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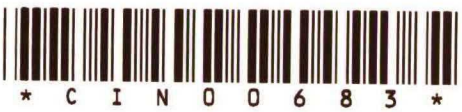

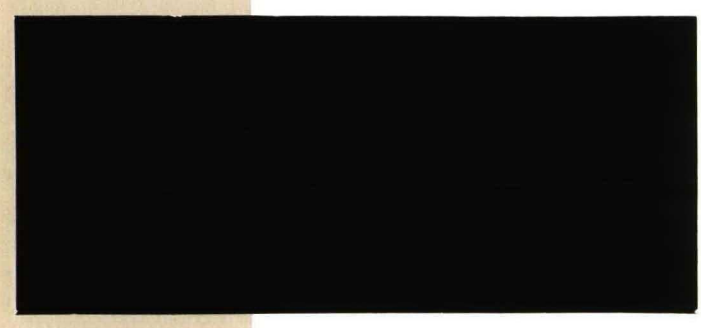

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SOURCES OF IDENTIFYING INFORMATION IN EVALUATION MODELS

by J.D. Angrist
and G.W. Imbens

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Sources of Identifying Information in Evaluation Models[1]

J. D. Angrist
G. W. Imbens

Harvard University[2]
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[2]Mailing Address: Department of Economics, Harvard University, Cambridge, MA 02138

Abstract

New results on using exclusion restrictions to identify and estimate average treatment effects are presented. Identification is achieved given a minimum of parametric assumptions, initially without reference to a latent index framework. Most econometric analyses of evaluation models motivate identifying assumptions using models of individual behavior. Our technical conditions do not fit easily into a conventional discrete choice framework, rather they fit into a framework where the source of identifying information is institutional knowledge regarding program administration. This framework also suggests an attractive experimental design for research using human subjects, in which eligible participants need not be denied treatment. We present a simple instrumental variables estimator for the average effect of treatment on program participants, and show that the estimator attains Chamberlain's semi-parametric efficiency bound. The bias of estimators that satisfy only exclusion restrictions is also considered. Stronger assumptions restricting participation behavior are introduced. Under these assumptions, satisfied by latent index models, semi-parametric indentification of average treatment effect remains possible.

1. INTRODUCTION. Do programs that subsidize education and training improve the labor market outcomes of program participants? Evaluation questions of this type are of great concern to government policy makers, private employers, and academic researchers. In any field where scientific research has policy implications, evaluation methodology is also of considerable importance. Discussions of evaluation methodology are discussions of the nature and credibility of scientific evidence. In medical research, for example, government regulations establish standards and procedures that researchers must follow for their results to be considered credible evidence for the efficacy and safety of new drugs. Standards here are quite clear: research guidelines for a new drug application clearly favor, but do not require, the randomized assigment of treatment and concurrent data collection on control groups (Center for Drug Evaluation and Research 1988, pp. 22, 56)

Social policy is arguably as important for human welfare as public health, yet no mutually agreed standard of evidence exists for establishing the effectiveness of social programs. On the one hand, critical research on econometric evaluation methodology by Lalonde (1986) and others has led to renewed interest in classical experimentation as a tool for social policy evaluations. Manski and Garfinkel (1991) note that the recent Job Partnership Training Act (JPTA) even mandates a particular sort of treatment–control evaluation design in which applicants for training are randomly denied treatment. On the other hand, Manski and Garfinkel (1991) and Heckman and Hotz (1989) argue persuasively that experiments can never be a complete substitute for evaluations using observational data. Disagreements over

evaluation methodology notwithstanding, research directed towards adapting experimental designs for social policy analysis and allowing for fewer assumptions in observational analyses is likely to remain important. This paper contributes to both the experimental and observational components of the evaluation research agenda by presenting new results on using exclusion restrictions to identify and estimate average treatment effects.

Our findings are related to results in a number of recent papers on theoretical identification in evaluation models. Like Chamberlain (1986), Heckman (1990a) and Heckman and Honoré (1990), we are concerned with identification given a minimum number of parametric assumptions. But, like Manski (1990), we avoid the additive latent index framework commonly invoked in econometric evaluations. Much of the previous work on identification presents some very general findings regarding the identification of distributions, but devotes relatively little attention to converting theoretical identification into empirically feasible estimators. In contrast, the formulation in this paper focuses on conditional means, and is immediately useful to applied researchers because it provides necessary and sufficient conditions for linear instrumental variables techniques to consistently estimate the average effect of treatment. In this, our approach is related to Angrist's (1991) use of instrumental variables to estimate treatment effects in nonlinear models, although here the identification conditions are not motivated by functional form restrictions.

We also show how to interpret the identifying assumptions as outlining a particular type of experimental design useful for research involving human subjects. Like Heckman (1990b), we

view social experiments as a source of identifying information, rather than as a replacement for economic modelling, and think that experiments should be designed with this in mind. An experimental design interpretation is important because the resulting design may be ethically more attractive than the conventional approach to randomization wherein eligible program applicants are randomly excluded from treatment. For example, some physicians have argued that randomization is incompatible with the Personal Care Principle in medical ethics, which requires doctors to put the welfare of their patients above the potential social gains from research (Royall 1991). JPTA program administrators are also reluctant to deny training to applicants randomized into a control group (Hotz 1991).

Our framework for experimental design essentially consists of first choosing an eligible population or evaluation site, either by randomized manipulation, or on the basis of ignorable (as defined by Rosenbaum and Rubin [1983]) covariates. Any eligible participant is then allowed to participate in the program if he or she likes. This approach may also identify parameters which are more likely to be useful for forecasting the impact of future programs.[3]

As a related by-product, our approach to inference also provides some insight regarding the problem of non–compliance in clinical trials, recently analyzed by Efron and Feldman (1991) and Robins (1989). Randomization of intention–to–treat, but not actual treatment, is one way to generate exclusion restrictions that will be sufficient to identify an average

[3]Harris (1985) and Moffit (1991b) also discuss randomization of sites versus randomization of individuals. However, a key distinction is that within sites these authors argue for saturation of treatment within sites while we do not. Different average treatment effects are therefore identified in the two types of site-randomization designs.

treatment effect. Not surprisingly, the estimator that uses these exclusion restrictions is a form of instrumental variables.

