bias. To see the bias in the second case, let $D = (W, X)$ and $\theta = (\tau, \xi)'$, the linear projection parameters are defined as follows:

$$\gamma_{LP} \equiv E[D'D]^{-1}E[D'Y] = \theta + E[D'D]^{-1}E\left[D'E[\varepsilon|X]\right].$$

Therefore, without further restriction on $E[\varepsilon|X]$, OLS does not produce a consistent estimate for θ, or τ in particular. In a worse scenario, X could be an outcome of W, in which X is referred to as "bad control" in Angrist and Pischke (2009). With the presence of bad control, the conditional independence is not likely to hold (Lechner, 2008), and it is also noted that the bad control can cause problems for identification even if treatments are randomly assigned (Wooldridge, 2005).

However, as is shown below, τ can still be identified as long as X is not solely a function of W. More formally, this extra condition is referred to as the measurable separability, as first introduced in Florens et al. (1990):

Definition 1. *W and X are measurably separated if, any function of W almost surely equal to a function of X must be almost surely equal to a constant.*

At its essence, this assumption ensures that we can vary the value of W while holding X at a particular value. Note that this still allows the distribution of X to depend on W, and vice versa. Although it does not rule out the bad control directly[1], it rules out situations where conditional independence is not likely to hold. Throughout the paper, we let $F(A, B, C)$, $F(A, B)$, and $F(A)$ denote the distribution functions of (A, B, C), (A, B), and A, respectively. Then, under this condition, for a continuous treatment W, τ is nonparametrically identified as follows:

$$\int_{\mathcal{W}\times\mathcal{X}} \partial_w E[Y|W = w, X = x]dF(w, x) = \tau + \int_{\mathcal{W}\times\mathcal{X}} \partial_w(x\xi + E[\varepsilon|X = x])dF(w, x) = \tau,$$

where the last result holds due to the measurable separability of W and X. To see why this condition is necessary, suppose the measurable separability does not hold, e.g., $X = f(W)$ almost surely and they are not constants, then conditioning on $W = w, X = x$ necessitates $W = w, X = f(w)$. In that case, we would have $\int_{\mathcal{W}\times\mathcal{X}} \partial_w(f(w)\xi + E[\varepsilon|X = f(w)])dF(w, x) \neq 0$.[2]

[1] For example, $X = W + e$ where $e \perp\!\!\!\perp W$, then we can vary W while fixing X, but in terms of potential outcome $X(w) \neq X$.

[2] Given the conditional mean independence and the measurable separability of W and X, indeed the

In the case of a binary W, measurable separability between W and X allows for conditioning on $W = 1$ and $W = 0$ at different values of X. Combining with the conditional independence condition, the identification of τ is achieved as follows:

$$\int_{\mathcal{X}} \left(E[Y|W = 1, X = x] - E[Y|W = 0, X = x] \right) dF(x) = \tau.$$

Example 1. *As a concrete example, consider the linear regression model relating district-level average test scores (avgscore) to district-level educational expenditure per student (expend) and average family income (avginc) from Wooldridge, 2019, Chapter 3:*

$$avgscore = \alpha + \tau \cdot expend + \xi \cdot avginc + \varepsilon.$$

Suppose we are interested in the partial effect of expend, τ. Since avginc is relevant for expend at the district level and avginc can also affect avgscore through other channels, e.g., private tutoring, including avginc as a control variable is sensible. However, avginc may also be correlated with other unobserved determinants that affect both avginc and avgscore, then the endogeneity of avginc can pollute the identification of (τ, ξ) and OLS fails to produce consistent estimates. However, τ is nonparametrically identified as long as: (1) expend is independent of ε conditional on avginc and (2) expend and avginc are measurably separated.

1.2 Does an IV Approach Solve the Problem?

When the instrument is genuinely exogenous and affects the outcome solely through the treatment, the answer is yes. However, in practice, control variables are often included to either increase efficiency or to strengthen the validity of the IV. In these cases, again, the endogeneity in those controls can cause problems for identification. To illustrate, let's

identification result can extend to nonparametric regression models such as

$$Y = f(W) + h(X) + \varepsilon, \;\; E[\varepsilon|W, X] = E[\varepsilon|X].$$

From the conditional mean function

$$E[Y|W = w, X = x] = f(w) + h(x) + E[\varepsilon|W = w, X = x] = f(w) + h(x) + E[\varepsilon|X = x],$$

$f(w)$ is identified due to the measurable separability, even if $h(x)$ is not identified (i.e. $E[\varepsilon|X = x] \neq 0$). Note that this result does not require differentiability of $E[Y|W = w, X = x]$ or $f(w)$.

consider the linear model again with an excludable IV:

$$Y = \tau W + \xi X + \varepsilon, \;\; E[\varepsilon|W, X] \neq E[\varepsilon|X]$$
$$W = \pi_Z Z + \pi_X X + \eta.$$

In this model, the IV is needed because W is not conditionally independent of ε even after conditioning on X. However, without controlling X, Z may not be a valid IV if Z affects Y through X too. Therefore, again, the endogeneity of X brings a dilemma, and neither τ nor ξ is identified by the usual IV or 2SLS projection. Nevertheless, τ can be nonparametrically identified through a control function approach[3]: If Z is independent of (ε, η) conditional on X, then W is independent of ε conditional on X and η. Accordingly, given the identification of η and the measurable separability between (X, η) and W[4], τ is identified:

$$\begin{aligned} &\int_{\mathcal{W}\times\mathcal{X}\times\mathcal{E}} \partial_w E[Y|W = w, X = x, \eta = e] dF(w, x, e) \\ =&\tau + \int_{\mathcal{W}\times\mathcal{X}\times\mathcal{E}} \partial_w E[\varepsilon|X = x, \eta = e] dF(w, x, e) = \tau. \end{aligned} \tag{2}$$

Example 2. *Consider a linear triangular model relating the individual wage to school attendance. In an influential paper that studies the causal impact of compulsory school attendance on earnings, Angrist and Krueger (1991) use quarter of birth (qbirth) as an instrument for educational attainment (totaledu) in wage equations, based on the observation that school-entry requirement and the compulsory schooling laws compel students born in the end of the year to attend school longer than students born in other months. However, this approach is also subject to some critiques that qbirth may not be truly exogenous because, for example, the family income level could affect conception planning*[5]*, which in turn affects birth. Therefore, researchers may consider including the parents' income (parinc) as a control. The heuristic model can be specified as follows:*

$$log(wage) = \alpha + \tau \cdot totaledu + \xi \cdot parinc + \varepsilon,$$
$$totaledu = \pi_0 + \pi_1 \cdot qbirth + \pi_2 \cdot parinc + \eta.$$

[3] It is not the only way of identification. In this linear triangular model, τ is also identified by $E[\partial_Z E[Y|X, Z]]/E[\partial_Z E[W|X, Z]] = \tau$.

[4] Because $W = Z\pi_Z + X\pi_X + \eta$ with $\pi_Z \neq 0$, the measurable separability between Z and X also implies the measurable separability between (X, η) and W in this case.

[5] For example, high-income households may have more control and flexibility on conception timing to deliberately avoid late birth for better schooling planning.

