# Model Specification in Instrumental-Variables Regression

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**Abstract**

In many applications of instrumental-variables regression, researchers seek to defend the plausibility of a key assumption: the instrumental variable is independent of the error term in a linear regression model. Although fulfilling this exogeneity criterion is necessary for a valid application of the instrumental variables approach, it is not sufficient. In the regression context, the identification of causal effects depends not just on the exogeneity of the instrument but also on the validity of the underlying model. In this paper, I focus on one feature of such models: the assumption that variation in the endogenous regressor that is related to the instrumental variable has the same effect as variation that is unrelated to the instrument. In many applications, this assumption may be quite strong, but relaxing it can limit our ability to estimate parameters of interest. After discussing two substantive examples, I develop analytic results (simulations are reported elsewhere). I also present a specification test that may be useful for determining the relevance of these issues in a given application.

# 1 Introduction

Social scientists often construct instrumental variables for use in regression analysis. The well-known idea is as follows. Consider the regression equation

$$Y_i = \alpha + \beta X_i + \epsilon_i. \tag{1}$$

The scalar $Y_i$ is an observation on a dependent variable for unit $i$, and $X_i$ is a scalar treatment variable. The parameter $\alpha$ is an intercept, $\beta$ is a regression coefficient, and $\epsilon_i$ is an unobserved, mean-zero error term. Here, $Y_i$, $X_i$, and $\epsilon_i$ are random variables. The parameters $\alpha$ and $\beta$ will be estimated from the data. Unlike the classical regression model, $X_i$ may be dependent on the error term, that is, endogenous. The Ordinary Least Squares (OLS) estimator will therefore be biased. Under additional assumptions, however, Instrumental Variables Least Squares (IVLS) regression provides a way to obtain consistent parameter estimates. To use IVLS, we must find an instrumental variable, namely, a random variable $Z_i$ that is statistically independent of the error term in equation (1). Moreover, $X_i$ and $Z_i$ must be reasonably well correlated. The latter condition can be checked (Bound et al. 1995); the former assumption cannot.[1] (Below, these ideas are generalized to apply to $p$ treatments and $q$ instruments). In applications, it is common to devote significant attention to defending the assumption of exogeneity.

The broad point I make in this article is the following. It is not merely the exogeneity of the instrument that allows for estimation of the effect of treatment. The inference also depends on a causal model that can be expressed in a regression equation like (1). Without the regression equation, there is no error term, no exogeneity and no causal inference by IVLS. Exogeneity, given the model, is therefore necessary but not sufficient for the instrumental-variables approach. The specification of the underlying causal model is at issue as well.

[1] Standard overidentification tests using multiple instrumental variables, for instance, assume that at least one instrument is exogenous (Greene 2003: 413-15).

While this general point has been raised by others,[2] I draw attention here to a particular, critical assumption: variation in the endogenous regressor related to the instrumental variable must have the same causal effect as variation unrelated to the instrument. In equation (1), for example, a single regression coefficient $\beta$ applies to endogenous as well as exogenous components of $X_i$. In many applications, this assumption of "homogenous partial effects" may be quite strong, but relaxing it can limit our ability to estimate parameters of interest.

For instance, let $X_i$ be a measure of income and $Y_i$ be a measure of political attitudes, such as opinions about taxation. In the example discussed in Section 2, the population of subjects is limited to participants in a prize lottery. The overall income of subject $i$ then consists of $X_i \equiv X_{1i} + X_{2i}$, where $X_{1i}$ is ordinary income and $X_{2i}$ measures lottery winnings. Overall income $X_i$ is likely to be endogenous, because factors associated with family background influence both ordinary income and political attitudes. However, lottery winnings are correlated with overall income and are also plausibly exogenous. As discussed below, lottery winnings can be used to instrument for overall income $X_i$.[3]

However, this approach requires the true data-generating process to be

$$Y_i = \alpha + \beta(X_{1i} + X_{2i}) + \epsilon_i, \tag{2}$$

as in equation (1). The model assumes that a marginal increment in lottery winnings has the same causal effect on political attitudes as a marginal increment in other kinds of income. Yet lottery winnings may be regarded by subjects as an unusual stream of windfall income and may influence attitudes differently than money earned through work. An alternative model to consider is

$$Y_i = \alpha + \beta_1 X_{1i} + \beta_2 X_{2i} + \epsilon_i \tag{3}$$

[2]See Angrist et al. (1996), Freedman (2006), Heckman et al. (1986, 2006), Imbens et al. (1994), and Rosenzweig et al. (2000).

