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Dan Ackerberg of UCLA on "Structural Identification of Production Functions"

April 24, 2006

I'm writing these up for my G604 class and Prof. Ackerberg both. Don't trust them too far– I don't understand the paper well.

Point (1): What does ω mean?

$$y_{it} = \beta_0 + \beta_k k_{it} + \beta_l l_{it} + \omega_{it} + \epsilon_{it} \tag{1}$$

y is output, k is log of capital, l is log of labor, and ϵ is i i d error.

So far, that is just a Cobb-Douglas production function.

ω is a variable unobserved by the econometrician but observed by the firm. Also, though the paper does not say it outright, it should be correlated with either k or l. If it isn't, it won't bias the β_l, β_k estimates.

Also, it matters, as Ackerberg did sort of say, that the unobserved variable ω affects k and l's marginal product equally. ω is separable from k and l in the equation above, whether it is multiplicative or additive. This is rather like "neutral" technical change, to allude to an old term for a technology variable that increases the impact of both captial and labor. It could be that firm i is especially well managed, for examlpe, so it gets more out of both labor and capital. What is *not* allowed is that firm i has a particularly good hiring agent, so it gets more out of each worker hired but has no particular advantage with respect to capital.

This knocks out my first interpretation of ω. I thought to myself: "A farm might ahve a big ω if the farmer knew the soil would particularly benefit from fertilizer, and so used more of it." In such a case, ω would increase output more if more land were used, but not, I think, if more captial were used. I am confused on this point, though, and it could use formal modelling.

It is a very important point. Remember that our whole model is premised upon not only ω being separable, but ω being correlated with k and l. So we are limited to unobserved variables that do increase the marginal product of k and l— so more of them will be used— but which also affect them in the same way, which is very special. Essentially, I think, we are limited to "more effective" and "less effective" firms, not "firms good at labor" and firms "good at capital". Of course, there is a reason this last is a harder problem: in essence, we are saying that β_k and β_l differ from firm to firm.

I think of this because I have an old paper on econometric bias when some agents are more effective than others. In my context, it is states that are better or worse in terms of the impact of welfare dollars on illegitimacy. This means that in the equation

$$illegitimacy_{state} = \beta_0 + \beta_{state} welfare_{state} + \epsilon_{state} \tag{2}$$

each state has a different coefficient. What we are trying to estimate is the average value of β across states. I show that OLS will lead to a biased estimate of that average, because states with higher β_{state} will choose lower values of $welfare_{state}$.

See Eric Rasmusen, "The Observed Choice Problem in Estimating the Cost of Policies, " Economics Letters (1998) 61(1): 13-15. A very short version of my Public Choice paper which makes the basic point that OLS estimation of the costs of deliberately chosen policies will be biased downwards. (http://rasmusen.org/published/Rasmusen_98.EC_LET.mchoice.pdf).

Eric Rasmusen, "Observed Choice, Estimation, and Optimism about Policy Changes, " Public Choice, (October 1998) 97(1-2): 65- 91. A policy will be used more heavily in a particular time and place where its marginal cost is lower. The analyst who treats times and places as identical will overestimate the policy's net benefit, especially for policy intensities greater than exist in his sample. In regression analysis, the problem can be solved by instrumental variables and a correction for heteroskedasticity. In an example using state- level data, the technique substantially increases the estimated responsiveness of the illegitimacy rate to transfer payments. (http://rasmusen.org/published/Rasmusen_98.PUB_CHOICE.choice.pdf). There is a mistake in one of the explanations, which I point out in some notes that also discuss estimation of intercepts.

Point (2). ω_{it} is Markov– it only depends on $\omega_{i,t-1}$. That's OK. What does it mean? I think it means that the firm, in forecasting ω_{it}, need only look at $\omega_{i,t-1}$. I asked out loud whether this rules out a firm using time-series regressions for prediction. It doesn't in the model as it stands, because a firm would only want to look at $\omega_{i,t-1}$, even if it was allowed to use $\omega_{i,t-2}$ in its forecast also. But in applying the model, it means it applies only to situations in which firms do not find it worthwhile to use time series regression analysis. If we think a firm ought to or does use a forecasting regression instead of just the previous year's value for its econoemtrician-unobserved variables, then this assumption is false.