However, the way parents' income affects children's wages varies and can be further correlated with other socioeconomic determinants, which makes it a potential endogenous control. In that case, IV or 2SLS does not yield consistent estimates for τ with or without including parinc linearly, but τ can be identified nonparametrically as (2).

1.3 More General Settings

Given the simple illustration of the endogenous control problem above, it seems that nonparametric methods are more immune to the endogenous control problem. This is first argued in Frölich (2008), but many questions remain: First, to what extent are the nonparametric methods immune to the endogenous control? For example, some extra regularity conditions may be needed, such as the aforementioned measurable separability, and it is important to know the implications of such restrictions. Furthermore, we would like to ask how general a class of models it can be where the nonparametric methods are valid under endogenous controls. If we can define such an admissible class of models, then the next question is, should researchers always use nonparametric methods? It is well-known that nonparametric methods may not be efficient, at the cost of being robust to specification, and, in this case, to endogenous control. Ideally, we would like to have a testing procedure that informs us about the potential endogenous controls in a general class of models.

As the first contribution, we study a class of parametric models that are nested in a nonseparable and nonparametric model. We show that under the usual conditional independence condition, the identification of the average response parameters is possible with the extra measurable separability condition in Section 2.1. Since the average response of a nonseparable model coincides with the average partial effects of any nested models under the usual conditional independence assumption, researchers can instead resort to the nonseparable model to avoid the endogenous bias caused by the controls. An analogous result for a nonseparable triangular model with IVs is also provided in Section 2.2.

Based on these results, we further propose a test for endogenous controls in Section 3. For linear models, the test follows from the general idea of Hausman's specification test. For more general setups, the test is based on an asymptotic linear representation of the difference of two estimators. Particularly, we focus on the average derivative estimator through series approximation and inference using the bootstrap. In a simulation study, we present some examples of this prevalent issue and showcase the finite sample performance of our methods. To illustrate our methods and provide practical implications, we revisit a classic study of the import-competition impact on labor market outcomes by Autor et al. (2013). We demonstrate how the inclusion of more control variables is likely to correct the bias from the concern of IV validity, while it may fail to correct the other source of bias due to the endogeneity of the control variables themselves, and how our approach can be utilized as a robustness check.

The issue of endogenous controls is prevalent in empirical research but is not well studied in econometrics literature. One exception outside our setting is regarding the regression discontinuity (RD) design, where Kim (2013) finds that endogenous control variables yield asymptotic bias in the RD estimator while the inclusion of these relevant controls may offset this bias and improve some higher-order properties of the estimator. Diegert et al. (2022) assesses the omitted variable bias when the controls are potentially correlated with the omitted variables in a sensitivity analysis framework. In a recent paper by Andrews et al. (2025), the issue of endogenous controls is attributed to misspecification, and their method of "strong exclusion" amounts to projecting out from the instrument Z a conditional mean function of Z given X, which is conceptually and econometrically equivalent to including more flexible functional forms of X as the control function.

The rest of the paper is outlined as follows: The main identification results are given in Section 2; The endogenous control test is proposed in Section 3. Section 4 presents the simulation study that compares our methods and those not robust to endogenous controls. Sample coverage and power results of our test are also presented. Section 5 provides the empirical application to illustrate the practical implications of our methods. Section 6 concludes the paper with empirical recommendations.

2 Nonseparable Models with Endogenous Controls

To investigate the impact of endogenous control variables in a more general setting, we consider a class of models as follows:

$$Y = m(W, X, \varepsilon) = h(W, X, \varepsilon; \theta) \tag{3}$$

where m is nonparametric but can be parameterized by h. Suppose h is known to the researcher up to the unknown parameters θ. A similar nonparametric and nonseparable model $Y = m(W, \varepsilon)$ has been studied by Altonji and Matzkin (2005) except that they consider X as the excluded instruments that do not enter the outcome equation.[6] With similar notation, we refer to the parameters of interest as average response (AR), local AR (LAR), and conditional LAR (CLAR), and they are defined as follows: For a continuous treatment W and a continuous outcome Y,

$$\begin{aligned}
\text{CLAR}: &\ \beta(w,x) = \int \partial_w m(w,x,\epsilon) f_{\varepsilon|W=w,X=x}(\epsilon) d\epsilon, \\
\text{LAR}: &\ \beta(w) = \int \beta(w,x) f_{X|W=w} dx, \\
\text{AR}: &\ \beta = \int \beta(w) f_W(w) dw
\end{aligned}$$

If Y is a binary outcome, we can write

$$\begin{aligned}
m(W,X,\varepsilon) =& 1\{m^*(W,X,\varepsilon) > 0\}, \\
h(w,X,\varepsilon;\theta) =& 1\{h^*(w,X,\varepsilon;\theta^*) > 0\},
\end{aligned}$$

where m^*, h^*, and θ^* are implicitly defined. Following Altonji and Matzkin (2005), we partition ε as (u,v) and implicitly define $u^* = u^*(W,X,v)$ as a solution of $m^*(W,X,u^*,v) = 0$. Suppose that, for fixed (w,x,v), $m^*(w,x,u,v)$ has at least one root in u and that $m^*(w,x,u,v)$ is strictly monotonic in u, then $u^*(W,X,v)$ is uniquely defined. Additionally, suppose $m^*(w,x,u,v)$ is continuously differentiable in w and u and that $\partial_u m^*(w,x,u^*,v) \neq 0$, then by implicit function theorem $u^*(w,x,v)$ is continuously differentiable in w. Then, we can define the binary response parameters as

$$\begin{aligned}
\text{BCLAR}: &\ \beta^*(w,x) = \int -\partial_w u^*(w,x,v) f_{u,v|W=w,X=x}(u^*(w,x,v),v) dv, \\
\text{BLAR}: &\ \beta^*(w) = \int \beta^*(w,x) f_{X|W=w} dx, \\
\text{BAR}: &\ \beta^* = \int \beta^*(w) f_W(w) dw
\end{aligned}$$

For a binary treatment, we can define a set of parameters analogously by replacing the derivatives above with the differences. For our main results, we will focus on the continuous treatment case to simplify the exposition.

[6] Our main motivation for model (3), which differs from Altonji and Matzkin (2005), is that empirical researchers often seek to include X as control variables in the outcome equation.

Example 3. *Consider a parametric nonseparable model* $Y = h(W, X, \varepsilon; \theta) = 1\{\tau W + \xi X + \varepsilon > 0\}$ *where* $\theta = (\tau, \xi)$ *and* ε *is Gaussian. Suppose we can partition* ε *as* $\varepsilon = u + v$*, and* v *is also Gaussian. Note that* $-u^*(W, X, v) = \tau W + \xi X + v$*, then* $-\partial_w u^*(w, x, v) = \tau$ *and*

$$\begin{aligned}
\beta(w,x) &= \tau \int f_{u,v|W=w,X=x}(-(\tau w + \xi x + v), v) dv \\
=&\tau \int f_{\varepsilon,v|W=w,X=x}(-(\tau w + \xi x), v) dv = \tau f_{\varepsilon|W=w,X=x}(-(\tau w + \xi x)) \\
=&\frac{\tau}{\sigma(w,x)} \phi\left(\frac{-(\tau w + \xi x) - \mu(w,x)}{\sigma(w,x)}\right)
\end{aligned}$$

where the second equality uses the fact that $\varepsilon = u + v$ *and the change-of-variables;* $\mu(W, X)$ *and* $\sigma(W, X)^2$ *are the conditional mean and variance since we don't assume* ε *is independent of* (W, X)*. If* $\varepsilon|W, X \sim N(0, 1)$*, then* $\beta(w, x) = \tau\phi(\tau w + \xi x)$*, which reduces to the usual average partial effect of a probit model.*

2.1 Identification under Conditional Independence

We first consider the scenario where ε is conditionally independent of W given X, but the dependence between X and ε is unrestricted.

