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Policy-Relevant Treatment Effects

By JAMES J. HECKMAN AND EDWARD VYTLACIL*

Accounting for individual-level heterogeneity in the response to treatment is a major development in the econometric literature on program evaluation. A substantial body of empirical evidence demonstrates that econometric models fit on individual-level data manifest heterogeneity in treatment effects that is present even after conditioning on observables.¹ An important distinction is the one between evaluation models where participation in the program being evaluated is based, at least in part, on unobserved idiosyncratic responses to treatment and models where participation is not based on unobserved idiosyncratic responses. This is the distinction between selection on unobservables and selection on observables.² The validity of entire classes of evaluation estimators hinges on whether or not they allow agents to act on unobserved idiosyncratic responses. In a wide variety of applications, the available evidence suggests that not only are *ex post* (post-enrollment) responses heterogeneous, but that *ex ante* decisions to participate in programs are based, in part, on these heterogeneous responses (Heckman and Vytlačil, 2000b, 2001).

I. Treatment Effects

An important consequence of these findings is that, in the presence of selection on idiosyncratic treatment effects, no single “effect” describes a program or intervention. A variety of treatment effects can be defined that depend on

the conditioning sets used to define “the” effect. Picking persons at random and entering them into a program and comparing their mean outcomes with those of persons denied access produces the average treatment effect (ATE). Picking persons at random who go into the program and comparing their average outcomes with those of the same type of people denied access to the programs defines the parameter “treatment on the treated” (TT). Assuming full compliance, this is the implicit parameter of interest in recent social experiments that deny access to otherwise acceptable applicants.

The mean effect of the program for those at the margin of participation in it (for given values of observables and conditioning on the unobservables in the program-participation equation) defines the marginal treatment effect (MTE).³ If the effect of treatment is the same for everyone with the same observables or if it varies among people with the same observables but enrollment decisions are not based on this variation, then all of the mean treatment parameters are the same, and there is a single effect of the intervention (Heckman, 2001).

In previous work, we present conditions under which it is possible to represent all of the conventional treatment parameters as weighted averages of the MTE, where different parameters correspond to different weights (see Heckman and Vytlačil, 2000a). Under the same conditions, we organize the econometric evaluation literature by classifying estimators on the basis of whether or not they assume that the MTE depends on the unobservables in the equation determining participation in the program. Ordinary linear instrumental variables (IV) is characterized as a weighted average of MTE

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¹ Heterogeneity of impacts in terms of observables is well established in studies of job-training programs (see the survey in Heckman et al. [1999]) and in rates of return to education in wage returns to other activities (Heckman, 2001).

² Heckman and Richard Robb (1986) introduce this distinction.

³ This parameter was introduced into the evaluation literature by Anders Björklund and Robert Moffitt (1987). It is the limit form of the LATE parameter. The LATE parameter was introduced in Guido Imbens and Joshua Angrist (1994), and the limit form of the LATE parameter was introduced in Heckman (1997) and Angrist et al. (2000); see also Heckman and Jeffrey Smith (1998).

where the weights in general differ from those used to define the standard treatment parameters (Heckman and Vytlačil, 2000a, b).

More precisely, consider a selection model with two potential outcomes (Y_0 , Y_1) and ignore general-equilibrium effects. Let X denote the observables in the outcome equations while Z denotes the observables in the participation equation. Write $Y_1 = \mu_1(X, U_1)$, and $Y_0 = \mu_0(X, U_0)$ where (U_0, U_1) are unobservables in the outcome equations. We observe Y_1 if $D = 1$; otherwise we observe Y_0 . Observed Y may be written as $Y = DY_1 + (1 - D)Y_0$. The individual treatment effect is $\Delta = Y_1 - Y_0$. The unobservables may be stochastically dependent on the observables, and μ_1 and μ_0 need not be additively separable in observables or unobservables. Our work links up to the discrete-choice literature by postulating a latent variable $D^* = \mu_D(Z) - U_D$ such that $D = 1$ if $D^* \geq 0$, and 0 otherwise. We assume: (i) $\mu_D(Z)$ is a nondegenerate random variable conditional on X ; (ii) U_D is absolutely continuous with respect to Lebesgue measure; (iii) (U_1, U_D) and (U_0, U_D) are independent of Z conditional on X ; (iv) Y_1 and Y_0 have finite first moments; and (v) $1 > \Pr(D = 1 | X = x) > 0$ for every $x \in \text{Supp}(X)$. Assumptions (i) and (iii) are “instrumental variable” assumptions that there is at least one instrument that determines participation in the program but not outcomes. Assumption (ii) is a technical assumption made primarily for convenience. Assumption (iv) guarantees that the parameters of interest will be well defined. Assumption (v) is the assumption that the population will contain both a treatment and a control group for each X . These conditions impose testable restrictions on the data. X does not have to be exogenous as long as one is evaluating programs in place rather than projecting to new populations.⁴

