

# LATE for School: Instrumental Variables and the Returns to Education\*

Charlie Gibbons

Juan Carlos Suárez Serrato<sup>†</sup>

Mike Urbancic

Department of Economics  
University of California, Berkeley

November 13, 2008

## Abstract

There are many reasons to believe that additional education increases an individual's future wages and reduces his propensity to commit crime. Difficulties arise in estimating these effects due to standard endogeneity concerns. Recent papers by Acemoglu and Angrist (2000) and Lochner and Moretti (2004) follow a common approach to address such concerns, notably the use of instrumental variables. This procedure may not estimate the causal effect of interest, however. To underscore this point, we first discuss the difference between the average treatment effect (ATE) and the local average treatment effect (LATE). The crux of our statement is to highlight a common logical fallacy invoked in the interpretation of these two distinct parameters. We provide hypothetical illustrations of the fallacy and demonstrate the difference between the LATE and the ATE empirically by comparing estimates of returns to education using a trio of standard instruments.

PRELIMINARY DRAFT—COMMENTS ARE APPRECIATED.

---

\*We would like to thank Jas Sekhon for his guidance in this project. Additionally, we appreciate the input of seminar participants at UC Berkeley, especially Rocio Titunik, and the Institute for Humane Studies conference. We also thank attendees of the 2008 AEA Pipeline Conference at UCSB, including Rodney Andrews, Henning Bohn, Yolanda Kodrzycki, Trevon Logan, Fernando Lozano, Todd Sorenen, and Doug Steigerwald, for their comments and suggestions. Of course, the usual disclaimers apply.

<sup>†</sup>Corresponding author; [jcsuarez@berkeley.edu](mailto:jcsuarez@berkeley.edu)

# 1 Introduction

Applied economics is plagued by problems of selection and endogeneity. While econometric techniques attempt to overcome these hurdles, our ability to obtain estimates of causal effects is limited. A common approach to analyzing observational data influenced by these potential biases is the use of instrumental variables (IV). Economists have come to appreciate that this framework imposes important assumptions upon the data, but may not carefully consider the precise treatment effect identified by this approach.

Specifically, the instrumental variables approach estimates the *local average treatment effect* (LATE), rather than the average treatment effect (ATE). Here, “local” refers to the subpopulation that changes its behavior in response to a change in the value of the instrument, a group known as the compliers; the composition of this group is instrument-dependent and unobservable. It has become common practice to use IV estimates to validate OLS estimates and thereby dismiss endogeneity bias, but this comparison fails to distinguish between the LATE and the ATE. These two effects are different if the responses of individuals are heterogeneous. If the covariate is in fact exogenous, then the LATE will equal the ATE, but the reverse is not true in the presence of heterogeneous treatment responses. Comparisons between OLS and IV estimates are based upon the false converse. The focus of this paper is to expose and illustrate this common fallacy.

We consider this logical fallacy both theoretically and empirically. We supply an intuitive explanation of how the LATE and the ATE differ. Regression discontinuity provides a framework familiar to economists that we use to clearly illustrate the identification of a local effect. Then, we use the results of Vytlačil (2002) and Heckman and Vytlačil (2005) to demonstrate the failure arising from comparisons of the ATE and LATE in the contexts of point estimation and in the estimation of distributional treatment effects. We show that the use of the Hausman test to compare OLS and IV estimates also flows from this fallacy.

Turning to an empirical example, we demonstrate these issues by using the instrumental variables approach to estimate both the private and social returns to education by examining increases in wages and decreases in criminal propensities respectively stemming from additional schooling. The literature on this subject provides a unique opportunity to examine the properties of our models. First, the topic has been covered by several important papers that guide our

inquiry; the influential works of Acemoglu and Angrist (2000) and Lochner and Moretti (2004) serve as touchstones that allow us to illustrate important concepts and interpretations behind IV models. Second, large data sets like the U.S. Census are readily available and quite familiar to the applied economist. Lastly, unique to the education literature, three different instruments have been found that may identify the effect of education in the presence of endogeneity. These qualities combine to provide an excellent empirical illustration of the logical flaw that we expose.

In Section 2, we discuss how applied economists approach the problem of endogeneity, with specific focus on the application of IV to the returns to education literature. In Section 3, we begin by offering a simple example illustrating the difference in identifying the LATE and the ATE. An explanation in the regression discontinuity context provides a more concrete formulation. Then, we use the definitions of the LATE and ATE to expose this fallacy analytically and in estimating single parameters and distributional effects. We show that, if there is no endogeneity bias, then the LATE equals the ATE, but the converse is false; if the LATE equals the ATE, that does *not* imply that endogeneity is absent. The interpretation of the Hausman test in the IV framework often follows this false converse. We use the returns to education literature to illustrate the fallacy empirically in Section 4. Lastly, we conclude in Section 5.

## 2 Causal Effects and the Returns to Education

Economists like clean designs that are unconfounded by unobserved variation to estimate parameters of interest. In this ideal, no bias arises from omitted variables. This design can be achieved by having either a truly randomized experiment or a “natural,” quasi-experiment that persuasively asserts that self-selection is based purely upon observable characteristics. Unfortunately, the implementation of OLS and even instrumental variables to the returns to education data are not consistent with the experimental framework.

The education literature considered here attempts to determine the effect of education on wages and incarceration. Ideally, this calculation would compare the outcomes under both the treatment and its absence—the treatment being an additional year of education. Alas, we cannot observe both of these outcomes for each student and the outcome that is observed is based upon endogenous treatment choices. As posited above, differences in mandatory schooling policies

may be exogenous to an individual's choice. The use of this condition as a treatment, however, would require identifying precisely those students for whom the treatment was binding (*i.e.*, the laws changed their behavior). While we cannot explicitly identify these individuals, this unknown population is used by the instrumental variables approach to identify the LATE, however (see Section 3).

Acemoglu and Angrist and Lochner and Moretti employ compulsory schooling age as an instrument for educational choice. The mandatory age for schooling has fluctuated in the U.S. across states and time and this variation can be used as an exogenous factor influencing educational attainment decisions.<sup>1</sup> A similar instrument is minimum employment age laws (Acemoglu and Angrist 2000). Rather than compelling students to attend school for a certain period of time, these laws foreclose the most promising alternative to school—work—and therefore reduce the opportunity cost of school attendance. These laws, too, vary across states and time.

Angrist and Krueger (1991) use a third instrument for educational attainment. Compulsory schooling laws often require students to attend school until they reach a certain age, rather than complete a certain number of years of education. The same laws require that students begin schooling at a certain age. As a result, students with a particular birth date will be systematically older or younger relative to their peers in that grade. Because education requirements are in terms of age, rather than in terms of years of enrollment, older students can drop out with lower educational attainment. Indeed, Angrist and Krueger find that older students in a given grade level were less likely to graduate.