[3]See Section 2 for details.

with $\beta_1 \neq \beta_2$. According to equation (3), there are heterogenous causal effects across components of $X_i$, that is, heterogenous partial effects. If the true model is equation (3), assuming (2) will produce estimates that are misleading.[4]

The model must be specified before IVLS or another technique can be used to estimate it. The assumption of homogenous partial effects is therefore a general issue, whether or not $X_i$ is endogenous. Applications of IVLS tend to bring the importance of this assumption to the fore, however. When analysts exploit natural experiments or other research designs to construct an instrumental variable $Z_i$, variation in $X_i$ related $Z_i$ may not have the same causal effect as variation unrelated to $Z_i$.[5] Unfortunately, it is often the desire to estimate the effect of variation unrelated to the instrument that motivates us to use IVLS in the first place. Otherwise, we could simply regress $Y_i$ on $Z_i$.

The issue arises in many settings. For instance, in a regression of civil conflict on economic growth, using data from sub-Saharan African countries, economic growth may be endogenous. Annual changes in rainfall may be used as an instrumental variable for economic growth. Yet as discussed in Section 3, different sources of economic growth, such as growth of agricultural or industrial productivity, may have different effects on the probability of civil war in Africa, and rainfall changes may be associated with the growth of agricultural but not industrial productivity. Economic growth, individual income, and other variables of interest to social scientists tend to be summary measures of many component inputs. These inputs may have different effects on the dependent variable, and instrumental variables will be related to some of these inputs but not to others.

The point is not that there is a general failure in IVLS applications. The assumption of

[4]For instance, if $X_{1i}$ and $X_{2i}$ are independent (as when subjects are randomized to levels of lottery winnings), IVLS asymptotically estimates $\beta_2$, the coefficient of the exogenous portion of treatment; see Section 4. In other cases, instrumental-variables regression may estimate a mixture of structural coefficients, but not necessarily a mixture of theoretical interest. On the other hand, estimating the correct model in equation (3) would require an additional instrument, since $X_{1i}$ is endogenous and $X_{2i}$ appears in the equation.

[5]A discussion of natural experiments can be found in Angrist and Krueger (2001) or Rosenzweig and Wolpin (2000); see also Dunning (2005, 2007).

homogenous partial effects may be innocuous in some settings, misleading in others. The examples discussed in this article include some of the strongest recent papers in the literature, in which innovative research designs supply good instruments. Yet the examples also remind us that in the regression context, the identification of causal effects using IVLS depends not just on the exogeneity of the instrument in relation to the model we posit, but also on the validity of the underlying model itself.[6] This is easily forgotten if we are focusing only on arguments about exogeneity.

Whether the assumption of homogenous partial effects is plausible in any given application is mostly a matter for *a priori* reasoning; supplementary evidence may help. At the end of this article, I present a statistical specification test that might be of some use. The specification test requires at least one additional instrument, however, and therefore may be of limited practical utility. The main goal of the paper is thus to underscore the importance of the assumption of homogenous partial effects and to encourage its discussion in applications. Specification of the model should be defended with the same energy used to defend exogeneity.

This discussion extends without difficulty to $p$ treatment variables and $q$ instruments. For instance, the matrix version of equation (1) is

$$Y = X\beta + \epsilon. \tag{4}$$

On the left hand side, $Y$ is an $n \times 1$ column vector. On the right hand side, $X$ is an $n \times p$ matrix with $n > p$. The parameter vector $\beta$ is $p \times 1$, while $\epsilon$ is an $n \times 1$ column vector. Here, $n$ is the number of units, and $p$ is the number of right-hand side variables (including the intercept if there is one). We can think of the rows of equation (4) as i.i.d. realizations of the data-generating process implied by equation (2) or (3), for units $i = 1, ...., n$.[7] The first column of $X$ may be all 1's, so that there is an

[6] Inferring causation from regression may demand a "response schedule" (Freedman 2005: 85-95, Heckman 2000). A response schedule says how one variable would respond, were we to intervene and manipulate other variables; it is a theory of how the data were generated.

[7] In many applications, we may only require that $\epsilon_i$ is i.i.d. across units.

intercept. To use IVLS, we must find an $n \times q$ matrix of instrumental variables $Z$, with $n > q \geq p$, such that (i) $Z'Z$ and $Z'X$ have full rank, and (ii) $Z$ is independent of the unobserved error term, that is, exogenous (Greene 2003: 74-80; Freedman 2005: 175). Exogenous columns of $X$ may be included in $Z$. The IVLS estimator can be written as

$$\hat{\beta}_{\text{IVLS}} = (\hat{X}'\hat{X})^{-1}\hat{X}'Y, \tag{5}$$

where $\hat{X} = Z(Z'Z)^{-1}Z'X$.[8]

Note that $\hat{X}$ is the projection of $X$ onto $Z$ and is (nearly) exogenous.[9] On the other hand, $X$ also has a projection orthogonal to $Z$, which is $e \equiv X - \hat{X}$. Rewriting $X = e + \hat{X}$ and substituting into equation (4), we have

$$Y = (e + \hat{X})\beta + \epsilon. \tag{6}$$

According to the model, $\beta$ applies to both pieces. If in truth these pieces have different coefficients, then the IVLS model is misspecified.