Assumption 1. $\varepsilon \perp\!\!\!\perp W \mid X$.

Assumption 1 is a key condition for identifying the response parameters. Importantly, it does not rule out X being endogenous. We emphasize that Assumption 1 itself may not be sufficient for identification due to the endogeneity of X: e.g., if W is some function of X only, which is dependent on ε, then the response parameters would not be identified by Assumption 1, and, in which case, even Assumption 1 itself is not likely to hold. Thus, an additional restriction is required to rule out such extreme cases. The next theorem shows that the measurable separability condition between the treatment and the control suffices for the identification of the response parameters of treatment.

Theorem 1. *Suppose* W *and* X *are measurably separated and Assumption 1 holds.*

1. *If* Y *is continuous, suppose* $m(w, x, e)$ *is differentiable in* w *and there exists an integrable dominating function of* $\partial_W m(W, X, \varepsilon)$*. Then,* $\beta(w, x) = \partial_w E[Y|W = w, X = x]$*,* $\beta(w) = E[\partial_w E[Y|W = w, X]|W = w]$*, and* $\beta = E[\partial_W E[Y|W, X]]$*.*

2. *If Y is binary, suppose $m^*(w,x,u,v)$ has at least one root and is strictly monotonic in u. Additionally, suppose $m^*(w,x,u,v)$ is continuously differentiable in w and u and $\partial_u m^*(w,x,u^*,e) \neq 0$, then $\beta^*(w,x) = \partial_w E[Y|W=w,X=x]$, $\beta^*(w) = E[\partial_w E[Y|W=w,X]|W=w]$, and $\beta^* = E\left[\partial_W E[Y|W,X]\right]$.*

Proof. By Assumptions 1 and the measurable separability between W and X, we have, for continuous W,

$$\partial_w f_{\varepsilon|W=w,X=x}(\epsilon) = \partial_w f_{\varepsilon|X=x}(\epsilon) = 0.$$

Then, applying Leibniz integral rule under the existence of an integrable dominating function and the chain rule gives

$$\begin{aligned}
&\partial_w E[Y|W=w,X=x] = \partial_w \int m(w,x,\epsilon) f_{\varepsilon|W=w,X=x}(\epsilon)d\epsilon \\
=& \int \partial_w m(w,x,\epsilon) f_{\varepsilon|W=w,X=x}(\epsilon)d\epsilon + \int m(w,x,\epsilon)\partial_w f_{\varepsilon|W=w,X=x}(\epsilon)d\epsilon \\
=& E[\partial_w m(w,x,\varepsilon)|W=w,X=x] = \beta(w,x)
\end{aligned}$$

The other two statements for the continuous-Y case simply follow from the definition of the conditional expectation.

When Y is binary, we can write

$$\begin{aligned}
&E[Y|W=x,X=x] = \int 1\{m^*(w,x,e)>0\} f_{\varepsilon|W=w,X=x}(e)de \\
=& \int_{1\{m^*(w,x,e)>0\}} f_{e|W=w,X=x}(e)de = \int \left[\int_{u>u^*(w,x,v)} f_{\varepsilon|W=w,X=x}(u,v)du\right] dv.
\end{aligned}$$

By Lebniz rule, chain rule, Assumption 1, and the measurable separability between W and X, we have

$$\partial_w E[Y|W=x,X=x] = \int -\partial_w u^*(w,x,v) f_{\varepsilon|W=w,X=x}(u^*(w,x,v),v)dv = \beta^*(w,x).$$

The other two statements for the binary-Y case follow from the definition of the conditional expectation, too.

□

Theorem 1 shows that even if ε and X are potentially dependent, the response parameters associated with the treatment can still be identified by the usual conditional expectations as long as the conditional independence and the measurable separability condition hold. This is a positive result: it justifies the prevalent use of potentially endogenous control variables, since the additional measurable separability condition is often reasonable in practice and holds in many common settings. Meanwhile, we have seen in previous examples that common linear parametric models are not immune to the endogeneity of control, so this result also encourages the use of fully flexible models in these scenarios. In the next example, we also show that a nonlinear parametric model is not immune to endogeneity of controls, while resorting to a nesting nonseparable model can get around this problem.

Example 3 continued. *For the binary model* $Y = 1\{\tau W + \xi X > \varepsilon\}$*, suppose* $\varepsilon | W, X \sim N(\mu(X), \Sigma(X))$*, i.e. Assumption 1 holds for this model. By Theorem 1,* β^* *is identified nonparametrically, which is also the common average partial effect of treatment on the response probability, i.e.* $E[\partial_W P(Y = 1|W, X)] = E\left[\frac{\tau}{\Sigma(X)}\phi\left(\frac{\tau W + \xi X - \mu(X)}{\Sigma(X)}\right)\right]$*. However, ignoring the endogeneity of the controls and employing a standard probit approach would produce an inconsistent estimate of the average partial effect.*

By its definition, measurable separability simply requires that W and X are not functions of each other only. It is intuitive, and the reasoning could be straightforward in practice for most economic variables. For identification purposes, this condition can be viewed as a very mild rank restriction. For further discussion, readers are referred to Florens et al. (2008), where they also provide primitive conditions on the data generating process under which measurable separability between two random variables is guaranteed.

2.2 Identification with IV

When the conditional independence condition is not plausible, it is common to consider an excluded IV for identification. In applications, additional control variables are often included to make the exogeneity condition of the IV more likely to hold. Although control variables are explicitly or implicitly assumed to be exogenous, we caution that they may be endogenous in practice, while finding IV for all endogenous controls is not possible. In this section, we study a nonseparable triangular model similar to the one in Imbens and Newey (2009) while explicitly allowing for endogenous controls.

We consider model 3 again, except now we don't impose the conditional independence between W and ε. Instead, suppose there exists an excludable variable independent of (ε, η), at least conditionally:

Assumption 2. $W = q(Z, X, \eta)$, $Z \perp\!\!\!\perp (\varepsilon, \eta) \mid X$.

Note that we don't impose the exogeneity condition of X with respect to either ε or η, yet X is allowed in both outcome and the reduced form equations in a nonseparable way. The requirement for IV is relaxed, and Z may well be dependent on X, which motivates the inclusion of X in the model.

From a control-function perspective, we are looking for a control variable V such that

$$\varepsilon \perp\!\!\!\perp W \mid (X, V) \tag{4}$$

and both X and V are measurably separated from W, then we can identify the response parameters by Theorem 1. In the next proposition, we show that under Assumption 2, η can serve such a purpose. If $q(Z, X, e)$ is one-to-one in e almost surely, then we might recover the information of η from W given Z and X. However, this is possible only when both Z and X are independent of η, which is not the case here since Z is only conditionally independent and X is allowed to depend on η. However, as we show in the next proposition, it turns out that we don't need to recover the information of η completely, and it suffices to construct V such that the sigma algebra generated by (X, η) is the same as that by (X, V).