Our empirical analysis compares the results obtained using this trio of instruments. We state immediately that we do not expect all these instruments to yield identical parameter estimates. Different “local” estimates arise from different instruments if students have heterogeneous returns to education. By examining the results generated by different instruments on our data, we show that the parameter estimates are indeed instrument dependent and, while one instrument may produce estimates similar to those arising from an OLS model, that does not imply that endogeneity bias is absent.

---

<sup>1</sup>See (*e.g.*, Lleras-Muney 2005) for additional applications of this instrument.

### 3 Comparing Estimates: LATE *v.* ATE

Before computing instrumental variables estimates, we consider their interpretation independently and relative to OLS estimates. Assuming that the models are correctly specified and that the instruments are valid, we can discern the treatment effects identified by each model. OLS models deliver the average treatment effect (ATE) in the absence of endogeneity. IV models provide the local average treatment effect (LATE) for those individuals for whom the instrument is binding (*i.e.*, those that change their educational attainment in response to changes in compulsory schooling laws, holding their observed characteristics fixed), given the necessary monotonicity assumption (Imbens and Angrist 1994).<sup>2</sup> Note that, because the identifying population is instrument-dependent, the LATE itself is identified only for this unobservable and unidentifiable subset of the population.<sup>3</sup>

To identify the LATE using IV, one may assume a homogeneous response to treatment for all individuals, that the response to treatment is increasing monotonically in treatment level, or the treatment effect is 0 for a subpopulation (Imbens and Angrist 1994). If endogeneity is not present or there is a homogeneous treatment effect, then the LATE will equal the ATE; if it is a factor, then the LATE may or may not equal the ATE.

#### 3.1 A Simple Example

Consider the following simple illustration of the relationship between the LATE and the ATE. Imagine that a population of interest is divided into three groups, each composed of otherwise identical individuals. One group has a high treatment response, one a low response, and one an average response. An individual's group membership is unobserved by the researcher. Since the LATE is instrument-specific and defined for some (unknown) population, in this example it can take a high, low, or average value depending upon the instrument. If the researcher happens to estimate the LATE for the average response group, then the LATE will equal the ATE. This obviously does not prove the absence of endogeneity; individuals will endogenously select into treatment based upon their personal benefits and costs of doing so. This simple situation shows that the equality

---

<sup>2</sup>Specifically, this assumption states that, if  $D_i(z)$  indicates the treatment that individual  $i$  chooses in response to instrument  $z$ , then either  $D_i(z) \geq D_i(z') \ \forall i$  or  $D_i(z) \leq D_i(z') \ \forall i \ \forall z'$ .

<sup>3</sup>Some authors attempt to characterize this hidden subpopulation; (see *e.g.*, Kling 2001). Even more importantly, this subpopulation may be completely different from the one relevant for policy analysis ((see *e.g.*, Heckman 1997)).

of the LATE and ATE does not imply exogeneity of educational choices.<sup>4</sup>

### 3.2 A LATE Example: Regression Discontinuity

As has already been mentioned, the “local” interpretation of the IV estimator arises because this parameter is only identified for an unknown subgroup of the sample. This interpretation may not be entirely intuitive. The regression discontinuity (RD) framework provides a clearer example of a local effect. Here, the probability of receiving treatment changes discontinuously at some threshold along a value scale. While the threshold is determined exogenously, the location of an individual relative to this point may be endogenous. Under either homogeneity of treatment responses or a selection-on-observables assumption, RD identifies the ATE. These assumptions permit the estimation of the ATE via several other strategies as well (*e.g.*, matching). RD is applied because it is able to handle endogeneity by assuming that selection *in the neighborhood of the threshold* is minimal. This belief relies upon assumed random variation in the precise location of an individual along the value scale; individuals near the threshold have roughly similar probabilities of landing above or below the cutoff, suggesting that individuals just above and just below are similar in terms of observed and unobserved characteristics.

Notice the use of “neighborhood,” “near,” and “just” above or below—RD estimates in the presence of endogeneity identify the treatment effect only for those individuals near the threshold.<sup>5</sup> Hence, RD identifies a local average treatment effect, a LATE (Hahn, Todd and der Klaauw 2001). The local nature is clearer here than in the IV context because the researcher limits the sample to a window around the threshold and this entire subsample identifies the LATE; *i.e.*, the subgroup for which the LATE is identified is clear and known. But, the subgroup defining the LATE in the IV framework (the compliers) is unknown and unobservable. This implies that the LATE generated by RD has a clearer interpretation and better informs its application to policy questions relative to a LATE given by IV.

---

<sup>4</sup>Ours is a critique of *identification*, not *estimation*. However, bias in estimation also makes the comparison of OLS and IV estimates more tenuous, since, in finite samples, the IV estimate is biased toward that given by OLS (Bound, Jaeger and Baker 1995). This critique of IV estimation serves to reinforce the error in comparing the LATE and the ATE, an issue of identification, which remains the primary point of this paper.

<sup>5</sup>There is no theoretical basis for defining “near” precisely, but Lee (2008) emphasizes that the “auxiliary prediction” of balance on the observed covariates should hold for those on either side of the threshold.

### 3.3 A Formal Comparison of LATE and ATE

In this section, we refine the distinction between the LATE and ATE and formalize the arguments outlined above. We offer definitions and relationships between the ATE, LATE, and marginal treatment effect (MTE). We then present a more precise, numerical example of the preceding simplified version to demonstrate that equality of IV and OLS estimates does not imply a lack of endogeneity. A second example further elucidates the source of heterogeneity by considering an analogous argument for the case of distributional treatment effects. Lastly, we examine the correct interpretation of the Hausman specification test as applied to the instrumental variables framework.

Imbens and Angrist (1994) present the conditions for which instrumental variables identify the average treatment effect for a subset of the sample, a parameter known as the local average treatment effect (LATE). Their assumptions relate the interpretation of instrumental variables estimates to the experimental framework and consider heterogeneous treatment effects.

In a recent paper, Vytlacil (2002) demonstrates that these assumptions are logically equivalent to those of a latent index model. In this model, treatment is triggered by an unobservable index surpassing a threshold. In particular, given the assumptions that identify the LATE, “there always exists a selection model that rationalizes the observed and counterfactual data” (Vytlacil 2002). Further, Heckman and Vytlacil (2005) demonstrate that the LATE and the ATE can be written as weighted averages of the marginal treatment effect (MTE) over different (endogenous) subsets of the sample. Using these results and an index model framework, we clarify the perils of comparing ATEs and LATEs.