The focus of this article differs from a related literature on instrumental-variables regression. In other papers, often formulated in the context of the Neyman-Holland-Rubin potential outcomes model, individuals or other units are assumed to have distinct responses to treatment; instruments may influence participation in treatment for only a subset of the units. Under suitable assumptions, instrumental variables can identify what Imbens and Angrist (1994) call "local average treatment effects," that is, average treatment effects for the subset of units whose participation in treatment is influenced by the instruments.[10]

[8]Equation (5) is the usual way of writing the two-stage least-squares (IISLS) estimator, $\hat{\beta}_{\text{IISLS}}$. See Freedman (2005:178-9) for a proof that $\hat{\beta}_{\text{IISLS}} = \hat{\beta}_{\text{IVLS}}$.

[9]$\hat{X}$ is not quite exogenous, because it is computed from $X$. This is the source of small-sample bias in the IVLS estimator; as the number of observations grows, the bias goes asymptotically to zero.

[10]See also Angrist et al. (1996) and Heckman et al. (1986, 2006). Rosenzweig and Wolpin (2000) and Freedman (2006) also show that what IVLS estimates depends on the underlying behavioral models that are posited. There is a large literature that discusses other aspects of IVLS (see Bartels 1991, Bound et al. 1995, Hanushek and Jackson 1977: 234-39, 244-45, and Kennedy 1985: 115).

In this paper, I ignore heterogeneity of treatment effects across individuals or units: in the regression models discussed here, coefficients are common to all units.[11] I instead investigate the consequences of heterogeneity across pieces of treatment variables – that is, causal heterogeneity across portions of *X*. I show that using IVLS to identify the effect of an endogenous regressor such as individual income or economic growth depends on specifying a regression model in which all of the inputs or component parts of this regressor have the same effect on the dependent variable. I call this the assumption of homogenous partial effects.

# 2 Political attitudes and lottery winnings

Doherty, Green and Gerber (2005, 2006) are interested in assessing the relationship between income and political attitudes.[12] They surveyed 342 people who had won a lottery in an unidentified Eastern state between 1983 and 2000 and asked a variety of questions about attitudes towards estate taxes, government redistribution, and social and economic policies more generally. Given the number and kinds of lottery tickets that individuals buy, the level of lottery winnings are randomly assigned among lottery players.[13] Abstracting from sample non-response and other issues that might threaten the validity of the inferences,[14] the authors can exploit the lottery to make compelling claims about the causal impact of winnings on political beliefs. It turns out that winning large amounts in a lottery has an effect on some relatively narrow political attitudes – e.g., those who win more in the lottery favor the estate tax less – but lottery winnings have relatively little impact on broader political attitudes, for instance, towards the proper role of government in the economy writ large.

[11]In the Neyman-Holland-Rubin potential outcomes framework, if all units have the same response to treatment, instrumental variables identify the average treatment effect. See Imbens and Angrist (1994: 469).

[12]Portions of the material in this section are based on Dunning (2005, 2007).

[13]Lottery winners are paid a large range of dollar amounts. In Doherty et al.'s sample, the minimum total prize was \$47,581, while the maximum was \$15.1 million, both awarded in annual installments.

[14]See Doherty et al. (2005, 2006) for further details.

However, a question of greater interest concerns the political effects of overall income, not lottery winnings per se. Does the strong research design allow us to generalize from the effect of lottery winnings to the effect of overall income? It does not, without making further assumptions. As Doherty et al. (2005: 8-10, 2006: 446-7) carefully point out, the effect on political attitudes of "windfall" lottery winnings may be very different from other kinds of income – for example, income earned through work, interest on wealth inherited from a rich parent, and so on.