The paper is organized as follows. Section 2 formally defines the average effect of treatment on program participants and presents the main theoretical results. Necessary and sufficient conditions are given for a data generating process to identify an average treatment effect under exclusion restrictions. These results are also compared to previous results on the identification of treatment effects. Section 3 outlines the instrumental variables interpretation of identifying information and discusses the type of experimental design or data generating processes that satisfy the identifying conditions. Some results on the efficient use of exclusion restrictions in estimating average treatment effects are also discussed. In section 4 we discuss what can be learned about treatment effects if the average treatment effect of interest is not identified. It is shown that under mild additional restrictions one might still be able to derive bounds for the average treatment effect or identify local average treatment effects. Section 5 offers a summary and some concluding thoughts on the nature of identifying information in models for the evaluation of social programs. An important distinction, and an underlying theme of the paper is the difference between identifying information derived from models of program participants' behavior and from information about program eligibility rules. We argue that the latter is more likely to provide a convincing empirical identification strategy.

2.1 IDENTIFICATION. Our framework is essentially similar to that advanced by Rubin (1977),

Heckman (1990), and others. Let Y_0 be the response variable for an individual if he or she does not participate in the program. We assume that Y_0 is well defined even if the individual is actually participating in the program. Similarly, Y_1 is the value of the response variable if the individual does participate in the program, and $Y_1 - Y_0$ is the treatment effect that we are interested in. We never observe both Y_0 and Y_1; all inferences about these differences are indirect and in terms of expectations. Let $f_0(y)$ and $f_1(y)$ denote the probability density functions of Y_0 and Y_1 respectively. P denotes an indicator for program participation, equal to one if an individual participates in the program, and equal to zero otherwise.

The average treatment effect can be defined in a number of ways (See, e.g., Heckman and Robb [1985], and Heckman [1990]). First, there is the expectation of $Y_1 - Y_0$ in the population:

$$(1) \quad \tilde{\alpha} = E[Y_1 - Y_0] = \int y[f_1(y) - f_0(y)]dy$$

This is the expected treatment effect if we take an individual randomly from the population and look at the difference between his response as a participant and nonparticipant. A second average treatment effect is defined by taking the expectation conditional on participation:

$$(2) \quad \alpha = E[Y_1 - Y_0|P = 1] = \int y[f_1(y|P = 1) - f_0(y|P = 1)]dy$$

This measures how much a participant gains from the program. Whether the focus is on the average treatment effect (ATE), $\tilde{\alpha}$, or on the selected average treatment effect (SATE), α, depends on the particular application. We are usually interested in forecasting the effects

of a program when it is extended to a larger part of society. If the program or treatment will potentially be used by all members of the population, $\tilde{\alpha}$ is appropriate. If the program will eventually be used by a population with characteristics similar to the population in the evaluation design, α is the relevant average treatment effect. The latter is probably more realistic in economic applications. We will therefore concentrate on identification of α, rather than $\tilde{\alpha}$.

The problem of estimating average treatment effects in our framework is one of sample selection exactly the same as that considered by Gronau (1974), Heckman (1979) and Manski (1990). We observe $Y = P \cdot Y_1 + (1 - P) \cdot Y_0$ and P. From this two conditional response distributions are identified:

$$f_1(y|P = 1) \quad \text{and} \quad f_0(y|P = 0),$$

along with the probability of participation, $q = Pr(P = 1)$. These distributions do not allow us to calculate $\tilde{\alpha}$ or α, for which we need to know the counterfactual expectation $\int y \cdot f_0(y|P = 1)dy$. The difference between the mean of Y_1 for those who participate and Y_0 for those who do not participate can be written as

$$E[Y_1|P = 1] - E[Y_0|P = 0]$$

$$= E[Y_1 - Y_0|P = 1] + E[Y_0|P = 1] - E[Y_0|P = 0] = \alpha + \beta$$

The average difference in outcomes between program participants and nonparticipants generally confounds the treatment effect α and the selection effect β. The exception is when

$f_i(y|P=1)$ is equal to $f_i(y|P=0)$ for $i=0,1$ and all y, in which case selection is sometimes said to be ignorable. This implies that the two response distributions (with and without participation) do not depend on the decision to participate. If this is not the case then selection is non–ignorable and it is clear that we need more information, or restrictions on $f_0(\cdot)$, to separate α and β. Below, we briefly review some identifying assumptions.

The first approach assumes that the selection problem can be solved simply by conditioning on the right covariates.

Condition 1 *There is an observable covariate X such that*

$$E[Y_i|P=1,X=x]=E[Y_i|P=0,X=x]$$

In this case we can condition on X to remove the selection effect if we observe (Y,P,X):

$$\alpha=\int E[Y_1|P=1,X=x]-E[Y_0|P=1,X=x]\cdot g(x|P=1)dx$$

$$=\int E[Y_1|P=1,X=x]-E[Y_0|P=0,X=x]\cdot g(x|P=1)dx$$

This is in terms of expectations and distributions that can usually be estimated. The selection effect is equal to:

$$\beta=\int E[Y_0|P=1,X=x]\cdot g(x|P=1)-E[Y_0|P=0,X=x]\cdot g(x|P=0)]dx$$

$$=\int E[Y|P=0,X=x]\cdot[g(x|P=1)-g(x|P=0)]dx$$

which can also be estimated. If $g(x|P=1) = g(x|P=0)$ for all x, implying that $Pr(P=1|x)$ does not depend on x, selection is ignorable after all and the selection effect is zero.

Conditioning on covariates corresponds to identification by adequately controlling for all factors related to both outcomes and treatment. References for this approach include Rubin (1977) and, in a regression framework, Barnow, Cain, and Goldberger (1981). A generalized control function methodology is outlined by Heckman and Robb (1985). An alternative approach to evaluation restricts the manner in which treatment is assigned. For example, treatment may be randomly assigned. In an experimental context, the distinction between approaches to causal inference based on control and randomization dates back at least to Fisher (1935). The econometric approach to restricting the manner of treatment assignment is to impose an exclusion restriction:

Condition 2 *There is a random variable Z such that for all z*

$$E[Y_0|Z=z] = E[Y_0]$$

and

$$E[P|Z=z] \quad \textit{is a non trivial function of } z$$

The covariate Z affects the participation probability, but is not related to the expected response in the absence of treatment.