Without loss of generality, we include the elements of X in Z . We define $P(Z)$ as the probability of receiving treatment: $P(z) \equiv \Pr(D = 1 | Z = z) = F_{U_D|X}(\mu_D(z)|x)$, where

⁴ Observe that there are no exogeneity requirements concerning X . A counterfactual “no feedback” condition corresponding to the classical superexogeneity assumption of structural econometrics is required for interpretability so that conditioning on X does not mask the effects of D . Letting X_d denote a value of X if D set to d , a sufficient condition that rules out feedback from D to X is $X_1 = X_0$. Heckman and Vytlačil (2000b) and Heckman (2001) discuss the role of exogeneity assumptions in projecting estimated treatment effects to new environments.

$F_{U_D|X}(\cdot | x)$ denotes the distribution of U_D conditional on $X = x$. Without loss of generality, we impose the normalization that $U_D \sim \text{Unif}[0, 1]$ so $\mu_D(z) = P(z)$. Vytlačil (2001) proves under conditions (i)–(v) that the selection model is equivalent to the LATE model of Imbens and Angrist (1994).

The average effect of treatment on those at the margin of participation in the program at level $U_D = u_D$ is the MTE: $\Delta^{\text{MTE}}(x, u_D) \equiv E(\Delta | X = x, U_D = u_D)$. It is the basis for unifying both estimators and treatment parameters. We develop the method of local instrumental variables (LIV) to estimate this parameter. Under conditions specified in Heckman and Vytlačil (2000b), we can estimate MTE by estimating the derivative of $E[Y | X = x, P(Z) = P(z)]$. The derivative of the conditional expectation can be estimated using any standard nonparametric regression technique.

We establish that under conditions (i)–(v) all of the population treatment parameters used in the evaluation literature are weighted versions of the MTE. Thus, for treatment parameter j ,

(1) Parameter $_j(x)$

$$= \int_0^1 \Delta^{\text{MTE}}(x, u_D) \omega_j(x, u_D) du_D$$

where the specific formulas for the weights for different parameters are given in our papers. The probability limit of IV estimators of treatment effects (as well as other estimators) may also be written as weighted averages of MTE.⁵ Thus conditional on $X = x$, the probability limit of estimator k is

(2) plim Estimator $_k(x)$

$$= \int_0^1 \Delta^{\text{MTE}}(x, u_D) \omega_k(x, u_D) du_D.$$

One can show that the weights corresponding to the conventional treatment parameters inte-

⁵ Expression (2) is related to similar expressions in Shlomo Yitzhaki (1996) and Angrist et al. (2000).

grate to unity, as do the weights corresponding to many of the estimators including IV using $P(Z)$ as an instrument. In general, the probability limits of the various estimators weight MTE differently than do the parameters.⁶ Notice that the parameters and estimators of the form given above coincide if responses to treatment do not vary among individuals (given $X = x$); if they do, the parameters and estimators will still coincide if agents do not participate in the program on the basis of such variation. In the more general case, which describes most of the studies in the literature, the estimators and parameters differ. When the support of $P(Z)$ is the full unit interval, we use LIV to estimate the treatment parameters by estimating MTE and using equation (1) to recover the parameter of interest. We also develop more general methods for estimating the parameters which do not require the support of $P(Z)$ to be the full unit interval (e.g., we allow Z to be discrete) and do not require estimating a derivative of a conditional expectation. We can replace MTE's by LATE's and integrals by sums. However, these methods still require support conditions on $P(Z)$, with the support condition depending on the particular parameter of interest. When these support conditions do not hold, we develop sharp bounds on the treatment parameters that exploit all of the information in the model and in the available data (Heckman and Vytlacil, 2000a, 2001).