These kinds of concerns may also limit our ability to use IVLS to estimate the causal effect of overall income on political attitudes. Let $A_i$ be a measure of the political attitudes of subject $i$.[15] Consider the regression equation

$$A_i = \beta I_i + \epsilon_i, \tag{7}$$

Here, $I_i$ is the self-reported income (from all sources) of subject $i$. The error term $\epsilon_i$ is a random variable, independently and identically distributed across respondents with $E(\epsilon_i) = 0$. For ease of exposition, the variables $A_i$ and $I_i$ are normalized to have zero mean, and covariates are not included.[16] The goal is to estimate the regression coefficient $\beta$, which measures the impact of overall income on political attitudes; by assumption, $\beta$ is the same for all respondents.[17]

Equation (7) is the standard linear regression set-up, except for one catch: the error term is not independent of income, because unobserved (unmeasured) variables may be associated with both overall income and political attitudes. For instance, rich parents may teach their children how to play the stock market and also influence their attitudes towards government intervention. Peer-group networks may influence both economic success and political values. Ideology may

[15] For instance, $A_i$ might be a measure of the extent to which respondents favor the estate tax, or a measure of opinions about the appropriate size of government.

[16] Doherty et al. (2005, 2006) present a similar linear regression model, though they report estimates of ordered probit models. Their equation (1) includes various covariates, including a vector of variables to control for the kind of lottery tickets bought.

[17] Notice that according to equation (7), subject $i$'s response depends on the values of $i$'s right-hand side variables; values for other subjects are irrelevant. The analog in Rubin's formulation of the Neyman model is the stable unit treatment value assumption (SUTVA) (Neyman 1923, Dabrowska and Speed 1990; Rubin 1974, 1978, 1980; see also Cox 1958, Holland 1986).

itself shape economic returns, perhaps through the channel of beliefs about the returns to hard work. Even if some of these variables could be measured and controlled, clearly there are many unobserved variables that could conceivably confound inferences about the causal impact of overall income on political attitudes.

Given the model in equation (7), however, the innovative research design supplies an excellent instrument – namely, a variable that is both correlated with the overall income of person $i$ and is independent of the error term in equation (7).[18] This variable is the level of lottery winnings of respondent $i$. The next equation is an accounting identity:

$$I_i \equiv O_i + W_i. \tag{8}$$

Here, $W_i$ is a measure of the lottery winnings of survey respondent $i$, while $O_i$ stands for the ordinary income of respondent $i$.[19] Equation (8) implies that

$$\mathrm{Cov}(I_i, W_i) \neq 0, \tag{9}$$

since the variable $W_i$ is a component of $I_i$.[20] Moreover, since levels of lottery winnings are randomly assigned to the lottery-playing survey respondents, winnings should be statistically independent of other characteristics of the respondents, including characteristics that might influence political attitudes. Thus

$$W_i \perp\!\!\!\perp \epsilon_i, \tag{10}$$

where A $\perp\!\!\!\perp$ B means "A is independent of B."

Viewed in the context of equation (7), equations (9) and (10) give the conditions for a valid

[18]Doherty et al. (2005) use instrumental variables.

[19]That is, $O_i$ is shorthand for the income of subject $i$, net of lottery winnings; this could include earned income from wages as well as rents, royalties, and so forth.

[20]This assumes (eminently plausibly) that $\mathrm{Cov}(O_i, W_i) \neq -\mathrm{var}(W_i)$.

instrument. The IVLS estimator is

$$\hat{\beta}_{\text{IVLS}} = \frac{\widehat{\text{Cov}}(W, A)}{\widehat{\text{Cov}}(W, I)}, \tag{11}$$

that is, the sample covariance of lottery winnings and attitudes divided by the sample covariance of lottery winnings and overall income. With these assumptions, equation (11) will provide a consistent estimator for $\beta$ in equation (7).

Note, however, that our ability to generalize from the effect of one treatment – lottery winnings – to the effect of another treatment – total income – is ensured only by the model in equation (7). We can use equation (8) to rewrite equation (7) as

$$A_i = \beta(O_i + W_i) + \epsilon_i. \tag{12}$$

According to the model, it does not matter whether income comes from lottery winnings or from other sources: a marginal increment in either lottery winnings or in ordinary income will be associated with the same expected marginal increment in political attitudes. This is because $\beta$ is assumed to be constant for all forms of income.

An alternative model to consider is

$$A_i = \beta_1 O_i + \beta_2 W_i + \epsilon_i, \tag{13}$$

with $\beta_1 \neq \beta_2$. Here, the variable $W_i$ is plausibly independent of the error term among lottery winners, due to the randomization provided by the natural experiment. However, $O_i$ remains endogenous, perhaps because factors such as education or parental attitudes influence both ordinary income and political attitudes. We could again resort to the instrumental variables approach, but since we need as many instruments as there are regressors in (13), we will need some new instrument in addition to $W_i$.