Assumption 3. *(i) $q(Z, X, e)$ is one-to-one in e almost surely; (ii) The conditional CDF $F_{\eta|X}(e)$ is continuous and strictly increasing in $e \in supp(\eta)$ almost surely.*

Proposition 1. *Suppose Assumption 2 holds for the nonseparable model (3). Then,*

(i) W is independent of ε conditional on (η, X).

(ii) If, additionally, Assumptions 3 holds, then $F_{W|Z,X}(W) = F_{\eta|X}(\eta)$, and condition (4) is satisfied with $V = F_{W|Z,X}(W)$.

Proof. For statement (i), let l be any continuous and bounded real function. Using the independence of Z and ε conditional on X, we first obtain the conditional mean independence as an intermediate result:

$$E[l(W)|\varepsilon, \eta, X] = \int l(q(z, X, \eta))dF_{Z|\varepsilon,\eta,X}(z) = \int l(q(z, X, \eta))dF_{Z|\eta,X}(z) = E[l(W)|\eta, X].$$

Then, we can check the conditional independence of W and ε given (η, X) by a conditional version of Theorem 2.1.12 of Durrett (2019). Let $a(\cdot)$ and $b(\cdot)$ be any continuous and bounded real functions, then

$$\begin{aligned}E[a(W)b(\varepsilon)|\eta, X] &= E[E[a(W)b(\varepsilon)|\varepsilon, \eta, X]|\eta, X] = E[E[a(W)|\varepsilon, \eta, X]b(\varepsilon)|\eta, X] \\ &= E[E[a(W)|\eta, X]b(\varepsilon)|\eta, X] = E[a(W)|\eta, X]E[b(\varepsilon)|\eta, X].\end{aligned}$$

Consider statement (ii). Under Assumption 3(i), there exists an inverse function $q^{-1}(Z, X, w) = e$. Then, we have

$$\begin{aligned}F_{W|Z,X}(w) &= Pr(W \leq w|Z, X) = Pr(q(Z, X, \eta) \leq w|Z, X) \\ &= Pr(\eta \leq q^{-1}(Z, X, w)|Z, X) = F_{\eta|X}(q^{-1}(Z, X, w)).\end{aligned}$$

where the last equality follows from the independence of Z and η conditional on X. Note that $\eta = q^{-1}(Z, X, W)$ a.s., so we have $V = F_{W|Z,X}(W) = F_{\eta|X}(\eta)$. Let $Q_{\eta|X}(u) = \inf\{e \in \mathbb{R} : F_{\eta|X}(e) \geq u\}$ be the conditional quantile function of η given X. Under Assumption 3(ii), $F_{\eta|X}(e)$ is a one-to-one function of e a.s., so we have $Q_{\eta|X}(F_{\eta|X}(\eta)) = \eta$. We note that $\sigma(F_{\eta|X}(\eta), X) \subset \sigma(\eta, X)$ since $F_{\eta|X}(\eta)$ is a function of η and X. Because $(\eta, X) = (Q_{\eta|X}(F_{\eta|X}(\eta)), X)$, we also have $\sigma(\eta, X) \subset \sigma(F_{\eta|X}(\eta), X)$. By setting $V = F_{\eta|X}(\eta)$, it follows that

$$\begin{aligned}E[a(W)b(\varepsilon)|V, X] &= E[a(W)b(\varepsilon)|\eta, X] \\ &= E[a(W)|\eta, X]E[b(\varepsilon)|\eta, X] = E[a(W)|V, X]E[b(\varepsilon)|V, X]\end{aligned}$$

which implies condition (4). □

By Proposition 1, $(X, V) = (X, F_{W|Z,X})$ together delivers the conditional independence condition. Then, the identification of response parameters associated with the treatment can be obtained by Theorem 1[7].

[7] Although V does not enter the outcome equation $m(W, X, \varepsilon)$, we can always rewrite $\tilde{m}(W, X, V, \varepsilon) = m(W, X, \varepsilon)$ and treat $\tilde{m}$ as the outcome equation in Theorem 1.

3 A Test for Endogenous Controls

In this section, we consider a test for the endogenous control in a large class of models, as an implication of the results presented in Section 2. Consider the parametric model $Y = h(W, X, \varepsilon; \theta)$ as in 3, which is nested by the nonparametric and nonseparable model $Y = m(W, X, \varepsilon)$. To focus on the main idea, we limit our attention to an i.i.d sample of size n and the case of the conditional independence in Assumption 1 with the conditioning set observable to the researcher[8].

Suppose we are interested in the average response of the treatment: $\beta := E[\partial_W h(W, X, \varepsilon; \theta)]$. For the linear model (1) in the introduction, we find $\beta = \tau$. For the nonlinear binary model in Example 3, we have $\beta = E\left[\frac{\tau}{\Sigma(X)}\phi\left(\frac{\tau W + \xi X - \mu(X)}{\Sigma(X)}\right)\right]$. For both cases, under the conditional independence assumption, β coincide with the average partial effects, as guaranteed by Theorem 1, and both are non-parametrically identified. However, we also note that neither is parametrically identified, and so the corresponding parametric estimators assuming exogeneity of the controls would be inconsistent. Meanwhile, when these models are correctly specified without endogenous controls, we know that the parametric estimators corresponding to these specifications can be efficient (attaining the Cramer-Rao lower bound) under certain conditions. This observation naturally leads to a Hausman-type test on the endogenous control.

Formally, we consider the null and the alternative hypotheses as follows:

$$
\begin{aligned}
H_0 &: X \text{ is } \; \text{exogenous}, \\
H_1 &: X \text{ is } \; \text{endogenous}.
\end{aligned}
$$

The null and alternative hypotheses are specified loosely to accommodate a large class of models. The meaning of the null and the alternative varies over the specification of $h(W, X, \varepsilon; \theta)$. For example, in the linear model, $E[X\varepsilon] = 0$ is necessary and sufficient for the null while $X \perp\!\!\!\perp \varepsilon$ is not necessary. However, in the endogenous probit model of Example 3, $X \perp\!\!\!\perp \varepsilon$ is necessary and sufficient for the null while $E[X\varepsilon] = 0$ is not sufficient. To make the null and alternative hypotheses concrete, we impose the following assumptions. We denote the distribution of (Y, W, X) as F and the empirical distribution as F_n.

[8]Note that the analysis can be extended to the case of Section 2.2 where the conditioning variable V needs to be estimated, but that would complicate the asymptotic analysis and deviates from the main idea of the test.

Assumption 4. *The parametric estimator $\hat{\beta}$ under consideration corresponding to model 3 has the following properties:*

1. *Under the null, β is parametrically identified as a functional $\beta = \Gamma_p(F)$, and the parametric estimator $\hat{\beta} = \Gamma_p(F_n)$ is asymptotically linear with an influence function $\varphi(W,X)$:*

$$\hat{\beta} - \beta = \Gamma_p(F_n) - \Gamma_p(F) = \int \varphi(W,X) d(F_n - F) + o_P(n^{-1/2}).$$

2. *$\hat{\beta}$ is asymptotically biased with bias $B > \varepsilon$ for some constant $\varepsilon > 0$, such that $\hat{\beta} - \beta - B = \Gamma_p(F_n) - \Gamma_p(F) - B = \int \varphi(W,X) d(F_n - F) + o_P(n^{-1/2})$.*

Under the linear model 1, Assumption 4 is easily verified. Note that there could be multiple estimators corresponding to a parametric model that satisfy these conditions, and the test is with respect to any one of those under consideration.