Exclusion restrictions are widely used in econometrics, usually in conjunction with other identifying restriction. One of the most influential approaches is that developed in a series

of papers by Heckman (1976, 1979). The following example is a simplification of the model used by Heckman (1979):

$$(3) \quad Y = P \cdot Y_1 + (1 - P) \cdot Y_0 = \mu + \alpha \cdot P + \varepsilon$$

$$(4) \quad P = I[\gamma \cdot Z + U \geq 0]$$

$$(5) \quad \begin{pmatrix} \varepsilon \\ U \end{pmatrix} \Big| Z \sim \mathcal{N}\left(\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \Sigma \right)$$

The conditional expectation of Y_0 given $Z = z$ is

$$E[Y_0|Z = z] = \mu + E[\varepsilon|Z = z] = \mu$$

and since participation depends on both Z and U, it satisfies Condition 2. Notice that the treatment effect α in (3) is identical for every subject, so it is equal to the average and selected average treatment effects.

Another example is Angrist's (1991) nonlinear model with an omitted variable, U, that is correlated with P, but independent of an excluded instrument Z:

$$E[Y|P = p, U = u, Z = z] = F(p, u; \beta)$$

$$E[P|U = u, Z = z) = G(u, z; \gamma)$$

and Z and U independent. Angrist shows that the average treatment effect

$$\tilde{\alpha} = E[F(1, U; \beta) - F(0, U; \beta)]$$

is identified if and only if F or G is additively separable. In most of the econometric literature identification is based on distributional assumptions, functional form assumptions regarding either the conditional expectation of the response function and the probability of participation or both. In our main result, we investigate when the exclusion restriction outlined in condition 2 is sufficient to identify the average treatment effect. Our approach is to invoke easily verifiable restrictions on the value of $h(z,u)$ and the distribution of Z.

Condition 3 *There is a set* $\mathcal{Z}_0$ *such that* $1 > Pr(Z \in \mathcal{Z}_0) > 0$, $Pr(P = 1|Z = z) = 0$ *for all* $z \in \mathcal{Z}_0$.

Theorem 1 *Conditions 2 and 3 are sufficient for identification of* α *with a random sample of* (Y, Z, P).

Proof: Let A be an indicator for the event $Z \notin \mathcal{Z}^0$. Then:

$$E[Y|A = 0] = E[Y_0]$$

$$E[Y|A = 1] = E[Y_0|A = 1] + Pr[P = 1|A = 1] \cdot E[Y_1 - Y_0|A = 1, P = 1]$$

$$= E[Y_0] + Pr[P = 1|A = 1] \cdot E[Y_1 - Y_0|P = 1]$$

Since we can consistently estimate $Pr[P = 1|A = 1]$, $E[Y|A = 0]$ and $E[Y|A = 1]$ we can identify $\alpha = E[Y_1 - Y_0|P = 1]$.

$\mathcal{QED}$.

The theorem above shows that it is sufficient for identification to have a value, or set of values, $\mathcal{Z}_0$, which is realized with non–zero probability and for which the probability of participation is zero. The question arises whether this is a necessary as well as a sufficient condition. A complete answer is difficult to give. But, a number of related results suggest that it is almost impossible to achieve identification otherwise. First we note that the key to identifying α is the identification of $E[Y_0]$:

Result 1 *α is identified if and only if $E[Y_0]$ is identified.*

Proof: By definition $\alpha = E[Y_1|P = 1] - E[Y_0|P = 1]$. Note that $E[Y_1|P = 1]$ is identified because we observe Y_1 if $P = 1$. Therefore identification of α is equivalent to identification of $E[Y_0|P = 1]$. This is equal to $\{E[Y_0] - (1 - Pr(P = 1)) \cdot E[Y_0|P = 0]\}/Pr(P = 1)$. Because $E[Y_0|P = 0]$ and $Pr(P = 1)$ are identified, identification of $E[Y_0|P = 1]$ is equivalent to identification of $E[Y_0]$.

$\mathcal{QED}$.

Second, we show that if Z is a discrete random variable, Condition 3 is indeed necessary for indentification of $E[Y_0]$ and therefore for identification of α:

Result 2 *Suppose Z is a discrete random variable with K points of support. If $Pr(P = 1|Z = z_k) > 0$ for all k, then $E[Y_0]$ is not identified without additional restrictions.*

Proof: We can identify from the sampling design, for $k = 1, \ldots, K$,

$$E[Y|Z = z_k] = E[Y_0] + Pr(P = 1|Z = z_k) \cdot E[Y_1 - Y_0|Z = z_k]$$

There are K equations in $K+1$ unknowns. Therefore we cannot identify $E[Y_0]$ without some restriction on $E[Y_1 - Y_0|Z = z_k]$ if $Pr(P = 1|Z = z_k] > 0$ for all k. Note that one restriction such as the equality of the conditional difference $E[Y_1 - Y_0|Z = z_k]$ for k_1 and k_0 is sufficient for identification of $E[Y_0]$.

QED.

The reason that Results 1 and 2 do not consitute a complete argument for sufficiency is that if Z is not discrete, it might be possible to identify $E[Y_0]$ in certain limiting cases, even when Condition 3 fails. In fact, this sort of "identification at infinity" is an underlying theme of a number of previous results on the identification of treatment effects.

2.2 COMPARISON WITH PREVIOUS IDENTIFICATION RESULTS. Conditions 2 and 3 and Theorem 1 are related to some recent results on semi-parametric identification. In latent index models like (3)–(5), if the disturbances are normal then there is clearly no set $\mathcal{Z}_0$ such that the participation probability is zero for that set. This implies that we cannot estimate $E[Y_0] = E[Y|A = 0]$, the expected response for those who had zero probability of participating, so that identification cannot be based on Theorem 1. However, one might be able to estimate $E[Y_0]$ in te limit. One such approach is Condition B in Heckman's (1990) theorem on nonparametric identification of treatment effects in a latent-index sample selection model. Heckman requires the support of $\gamma \cdot Z$ in the latent index to be the real line. Therefore, there is a sequence of sets $\mathcal{Z}_n$ such that the probability of participating goes to zero in the limit. That is, there is a sequence of sets $\mathcal{Z}_n$, such that for all sequences of real

numbers $\eta_n > 0$ and $\delta_n > 0$ converging to zero, $Pr(Z \in \mathcal{Z}_n) > \delta_n$, and $Pr(P = 1|Z = z) < \eta_n$ for all $z \in \mathcal{Z}_n$. If the limit $\lim_{n\to\infty} E[Y_0|P = 0, Z \in \mathcal{Z}_n] = E[Y_0]$ for all such sequences η_n and δ_n, then an estimate based on such a sequence can take the place of $E[Y_0|A = 0]$ in the proof of Theorem 1 and identification is still obtained. This is similar to an earlier result in Chamberlain (1986) regarding semi–parametric identification of censored regression models. But Chamberlain (p. 205) and Heckman (p. 317) seem to feel that this sort of "identification at infinity" is not a very compelling foundation for inference. Chamberlain explores the possibility of imposing additional mild restrictions that would actually rule out this result.