II. Policy Relevant Treatment Parameters

The conventional treatment parameters are justified on intuitive grounds. The link to cost-benefit analysis and interpretable economic frameworks is obscure. Heckman and Smith (1998) develop the relationship between these parameters and the parameters of cost-benefit analysis. Sometimes the traditional parameters answer interesting policy questions, and sometimes they do not.

A more direct approach to defining economic treatment parameters pursued in our research is to postulate a policy question or decision prob-

lem of interest and to derive and estimate the parameter that answers it. Taking this approach does not in general produce the conventional treatment parameters.

We consider a class of policies that affect P , the probability of participation in a program, but do not directly affect MTE. An example from the economics of education would be policies that change tuition or distance to school but do not directly affect the gross returns to schooling.⁷ Define P as the baseline probability and define P^* as the probability produced under an alternative policy regime. In this paper, we compare policies using a Benthamite criterion and consider the effect of the policies on the mean utility of individuals with a given level of $X = x$. We obtain the following for utility V :

$$\begin{aligned} (3) \quad & E[V(Y)|\text{under policy } *, X = x] \\ & - E[V(Y)|\text{under baseline}, X = x] \\ & = \int_0^1 \Delta_V^{\text{MTE}}(x, u_D) \omega^*(x, u_D) du_D \end{aligned}$$

where the policy weights are $\omega^*(x, u_D) = F_{P|X}(u_D|x) - F_{P^*|X}(u_D|x)$, where $F_{P|X}(\cdot|x)$ is the distribution of P conditional on $X = x$,⁸

⁷ We abstract from general-equilibrium effects.

⁸ Keeping the conditioning on X implicit, we have $E[V(Y)|\text{baseline}]$

$$\begin{aligned} & = \int_0^1 E[V(Y)|P(Z) = p] dF_P(p) \\ & = \int_0^1 \left(\int_0^1 \mathbf{1}_{[0,p]}(u) E[V(Y_1)|U = u] \right. \\ & \quad \left. + \mathbf{1}_{(p,1]}(u) E[V(Y_0)|U = u] du \right) dF_P \\ & = \int_0^1 ([1 - F_P(u)] E[V(Y_1)|U = u] \\ & \quad + F_P(u) E[V(Y_0)|U = u]) du \end{aligned}$$

where $\mathbf{1}_{\mathcal{A}}(u)$ is an indicator function for the event $u \in \mathcal{A}$. Thus, comparing the baseline to the new regime,

$$\begin{aligned} & E_{P^*}[V(Y)] - E_P[V(Y)] \\ & = \int_0^1 E(\Delta_V|U = u)[F_P(u) - F_{P^*}(u)] du. \end{aligned}$$

⁶ Under conditions (i)–(v) the maximum possible difference between any two policy parameters representable in the form of (2) or estimators and parameters can be written as a product of the difference between the largest and smallest possible value of MTE and a measure of the distance between the two weights.

and $\Delta_V^{\text{MTE}}(x, u_D) = E[V(Y_1) - V(Y_0)|X = x, U_D = u_D] = E(\Delta_V|X = x, U_D = u_D)$ where $\Delta_V = V(Y_1) - V(Y_0)$. When V is the identity function, we compare mean outcomes, as in conventional cost-benefit analysis. The policy parameter is a weighted average of the MTE as previously defined. Instead of hoping that conventional treatment parameters answer interesting economic questions, a better approach is to estimate Δ_V^{MTE} and weight is by the appropriate weight that is determined by how the policy changes the distribution of P .

An alternative approach to policy evaluation is to produce a policy-weighted instrumental variable based on a specific choice for $\omega(x, u_D)$ that captures the effect of the policy change. If we choose the weights for the estimator $\omega_k(x, u_D)$ in (2) to coincide with the weights for the policy change, $\omega^*(x, u_D)$, in (3) we can produce an estimator that is tailored to the policy change of interest. In Heckman and Vytlačil (2000b) we establish that the policy-relevant instrumental variable is $[f_{P^*}(P)/f_P(P)] - 1$, where f_{P^*} and f_P are the densities of P^* and P , respectively. It is possible to determine the distribution of P and P^* independently of determining the other ingredients required for forming the policy-relevant treatment parameter.