For concreteness, we consider a series estimator. Suppose (W,X) is d-dimensional. For some given K, we define a power series $\{\tilde{g}_k\}_{1\le k\le K}$ to approximate $g(W,X) := E[Y|W,X]$:

$$\begin{aligned}\tilde{g} :=& \sum_{k=1}^{K} \tilde{g}_k' \pi_k = G'\Pi, \\ \tilde{\beta} :=& E[\partial_W [G(W,X)'\Pi]]\end{aligned}$$

where $G = (\tilde{g}_1', ..., \tilde{g}_K')'$; for each k,

$$\tilde{g}_k = \mathrm{vec}\left\{\prod_{j=1}^{d} q_j^{\lambda_j} : \sum_{j=1}^{d} \lambda_j = k, (q_1, ..., q_d) = (l_1(W), l(X_1), ..., l(X_{d-1}))\right\},$$

and $l = (l_1, ..., l_K)$ is some transformation function differentiable to all orders with bounded derivatives and has $\det(\partial_z l(z))$ bounded away from zero; $\Pi = (\pi_1', ..., \pi_K)'$ are the corresponding linear projection parameters. The series estimator of β is defined as

$$\hat{\tilde{\beta}} := \frac{1}{n}\sum_{i=1}^{n} \partial_W \left[G(W_i, X_i)'\hat{\Pi}\right],$$

where $\hat{\Pi}$ are the least square estimators of Π. $\hat{\tilde{\beta}}$ is an average derivative estimator studied in Example 3 of Newey (1994): it is shown that, with growing K and other regularity condition given in Theorem 7.2 of the same paper, $\hat{\tilde{\beta}}$ is asymptotically linear with an influence function ϕ:

$$\sqrt{n}(\hat{\tilde{\beta}} - E[\partial_W g(W,X)]) = \frac{1}{\sqrt{n}}\sum_{i=1}^{n}\phi(W_i,X_i) + o_P(1), \tag{5}$$
$$\phi(W,X) = \partial_W g(W,X) - \beta + \partial_W \log f_{W,X}(W,X)(Y - g(W,X)),$$

where $f_{W,X}(W,X)$ is the joint density function of (W,X).

When Assumption 1 holds, we have $\beta = E[\partial_W g(W,X)]$ under both the null and the alternative. Therefore, under the null, $\tilde{\beta} - \hat{\beta}$ is asymptotically normal with the mean-zero influence function $\psi(W,X) = \phi(W,X) - \varphi(W,D)$:

$$\sqrt{n}\left(\tilde{\beta} - \hat{\beta}\right) = \frac{1}{\sqrt{n}}\sum_{i=1}^{n}\psi(W_i,X_i) + o_P(1),$$

and the asymptotic variance $V = \lim_{n\to\infty} Var\left(\frac{1}{\sqrt{n}}\sum_{i=1}^{n}\psi(W_i,D_i)\right)$. When the parametric estimator $\hat{\beta}$ is efficient under the null, we can appeal to the classic result of Hausman (1978) to express the V as the difference of the asymptotic variances of $\tilde{\beta}$ and $\hat{\beta}$. In general, however, assuming efficiency of the parametric estimator is not desirable. Moreover, $\varphi(W,X)$ depends on the specific parametric estimator, and $\phi(W,X)$ involves estimation of a joint density function. Alternatively, we consider bootstrap the non-studentized statistic $\sqrt{n}\left(\tilde{\beta} - \hat{\beta}\right)$. Let $\theta_0 = \tilde{\beta} - \beta$ and $\hat{\theta} = \hat{\tilde{\beta}} - \hat{\beta}$. Let J_n^* denote the bootstrap distribution of $\sqrt{n}(\hat{\theta} - \theta_0)$. We consider the bootstrap confidence region

$$\mathcal{B}_n(\alpha) = \{\theta \in \Theta : \hat{\theta} - n^{-1/2}\inf\{t : J_n^*(t) \geq \alpha/2\} \leq \theta \leq \hat{\theta} + n^{-1/2}\inf\{t : J_n^*(t) \geq 1 - \alpha/2\}\}.$$

The following result shows that this bootstrap confidence region is asymptotically valid.

Proposition 2. *Suppose Assumptions 4 and conditions of Theorem 1 hold, and 5 is satisfied. Additionally, (a) F has a finite support, (b) $0 < Var(\psi(W,X)) < \infty$, (c) $E_n[\psi^2] \to E[\psi^2]$, (d) $\limsup_n n^{1/2}\|F_n - F\|_\infty < \infty$, then we have, as $n \to \infty$:*

1. *Under the null,*
$$P\{0 \in \mathcal{B}_n(\alpha)\} \to 1 - \alpha.$$

2. Under the alternative,

$$P\{0 \notin \mathcal{B}_n(\alpha)\} \to 1.$$

Proof. By Theorem 1, we have under both the null and the alternative,

$$\beta = E[\partial_W h(W, X, \varepsilon; \theta)] = E[\partial_W E[Y|W, X]] = E[\partial_W g(W, X)].$$

Then, under the null and Assumption 4 as well as condition 5, we can write

$$\begin{aligned}
\hat{\hat{\beta}} - \hat{\beta} =& \hat{\hat{\beta}} - E[\partial_W g(W, X)] + \beta - \hat{\beta} \\
=& \int \phi(W, X) d(F_n - F) + \int \varphi(W, X) d(F - F_n) + o_P\left(n^{-1/2}\right) \\
=& \int \psi(W, X) d(F_n - F) + o_P\left(n^{-1/2}\right)
\end{aligned}$$

Under the extra condition listed in the statement, we can apply Theorem 1.6.3 of Politis et al. (1999), which concludes the first statement.

Under the alternative, we have

$$\hat{\hat{\beta}} - \hat{\beta} - B = \int \psi(W, X) d(F_n - F) + o_P\left(n^{-1/2}\right)$$

Let $\mathcal{B}_n(\alpha) - B = \{\theta - B : \theta \in \mathcal{B}_n(\alpha)\}$. Then, applying Theorem 1.6.3 of Politis et al. (1999) to the reentered difference $\hat{\hat{\beta}} - \hat{\beta} - B$ gives,

$$P\{0 \in \mathcal{B}_n(\alpha) - B\} \to 1.$$

As $n \to \infty$, $n^{-1/2} \inf\{t : J_n^*(t) \geq 1 - \alpha/2\} - n^{-1/2} \inf\{t : J_n^*(t) \geq \alpha/2 \to 0$ because $J_n^*(t)$ is an asymptotical normal distribution by Theorem 1.6.3 of Politis et al. (1999). Since $B > \varepsilon > 0$, we have $P\{0 \notin \mathcal{B}_n(\alpha)\} \to 1$, which completes the proof. □

4 Simulation

In this section, we use Monte Carlo simulations to demonstrate in finite samples: (1) the severe bias due to the endogenous control, (2) how our constructive identification results are immune to the endogeneity of controls, and (3) coverage and power of the proposed test of the endogenous control. We consider two data generating processes (DGPs) that correspond to the two scenarios covered in Sections 2.1 and 2.2.

