STATA Exercise 4: Instrumental variable estimation and the returns to education [ANSWERS]

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1. With the most simple OLS regression, we find an estimate of the returns to schooling of 7.1%, i.e. one more year of education increases the weekly wage by 7.1%. Including more explanatory variables as specified by b) reduces the returns to education to 6.3%.

2. The problem with the wage equation is that education may be endogenous, that is education may be correlated with omitted variables in the error term. Two candidates of such omitted variables are ability and motivation: More able/ motivated persons are more likely to have higher schooling levels and earn higher wages. This implies that OLS is biased and inconsistent and in labor economics this is often referred to as ability bias. What is the direction of the bias? Ability bias will imply that OLS estimate of returns to education is higher than the true returns.

3. As an introduction to the Angrist and Krueger (1991) paper, it is useful to emphasize that "The best of today's design-based studies make a strong institutional case, backed up with empirical evidence, for the variation thought to generate a useful natural experiment" (Angrist and Pischke, 2010 p. 12).[1] Angrist and Krueger provide intuitive reasons for why the compulsory school laws are effective and provide descriptive evidence, which supports the intuition. It is important to examine the type of variation in the data, we are using in the empirical set-up.

   A) The graph, which depicts the first-stage, shows a saw-toothed relationship between quarter of birth and the average years of schooling. The difference in educational levels between individuals born in different quarters is fairly small, but there seems to be a distinct pattern that individuals born in 3rd and 4th quarter have higher educational levels.

   B) We find a similar saw-toothed relationship reduced-form relationship (i.e. between quarter of birth and the average log weekly earnings). It is

[1] Angrist, J.D. and J.-S. Pischke (2010), "The Credibility Revolution in Empirical Economics: How Better Research Design is Taking the Con out of Econometrics", *Journal of Economic Perspectives*, Vol. 24, No. 2, pp. 3-30.

important to notice that the weakly earnings are not trending much over the birth cohorts 1930-1939. This is useful to have in mind when we consider the Wald estimator in question 5 since we worry less about age trends in the wages. Angrist and Krueger (1991) discuss whether other factors than compulsory school laws could induce this pattern. Putting the effect of compulsory schooling laws aside, previous and subsequent research (primarily by educational psychologists) indicate that there is a relationship between school starting age and academic performance. However if anything, the consensus seems to be that students starting at an older age are more mature and perform better in school. If this unobserved ability is also rewarded in the labor market, this would imply that individuals born in the first quarter of the year earn more. Hence, this effect will bias the estimate of the returns to schooling downwards. However, see also Bound, Jaeger and Baker (1995, pp. 446-447)[2] for a more critical discussion of this.

4. If compulsory schooling laws should have a plausible effect on years of education, we would expect that they mainly increase years of education for relatively low educational levels. Here, we more or less follow Angrist and Krueger and show that the seasonal pattern in years of education is much less pronounced for persons having a high school degree compared to the full sample. Furthermore, whereas the years of schooling is increasing with the quarter of birth for the full sample, it is largest in the second quarter for persons having at least graduated from high school. Furthermore, when we turn to the proportions of respectively high school graduates, college graduates and persons with a doctoral degree, we only see a similar distinct pattern for proportion the high school graduates, which is increasing in quarter of birth.

5.
$$\hat{E}(Y|Z=0) = 5.903$$
$$\hat{E}(Y|Z=1) = 5.892$$
$$\hat{E}(Y|Z=1) - \hat{E}(Y|Z=0) = -0.0111$$

$$\hat{E}(S|Z=0) = 12.688$$
$$\hat{E}(S|Z=1) = 12.797$$
$$\hat{E}(S|Z=1) - \hat{E}(S|Z=0) = -0.109$$

so the Wald estimate is given by
$$\frac{\hat{E}(Y|Z=1) - \hat{E}(Y|Z=0)}{\hat{E}(S|Z=1) - \hat{E}(S|Z=0)} = \frac{-0.0111}{-0.109} = 0.102$$

[2] Bound, J, D.A. Jaeger and R.M. Baker (1995), "Problems with Instrumental Variables Estimation When the Correlation Between the Instruments and the Endogeneous Explanatory Variable is Weak", *Journal of the American Statistical Association*, Vol. 90, No. 430, pp. 443-450.

6. This implies that increasing education by one year increases the wage by 10.2%. Hence, we actually find a *higher* estimate using an instrument to take account of the endogeneity even though we thought the OLS estimate was already upward-biased.[3] What could be the reason this? It could be classical measurement error in schooling, but a, perhaps, more convincing explanation is that we only estimate the local average treatment effects (LATE) and the compliers have higher returns to additional schooling. We have already seen that it is mainly low educational levels, which are affected. The hypothesis is that the compliers have higher discount rates or face higher interest rates, so they choose a lower educational level.[4]

7. We cannot identify the compliers, but we characterize them.

   i) With the notation that $D = 1$ if high school graduate and 0 otherwise and $Z = 1$ if born in the third and fourth quarter of births, the share of compliers is simply

$$\hat{P}\left(complier\right) = \hat{E}\left(D|Z=1\right) - \hat{E}\left(D|Z=0\right) = 0.779 - 0.763 = 0.017$$