Suppose the data were generated according to equation (13), and we erroneously assume equation (12). As I show analytically in Section 4, if we estimate (12) using $W_i$ as an instrument for $I_i$, IVLS estimates $\beta_2$ rather than $\beta_1$.[21] Given that the coefficient of $O_i$ is of interest, this may substantially limit the utility of instrumental variables. After all, if we only cared about $\beta_2$, we could simply regress $Y_i$ on $W_i$. The point is not that there is a general flaw in the IVLS approach. The point is that model specification matters; for IVLS to estimate the parameter of interest, the data must be generated according to equation (12), not equation (13).

# 3 Civil war and rainfall

Miguel, Satyanath, and Sergenti (2004) study the effects of economic growth on the likelihood of civil conflict in Africa. According to the influential models of Collier and Hoeffler (1998, 2001), economic factors influence the incidence of civil war because of the important role they play in rebel recruitment (see also Weinstein 2007). Miguel et al. (2004: 727) summarize the approach as follows: "Collier and Hoeffler stress the gap between the returns from taking up arms relative to those from conventional economic activities, such as farming, as the causal mechanism linking low income to the incidence of civil war."[22] According to Collier and Hoeffler, the economic incentives of potential rebels outweigh other factors, such as social injustice, in explaining the incidence of rebellion. In their well-known formulation, it is greed, not grievance, that mainly explains variation in the occurrence of civil wars.

However, there is an important problem for purposes of testing such theories about the influence of economic conditions on civil conflict. As Miguel et al. (2004: 726) point out, "the existing literature does not adequately address the endogeneity of economic variables to civil war

[21]This depends on the independence of $O_i$ and $W_i$, which is due here to the randomization of units to levels of lottery winnings. If the true model is (13) but $O_i$ and $W_i$ are correlated, IVLS will estimate a mixture of $\beta_1$ and $\beta_2$; see Section 4.

[22]Fearon and Laitin (2003), in an alternative though possibly complementary approach, emphasize the importance of state capacity and roughness of terrain in explaining the outbreak and duration of civil war.

and thus does not convincingly establish a causal relationship. In addition to endogeneity, omitted variables – for example, government institutional quality – may drive both economic outcomes and conflict, producing misleading cross-country estimates." Civil conflict may influence economic conditions, and there may be confounding too.

Miguel et al. (2004) posit that the probability of civil conflict in a given country and year is given by

$$\text{Prob}\{C_{it} = 1 | G_{it}, \epsilon_{it}\} = \alpha + \beta G_{it} + \epsilon_{it}. \tag{14}$$

Here, $C_{it}$ is a binary variable for conflict in country $i$ in year $t$, with $C_{it} = 1$ indicating conflict. The economic growth rate of country $i$ in year $t$ is $G_{it}$, $\alpha$ is an intercept, $\beta$ is a regression coefficient, and $\epsilon_{it}$ is a mean-zero random variable.[23] According to the model, if we intervene to increase the economic growth rate in country $i$ and year $t$ by one unit, the probability of conflict in that country-year is expected to increase by $\beta$ units (or to decrease, if $\beta$ is negative). The problem is that $G_{it}$ and $\epsilon_{it}$ are not independent. The proposed solution is instrumental variables regression.

Annual changes in rainfall provide the instrument for economic growth. In sub-Saharan Africa, as the authors demonstrate, there is a positive correlation between percentage change in rainfall over the previous year and economic growth, so the change in rainfall passes one key requirement for a potential instrument. The other key requirement is that rainfall changes are independent of the error term.[24] This is essentially untestable, but Miguel et al. probe its plausibility at length, and the idea seems very sensible.[25] The IVLS estimates presented by Miguel et al. suggest a strong negative relationship between economic growth and civil conflict.[26] This appears to be

[23] Equation (14) resembles the main equation found in Miguel et al. (2004: 737), although I use $G_{it}$ in place of Miguel et al.'s notation for economic growth, and I ignore control variables as well as lagged growth values for ease of presentation. The specification in Miguel et al. is $C_{it} = \gamma G_{it} + X'_{it}\beta + \epsilon_{it}$, so the dichotomous variable $C_{it}$ is assumed to be a linear combination of continuous right-hand side covariates and a continuous error term. The authors clearly have in mind a linear probability model, so in the text I write equation (14) instead.

[24] An exclusion restriction is necessary in this context: $Z$ cannot appear in equation (14). This would be violated if rainfall had a direct effect on warfare, above and beyond its influence on the economy.

[25] Exogeneity of the instrument is not the issue here; for purposes of this discussion, I will assume the change in annual rainfall is exogenous.

[26] "A five-percentage-point drop in annual economic growth increases the likelihood of a civil conflict...in the fol-

compelling evidence of a causal relationship, and Miguel et al. also have a plausible mechanism to explain the effect – namely, the impact of drought on the recruitment of rebel soldiers.