For simplicity, we consider the situation of a binary treatment. Define the potential outcomes under the treatment,  $Y_1$ , and control regimes,  $Y_0$ , as functions of observable covariates  $X$  and unobservable factors in the treatment,  $U_1$ , and control,  $U_0$  states, given by

$$Y_1 = \mu_1(X, U_1) \text{ and } Y_0 = \mu_0(X, U_0).$$

An individual will choose to accept treatment if  $Y_1 \geq Y_0$ . Since  $U_1$  and  $U_0$  are unobservable, we cannot predict this choice directly. Instead, we create a new model of the value of treatment,  $\mu_D$ , which is a function of the observable covariates and instruments  $Z$ , but not of any unobserved

factors. This formulation implies the following latent index model:

$$\begin{aligned} D^* &= \mu_D(Z) - U_D \\ D &= \begin{cases} 1 & \text{if } D^* \geq 0 \\ 0 & \text{otherwise} \end{cases}. \end{aligned}$$

$U_D$  can be a function of  $U_0$  and  $U_1$ , *e.g.*,  $U_D = U_0 - U_1$ . This variable can be viewed as an individual's unobserved value (or utility) from not taking treatment. Without loss of generality,  $U_D$  is normalized to be distributed Uniform[0, 1]. Given that  $\mu_D$  is an arbitrary and unknown function on [0, 1]; as a result of our normalization of  $U_D$ ,  $\mu_D$  is an individual's *p*-score—the probability of taking up treatment conditional on  $Z$  and  $X$ . To see this, note that

$$\Pr(D = 1 | X, Z) = \Pr(D^* \geq 0 | X, Z) = \Pr(\mu_D(Z) - U_D \geq 0 | X, Z) = \Pr(U_D \leq \mu_D(Z) | X, Z) = \mu_D(Z),$$

where the last equivalence follows from the uniform assumption on  $U_D$  and the assumption that  $\mu_D(Z)$  is conditional on  $X$ .

This formulation shows that treatment status  $D$  depends upon both the observable instruments  $Z$  and the unobservable element  $U_D$ . Instead, suppose that there were no unobservable aspects of an individual's choice in this model. Then, again without loss of generality,  $U_D$  would be a degenerate random variable equal to 0. The probability line above would yield a *p*-score that is simply 0 or 1 depending upon the value of  $\mu_D(Z)$ , namely observed covariates. If there is no unobserved component in the problem—*i.e.*, there is no endogeneity—treatment assignment can be perfectly predicted based upon the value of observed covariates.

Write the observed outcome as  $Y = DY_1 + (1 - D)Y_0$ . Assuming that  $Z$  is a set of valid instrumental variables and under the necessary regularity conditions, Vytlačil (2002) shows the model above is equivalent to the assumptions that Imbens and Angrist (1994) supply to identify



the LATE.<sup>6</sup>

Treatment effects can now be analyzed within this structure. Let  $\Delta = Y_1 - Y_0$ . The ATE (conditional on  $X$ ) is defined by<sup>7</sup>

$$\Delta^{ATE}(x) \equiv \mathbb{E}[\Delta | X = x].$$

This would yield a true treatment effect if treatment could be randomly assigned among individuals with  $X = x$ , under full compliance, and in the absence of general equilibrium effects. Under these assumptions, OLS estimates the ATE.

The LATE under instrument  $z$  is defined as the average treatment effect for individuals who did not undertake treatment under the  $z'$  regime, but do take treatment under  $z$ , conditional upon  $X$ ; members of this group change their behavior in response to an (exogenous) change in the instrument and are known as “compliers” (Angrist, Imbens and Rubin 1996). Let  $z, z'$  be two realizations of the instrument. Given this formulation, the LATE can be written as

$$\begin{aligned} \Delta^{LATE}(x, \mu_D(z), \mu_D(z')) &\equiv \mathbb{E}[\Delta | X = x, D_z = 1, D_{z'} = 0] \\ &= \mathbb{E}[\Delta | X = x, \mu_D(z') < U_D \leq \mu_D(z)]. \end{aligned}$$

The second line follows the following logic: if an individual did not take treatment under  $z'$ , then his value of the alternative to treatment,  $U_D$ , must be greater than the value of taking that treatment conditional on the instrument  $z'$ , which is  $\mu_D(z')$ ; the reverse logic implies that  $U_D \leq \mu_D(z)$ ,

---

<sup>6</sup>The assumptions, as stated in Heckman and Vytlačil (2005), are:

1.  $\mu_D(Z)$  is a non-trivial function of  $Z$  conditional on  $X$ .
2.  $(U_0, U_1, U_D)$  are independent of  $Z$  conditional on  $X$  (exclusion restriction of instrument  $Z$ ).
3.  $U_D$  has an absolutely continuous distribution with respect to Lebesgue measure.
4.  $\mathbb{E}[Y_1]$  and  $\mathbb{E}[Y_0]$  are finite.
5.  $0 < \Pr(D = 1 | X) < 1$  (there are both treatment and control groups).
6.  $X_1 = X_0$  a.e. (the treatment and control groups have common support over exogenous parameters).

<sup>7</sup>When we typically think of the ATE or LATE, the parameter is typically unconditional, *i.e.*, integrated over the set of  $x$  covariates. Hence, our usual conception for the ATE is

$$ATE = \int_{x \in X} \Delta^{ATE}(x) dF(x)$$

We maintain the definition above for simplicity and concreteness.

yielding the given expression.

This notation emphasizes that the LATE is an expectation taken over a distribution of unobserved characteristics  $U_D$  defined by the instruments  $z$  and  $z'$ . This subpopulation is the set of compliers, defined as  $\{U_D : \mu_D(z') < U_D \leq \mu_D(z)\}$ . Since  $U_D$  is unobserved, the set of compliers is also unobserved. Lastly, a different pair of instruments would change the set of compliers and, therefore, the LATE.

We continue to refine our exposition by defining the marginal treatment effect (MTE). Take the limit of the LATE as  $\mu_D(z) \rightarrow u_D$  (under the regularity conditions imposed above), giving

$$\Delta^{MTE}(x, u_D) \equiv \lim_{\mu_D(z) \rightarrow u_D} \Delta^{LATE}(x, u_D, \mu_D(z)) = \mathbb{E}[\Delta | X = x, U_D = u_D].$$

This is simply the average treatment effect of individuals with observable characteristics  $x$  and unobservable characteristics  $u_D$ . If  $\Delta$  is a value measure, then the MTE is an individual's willingness-to-pay for treatment given his observed and unobserved characteristics.<sup>8</sup>

At last we are able to express the relationship between the three treatment effects. Specifically, the ATE and LATE can be written

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

$$\Delta^{LATE}(x, \mu_D(z), \mu_D(z')) = \frac{1}{\mu_D(z) - \mu_D(z')} \int_{\mu_D(z')}^{\mu_D(z)} \Delta^{MTE}(x, u_D) du_D. \tag{2}$$

These equations elucidate several important points. First, the LATE is instrument-dependent, while the ATE is not. The limits of integration of the LATE (*i.e.*, the set of compliers) and a multiplicative factor are both functions of the instruments, thus making the parameter itself dependent upon the instrument.