   We see that the share of compliers is low so there is no reason to believe that LATE is close to ATE. However, as we saw in question 4, the instrument pushes primarily individuals with low levels of education. Many policies including compulsory school laws target this group so from this perspective the LATE may be more relevant than ATE or ATT.

   ii) The share of compliers among treated is given by

$$\begin{aligned} P\left(complier|D=1\right) &= \frac{P\left(D=1|complier\right)P\left(complier\right)}{P\left(D=1\right)} \\ &= \frac{P\left(Z=1\right)\left(E\left(D|Z=1\right)-E\left(D|Z=0\right)\right)}{P\left(D=1\right)} \end{aligned}$$

   The estimated version is

$$\begin{aligned} \hat{P}\left(complier|D=1\right) &= \frac{\hat{P}\left(Z=1\right)\left(\hat{E}\left(D|Z=1\right)-\hat{E}\left(D|Z=0\right)\right)}{\hat{P}\left(D=1\right)} \\ &= \frac{0.509 \cdot (0.779 - 0.763)}{0.771} \\ &= 0.011 \end{aligned}$$

[3] Card (2001) surveys the literature and he finds that IV estimates of the returns to schooling are generally larger than OLS estimates, and often larger by 30% or more.

[4] According to Becker's (1964) model of human capital, the marginal returns to education should equalize the discount rate.

iii) The estimated share of compliers among non-treated is given by

$$\begin{aligned}\hat{P}\left(complier|D=0\right) &= \frac{\hat{P}\left(Z=0\right)\left(\hat{E}\left(D|Z=1\right)-\hat{E}\left(D|Z=0\right)\right)}{\hat{P}\left(D=0\right)} \\ &= \frac{0.491\cdot\left(0.779-0.763\right)}{0.229} \\ &= 0.036\end{aligned}$$

iv) Finally, we can compute the share of compliers among blacks as

$$\begin{aligned}\hat{P}\left(complier|black\right) &= \hat{E}\left(D|Z=1,black\right)-\hat{E}\left(D|Z=0,black\right) \\ &= 0.584-0.561 \\ &= 0.024\end{aligned}$$

8. We find the following 2SLS estimates

Instruments	1st quarter	1st quarter	1st-3rd quarter	1st-3rd quarter
Explanatory variables	A	B	A	B
Returns to education	0.105	0.095	0.105	0.099
	(0.02)	(0.03)	(0.02)	(0.02)

The results are very similar to the Wald estimates. When the instrument is truly exogenous and relevant, the IV estimate is consistent whether or not we include explanatory variables. The main reason for including explanatory variables is to obtain more efficient estimates.

9. The 2SLS estimates with quarter of birth - year of birth interactions are given in the table below.

Explanatory variables	A	B
Returns to education	0.089	0.081
	(0.02)	(0.02)
No. of instruments	29	29

Here, we see that estimates which are somewhat lower than previous Wald estimates and more in the range of the initial OLS estimates. Since the year of birth dummies are included in the wage equation, the effect of education on the wage is identified by variation in education across quarters of births within each birth year.

10. Looking at the first stage, we see that not all instruments are significant. It turns out that we have a weak instruments problem. In fact, Angrist and Krueger (1991) paper started a literature on how to deal with weak instruments. We are less ambitious here. We want to be able to detect signs of weak instruments. It can be shown that the bias of 2SLS vanishes in large samples when F-statistic on the excluded instruments gets large. However, when the instruments are weak, the F-statistic varies inversely with the number of instruments. Stock, Wright, and Yogo (2002) suggest

the rule-of-thumb that one needs a F-statistics above 10 to be in the safe zone. Clearly, the F-statistic is well above 10 when using only 3 instruments, but with the interactions between quarter and year of birth dummies, the F-statistic suggests that the estimates suffer from a weak instrument problem.

Explanatory variables	A	A	B
Returns to education	0.105	0.089	0.081
	(0.02)	(0.02)	(0.02)
No. of instruments	3	29	29
F statistic	32.3	4.9	4.7

11. LIML gives almost the same estimate for the cases with the three quarter of birth instruments. This is as we would expect with a F-statistic above 30. With the 29 instruments, where the F-test is below 5, LIML gives slightly higher estimates tham 2SLS (9.3% vs. 8.9%) and (8.4% vs. 8.1%), but the LIML estimates are not significant different from the 2SLS estimates. Furthermore, the standard errors are only slightly larger. Therefore, the LIML estimates indicate that the weak instruments problem does not seem to be a major concern.

LIML				
Explanatory variables	A	B	A	B
Returns to education	0.106	0.100	0.093	0.084
	(0.02)	(0.02)	(0.02)	(0.02)
No. of instruments	3	3	29	29

On the other hand, JIVE gives larger estimates and the estimate is only marginally affected by the changing the specification and by interacting dummies between the three first quarters of birth dummies and year of birth dummies. From JIVE estimates, it seems more likely that we are faced with a weak instrument problem.

JIVE				
Explanatory variables	A	B	A	B
Returns to education	0.111	0.112	0.108	0.109
	(0.02)	(0.03)	(0.03)	(0.04)
No. of instruments	3	3	29	29