Identification at infinity is unnatural in latent index models partly because many, if not most, regressors in economics have bounded support and are discrete. Most importantly, however, the latent index framework is usually motivated from a model of individual choice. Although the economic theory of discrete choice is well-developed and generally accepted, the details of empirical implementation are not. Identification at infinity requires not only covariates shifting choices but excluded from outcomes, but also covariate–choice relationship that obey certain very special restrictions without intrinsic behavioral content.

Both our Theorem 1 and previous results rely on exclusion restrictions and restrictions on the probability of participating for certain groups. Therefore, identification under Theorem 1 is similar to identification under the results of Chamberlain (1986) and Heckman (1990). One essential feature, however, distinguishes our approach from the traditional econometric

viewpoint: In Theorem 1, the main source of identifying information – the set of covariates for whom the probability of participation is zero – is obtained from the knowledge that the program was simply not offered to certain individuals or groups. A latent index framework in this case is unnnatural and unnecessary; with this sort of prior information there is no need to rely on limiting behavior. The argument we make for identifying information from program eligibility rules is exactly that made informally in a recent paper by Moffit (1991).

Finally, we note that Manski (1990) presents similar results regarding identification of density functions in selection models without reference to a latent index framework. Manski's Corollary 2 (p. 30) shows that given certain level–set restrictions, nonparametric bounds on density functions coincide, and therefore the density function is identified, if and only if the probability of selection is one for some part of the population. Like Heckman and Chamberlain, however, Manski (p. 30) seems to feel that identifying with level–set restrictions is "rarely identifying in practice." Part of the reason for this is that while the results by Manski and Heckman give identification in principle, Chamberlain proves that the information bound can be zero for these models. Our approach requires that $Pr(Z \in \mathcal{Z}_0) > 0$, which implies that the treatment effect is estimable at rate $\sqrt{N}$.

Recent empirical examples of evaluations in this framework include the geographically randomized Educational Assistance Test Program (EATP) and Multiple Option Recruiting Experiment (MORE), in which different packages of veterans educational benefits were randomized over military recruitment stations (Fernandez [1982]). In the EATP and MORE, no

new benefit packages were offered to a random subsample of stations. Observational examples where the source of identifying information is derived from institutions include Angrist's (1990) use of the draft lottery to estimate the labor market consequences of Vietnam–era military service, Angrist and Krueger's (1989) use of birthday–ordering to estimate the effects of World War II military service, and Angrist and Krueger's (1991) use of the interaction between compulsory school attendance laws and quarter of birth to estimate the effects of compulsory schooling on earnings.

As in most econometric applications the example listed above were implemented using statistical models with a constant treatment effect, so that the exclusion restrictions alone are sufficient for indentification. But selected average treatment effects may also be identified in some of these cases. For example, in the compulsory schooling application, virtually all students born in certain quarters were compelled to complete an additional year of schooling. Other students chose whether or not to go on; the treatment in this case is failure to complete an additional year of schooling. Likewise, in the Vietnam–era draft lottery, virtually all non–deferred men with low lottery numbers were drafted.[4]

2.3 Average Treatment Effects

If instead of the set $\mathcal{Z}_0$ we had a set $\mathcal{Z}_1$ such that $Pr(P = 1|Z = z) = 1$ for all $z \in \mathcal{Z}_1$, we would be able to identify the selected average non–treatment effect:

$$-\bar{\alpha} = E[Y_0 - Y_1|P = 0]$$

[4]For the identification results of this paper to hold in the lottery example, deferment would have to be an ignorable covariate.

This measure how much non–participants gain (or lose) from not participating in the program. This result is obvious if we reverse what we call treatment and non–treatment. If there is both a set $\mathcal{Z}_0$ that satisfies Condition 3 and a set $\mathcal{Z}_1$ that satisfies the above condition we can identify the average treatment effect $\tilde{\alpha}$. The three treatment effects are related by the following identity:

$$\tilde{\alpha} = Pr[P = 1] \cdot \alpha + (1 - Pr[P = 1]) \cdot \bar{\alpha}$$

Intuition for why α is identified is apparent from the proof of Theorem 1: $E[Y_0]$ is identified in the sample where $Z \in \mathcal{Z}_0$, and $E[Y_1]$ is identified in the set where $Z \in \mathcal{Z}_1$. If the treatment effect is identical for everybody then $\alpha = \tilde{\alpha} = \bar{\alpha}$. In general however, the treatment effects for participants and non–participants can be different, and in that case identification of the average treatment effect (ATE) requires stronger assumptions than does identification of the selected treatment effect (SATE).

3.1 Eligibility-Randomization as an Experimental Design. Social experiments can be based on randomly assigned eligibility wherein individuals freely choose whether or not to participate. Let D be an indicator for the willingness to participate, equal to 1 if someone is willing to participate and zero otherwise. Suppose there is some characteristic, indicated by a binary variable A, where only people with $A = 1$ are eligible for treatment, and the joint distribution of response Y_i and willingness to participate D does not depend on A. Formally, we can write

$$P = A \cdot D$$

and

$$f(Y_i, D|A = 1) = f(Y_i, D|A = 0) \quad \text{for } i = 0, 1$$

This is clearly much stronger than needed, but it makes the identification strategy transparent. A direct consequence is $E[Y_0|A = a] = E[Y_0]$ and therefore A satisfies both conditions 2 and 3 and we can identify the selected average treatment effect

$$\alpha = E[Y_1 - Y_0|P = 1] = E[Y_1 - Y_0|D = 1] = \frac{E[Y|A = 1] - E[Y|A = 0]}{Pr(P = 1|A = 1)}.$$

The SATE is in this case the expected treatment effect for all participants if eligibility were to be extended to the entire population, i.e. if $A = 1$ for all individuals. Our result differs from that in Heckman (1990b, p. 27) because we compare eligibles and ineligibles where Heckman compares participants and ineligibles. The combination of the ineligibles and eligible non–participants allows us to identify the distribution of Y_0 for those who are willing to participate:

$$f(Y_0|D = 1) = \frac{1}{Pr(D = 1)} f(Y_0|A = 0) - \frac{1 - Pr(D = 1)}{Pr(D = 1)} f(Y_0|A = 1, P = 0),$$

where $Pr(D = 1) = Pr(D = 1|A = 1)$ is identified from the proportion of participants among eligibles.