Yet have Miguel et al. estimated the effect of economic growth on conflict? Making this assertion depends on how growth produces conflict. In particular, it depends on positing a model in which economic growth has a constant effect on civil conflict – constant, that is, across the components of growth. Notice, for instance, that equation (14) is agnostic about the sector of the economy experiencing growth. According to the equation, if we want to influence the probability of conflict we can consider different interventions to boost growth: for example, we might target foreign aid with an eye to increasing industrial productivity, or we might subsidize farming inputs in order to boost agricultural productivity.

Suppose instead that growth in agriculture and growth in industry – which both influence overall economic growth – have different effects on conflict, as in the following model:

$$\text{Prob}\{C_{it} = 1 | I_{it}, A_{it}, \epsilon_{it}\} = \alpha + \beta_1 I_{it} + \beta_2 A_{it} + \epsilon_{it}. \tag{15}$$

Here, $I_{it}$ and $A_{it}$ are the annual growth rates of industry and agriculture, respectively, in country $i$ and year $t$.[27] What might motivate such an alternative model? Decreases in agricultural productivity may increase the difference in returns to taking up arms and farming, making it more likely that the rebel force will grow and civil conflict will increase. Yet in a context in which many rebels are recruited from the countryside, as recent studies have emphasized, changes in (urban) industrial productivity may have no, or at least different, effects on the probability of conflict.[28] In this context, heterogenous effects on the probability of conflict across components of growth may be the conservative assumption.

lowing year by over 12 percentage points – which amounts to an increase of more than one-half in the likelihood of civil war" (Miguel et al. 2004: 727). A civil conflict is coded as occurring if there are more than 25 (alternatively, 1,000) battle deaths in a given country in a given year.

[27] The use of the same notation for coefficients as in Section 2 is merely for convenience; for instance, there is no claim here that overall economic growth is an additive function of growth in the industrial and agricultural sectors.

[28] Kocher (2007), for example, emphasizes the rural basis of contemporary civil wars.

If the true data-generating process is equation (14), but economic growth is endogenous, instrumental-variables regression delivers the goods. On the other hand, if the data-generating process is equation (15), another approach may be needed. If $\beta_2$ is the coefficient of theoretical interest, we might use rainfall changes to instrument for agricultural growth in equation (15). However, industrial growth and agricultural growth may both be dependent on the error term in equation (15), in which case a different instrument for industrial growth would be required.[29]

The point for present purposes is not to try to specify the correct model for this substantive context. The objective is to point out that what IVLS estimates depends on the assumed model, and not just on the exogeneity of the instrument in relation to the model. There are important policy implications, of course: if growth reduces conflict no matter what the source, we might counsel more foreign aid for the urban industrial sector, while if only agricultural productivity matters, the policy recommendations would be quite different. Discussing and defending the specification of the model, and not just the plausibility of exogeneity, is therefore a crucial part of IVLS applications.

# 4 What does IVLS estimate when the model is wrong?

If the data-generating process involves heterogenous partial effects and we erroneously assume homogenous effects, what does IVLS estimate? In this section, I analyze a case akin to the example in Section 2, where an endogenous regressor breaks down into the sum of independent exogenous and endogenous pieces. I show that in this case, IVLS asymptotically estimates the impact of the exogenous portion of treatment, not the endogenous piece or a mixture of endogenous and exogenous pieces.

For each observation $i$, the true data-generating process is

$$Y_i = \beta_1 X_{1i} + \beta_2 X_{2i} + \epsilon_i, \tag{16}$$

[29] For instance, conflict may depress agricultural growth, and harm urban productivity as well.

where $\beta_1$ and $\beta_2$ are parameters and $\beta_1 \neq \beta_2$. The subjects are i.i.d., and $\mathrm{E}(\epsilon_\mathrm{i}) = E(X_{1i}) = E(X_{2i}) = 0$. Equation (16) is identical to equation (13) in Section 2, with $X_{1i}$ equal to ordinary income and $X_{2i}$ equal to lottery winnings. Here, $X_{1i}$ is endogenous and $X_{2i}$ is exogenous. In symbols,

$$\mathrm{Cov}(\epsilon_i, X_{1i}) \neq 0 \tag{17}$$

but

$$X_{2i} \perp\!\!\!\perp \epsilon_i, \tag{18}$$

that is, $X_{2i}$ and $\epsilon_i$ are independent. Also, $\mathrm{X}_{1i} \perp\!\!\!\perp X_{2i}$.[30]

Suppose we erroneously assume that data were generated according to

$$Y_i = \beta X_{\mathrm{T}i} + \epsilon_i, \tag{19}$$

where $X_{\mathrm{T}i} \equiv X_{1i} + X_{2i}$ (with "T" for "total"). Equation (19) is the usual regression model, with one exception: $X_{\mathrm{T}i}$ is endogenous, because $X_{1i}$ and $\epsilon_i$ are dependent. However, by construction we have a valid instrument, since $X_{2i}$ is correlated with the endogenous regressor but also independent of the error term.