Suppose that there is no endogeneity in this framework. Then,  $\Delta^{MTE}(x, u_D) = \Delta^{MTE}(x)$ .

---

<sup>8</sup>The MTE is often the desired parameter of interest because it permits empirical examinations of the relationship between marginal benefits and costs. It is precisely these values that economic theory uses to design optimal policies. Heckman and Vytlačil (2001) and Heckman and Vytlačil (2005) pursue these considerations, but are beyond the scope of our exposition.

Hence,

$$\begin{aligned}
\Delta^{ATE}(x) &= \int_0^1 \Delta^{MTE}(x, u_D) du_D = \Delta^{MTE}(x) \int_0^1 du_D = \Delta^{MTE}(x), \text{ and} \\
\Delta^{LATE}(x, \mu_D(z), \mu_D(z')) &= \frac{1}{\mu_D(z) - \mu_D(z')} \int_{\mu_D(z')}^{\mu_D(z)} \Delta^{MTE}(x, u_D) du_D \\
&= \Delta^{MTE}(x) \frac{1}{\mu_D(z) - \mu_D(z')} \int_{\mu_D(z')}^{\mu_D(z)} du_D = \Delta^{MTE}(x).
\end{aligned}$$

A second major result is: if there is no endogeneity, then

$$\Delta^{ATE}(x) = \Delta^{LATE}(x) = \Delta^{MTE}(x).$$

We reiterate, however, that the converse is not true—specifically, if the LATE equals the ATE, *it does not follow* that there is no endogeneity. It is conceivable that Equation 1 equals Equation 2, while the latter is still integrated over an endogenous subpopulation. Examples of this fallacy follow below.

As an aside, the following intuition may help explain the relationship between the LATE and the ATE. If there is no endogeneity bias present, then, setting the LATE equal to the ATE, we see that every individual in the sample can be considered a complier; *i.e.*,  $\mu_D(z') = 0$  and  $\mu_D(z) = 1$ . Intuitively, if there is no endogeneity in the model, then the best instrument of treatment is treatment itself. Since treatment is exogenously assigned, it is independent of unobserved characteristics and thus it is a valid instrument. Changing the instrument, which is treatment itself, obviously changes the individual's treatment and thus every individual is tautologically a complier.

This formalization aims to clarify the distinction that we make between the LATE and the ATE. After providing a few examples to illustrate this result concretely, we move to an examination of the empirical literature on the returns to education. The theoretical results demonstrated above outline the reasons why the three common instruments employed in the education literature—compulsory schooling laws, minimum employment ages, and quarter-of-birth—should not be expected to yield the same parameter estimates if educational attainment is an endogenous

choice.

### Example 1: Comparing LATE and ATE

Consider an estimation problem where the LATE is identified following the assumptions above and the MTE has the following form:

$$\Delta^{MTE}(x, u_D) = \begin{cases} 2\% & \text{if } u_D \leq \frac{1}{3}, \\ 1\% & \text{if } u_D \in \left(\frac{1}{3}, \frac{2}{3}\right), \\ 0\% & \text{if } u_D \geq \frac{2}{3}. \end{cases} \quad (3)$$

Treatment here could be, as an example, a job training program that assists those individuals with a low opportunity cost of treatment  $u_D$  more than those with high-valued alternatives. Now, suppose that the instrument is chosen such that the compliers consist of those individuals with  $u_D \in \left(\frac{1}{3}, \frac{2}{3}\right)$ . It then follows that

$$\begin{aligned} \Delta^{ATE}(x) &= \int_0^1 \Delta^{MTE}(x, u_D) du_D = 1\% \quad \text{and} \\ \Delta^{LATE}(x) &= \frac{1}{\frac{2}{3} - \frac{1}{3}} \int_{\frac{1}{3}}^{\frac{2}{3}} \Delta^{MTE}(x, u_D) du_D = 1\%. \end{aligned}$$

The LATE equals the ATE, yet the effect of treatment is not exogenous—it is defined by unobserved factors. Hence, relying upon the equality between the LATE and the ATE to conclude that endogeneity is absent from this model is fallacious. This leap is commonly made throughout the applied econometrics literature, despite it being erroneous; one of the central themes of this paper is to highlight this error.

### Example 2: Distributional Treatment Effects

The result in the previous example arises because treatment varies with unobserved factors. Here we offer an analogous example that estimates not averages of outcomes, but rather distributions of outcomes. We discuss the proper interpretation imparted to these estimates.

The literature on non-parametric estimation of treatment effects has recently explored the

identification of distributions of potential outcomes (see, *e.g.*, Abadie, Angrist and Imbens 2002, Imbens and Rubin 1997, Abadie 2002). Abadie (2002) demonstrates that the same assumptions employed by Imbens and Angrist (1994) to identify the LATE can be generalized to identify functions of potential outcomes. Specifically, by using an instrumental variables approach, he is able to identify the entire cumulative distribution function (CDF) of potential outcomes under treatment for the set of compliers.

Consider the following example where individuals self-select into a treatment regime, by choosing a level of education, for example. Assume that the CDF of potential outcomes (*e.g.*, income) both prior to treatment and under the control regime is

$$F_0(y|u_D) = \frac{1}{100} \times y \quad \forall u_D,$$

where  $y$  is income measured in thousands of dollars. In words, the income distribution is uniform and bounded between \$0 and \$100,000. Additionally, assume that the marginal effect of treatment is proportional to the individual's pre-treatment income,  $y_0$ :

$$\Delta^{MTE}(y_0, u_D) = \begin{cases} 3y_0 & \text{if } u_D \leq \frac{1}{3}, \\ 1.5y_0 & \text{if } u_D \in (\frac{1}{3}, \frac{2}{3}), \\ y_0 & \text{if } u_D \geq \frac{2}{3}. \end{cases}$$

Lastly, assume the existence of an instrument that identifies potential outcomes for the subset of the population with  $U_D \in (\frac{1}{3}, \frac{2}{3})$ .

Now, we can apply the instrumental variables method of Abadie (2002) to estimate the distribution of income of the compliers under the treatment regime. In his paper, he divides the sample based upon the value of the instrument for each individual and calculates the empirical CDF of the outcomes for each value of the instrument.<sup>9</sup> Then, he develops a bootstrapping strategy to test the hypothesis of equivalence of these distributions. In our example, we would get an estimate

---

<sup>9</sup>For expositional purposes, he describes the procedure using a binary instrument. See his paper for additional details.

of the CDF for the compliers, which is:

$$\begin{aligned}
F_1 \left( y_1 \middle| u_D \in \left( \frac{1}{3}, \frac{2}{3} \right) \right) &= F_0(1.5y_0) \\
&= \Pr \left( \frac{3}{2}y_0 \leq y \right) = \Pr \left( y_0 \leq \frac{2}{3}y \right) \\
&= \frac{1}{100} \times \frac{2y}{3} = \frac{y}{150}.
\end{aligned}$$

The first equality follows because  $F_0$  is the distribution of income regardless of the unobserved covariates. Here we see an estimate that is analogous to the LATE, though it estimates an entire CDF, rather than an average. An estimate of the CDF that does not use instruments and thus ignores the endogeneity of the example would generate

$$F_1(y) = \frac{1}{3} \frac{y}{100} + \frac{1}{3} \frac{y}{150} + \frac{1}{3} \frac{y}{300} = \frac{y}{150}$$

This is a CDF analog of the ATE.