An example of a research design based on this principle might use the fact that a program is started in a particular community, and only members of that community are eligible to

enter the program. If there is a neighbouring community with similar members, we can use the members of that community to control for the selection effect that would arise if we just compared the participants and non -participants of the community that started the program.

Harris (1985), Garfinkel, Manski and Michalopoulos (1991) and Moffit (1991) refer to experiments of this type as macroexperiments, in contrast to microexperiments in which individuals within a site are randomly assigned to treatment and control groups. These authors stress that such macroexperiments can potentially identify macro treatment effects that result from interaction between individuals. An important difference between our approach and previous discussion of macro experiments, however, is that we are not arguing for saturation of treatment within eligible sites.[5]

A further advantage of an experiment in which eligibility is randomly assigned is that there is no formal application process for subjects who will later be randomized out. The need to deny treatment is a major factor in the dissatisfaction of job training centers with randomized assigment (Manski and Garfinkel 1991). Moreoever, in medical research, eligibility randomization does not require that individual physicians deny a treatment they feel is beneficial (as ocurred in the controversial ECMO [extracorporeal membrane oxygenation] study of infant mortality; see Royall, [1991]). Instead of randomizing treatment within hospitals, randomly chosen hospitals could have been selected for study, with physicians freely choosing the most approriate treatment within eligible sites, and data collected on outcomes

[5]The Wisconsin Child Support Demonstration (Garfinkel 1983) follows this basic approach. Treatments in this study are randomized over Wisconsin counties, although the focus is on county-level outcomes and not the individual outcomes captured by SATE as defined here.

at all sites. Another issue of interest in medical research is the question of non–compliance in conventional clinical trials. It is clear that as long as eligibility for treatment ("intention–to–treat" in biometric jargon) is randomized, the effect of a binary treatment on participants is identified.

3.2 LINEAR INSTRUMENTAL VARIABLES ESTIMATION. In this section we show that if conditions 2 and 3 are satisfied we can estimate α in a straightforward manner. First we discuss the case where we observe Y, P and A, an indicator for the event $Z \notin \mathcal{Z}_0$. In this case we can estimate α by linear instrumental variables. Second, we analyze the case where we observe Y, P and Z. It turns out that this does not necessarily increase the efficiency of our estimate of the treatment effect. Finally we discuss estimation if we do not observe A itself, but a variable correlated with A.

The first estimator is a linear instrumental variables estimator. The variable A is an instrument for the endogenous regressor P in the regression function

$$(6) \quad E[Y|A] = E[Y_0] + E[P|A] \cdot E[Y_1 - Y_0|P = 1]$$

The sample analog of the solution for α is an estimate of $\text{Cov}(y, A)/\text{Cov}(P, A)$:

$$\hat{\alpha} = \frac{\bar{Y}_{A=1} - \bar{Y}_{A=0}}{\bar{P}_{A=1}}$$

where

$$\bar{Y}_{A=1} = \sum_{n=1}^{N} A_n \cdot Y_n \Big/ \sum_{n=1}^{N} A_n$$

$$\bar{Y}_{A=0} = \sum_{n=1}^{N}(1 - A_n) \cdot Y_n \Big/ \sum_{n=1}^{N}(1 - A_n)$$

$$\bar{P}_{A=1} = \sum_{n=1}^{N} A_n \cdot P_n \Big/ \sum_{n=1}^{N} A_n$$

The question naturally arises whether we can improve on this estimate of the selected average treatment effect if we observe Z as well as A. Using Chamberlain's (1987) approach to semi-parametric efficiency bounds one can show that this is only possible if Z affects the conditional variance of Y_0:

Theorem 2 *If the conditional variance of* Y_0, $E[(Y_0 - E[Y_0])^2|Z = z]$ *does not depend on* z. *then* $\hat{\alpha}$ *is an efficient estimator for* α. *If the conditional variance does depend on* z, *one can obtain a more efficient estimator by replacing* $\bar{Y}_{A=0}$ *in the formula for* $\hat{\alpha}$ *by an efficient estimator for* $E[Y_0]$ *that adapts for the heteroscedasticity.*

Proof: see appendix.

This theorem shows that the only information in observing Z lies in the heteroscedasticity of Y_0. However, it is unusual to have a case where one has a convincing argument that Z does not belong in the conditional mean function, but does belong in the conditional variance function. Therefore, in most cases the instrumental variables estimator based on A will be efficient.

Finally, note that if conditions 2 and 3 are satisfied but we do not observe A, we can still consistently estimate α if we observe a random variable X satisfying

Condition 4 $E[Y|A,X] = E[Y|A]$ $E[P|A,X] = E[P|A]$

Condition 4 implies that X affects mean outcomes and treatment probabilities only through its effect on eligibility A. In this case we can use X as an instrument instead of A. To see this, note that from (6);

$$E\Big\{E[Y|A]|X\Big\} = E[Y_0] + E[Y_1 - Y_0|P = 1] \cdot E\Big\{E[P|A]|X\Big\}$$

which simplifies to

$$E[Y|X] = E[Y_0] + E[Y_1 - Y_0|P = 1] \cdot E[P|X]$$

This implies that X is a valid instrument. It is clear that using both X and A as instruments is equivalent to using just A because X does not add any information once A is known. However, X may be useful if A is not observed. In the example of the draft lottery, one might envision knowing the week a person was born, but not the exact day. In that case the week is the inaccurate instrument X while the actual day on which a person is born would be the accurate instrument A.

4. Inference when the Selected Average Treatment Effect is not Identified.

In this section we discuss what can be learned about treatment effects when Condition 3 is not satisfied. First we investigate what we *can* learn about the selected treatment effect even if we cannot estimate it consistently. The second approach investigates whether there are other interesting average treatment effects that we can estimate consistently even if Condition 3 is not satisfied.