4.1 Endogenous Control Bias and the Remedy

The first DGP covers the scenario where both the treatment and the controls are endogenous, while the treatment is conditionally independent of the unobserved determinants:

$$\text{DGP(1)}: Y = \tau W + \xi_1 X_1 + \xi_2 X_2 + U, \quad W = \frac{1}{2}\exp(a) + N(0,1)$$
$$X_1 = a + \frac{1}{2}\exp(p), \quad X_2 = p, \quad U = \frac{1}{2}\exp(b) + \frac{1}{2}\exp(q) + N(0,1),$$

where $\tau = \pi_1 = \pi_2 = 1$; (a, b) and (p, q) are independent of each other, and each are jointly normal with mean zero, variance one, and covariance $\rho = 0.75$; $N(0, 1)$ denotes a random draw from a standard normal distribution, independent of (a, b) and (c, d). We observe that (1) $X = (X_1, X_2)'$ are relevant for both Y and W; (2) W and X are dependent on U; (3) Conditional on X, W is independent of U; and (4) W and X are measurably separated. As a result, linear projection parameters do not identify τ. Meanwhile, τ is also the average response of W, and it is nonparametrically identified by Theorem 1.

The second DGP covers the scenario where the IV is only conditionally valid, and the control is endogenous:

$$\text{DGP(2)}: Y = \tau W + \pi_1 X_1 + \pi_2 X_2 + U, \quad W = X_1 + X_2 + Z + \eta$$
$$X_1 = a + \frac{1}{2}\exp(p), \quad X_2 = p, \quad Z = \frac{1}{2}\exp(a) + N(0,1)$$
$$\eta = \frac{1}{2}\exp(\xi), \quad U = \frac{1}{2}\exp(b) + \frac{1}{2}\exp(q) + \frac{1}{2}\exp(\zeta) + N(0,1),$$

where (ξ, ζ) are also jointly normal with mean zero, variance one, and covariance $\rho = 0.75$. We observe that (i) W is not conditionally independent given X; (ii) Z is a valid IV only when conditional on X, but X is endogenous; (iii) Z and X are measurably separated. (iv) The measurable separability between Z and X here also implies the measurable separability between W and (X, η). As a result, τ is not identified by the usual IV projection, while τ is again, as an average response, nonparametrically identified: constructive identification is given by (2) or footnote 3 when the outcome equation is linear, and Theorem 1 gives a more general approach when the outcome equation is nonparametric and nonseparable.

Table 1 compares the estimates of τ using (i) OLS without control, (ii) OLS with control, and (iii) the nonparametric estimations through the third-order polynomial series regression.

Table 1: Simulation for DGP(1)

ρ	Methods	OLS w.o. X	OLS w. X	Series
	Bias	0.732	0.156	0.013
0.75	SD	0.091	0.080	0.048
	MSE	0.545	0.031	0.002
	Bias	0.588	0.088	0.004
0.5	SD	0.083	0.063	0.054
	MSE	0.352	0.012	0.003
	Bias	0.383	0.000	0.000
0	SD	0.065	0.045	0.058
	MSE	0.151	0.002	0.003

Note: Simulation results are based on $10,000$ replications and random samples of size $n = 1000$. Series regression uses 3rd-order polynomials.

The results are clear: while conventional methods that assume the exogeneity of controls are severely biased, the nonparametric methods perform much better in terms of bias.

Table 2: Simulation Results for DGP(2)

ρ	Methods	IV w.o. X	IV w. X	Series 1	Series 2
	Bias	0.528	0.156	0.013	0.020
0.75	SD	0.057	0.083	0.059	0.063
	MSE	0.282	0.031	0.004	0.004
	Bias	0.424	0.089	0.005	0.001
0.5	SD	0.052	0.068	0.064	0.069
	MSE	0.182	0.012	0.004	0.005
	Bias	0.277	0.009	0.005	0.004
0	SD	0.044	0.052	0.068	0.073
	MSE	0.079	0.003	0.005	0.005

Note: Simulation results are based on $10,000$ replications and random samples of size $n = 1000$. The conditional CDF is estimated based on a grid of 10 quantiles between 0.05 and 0.95 using the empirical support of W. Series 1 corresponds to the constructive identification in footnote 3 and Series 2 corresponds to the approach due to Theorem 1. Both series approaches use the 3rd-order polynomials.

Table 2 compares the following IV without control, IV with control, nonparametric approach as in footnote 3 using series regression, two-step control function approach in Theorem 1 using series regression for the second step. The results are as expected by theory. The IV-based methods that implicitly impose the exogeneity of the controls fail to produce consistent estimates of the partial effects of interest, and the alternative identification methods paired with usual nonparametric estimators perform well in finite samples.

4.2 Test for Endogenous Controls

In this section, we will focus on DGP(1) and demonstrate the finite sample performance of the proposed test in terms of the empirical coverage and power.

In the setting of DGP (1), the covariance parameter ρ controls the degree of endogeneity in controls. As a result, the null and alternative hypotheses of the test can be translated as

$$H_0 : \rho = 0,$$
$$H_1 : \rho \neq 0.$$

Therefore, we can simulate the coverage probability under the null $\rho = 0$, and we can choose a grid for $\rho > 0$ to simulate the power of the test.

Table 3 reports the empirical coverage under different sample sizes and number of Bootstrap replications. We find that (1) the coverage gets closer to the nominal rate as the sample size increases, and the test tends to be conservative when the sample size is small; (2) the increment of Bootstrap replications from 500 to 1000 does not matter much.

We also provide the empirical power of the test along a sequence of alternatives characterized by ρ. We find the test is slightly conservative for small values of ρ and when the sample size is small.

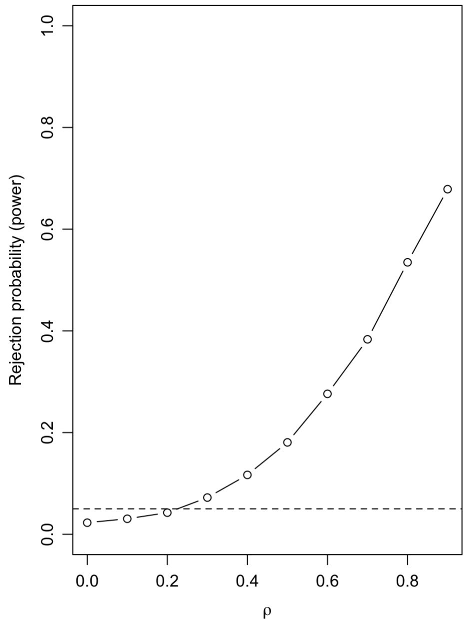
Table 3: Coverage probabilities with a nominal rate 0.95

Sample size	1000		500		100	
Bootstrap reps	1000	500	1000	500	1000	500
Coverage(%)	96.6	95.9	97.3	97.7	99.9	99.9

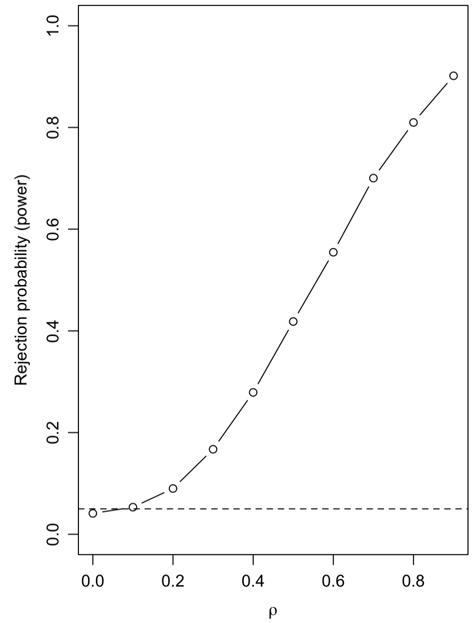
Note: Simulation results are based on $10,000$ replications and random samples of size $n = 1000$. The series approach uses the 3rd-order polynomials.