The instrumental variables estimator is

$$\hat{\beta}_{\mathrm{IVLS}} = \frac{\mathrm{Cov}(X_2, Y)}{\mathrm{Cov}(X_2, X_\mathrm{T})}, \tag{20}$$

where the covariances are taken over data.[31] Now, substituting for $\mathrm{X_T}$ and distributing covariances, we have

$$\hat{\beta}_{\mathrm{IVLS}} = \frac{\mathrm{Cov}(X_2, Y)}{\mathrm{Cov}(X_2, X_1) + \mathrm{Var}(X_2)}. \tag{21}$$

[30]This is as in the example on lottery winnings: subjects are randomized to levels of $X_{2i}$.

[31]Equation (20) is valid because $X_1$ and $\mathrm{X}_2$ have been normalized to have a mean of zero.

By assumption, $X_1$ and $X_2$ are independent, so Cov($X_2$,$X_1$) should be near zero, and

$$\lim_{n\to\infty} \frac{\mathrm{Cov(X_2, Y)}}{\mathrm{Var(X_2)}} = \beta_2. \tag{22}$$

Thus, IVLS asymptotically estimates the impact of the exogenous portion of treatment.[32] It does not estimate the effect of endogenous portion of the aggregate variable of interest, $X_\mathrm{T}$. Yet we are ultimately interested in the effect of $X_1$, which is $\beta_1$, or at least the effect of $X_\mathrm{T}$. Otherwise, we could simply regress $Y$ on $X_2$.

In other cases, the situation may be somewhat more complicated. For instance, when $\mathrm{Cov}(X_1, X_2) \neq 0$, the IVLS estimate of $\beta$ in equation (19) will converge to a mixture of $\beta_1$ and $\beta_2$, the weights being $w = \mathrm{Cov}(X_{2i}, X_{1i})/[\mathrm{Cov}(X_{2i}, X_{1i}) + \mathrm{Var}(X_{2i})]$ on $\beta_1$ and $1 - w$ on $\beta_2$.[33] In simulations reported online, I investigate what IVLS estimates under a range of other assumptions about the true data-generating process.[34]

In short, if the true data-generating process involves different coefficients for different components of the treatment variable $X_i$, and we assume that these components have the same coefficients, IVLS may estimate some data-dependent mixture of the structural parameters, which may not be the quantity of interest. For a more general discussion, see Angrist, Imbens, and Rubin (1996). The analytic results in this section therefore underscore the key role played by model specification: exogeneity of the instruments, given the model, is necessary but not sufficient for valid application of IVLS.

[32] This depends on the independence of $X_{1i}$ and $X_{2i}$ in this example: see equation (21).

[33] In the formula for $w$, Var and Cov operate on random variables, and $w$ could be negative.

[34] See the *Political Analysis* website. Also posted at http://pantheon.yale.edu/~td244/research.html.

## 5 A model specification test

The discussion above suggests a natural specification test, which requires the availability of an additional instrument, $Z_{1i}$, such that

$$Z_{1i} \perp\!\!\!\perp \epsilon \tag{23}$$

and

$$Cov(Z_{1i}, X_{1i}) \neq 0, \tag{24}$$

where the notation is as in the previous section. We will then use IVLS to estimate the model in equation (16) above, that is,

$$Y_i = \beta_1 X_{1i} + \beta_2 X_{2i} + \epsilon_i, \tag{25}$$

using $Z_{1i}$ and $X_{2i}$ (which is exogenous) as the instruments.

Let $\widehat{\Sigma}$ be the estimated variance-covariance matrix for the coefficient estimates:

$$\widehat{\text{Cov}}(\hat{\beta}_1, \hat{\beta}_2 | X_1, Z_1) = \widehat{\Sigma} \tag{26}$$

Using the diagonal and off-diagonal elements of this $2 \times 2$ matrix, we can calculate

$$\widehat{\text{Var}}\,(\hat{\beta}_1 - \hat{\beta}_2) = \widehat{\text{Var}}\,\hat{\beta}_1 + \widehat{\text{Var}}\,\hat{\beta}_2 - 2\,\widehat{\text{Cov}}\,(\hat{\beta}_1, \hat{\beta}_2) \tag{27}$$

The coefficient estimates are asymptotically normal, and $z$-tests for the difference can be applied (see Greene 2003: 77-78 for details). If pooling is appropriate, then the estimated coefficient on $X_1$ should be the same as the estimated coefficient on $X_2$, up to random error. Statistical tests should therefore fail to reject the null hypothesis that $\beta_1$ and $\beta_2$ are equal.