We assume that Condition 2 is satisfied, but not Condition 3. There is no control group of ineligibles, and therefore an essential component of the instrumental variables approach discussed in the previous section is missing. We also assume that Z is discrete with points of support $z_1, z_2, \ldots, z_L$. Let $p_{z_k} = Pr(P = 1|Z = z_k)$, $\alpha_{z_k} = E[Y_1 - Y_0|P = 1, Z = z_k]$, $\pi_k = Pr(Z = z_k)$ and $Q = Pr(P = 1) = \sum \pi_k p_{z_k}$. In terms of these parameters, the selected average treatment effect is equal to

$$\alpha = \sum_k \frac{\pi_k \cdot p_{z_k}}{Q} \cdot \alpha_{z_k}$$

The probability limit of the instrumental variables estimator for α, using Z as an instrument for P can be derived as follows: Note that

$$E[Y|Z = z_k] = E[Y_0] + \alpha_{z_k} \cdot p_{z_k}$$

Define

$$\lambda_k = \frac{p_{z_k}(p_{z_k} - Q)}{E[p_{z_k}(p_{z_k} - Q)]}$$

and

$$\lambda = \frac{E[p_{z_k}^2]}{E[p_{z_k}(p_{z_k} - Q)]}$$

The λ_k are weights that have expectation equal to one, but they can be negative.

Result 3 *The instrumental variables estimator for α using Z as an instrument for P has probability limit*

$$\alpha_\lambda = E[\lambda_k \cdot \alpha_{z_k}] = \lambda \cdot \alpha_0 + (1-\lambda) \cdot \alpha$$

where

$$\alpha_0 = E\frac{p_{z_k}^2}{E(p_{z_k}^2)}\alpha_{z_k}$$

Proof: The first part follows directly from the expression for $E[Y|Z = z_k]$ and the definitions for α_{z_k} and λ_k. To see the second part, write:

$$\frac{1}{1-\lambda} \cdot [\alpha_\lambda - \lambda\alpha_0]$$

$$= \frac{1}{1-\lambda}\Big[\frac{E[p_{z_k}(p_{z_k} - Q)\alpha_{z_k}]}{E[p_{z_k}(p_{z_k} - Q)]} - \frac{E[p_{z_k}^2]}{E[p_{z_k}(p_{z_k} - Q)]} \cdot \frac{E[p_{z_k}^2]}{E[p_{z_k}^2]}\alpha_{z_k}\Big]$$

which simplifies to $E[p_{z_k}\alpha_{z_k}/Q]$ which is equal to α. $\mathcal{QED}$.

α_0 is a weighted average treatment effect, with weights proportional to $p_{z_k}^2$. If the treatment effect is constant, then both α_0 and α_λ coincide with the selected average treatment effect α. Therefore if we are prepared to bound the treatment effect heterogeneity, we can calculate corresponding bounds for the selected average treatment effect. Note that λ in the above result is estimable from the data.[6] Define $c = \alpha_0/\alpha$. If there is no treatment effect heterogeneity, then $c = 1$. The bias of the IV estimator is, in terms of c and λ:

$$\frac{\alpha}{\alpha_\lambda} = \frac{1}{\lambda \cdot c + (1-\lambda)}$$

λ measures how big the bias of the IV estimator can be because of treatment heterogeneity.

[6] In Angrist and Krueger (1990) λ is estimated to be about 2.5 for the relation between quarter of birth and high school graduation.

Now we will turn to the question whether there is any other average treatment effect that can be estimated if Condition 3 is not satisfied. We start by strengthening Condition 2:

Condition 5 *Let Z be any observed random variable and U be any random variable such that*

$$E[Y_i|U = u, Z = z] = E[Y_i|U = u] \quad \text{for } i = 0, 1$$

$$P = h(U, Z)$$

U and Z are independent, and $E[h(U, Z)|Z = z]$ is a non-trivial function of z.

This condition is stronger than Condition 2 for two reasons. First, $E[Y_0|U = u, Z = z] = E[Y_0|U = u]$ combined with independence of Z and U implies that $E[Y_0|Z = z] = E[Y_0]$ but not the other way round. Second, this condition puts restrictions on the expected value of Y_1 given $Z = z$, whereas Condition 2 does not put any restrictions on the distribution of Y_1 given Z at all. The modelling in terms of unobserved characteristics allows us to restrict the participation equation in a way that might reflect our assumptions about underlying behavior. Let $p_z = Pr(P = 1|Z = z) > 0$ for all z.

Condition 6 *For all z_0, z_1, such that $p_{z_1} \neq p_{z_0}$,*

$$(p_{z_1} - p_{z_0}) \cdot [h(z_1, u) - h(z_0, u)] \geq 0 \quad \text{for all } u$$

If $p_{z_1} = p_{z_0}$ then $h(z_1, u) = h(z_0, u)$ for all u.

This condition implies that there is some monotonicity in the participation decision. If someone with $U = u_1$ will participate if $Z = z_1$ but not if $Z = z_0$, then there is nobody (i.e no value of u) who will participate with $Z = z_0$ but not with $Z = z_1$. In other words, the effect of a change in Z has the same sign no matter what the value of U. If Condition 6 does not hold, it is difficult to compare the average responses for participants and non-participants for different value of Z because one cannot say anything about the difference in the distribution of u between participants with $Z = z_0$ and participants with $Z = z_1$.

Suppose that participation is determined by a additive latent index model

$$P = I[\gamma \cdot Z + U > 0]$$

In that case (but not only in that case) condition 6 is satisfied. This shows that the specification of the additive latent index model is indeed more restrictive than the specification in Condition 2.

Another way of stating the same restriction on the participation decision is the following condition. It compares the distribution of U for participants with different values of Z.

Condition 7 *If for any two values* z_1 *and* z_0, $p_{z_1} > p_{z_0}$ *then*

$$f(u|P = 1, Z = z_1) = \frac{p_{z_0}}{p_{z_1}} f(u|P = 1, Z = z_0) + \frac{p_{z_1} - p_{z_0}}{p_{z_1}} \tilde{f} z_1, z_0(u)$$

and if $p_{z_1} < p_{z_0}$

$$f(u|P = 1, Z = z_0) = \frac{p_{z_1}}{p_{z_0}} f(u|P = 1, Z = z_1) + \frac{p_{z_0} - p_{z_1}}{p_{z_0}} \tilde{f} z_1, z_0(u)$$

for some $\tilde{f}_{z_1,z_0}(u) \geq 0$ *satisfying* $\int \tilde{f} z_1, z_0(u)du = 1$.