5 Application: The China Syndrome Revisit

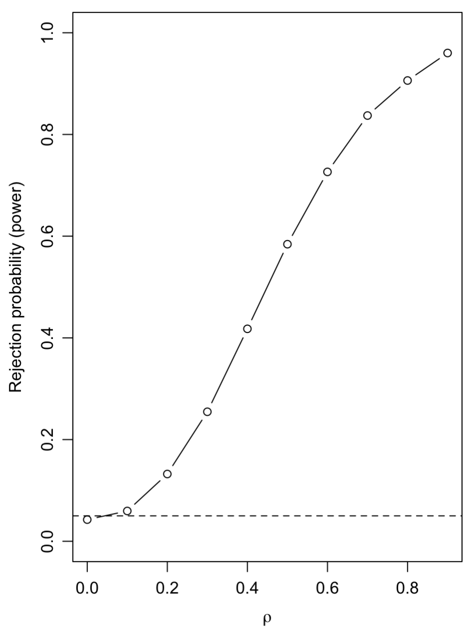
In this section, we revisit an empirical study of the import competition effect on the US labor market. During the late 20th and early 21st century, the world has witnessed a drastic surge



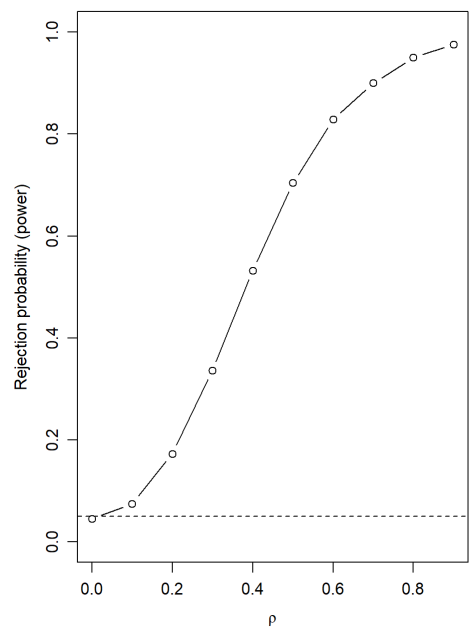
(a) Power with $n = 500$, $B = 500$



(b) Power with $n = 1000$, $B = 500$



(c) Power with $n = 500$, $B = 1000$



(d) Power with $n = 1000$, $B = 1000$

Figure 1: Power of the test with different sample sizes and bootstrap replications.

Note: Simulation results are based on $10,000$ replications. The test is based on the difference between the OLS estimator with controls and the series estimator with the 3rd-order polynomials.

of imports from the Chinese market. Meanwhile, there was a downturn of import-competing manufacturing in certain regions of the US, and it came with a higher unemployment rate and wage inequality in these regions. Therefore, a natural question arises: was the disruption of the local labor market mainly caused by the import competition, or was it rather a result of an overall economic transition in the US?

In a classic paper by Autor et al. (2013), the authors answer this question by exploiting the import-competition exposure variation across regional markets in the US. The idea is that regions with more initial specialization in labor-intensive industries are more exposed to the Chinese import competition. They measure the *share* of those industries at the start of the observation period and multiply it by the growth in US imports from China (*shift*), which produces a measure of import competition varied by regions. By taking commuting zones as analysis units, they are able to estimate effects on various labor market outcomes with a reasonable sample size.

There are two potential sources of endogeneity in this setup. Firstly, the import growth from China could be driven by unobserved local market transitions, such as industrial reallocation that causes a supply shortage. In that case, the growth in US imports from China could be driven by unobserved shocks that also move local outcomes. Secondly, the start-of-period measure of share in labor-intensive industries may also be related to other regional characteristics, such as educational attainment, which also determines the local labor market outcomes. To deal with the first type of endogeneity, they employ a measure of import growth in other high-income countries as an exogenous *shift* and multiply it by the same *share* variable to obtain the IV. They further assume that the growth in imports from China is not driven by demand shocks that are shared by the US and other high-income countries, so as to ensure the validity of the IV. For the second type of endogeneity, they take two approaches: (1) They first-difference both the treatment and the outcomes so that they are in terms of per capita changes. In a way, this is similar to removing the fixed effects in a linear panel model. (2) They add other start-of-period measures of regional market conditions and demographics to make the exogeneity of the *share* variables more plausible.

The results in Table 3 of Autor et al. (2013) show that adding more controls reduces the size of the estimated (negative) impact due to Chinese imports. This can be regarded as correcting the bias due to the second type of endogeneity. However, adding more controls may not fully capture the unobserved determinants that jeopardize the exogeneity of the *share* variables, and doing so may introduce extra endogeneity from the control variables. To make the exogeneity condition of the IV more likely to hold while avoiding the extra bias due to endogenous controls, we propose to implement the non-parametric control function approach in Section 2.2. We also illustrate how the endogenous control test works and its practical implications.

Our approach is also based on the exogeneity of the IV *shift* variables[9]. Thus, we do not tend to evaluate or improve upon the proposed IV in the original study; instead, we intend to make this approach more robust by allowing more controls to achieve conditional validity of IV while relaxing the exogeneity requirement of the control variables.

To focus on our main concern, readers are referred to Autor et al. (2013) for a detailed construction of the data. The main outcome of interest, ΔL, is the regional decade-change (% pts) in share of manufacturing employment; the explanatory variable of focus, ΔI_US, is a measure of changes in Chinese import exposure per worker in each region; and the IV, I_O, is the same measure of changes in exposure but with Chinese imports to the US replaced by those to other high-income countries. Two periods of decade-long first-difference cross-sectional data are stacked together as a panel. Five sets of time-invariant control variables, X, are augmented. The baseline model is given as follows:

$$\Delta L_{it} = \gamma_t + \tau \Delta I\_US_{it} + X_i + e_{it}$$

where γ_t is a time fixed effect and e_{it} is the stochastic error term. Under the IV conditions and regularity conditions, τ is exactly identified by the linear IV approach, given that X_i is uncorrelated to e_{it}.