This problem illustrates precisely the same issue raised in the prior example. Now, the CDF of the potential outcomes for the compliers (a “local” CDF, perhaps) is the same as the function for all the treated individuals, which ignores the presence of endogeneity. Again, in this case, we must resist the temptation to believe that this equality falsifies claims of endogeneity. This example shows how the logic of our inquiry applies to not only average treatment effects, but functions of outcomes in the presence of endogeneity more generally.

### Example 3: Hausman Specification Tests

In applied econometrics, it is common to compare two estimators with the same asymptotic limit as a specification test of the estimation strategy. In a classic paper, Hausman (1978) outlines the following procedure. First, choose two consistent, asymptotically-normal estimates of a parameter (vector)  $\beta$ ; the first,  $\hat{\beta}_0$ , must be efficient (in that it achieves the Cramér-Rao Lower Bound) under the null hypothesis of correct specification, but, under the alternative hypothesis, may be biased. The second estimate,  $\hat{\beta}_1$ , must be consistent under both the null and alternative hypotheses, but may not be efficient. Then,  $\hat{\beta}_0$  has a zero covariance (matrix) with the difference between the estimates,  $\hat{q} = \hat{\beta}_1 - \hat{\beta}_0$ . Now, the familiar *Hausman test* of  $H_0 : \hat{q} = 0$  can be created.

Many authors in applied econometrics have used the Hausman test to determine the presence of endogeneity by comparing an OLS estimate, which is the efficient estimate under the null hypothesis of exogeneity, to an IV estimate, which is the estimate that is robust to the proposed alternative hypothesis of endogeneity. If  $\beta$  is truly exogenous, then the OLS and IV estimates have the same asymptotic limit; this is because the LATE equals the ATE under exogeneity. This implies that, if the parameters are exogenous, then you cannot reject the hypothesis that the difference between the OLS and IV estimates is 0. The contrapositive is also true; namely, if you can reject the hypothesis that the LATE equals the ATE, then endogeneity is present. But the converse is *not* true; if you cannot reject the null hypothesis, then it is not necessarily the case that the parameters are exogenous. Indeed, this could be another situation, like Example 1, in which the LATE equals the ATE *despite* the presence of endogeneity.

Note that this is a matter entirely separate from the type II error of erroneously failing to reject the null hypothesis. Even if a test of power 1 could be created, failing to reject the null hypothesis does not imply exogeneity. This is because the null hypothesis being tested is *not* exogeneity, but rather the equality of the LATE and the ATE. This equality could arise under three circumstances:

1. There is no endogeneity bias (the conclusion that the researcher would like to draw from failure to reject parameter equality).
2. There is insufficient power to distinguish between the two parameters, which are, in fact different (*i.e.*, a type II error is made).
3. The LATE over an endogenous set of compliers happens to equal the ATE for the entire population (a chance equality between two distinct parameters).

The researcher would like to make the simple claim of (1), accepting (2) as a necessary consequence of statistical estimation. But this assertion ignores the possibility of (3), an entirely separate concern that confounds the analysis.

In summary, if the Hausman test leads you to reject the null hypothesis that the LATE equals the ATE, then endogeneity is present. But, if you cannot reject equality of these two parameters, then you cannot conclude that the covariates are exogenous.<sup>10</sup> This latter conclusion

---

<sup>10</sup>Though it is true that you cannot reject the hypothesis of exogeneity.

is another manifestation of the confusion incumbent in comparing the LATE and the ATE.

### 3.4 LATE *v.* ATE in the Returns to Education Literature

To motivate reconsidering the interpretation of IV estimates, we provide some claims from the returns to education literature that may be based upon the logical flaw that we discuss. Specifically, these quotes suggest that, if the IV and OLS estimates are equal, then there is no endogeneity. Yet, as we have seen, this assertion is false. For example, Lochner and Moretti (2004) state that

[t]hese [instrumental variable] estimates are stable across specifications and nearly identical to the corresponding OLS estimates . . . This indicates that the endogeneity bias is not quantitatively important after controlling for age, time, state of residence and state of birth.

In analyzing the private returns to education and hypothesizing about education externalities, Acemoglu and Angrist (2000) claim that

it is noteworthy that the IV estimates using quarter of birth are very close to the OLS estimates for the same period . . . Thus, estimates of external returns that treat individual schooling as exogenous and endogenous should give similar results, at least for [that period].

These pronouncements follow the converse of the results shown in Section 3.3, however, and are not necessarily true. Lastly, Angrist and Krueger (1991) state that

Using season of birth as an instrument for education in an earnings equation, we find a remarkable similarity between the OLS and the TSLS [two-stage least squares] estimates of the monetary return to education. . . This evidence casts doubt on the importance of omitted variables bias in OLS estimates of the return to education, at least for years of schooling around the compulsory schooling level.

This quote provides a more balanced statement of the results, namely that equality of IV and OLS estimates does not necessarily rule out endogeneity bias, but it does not preclude the possibility that the bias is significant.

## 4 Instrumental Variables Estimation

We illustrate the difference between the ATE and the LATE using the returns to education literature. Clearly, an individual's choice of educational attainment is based upon unobservable



characteristics; an instrumental variables approach attempts to combat this endogeneity problem. We are able to identify three different LATEs because we employ three different sets of instruments. This trio helps to illustrate empirically the difference between the LATE and the ATE.

While Lochner and Moretti only use compulsory schooling laws as an instrument, we augment their data set by adding quarter of birth in order to implement Angrist and Krueger’s (1991) approach as well.<sup>11</sup> Acemoglu and Angrist use these instruments along with minimum employment age laws and we employ the same trio.<sup>12</sup> Since the LATE is instrument-dependent, we do not anticipate the parameter estimates being the same across instruments *because we are estimating the parameters over different subpopulations in each instance*. The use of several instruments underscores that the LATE and ATE are different parameters.