This adaptation of a standard test compares a pooling estimator to a splitting estimator; it could be viewed as a Hausmann test, in which an additional instrument is needed to test the pooling restriction because $x_1$ is endogenous. In simulations, the specification test is able to detect model specification failures with a high degree of accuracy. Of course, like most specification tests, this one is robust only against a limited class of alternatives: we stipulate that the data are generated according to equation (16), and the alternatives are that $\beta_1 = \beta_2$ or $\beta_1 \neq \beta_2$. Moreover, since the test requires the availability of an additional instrument, it may only be useful in certain classes of applications.[35]

# 6 Conclusion

Social scientists often construct instrumental variables for use in regression analysis. A valid instrumental variable $Z_i$ must be correlated with an endogenous regressor $X_i$, and it must itself be exogenous, that is, independent of the error term in the underlying regression model. The first assumption can be checked from the data. The second assumption is generally the more difficult to satisfy, and it is essentially untestable. In applications, analysts often seek to use natural experiments or other research designs to generate plausible instruments (Angrist and Krueger 2001, Dunning 2007, Rosenzweig and Wolpin 2000).

However, it is not enough to have a valid instrument. The regression model linking $Y_i$ to $X_i$ must also be valid. While this may seem obvious, in this article I have drawn attention to a too-infrequently remarked feature of the canonical IVLS regression model: the assumption of homogenous causal effects across portions of the endogenous regressor $X_i$, that is, the assumption of homogenous partial effects.

Violations of this assumption can limit the ability of the instrumental-variables approach to recover causal parameters. For example, in order to use lottery income to estimate the effect of overall income on political attitudes, we must assume that the effects of lottery income and

[35] For instance, I do not attempt to key the test to data from the examples discussed above because I do not see an available additional instrument.

ordinary income are the same. To use rainfall changes to estimate the effect of economic growth on civil conflict, we must assume that growth in the agricultural sector has the same effect as growth in the industrial sector. In short, we need to assume that variation in the endogenous regressor that is related to the instrumental variable has the same effect as variation that is unrelated to the instrument. In many applications, this assumption may be quite strong, and it should be defended with same energy used to defend exogeneity.

If the assumption of homogenous partial effects is wrong, then IVLS estimates can be quite misleading. When heterogeneity takes the simple form discussed in the example on lottery winnings – that is, the endogenous regressor is a sum of independent exogenous and endogenous portions – instrumental variables regression simply estimates the coefficient of the exogenous portion of treatment. In more complicated settings, IVLS may estimate a mixture of the true coefficients, but it will not necessarily estimate a mixture of theoretical interest. Thus, if the model is incorrectly specified, exogeneity may not be much help. The point here is not that a different estimation strategy would be better than IVLS. What is at issue is the specification of the model.

Ultimately, the question of model specification is a theoretical and not a technical one. Whether it is proper to specify constant coefficients across exogenous and endogenous portions of a treatment variable, in examples like those discussed in this paper, is a matter for theoretical consideration, to be decided on theoretical grounds. Supplemental evidence may also provide insight into the appropriateness of the assumption of homogenous partial effects. The issues discussed in this article are not unique to applications of IVLS – indeed, similar issues may arise even if there is no endogeneity – yet special issues are raised with IVLS because we often hope to use the technique to recover the causal impact of endogenous portions of treatment.

What about the potential problem of infinite regress? In the lottery example, for instance, it might well be that different kinds of ordinary income have different impacts on political attitudes; in the Africa example, different sources of agricultural productivity growth could have different effects on conflict. To test many permutations, given the endogeneity of the variables, we would need many instruments, and these are not usually available. This is exactly the point. Deciding when it is appropriate to assume homogenous partial effects is a crucial theoretical issue. That issue tends to be given short shrift in typical applications of the instrumental-variables approach, where the focus is on exogeneity.

The point here is not to encourage data analysis or regression diagnostics (although more data analysis might well be a good idea). Rather, in any particular application, a priori and theoretical reasoning as well as supplementary evidence should be used to justify the specification of the underlying regression model. In some settings, the assumption of homogenous partial effects may be innocuous; in other settings, it will be wrong, and IVLS will deliver misleading estimates. Exploiting a natural experiment that randomly assigns units to various levels of $Z_i$ may not be enough to recover the causal impact of $X_i$, if the regression model that is being estimated is itself incorrect.

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