Result 4 *Conditions 6 and 7 are equivalent if Condition 1 holds.*

Proof: See Appendix.

These two conditions allow us to make comparisons between the difference in average response between those who participate and those who do not, for the two groups, those with $Z = z_0$ and those with $Z = z_1$. We cannot compare these differences in general, because we have no way of comparing the associated U distributions, but condition 7 gives us a handle on this comparison.

Result 5 *If Conditions 5 and 7 hold, then we can identify the following average treatment effect:*

$$\alpha_{z_1,z_0} = E[Y_1 - Y_0|h(z_1,U) \neq h(z_0,U)] = \int E[Y_1 - Y_0|U = u]\tilde{f} z_1, z_0(u)du$$

Proof: See Appendix.

The interpretation of this result is that we can identify the average treatment effect for the "changers", or the "local" average treatment effect. These individuals are characterized by a value of U such that a change from $Z = z_0$ to $Z = z_1$ induces them to start or stop participating. α_{z_1,z_0} measures the average gain that this group makes from participating.

To see how far this result can take us towards identification of the selected average treatment effect α, consider the case where Z has a discrete distribution with points of support $z_0, z_1, \ldots, z_K$. Let $p_{z_k} = Pr(P = 1|Z = z_k)$, $\pi_k = Pr(Z = z_k)$, and $Q = Pr(P =$

1) $= \sum_k \pi_k p_{z_k}$. Let the z_k be ordered in such a way that $p_{z_k} < p_{z_{k+1}}$. For any pair (z_k, z_l) we can identify the average treatment effect α_{z_l,z_k}, defined in Result 4. If we have three points of support z_k, z_l and z_m with $p_{z_k} < p_{z_l} < p_{z_m}$, the following relation between the three average treatment effects holds:

$$\alpha_{z_m,z_k} = \frac{p_{z_l} - p_{z_k}}{p_{z_m} - p_{z_k}} \alpha_{z_l,z_k} + \frac{p_{z_m} - p_{z_l}}{p_{z_m} - p_{z_k}} \alpha_{z_m,z_l}$$

For example suppose that $p_{z_0} = 0$. Then Condition 3 is satisfied and the selected average treatment effect is identified.[7] The SATE is now related to the pairwise average treatment effects in the following way:

$$(7) \quad \alpha = \sum_{k=1}^{K} \frac{\pi_k p_{z_k}}{Q} \alpha_{z_k,z_0} = \sum_{k=2}^{K} \frac{\tilde{\pi}_k (p_{z_k} - p_{z_1})}{\tilde{Q}} \alpha_{z_k,z_1} + \frac{p_{z_1}}{\tilde{Q}} \alpha_{z_1,z_0}$$

where $\tilde{\pi}_k = Pr(Z = z_k | Z \neq z_0)$ and $\tilde{Q} = Pr(P = 1 | Z \neq z_0)$. Equation (7) shows how close we can get to identification of α if Condition 3 is not satisfied and we have no value z_0 with zero participation probability. The first term, $\sum_{k=2}^{K} \frac{\tilde{\pi}_k (p_{z_k} - p_{z_1})}{\tilde{Q}} \alpha_{z_k,z_1}$, is identified without the zero participation control group. From the second term $p_{z_1} \alpha_{z_1,z_0} / \tilde{Q}$, the factors p_{z_1} and $\tilde{Q}$ are identified and the only factor that is not identified is α_{z_1,z_0}. If we can bound this factor, the average treatment effect for the group who is already participating when $Z = z_1$, we can get a bound for α, using the approach to selection models advocated by Manski (1990a,1990b). Especially if p_{z_1} is small, i.e. if there is a control group with a small participation probability, this bound can be sharp.

[7] In this case the previously defined α_{z_k} is equal to α_{z_k,z_0}.

5. CONCLUSION. The SATE measures the average difference between the outcomes of program participants and what participants' outcomes would have been had they not been treated. When some individuals or groups are ineligible to participate in a program, and eligibility does not affect outcomes for other reasons, the SATE is identified using a simple instrumental variables estimator. This estimator is efficient - it makes full use of the identifying information provided by program eligibility rules.

The possibility of identification through eligibility rules is established using the same logic as recent arguments for identification based on the existence of a set of covariates for which the probability of treatment approaches zero in the limit. The source of identifying information is different, however, and likely to be more credible than identification through latent index models of individual behavior. Program rules are a matter of public record, and observed data can be used to verify enforcement of the rules. Identification through eligibility rules may also provide a good forecast of future program effects under the same rules. Another attractive feature of this approach is that no eligible participant need be denied treatment in experimental designs based on this principle.

Finally, we show that with mild additional assumptions about the participation decision, we can identify a local average treatment effect even if there are no strictly ineligible groups in the sample.

Appendix

Proof of Theorem 2: The selected average treatment effect α is equal to $(E[Y|A =$

$1] - E[Y|A = 0])/(Pr(P = 1|A = 1)$. An efficient estimator can therefore be obtained by substituting efficient estimators for $E[Y|A = 1]$, $E[Y|A = 0]$ and $Pr(P = 1|A = 1)$ in this formula. We will show that

1) $\bar{Y}_{A=1}$ is an efficient estimator for $E[Y|A = 1]$,

2) $\bar{P}_{A=1}$ is an efficient estimator for $Pr(P = 1|A = 1)$,

3) $\bar{Y}_{A=0}$ is an efficient estimator for $E[Y|A = 0]$ if the conditional variance of Y_0 given Z does not depend on Z.

There are two steps. First we show that the model can be characterized by a finite number of conditional moment restrictions. Second we show that given those moment restrictions the three estimators are efficient.