#### 4.1 IV estimates

The education literature considered here uses U.S. Census data on black and white men from 1960, 1970, and 1980. Note that, while these files identify incarcerated respondents, they do not provide information on the crimes committed. Additionally, the Census data only identify individuals in prison at the time of the Census and not individuals that have ever been to prison or have committed crimes. To use incarceration as a proxy for crime *per se*, Lochner and Moretti assume that education does not affect the probabilities of arrest or incarceration or sentence length.<sup>13</sup> Since younger men are more likely to be incarcerated than older men at a given time, there is a significant age trend in these data (Gibbons, Suárez Serrato and Urbancic 2008). Our approach employs a base set of explanatory variables in each specification. Fixed effects for age (categorized into 14 dummies spanning three-year intervals: 20–22, 23–25, . . . ), state of birth, state of residence, and

---

<sup>11</sup>Angrist and Krueger find a “small but persistent pattern” in educational attainment by quarter of birth. This seasonal pattern is only present through the twelfth grade and was not evident in levels of higher education. This result suggests that an individual’s birth date affects educational attainment only through compulsory schooling laws, a necessary prerequisite for an appropriate instrument. This approach is criticized by Bound, Jaeger and Baker (1995) due to weak instruments and incorrect confidence interval coverage. Imbens and Rosenbaum (2005) reply offering an improved estimator for instrumental variables standard errors and deride the use of asymptotic normal approximations to this distribution. Their replication found statistically-similar results to Angrist and Krueger, though with wider confidence intervals boasting correct coverage. Under the assumption that quarter-of-birth does not affect imprisonment other than via education, we apply this instrument to the Lochner and Moretti data. The Bound, Jaeger and Baker (1995) critique is less of a concern here, since we are not confronted by a weak instruments issue (see Appendix II).

<sup>12</sup>See Appendix I for more detailed information regarding the data that we use.

<sup>13</sup>Lochner and Moretti offer a detailed examination of these limitations.

year are included.<sup>14</sup>

The results of the IV regressions (plus the OLS baseline) are given in Table 1.<sup>15</sup> For the incarceration regressions, the parameter estimates are the change in percentage points of an individual’s propensity to be incarcerated at the time of the Census for each additional year of education. In the wage regressions, the estimates are the change in an individual’s log weekly wage for an additional year of education. In the incarceration regressions for whites, we find that the LATE for the compulsory schooling age instrument is similar to the ATE given by OLS. But examination of additional instruments shows that the LATE differs from the ATE. Based upon the arguments of Section 3, it is impossible to conclude that “endogeneity bias is not quantitatively important” (Lochner and Moretti 2004). The parameter estimates for whites vary by a factor of nine; one estimate for blacks is positive (though insignificant). These divergent results demonstrate that the LATE is instrument dependent and, while one LATE estimate is near the ATE, these results suggest that endogeneity bias *is* an important factor in the returns to education.<sup>16</sup>

The results here suggest that, for both blacks and whites, there is a subpopulation for which education does reduce criminal propensity, though there is also a subpopulation for which this effect is insignificant. All the instruments in the wage regressions yield significant, positive effects of education on wages, suggesting that there is a subpopulation for both blacks and whites for which education increases their wages. Even for these regressions that produce unanimous verdicts on significance and direction, we cannot claim that this result holds for *all* individuals, since we do not know which individuals are compliers in any of these regressions. We cannot even identify the proportion of the population for which this result holds.

In Example 3 of Section 3.3, we discuss the application of the Hausman test to compare IV estimates to those of OLS. To recall, if we can reject the hypothesis that the OLS estimate equals the IV estimate, then we can reject exogeneity of educational attainment. But, if we cannot

---

<sup>14</sup>These specifications correspond to those used in Lochner and Moretti, but are broadly consistent with the approach of Acemoglu and Angrist as well. We use the same approach for studying both wages and crime to unite these strands and illustrate the desired methodological points, rather than perform strict replications. Since we add additional instruments to our analysis, we do not use the data provided openly by either pair of authors; instead, we acquire the raw data from their source. See Appendix I for a thorough data description. It should be noted that we do not believe that the regression procedure above gives the best estimates of the OLS parameters—see Gibbons, Suárez Serrato and Urbancic (2008).

<sup>15</sup>To emphasize the note to the tables of the paper, we deviate from convention and indicate significance at the 5% level using a single star and at the 1% level using a pair.

<sup>16</sup>See Section 3.3.

Table 1: IV estimates

(a) Effect of education on imprisonment

|                          | Whites              |      | Blacks              |       |
|--------------------------|---------------------|------|---------------------|-------|
|                          | Estimate            | HS   | Estimate            | HS    |
| OLS estimates            | -0.095**<br>(0.002) | —    | -0.364**<br>(0.014) | —     |
| Compulsory schooling age | -0.087*<br>(0.044)  | 0.04 | -0.589**<br>(0.182) | 1.53  |
| Minimum employment age   | -0.033<br>(0.044)   | 2.05 | 0.198<br>(0.226)    | 6.21* |
| Quarter-of-birth         | -0.275**<br>(0.100) | 3.27 | -0.064<br>(0.553)   | 0.29  |
| All instruments          | -0.083**<br>(0.036) | 0.11 | -0.268<br>(0.153)   | 0.40  |

(b) Effect of education on wages

|                          | Whites             |         | Blacks             |         |
|--------------------------|--------------------|---------|--------------------|---------|
|                          | Estimate           | HS      | Estimate           | HS      |
| OLS estimates            | 0.052**<br>(0.001) | —       | 0.053**<br>(0.001) | —       |
| Compulsory schooling age | 0.139**<br>(0.014) | 35.71** | 0.135**<br>(0.015) | 30.62** |
| Minimum employment age   | 0.374**<br>(0.076) | 18.02** | 0.047<br>(0.024)   | 0.04    |
| Quarter-of-birth         | 0.212**<br>(0.016) | 98.47** | 0.072**<br>(0.026) | 0.57    |
| All instruments          | 0.160**<br>(0.013) | 70.11** | 0.111**<br>(0.013) | 22.27** |

Notes: Parameters for the incarceration regressions are in percentage terms. For all regressions, standard errors are clustered by state-year and appear in parentheses. Sample includes individuals 60 years old and younger. All specifications contain dummies for age category (20–22, 23–25, ...), year, state of birth, and state of residence. Regressions for blacks include a dummy for individuals in the south who turned 14 in 1958 or later to account for the impact of *Brown v. Board of Education*. Hausman test statistics (HS) test for equality between OLS and IV estimates. For the parameter estimates and Hausman tests, one star indicates significance at the 5% level; two stars indicate significance at the 1% level.

reject the null hypothesis of equality, we nonetheless are not able to accept exogeneity. We may find the results suggestive, especially when this equality holds for several instruments with respect to the OLS estimate. The Hausman test statistics (HS) are presented in Table 1. In all but one instance, we cannot reject the null hypothesis of equality for the incarceration regressions. This might lead us to believe that education is exogenous to incarceration, though we cannot provide a level of statistical significance for this assertion. Contrarily, we can reject equality for many of

the estimates in the wage equations. This suggests that education and income are endogenously related, while educational attainment may be exogenous relative to incarceration. By applying and comparing the results of three instruments on these data, we are able to clearly illustrate the significance of the *local* aspect of the LATE.