Figure 1, taken from Heckman and Vytlačil (2000b), plots the estimated MTE (where $V(Y) = \ln Y$) comparing the economic returns to college education with those for high-school education using National Longitudinal Survey of Youth (NLSY) data. It also plots the weights on MTE for linear IV using $P(Z)$ as an instrument. The figure also plots the policy weights on MTE for three tuition policies identified at the base of the figure. To produce the results displayed in the figure, we divide the policy weights by the change in the proportion of people going to college induced by the policy change being characterized. When integrated against the MTE, this produces the per capita effect of the policy on those induced to change by it. The renormalized policy weights are thus conceptually comparable to the IV weights.

IV weights the MTE very differently from the weighting required to evaluate policies I and II. It roughly approximates the weighting required to evaluate policy III. The policy-relevant instrumental-variable weights for each policy reproduce the weights for each policy. If the MTE is flat, there is

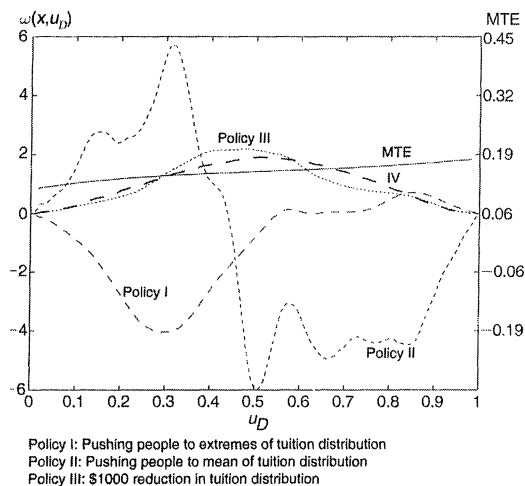


FIGURE 1. MARGINAL TREATMENT EFFECT (MTE) VERSUS POLICY AND IV WEIGHTS

Note: The figure depicts a baseline model without interactions. (Source: Heckman and Vytlačil, 2000b).

one single mean “effect” of all policies, and IV estimates that effect. In the general case, which covers most of the studies surveyed by Heckman and Vytlačil (2000b) and Heckman (2001), different policies are associated with different weights, and only by accident would linear IV identify the appropriate policy response.

When the support of P^* is not contained in the support of P , so that the policy intervention being studied extends P^* outside of historical data, it is necessary to make additional assumptions in order to perform a principled policy analysis. If parametric assumptions are made about $P(z)$, if the probability is determined by historical data, and if the intervention being studied changes the distribution of Z in a known way, it is straightforward to determine the distribution of $P^*(z)$, including the new support. However, MTE is only nonparametrically identified over the support of P .

The case where the distribution of $P^*(Z)$ is known is the most straightforward to exposit. We identify $\omega^*(u_D)$, the weighting function defined above. Let \mathcal{P}_x denote the support of $P(Z)$ conditional on $X = x$, and let $\mathcal{P}_x^c = [0, 1] \setminus \mathcal{P}_x$. Assume that there exists at least one component of Z that is continuous and that $P(\cdot)$ is a nontrivial, continuous function of that component, so that it is possible to identify $\Delta^{\text{MTE}}(x, u_D)$ for all u_D values in \mathcal{P}_x .

Using equation (3) with V equal to the identity function, we have

$$\begin{aligned} & E(Y|\text{under policy } *, X = x) \\ & - E(Y|\text{under baseline}, X = x) \\ & = \int_0^1 \Delta^{\text{MTE}}(x, u_D) \omega^*(x, u_D) du_D \\ & = \int_{\mathcal{P}_x} \Delta^{\text{MTE}}(x, u_D) \omega^*(x, u_D) du_D \\ & + \int_{\mathcal{P}_x^c} \Delta^{\text{MTE}}(x, u_D) \omega^*(x, u_D) du_D. \end{aligned}$$

The first term of the sum is identified, but not the second term. However, we identify $\omega^*(u_D)$, and if MTE is bounded, then we can bound the second term (see Heckman and Vytlacil, 2000b).⁹

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⁹ Hidehiko Ichimura and Christopher Taber (2000) present a related analysis of policy evaluation under limited support. Their framework does not use the MTE to unify estimators and policy counterfactuals. However, their setup is more general than the index model we use. The index model leads to simple expressions.