To implement our approach as in Section 2.2, we use the third-order Hermite polynomial series for approximating the non-parametric functions. Specifically, we estimate $F_{\Delta I\_US|\Delta I\_O,X}$ at a grid of values, taken from the unconditional quantiles of ΔI_US, using the third-order Hermite polynomial basis functions of $(\Delta I\_O, X)$. Due to a large number of basis functions as the dimension of control increases, we add a ridge penalty to the least square estimation of the finite series regression, which is also a common practice in non-parametric series IV for better computation properties as suggested by Newey and Powell (2003). The times fixed effects and dummy variables in X are included linearly and not penalized. Using the ridge estimates, we obtain the fitted conditional CDF $\widehat{V}$ for each observation using linear interpolation. The conditional expectation $E(Y|W,V)$ is also approximated by the third-order Hermite polynomials with plugged-in $\widehat{V}$, and then the average

[9] Suppose the growth in imports from China is partially driven by positive demand shocks too, then the estimates of import-competition impact would look smaller because the positive demand would also generate positive outcomes in the US markets.

derivative estimator is obtained from the least square estimator of the series regression with ridge penalty. Both ridge penalty levels are chosen by cross-validation. Due to the generated regressor $\widehat{V}$, the influence function for the average derivative estimator is not valid anymore, for inference we instead resort to confidence intervals obtained from the i.i.d. bootstrap that clusters at the state level.

Table 4: IV, APE (Ridge-penalized), and Endogenous Control Test

	IV			APE			IV–APE		
Spec	$\hat{\beta}$	SE	95% CI	$\hat{\hat{\beta}}$	SE	95% CI	$\hat{\theta}$	SE	95% CI
(1)	−0.746	0.070	[−0.883, −0.613]	−1.036	0.144	[−1.289, −0.738]	0.290	0.111	[0.072, 0.510]
(2)	−0.610	0.102	[−0.793, −0.399]	−0.498	0.082	[−0.679, −0.361]	−0.113	0.124	[−0.315, 0.108]
(3)	−0.538	0.094	[−0.687, −0.336]	−0.420	0.069	[−0.559, −0.288]	−0.118	0.115	[−0.262, 0.111]
(4)	−0.508	0.082	[−0.648, −0.362]	−0.182	0.083	[−0.424, −0.109]	−0.326	0.111	[−0.518, −0.118]
(5)	−0.562	0.085	[−0.725, −0.405]	−0.126	0.075	[−0.285, −0.008]	−0.436	0.130	[−0.668, −0.184]
(6)	−0.596	0.095	[−0.759, −0.419]	−0.037	0.068	[−0.198, 0.066]	−0.559	0.127	[−0.780, −0.277]

Note: The ridge penalty level is chosen by the 5-fold cross-validation with the minimum-square-error rule. The bootstrap confidence interval for $\hat{\theta}$ is defined as $\mathcal{B}_n(\alpha)$ in Section 3, with $\alpha = 0.05$; it is similarly defined for the other two estimators.

Table 4 displays our results. The first IV section replicates the results of Table 3 of Autor et al. (2013), except that the standard errors and confidence intervals are replaced by the bootstrap versions to match those used for other estimators. APE denotes the average derivative estimator with plugged-in $\widehat{V}$. The last section IV-APE gives the difference of these two estimators and is used as a test for endogenous control as proposed in Section 3. Six specifications of controls X are as follows: (1) No control; (2) start-of-period employment; (3) Census-division specific fixed effects + (1); (4) start-of-period shares of college attainment, immigrants, female employment + (2); (5) Routine-intensive occupations, offshorability index + (2); (6) All controls mentioned above.

First, looking at the IV estimates, we find that as more controls are included, the estimated effects get smaller: it drops from 0.746 (no control), to 0.610 (one control), 0.538 (census division dummies), 0.508 (demographics), 0.562 (industrial types), and 0.596 (all controls), all with a negative sign. The estimates of our approach display a similar pattern: the absolute sizes of the estimates reduce as more controls are included. As we discussed above, the inclusion of more controls may solve the first type of bias but further introduces the second type, and our results provide alternative estimates that are robust against the the endogenous controls. If we believe more control variables help guard against the first type of bias, then the results in (6) suggest that after correcting the bias from the second type, the causal impact of Chinese import competition on the US market is not as large as we would expect.

The standard errors of the nonparametric method are reasonably small, and some are even smaller than the linear IV estimates, which is attributed to the ridge regularization. However, the ridge penalty also introduces shrinkage bias on the slope estimators of the series regression, which in turn may cause shrinkage bias in the average derivative estimator. Therefore, we further conduct a robustness check by only penalizing the conditional CDF estimation and leaving the second-step average derivative estimator unpenalized. The results are displayed in Table 5. We find that the estimates without penalization are indeed larger than their counterparts in Table 4, but the pattern remains: although the standard errors increase, we still find our approach generates smaller estimates except for the first case, where no controls are included.

Table 5: IV, APE (un-penalized), and Endogenous Control Test

	IV			APE			IV–APE		
Spec	$\hat{\beta}$	SE	95% CI	$\hat{\hat{\beta}}$	SE	95% CI	$\hat{\theta}$	SE	95% CI
(1)	−0.746	0.070	[−0.883, −0.613]	−1.317	0.200	[−1.682, −0.901]	0.571	0.178	[0.237, 0.917]
(2)	−0.610	0.102	[−0.787, −0.409]	−0.602	0.182	[−0.972, −0.251]	−0.009	0.212	[−0.346, 0.432]
(3)	−0.538	0.107	[−0.728, −0.329]	−0.521	0.173	[−0.990, −0.294]	−0.017	0.199	[−0.328, 0.470]
(4)	−0.508	0.084	[−0.673, −0.351]	−0.311	0.207	[−0.947, −0.078]	−0.198	0.227	[−0.467, 0.427]
(5)	−0.562	0.091	[−0.723, −0.378]	−0.138	0.212	[−0.650, 0.151]	−0.424	0.236	[−0.788, 0.136]
(6)	−0.596	0.101	[−0.787, −0.401]	−0.286	0.217	[−0.914, −0.067]	−0.311	0.242	[−0.586, 0.349]

Note: The ridge penalty level (for the conditional CDF estimation) is chosen by the 5-fold cross-validation with the minimum-square-error rule. The bootstrap confidence interval for $\hat{\theta}$ is defined as $\mathcal{B}_n(\alpha)$ in Section 3, with $\alpha = 0.05$; it is similarly defined for the other two estimators.

Lastly, we conduct the endogenous control test through the last column. Since row (1) does not include a control variable, our test does not apply there. Examining rows (2) - (6) in Table 5, we find rejections of no endogenous control in the last three rows, suggesting the average derivative estimators that are robust to the endogeneity of controls are preferable in specifications (4) - (6). Due to larger standard errors, the test results in Table 5 are not very informative, given that none of the tests are rejected.

6 Conclusion

We address a critical, prevalent, yet often overlooked problem in empirical research: the endogeneity of control variables. Building on the insightful observation and discussion in Frölich (2008) that nonparametric estimation can help with the endogenous control problem, we provide constructive identification results for marginal effects in a simple linear model with or without the presence of IVs, and extend the results to a general class of nonseparable models.

Our results not only provide solutions for identifying marginal effects of the treatment in the presence of endogenous controls but also have important implications: Because the additional measurable separability condition we introduce imposes minimal practical restrictions, endogenous controls are generally innocuous in a nonparametric model. Furthermore, based on the identification results, we propose a test for endogenous controls.

For empirical studies, our results invite researchers to conduct robustness checks using nonparametric methods and the proposed test. In general, nonparametric approaches are more robust, not only with respect to specification concerns but also in light of endogenous controls.

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