## 4.2 Validity of the Three Instruments

Two major conditions must be fulfilled for an instrument to be valid. First, the instrument must be correlated with the endogenous regressor. This is known as the *inclusionary restriction*. Here, the instruments must change the educational attainment of some students, holding their other characteristics constant. Additionally, this relationship must be sufficiently strong to produce consistent estimates (Bound, Jaeger and Baker 1995). Appendix II tests for weak instruments.

Second, the *exclusionary restriction* requires that the instrument be uncorrelated with the error term in the second stage regression. This ensures that the instrument only operates on the outcome via the endogenous variable. This, too, is necessary for the instrumental variables estimate to be consistent. For this study, the enactment and amendment of the compulsory schooling and minimum employment laws are assumed to be independent of crime and wages. But, if policy makers during the period of investigation believed the results of the literature (*i.e.*, that more education reduces crime and increases wages) and increased compulsory schooling laws or minimum employment ages in response to increased crime or depressed wages, then the instruments would not be valid. Despite its importance, this condition is untestable.

An additional condition is required to permit the LATE interpretation of the IV estimates—the monotonicity assumption (see Footnote 2). This assumption implies that the instrument alters the decision to participate in treatment in a monotonic fashion for every individual. If, for example, as the value of the instrument increases, people on the whole are more likely to select into treatment, then every individual must be more likely to enter treatment for any increase in the instrument. In our instance, as, say, the compulsory schooling age increases, every individual must be more likely to (weakly) extend their educational career. While this seems to be a plausible assumption in our case, it is untestable.

## 5 Conclusion

Instrumental variables are common in contemporary econometrics. Despite their widespread use, it is difficult to correctly interpret the results that they provide. Specifically, we distinguish between the ATE that arises from a correctly-specified OLS model and the LATE generated by a correctly-specified IV model. If there are homogeneous treatment effects or no endogeneity bias, then the LATE will equal the ATE. Several authors in the returns to education literature, however, assert the converse; if the LATE equals the ATE, then endogeneity bias is minimal. A simple counterexample shows that this logic is flawed. Further, taking advantage of recent developments uniting the structural and non-parametric literatures on the identification of treatment effects, we present a theoretically-precise illustration. Interpretation of the Hausman test in the IV context often follows flawed reasoning; while it can provide satisfactory evidence of endogeneity, it cannot provide such evidence for the null hypothesis of exogeneity.

We demonstrate the relationship between the LATE and the ATE empirically by examining the returns to education. This literature is well-suited to this endeavor because it provides three separate instruments for educational attainment. While some IV estimates generate results equal to those of OLS models, the IV estimates differ from one another. Put differently, different instruments produce different LATEs, thereby implying that heterogeneity and endogeneity are important concerns in this literature. This illustrates that, though the LATE may equal the ATE, this does not imply that endogeneity bias is absent.

Lastly, we must remember that the LATE in IV is identified for an unknown subsample that may well be irrelevant to the policy questions that we are pondering. Hence, the LATE stemming from IV may provide little or no information applicable to a policy problem; indeed, this is one of Heckman's primary critiques of IV. Regression discontinuity is more satisfying here because, though it identifies a LATE, this parameter covers a known subpopulation. While the LATE may be a valuable causal estimate, its applicability for policymaking is questionable.

## References

- Abadie, Alberto. 2002. "Bootstrap Tests for Distributional Treatment Effects in Instrumental Variable Models." *Journal of the American Statistical Association* 97:284–292.
- Abadie, Alberto, Joshua Angrist and Guido Imbens. 2002. "Instrumental Variables Estimates of the Effect of Subsidized Training on the Quantiles of Trainee Earnings." *Econometrica* 70(1):91–117.
- Acemoglu, Daron and Joshua Angrist. 2000. "How Large are Human Capital Externalities? Evidence from Compulsory Schooling Laws." *NBER Macroeconomics Annual* 15:9–59.
- Angrist, Joshua D. and Alan B. Krueger. 1991. "Does Compulsory School Attendance Affect Schooling and Earnings?" *Quarterly Journal of Economics* 106(4):979–1014.
- Angrist, Joshua D., Guido W. Imbens and Donald B. Rubin. 1996. "Identification of Causal Effects Using Instrumental Variables." *Journal of the American Statistical Association* 91(434):444–455.
- Bound, John, David A. Jaeger and Regina M. Baker. 1995. "Problems with Instrumental Variable Estimation when the Correlation Between the Instruments and the Endogenous Explanatory Variable is Weak." *Journal of the American Statistical Association* 90(430):443–450.
- Gibbons, Charles E., Juan Carlos Suárez Serrato and Mike Urbancic. 2008. "Broken or Fixed Effects?" *Working Paper*.
- Hahn, Jinyong, Petra Todd and Wilbert Van der Klaauw. 2001. "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design." *Econometrica* 86(1):201–209.
- Hausman, Jerry A. 1978. "Specification Tests in Econometrics." *Econometrica* 46(6):1251–71.
- Heckman, James J. 1997. "Instrumental Variables: A Study of Implicit Behavioral Assumptions Used in Making Program Evaluations." *Journal of Human Resources* 32(3):441–462.
- Heckman, James J. and Edward Vytlacil. 2001. "Policy-Relevant Treatment Effects." *American Economic Review* 91(2):107–111.
- Heckman, James J. and Edward Vytlacil. 2005. "Structural Equations, Treatment Effects, and Econometric Policy Evaluation." *Econometrica* 73(3):669–738.
- Imbens, Guido and Paul Rosenbaum. 2005. "Robust, Accurate Confidence Intervals with a Weak Instrument." *Journal of the Royal Statistical Society* 168(1):109–126.
- Imbens, Guido W and Donald B Rubin. 1997. "Estimating Outcome Distributions for Compliers in Instrumental Variables Models." *Review of Economic Studies* 64(4):555–74.
- Imbens, Guido W. and Joshua D. Angrist. 1994. "Identification and Estimation of Local Average Treatment Effects." *Econometrica* 62(2):467–475.
- Kling, Jeffrey R. 2001. "Interpreting Instrumental Variables Estimates of the Returns to Schooling." *Journal of Business & Economic Statistics* 19(3):358–364.
- Lee, David S. 2008. "Randomized Experiments from Non-random Selection in U.S. House Elections." *Journal of Econometrics* 142(2):675–697.

- Lleras-Muney, Adriana. 2005. "The Relationship Between Education and Adult Mortality in the United States." *Review of Economic Studies* 72:189–221.
- Lochner, Lance and Enrico Moretti. 2004. "The Effect of Education on Crime: Evidence from Prison Inmates, Arrests, and Self-Reports." *American Economic Review* 94(1):155–189.
- Mikusheva, Anna and Brian P. Poi. 2006. "Tests and Confidence Sets with Correct Size in the Simultaneous Equations Model When Instruments are Potentially Weak." *The STATA Journal* 6(3):335–347.
- Stock, James H. and Motohiro Yogo. 2002. "Testing for Weak Instruments in Linear IV Regression." *NBER Working Paper* .
- Vytlacil, Edward. 2002. "Independence, Monotonicity, and Latent Index Models: An Equivalence Result." *Econometrica* 70(1):331–341.