The model implies the following conditional moment restrictions: If $A = 0$ then

$$E[Y - \theta|Z] = 0 \quad E[P|Z] = 0$$

If $A = 1$ then

$$E[P - h_1(Z)|Z] = 0 \quad E[Y - h_2(Z)|Z] = 0$$

The model does not imply any other restrictions. It is essential to show this before proving efficiency using the Chamberlain bounds The argument goes as follows. Suppose we have a datagenerating process for (Y, Z, P) with P binary, satisfying the moment conditions. Then we can always construct a model that satisfies (1) and (2) as follows:

$$E[Y_0|Z] = \theta$$

Choose any non-constant function for $E[Y_0|Z, P = 0]$, let

$$E[Y_0|Z, P = 1] = \{\theta - (1 - h_1(Z))E[Y_0|Z, P = 0]\}/h_1(Z)$$

and then we complete the model by choosing for all $Z \notin \mathcal{Z}_0$

$$E[Y_1|Z, P = 1] = \{h_2(Z) - \theta + h_1(Z) \cdot E[Y_0|Z, P = 1]\}/h_1(Z)$$

This constructed model satisfies $E[Y_0|Z] = 0$ for all Z. For this construction it is essential to have $h_1(Z) > 0$ which is true when $A = 1$ by definition.

Given that the model is fully characterized by the conditional moments, it is straightforward to derive the bounds for the three quantities of interest: $E[h_1(Z)]$, $E[h_2(Z)]$ and θ. The formulas in Chamberlain (1990, p 7) can be applied and simplified directly. First θ. Given a set of N_0 observations with $A = 0$, the bound on the variance of $\sqrt{N_0}(\hat{\theta} - \theta^*)$ is

$$\left\{E\left[\frac{1}{E[(Y_0 - \theta^*)^2|Z]}\right]\right\}^{-1}$$

This simplifies to $E[(Y_0 - \theta^*)^2]$ if there is no heteroskedasticity. The variance of $\sqrt{N_0}(\bar{Y}_{A=0} - \theta^*)$ is $E[(Y_0 - \theta^*)^2]$. Therefore $\bar{Y}_{A=0}$ is efficient if there is no heteroskedasticity. In exactly the same way we look at the variance bound for $E[h_1(Z)]$ and $E[h_2(Z)]$ given a set of N_1 observations with $A = 1$. In both cases the variance is equal to the variance of the average. In other words, $\bar{Y}_{A=1}$ and $\bar{P}_{A=1}$ are efficient estimators.

$\mathcal{QED}$.

Proof of Result 3: Suppose, without losing generality, $p_{z_1} > p_{z_0}$. For all u such that $h(u, z) = 1$, the conditional density of U given $P = 1$ and $Z = z$ is equal to

$$f(u|P=1,Z=z)=\frac{f(u)\cdot h(u,z)}{p_z}$$

To prove 2 $\longrightarrow$ 3 note that if Condition 2 holds, we have

$$(8)\quad \tilde{f}(u)=\frac{p_{z_1}}{p_{z_1}-p_{z_0}}\cdot\left[f(u|P=1,Z=z_1)-\frac{p_{z_0}}{p_{z_1}}f(u|P=1,Z=z_0)\right]$$

$$=f(u)\frac{h(u,z_1)-h(u,z_0)}{p_{z_1}-p_{z_0}}$$

which is non-negative because of Condition 2 and integrates out to one. To prove 3 $\longrightarrow$ 2 note that if $\tilde{f}(u)$ in (1) is non-negative, it must be true that $h(u,z_1)\geq h(u,z_0)$ which implies condition 2 because we assumed $p_{z_1}>p_{z_0}$.

$\mathcal{QED}$.

Proof of Result 4: First note that:

$$f(u)=f(u|Z=z)=p_z\cdot f(u|P=1|Z=z)+(1-p_z)\cdot f(u|P=0,Z=z)$$

We will use $Y_i(u)$ as shorthand for $E[Y_i|U=u]$. Consider the conditional expectation of Y_1 given $P=1$ and $Z=z_1$. It is equal to

$$\int Y_1(u)\cdot f(u|P=1,Z=z_1)du=$$

$$\int Y_1(u)\cdot\left[\frac{p_{z_0}}{p_{z_1}}\cdot f(u|P=1,Z=z_0)+\frac{p_{z_1}-p_{z_0}}{p_{z_1}}\tilde{f}(u)\right]du$$

$$=\frac{p_{z_0}}{p_{z_1}}E[Y_1|P=1,Z=z_0]+\frac{p_{z_1}-p_{z_0}}{p_{z_1}}\cdot\int Y_1(u)\tilde{f}(u)du$$

We can combine this with the conditional expectation of Y_1 given $P=1$ and $Z=z_0$ to get:

$$\frac{p_{z_1}}{p_{z_1}-p_{z_0}} \cdot \Big[E[Y_1|P=1,Z=z_1] - \frac{p_{z_0}}{p_{z_1}} E[Y_1|P=1,Z=z_0]\Big] = \int Y_1(u)\tilde{f}(u)du$$

Next, consider the conditional expectation of Y_0 given $P=1$ and $Z=z_0$. It equals

$$\int Y_0(u)f(u|P=0,Z=z_0)du = \int Y_0(u)\Big[\frac{1}{1-p_{z_0}}f(u) - \frac{p_{z_0}}{1-p_{z_0}}f(u|P=1,Z=z_0)\Big]du$$

The third expectation of interest is that of Y_0 given $P=0$ and $Z=z_1$. It equals

$$\int Y_0(u)f(u|P=0,Z=z_1)du = \int Y_0(u)\Big[\frac{1}{1-p_{z_1}}f(u) - \frac{p_{z_1}}{1-p_{z_1}}f(u|P=1,Z=z_1)\Big]du$$

$$= \int Y_0(u)\cdot\Big[\frac{1}{1-p_{z_1}}f(u) - \frac{p_{z_0}}{1-p_{z_1}}f(u|P=1,Z=z_0) + \frac{p_{z_1}-p_{z_0}}{1-p_{z_1}}\tilde{f}(u)\Big]du$$

The two conditional expectations of Y_0 combine to give

$$\frac{1-p_{z_1}}{p_{z_1}-p_{z_0}} \cdot \Big[E[Y_0|p=1,Z=z_1] - \frac{1-p_{z_0}}{1-p_{z_1}}E[Y_0|P=1,Z=z_0]\Big] = \int Y_0(u)\tilde{f}(u)du$$

Therefore we can identify the average treatment effect

$$\int [Y_1(u)-Y_0(u)]\tilde{f}(u)du$$

which is the average treatment effect for the group that changes its treatment status with the change in the instrument.

QED.

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