It's just a simplifying assumption, though, so I don't know that this causes much harm.

Point (2.5). I think, too, that we need ω_{it} to be serially correlated across time. A given firm must have a basic advantage, even though it varies across time; OR, two adjacent time period of a firm must have correlated values, even if all firms are basically equal.

The paper says that the fixed-effects method of estimating this problem assumes perfect correlation of ω_{it} across time; that is, it is ω_i. Then, the econometrician can in essence look at a firm across time to get an estimate of ω_i, and, actually, consistent estimates of β_l and β_k. After all, having an error term with a non-zero mean, which is what $\omega + \epsilon$ would be, only biases the constant, not the other coefficients. Then he can use those estimates of firm-specific effects to combine information from all the firms and improve the estimates.

I think the paper exaggerates when it says taht fixed-effects estimation *requires* perfect correlation. To the extent that it is not perfectly correlated, but only partially correlated, wouldn't we just need to use a serial correlation correction in the first stage (as I described it) of fixed-effects estimation? This would perhaps be lik the old "random effects" idea.

Point (3). Given equation (1) above, OLS will be biased, because k and l are correlated with the unobserved error, $\omega_{it} + \epsilon_{it}$. If we can instrument for k and l, we can solve that problem. The literature is trying to find clever ways to do that instrumenting.

Point (4). Olley and Pakes are using the following idea. Suppose k_{it} is chosen a period in advance, when the firm does not yet know ω_{it}. Then k_{it} is not at the optimum value for period t— it has a random mistake in it. That gives us random variation in the x-variable, variation uncorrelated with the error term, like a natural experiment.

Other kinds of mistakes by the firm– poor information, or bad implementation, or irrationality— woudl help in the same way, as Ackerberg mentioned towards the end.

So we can look at how k changes over time– via investment and depreciation– and use k and investment to get an estimate for ω. We'll use just gross investment, because depreciation is hard to measure, and that means net investment will be badly measured also. We can only do this if investment is monotonic in ω, but that is a reasonable assumption. If investment is NOT monotonic in ω, then a bigger investment would not always be a sign of a firm thinking ω is bigger, which is why we couldn't make a usable estimate. (Isn't there *some* useful estimate we could make? I bet there is— but things would get uselessly complicated.)

So the crucial thing here is a distinction between short run and long run inputs, here labor and capital. If we had various kinds of medium runs we could use them too.

Point (5). A problem with Olley-Pakes is that often investment is zero, so we get lots of zeroes in the data, and we can't back out the ω's being different if a bunch of observations all have $I = 0$. Prof. Glomm brought that up, I think. (Note taht *net* 8investment woudl not have this problem, because it can easily be negative, if gross investment is less than depreciation.)

Ackerberg's reply was that Olley-Pakes can jst drop their 0-I observations. I think I see why that is. That is selection on the Right-hand-side variable, which is generally OK. It just means they've lost some observations. It's never good to lose data points, though, so Levinsohn and Petrin use materials instead of capital for their method.

Point (6). The problem Ackerberg finds is what he calls Collinearity. It is not a problem in Olley-Pakes, but it is in the related tehcnique by Levinsohn and Petrin. What they do is bring in Materials as an input. Materials are variable in the short run. They are a function of capital and the current unobserved variable ω.

$$m_{it} = f_t(\omega_{it}, k_{it}) \tag{3}$$

Since k_{it} is chosen in the previous period,using m_{it} and k_{it} we can back out ω_{it}, as before. But that will not help us, because l_{it} and our estimate of ω_{it} will be collinear.

I'd call it perfect multicollinearity, actually. If you have enough data, you solve collinearity.

Here, with enough data, your statistical significance is sure to go zero. Having more data confirms your problem, making sure that you don't accidentally get any result that look significant.

Point (7). Could it be sensible to assume $\epsilon = 0$, that is, it may be realistic that there are no variables (in the *production* function, not external functions) unobserved by the firm? I think so. Then the dynamic panel approach works better.

I ran out of time, so I will end these notes here, before the paper really gets into its original contributions.