## Appendix I Data

Lochner and Moretti (2004) use data from the 1960–1980 Censuses in their study. Because we extend their analysis, specifically by the introduction of two additional instruments, we cannot use the version of their data that they make publicly available. Instead, we recollect the data from the Integrated Public Use Microdata Series (IPUMS) at the University of Minnesota. We add quarter of birth for all the Census observations and identify the minimum employment age requirement applicable during their youth. The compulsory schooling age and minimum employment age data are only provided from 1914 forward.<sup>17</sup> Hence, Lochner and Moretti limit their observations to individuals between the ages of 20 and 60 in the 1960–1980 Censuses. We expand this set by including ages 60–70 in the 1970 and 60–80 in the 1980 Censuses.<sup>18</sup> Due to computational limitations, Lochner and Moretti use a 90% sample of their Census file in their paper.<sup>19</sup> Since the seed of this sample was not retained, we are not able to perfectly replicate their sample in our data.<sup>20</sup> Our study takes advantage of increased computing power to examine the entire sample plus the additions listed above. These additions make maximum use of the instruments available.

We follow Lochner and Moretti in analyzing the regression models separately for blacks and whites. This division controls for the inequalities in education experienced by the two races. The regressions for blacks include a dummy variable indicating if the respondent was a black man born in the South who turned 14 in 1958 or later. This incorporates changes in education quality resulting from the Supreme Court decision *Brown v. Board of Education*.<sup>21</sup>

## Appendix II Testing for Weak Instruments

As an initial test for weak instruments, we regress educational attainment and wages on each set of instrument dummies and the base set of explanatory variables (*i.e.*, perform the first stage of

---

<sup>17</sup>Compulsory schooling and minimum employment age data are found in Acemoglu and Angrist (2000).

<sup>18</sup>We also add ages 20–50 from 1950, but most individuals in the 1950 Census do not have educational attainment records. When we include those that do have attainment variables, our results do not change markedly. We cannot utilize the 1990 Census because incarceration status is not made publicly available by the Census bureau.

<sup>19</sup>Personal communication with the author.

<sup>20</sup>In our paper, when we refer to the Lochner-Moretti sample, we are referring to our approximation to their sample; specifically, all black and white men in the 1960–1980 Censuses aged 20–60 born in the contiguous 48 states (Alaska and Hawaii were not part of the union until 1959).

<sup>21</sup>We define the south as those states whose Congressmen supported the Southern Manifesto in protest of the *Brown v. Board* decision, namely Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia.



a two-stage instrumental variables procedure). An initial inclusionary restriction test is that the coefficients on the dummies are significant and positive. This implies that the dummies explain educational attainment and operate as expected. This is an *ad hoc* heuristic, however.

A more reliable test for weak instruments examines the  $F$ -statistic on the joint hypothesis that the coefficients on all the instruments are zero. Stock and Yogo (2002) develop a distribution for this test and our results are listed in Table 2. We can reject the weak instruments hypothesis for most of the instruments, except for some restricted to the 30 years-old and under group and the minimum employment age for all the white wage regressions. The quarter-of-birth instrument overall yields the strongest relationship to education.

Table 2: First-stage  $F$ -tests for weak instruments

| (a) Incarceration regressions |        |        | (b) Wage regressions     |        |        |
|-------------------------------|--------|--------|--------------------------|--------|--------|
|                               | Whites | Blacks |                          | Whites | Blacks |
| Compulsory schooling age      | 53.70  | 31.94  | Compulsory schooling age | 51.47  | 34.75  |
| Minimum employment age        | 51.96  | 29.50  | Minimum employment age   | 4.54   | 22.88  |
| Quarter-of-birth              | 94.20  | 33.78  | Quarter-of-birth         | 81.88  | 30.91  |
| All instruments combined      | 52.91  | 31.17  | All instruments combined | 41.98  | 32.95  |

Notes: Stock and Yogo (2002) provide critical values for a weak instruments test based upon the first-stage  $F$ -statistics. For each instrument separately, the 5% critical value is 13.91; for the joint application of the instruments, this statistic is 20.53.

As an additional safeguard against troubles arising from weak instruments, we use the robust instrumental variables procedure of Mikusheva and Poi (2006). These results are presented in Table 3 and yield roughly the same estimates and significance as standard IV estimates and do not contradict the results of standard IV estimation. This further suggests that our IV estimates are reliable.

Table 3: Robust IV estimates

(a) Effect of education on incarceration

| WHITES     |          |                     |                 |
|------------|----------|---------------------|-----------------|
| Instrument | Estimate | Confidence interval | <i>p</i> -value |
| CSL        | -0.091*  | (-0.164, -0.019)    | 0.014           |
| MEA        | -0.032   | (-0.109, 0.046)     | 0.423           |
| QOB        | -0.277** | (-0.475, -0.083)    | 0.005           |
| All        | -0.088** | (-0.148, -0.028)    | 0.004           |
| BLACKS     |          |                     |                 |
| Instrument | Estimate | Confidence interval | <i>p</i> -value |
| CSL        | -0.618** | (-1.011, -0.229)    | 0.002           |
| MEA        | 0.159    | (-0.308, 0.634)     | 0.506           |
| QOB        | -0.066   | (-1.098, 0.990)     | 0.897           |
| All        | -0.293   | (-0.619, 0.033)     | 0.078           |

(b) Effect of education on wages

| WHITES     |          |                     |                 |
|------------|----------|---------------------|-----------------|
| Instrument | Estimate | Confidence interval | <i>p</i> -value |
| CSL        | 0.143**  | (0.135, 0.152)      | 0.000           |
| MEA        | 0.459**  | (0.393, 0.546)      | 0.000           |
| QOB        | 0.348**  | (0.301, 0.408)      | 0.000           |
| All        | 0.184**  | (0.175, 0.194)      | 0.000           |
| BLACKS     |          |                     |                 |
| Instrument | Estimate | Confidence interval | <i>p</i> -value |
| CSL        | 0.135**  | (0.116, 0.156)      | 0.000           |
| MEA        | 0.047**  | (0.015, 0.078)      | 0.000           |
| QOB        | 0.095**  | (0.015, 0.184)      | 0.000           |
| All        | 0.118**  | (0.101, 0.136)      | 0.000           |

Notes: Parameters for the incarceration regressions are in percentage terms. Confidence intervals of 95% are given. Sample includes individuals 60 years old and younger. One star indicates significance at the 5% level; two stars indicate significance at the 1% level.