Labour Economics
Supervision 4

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Question 1

i.
(a)

It seems unlikely. A suitable instrument should be correlated with the treatment variable but not the error term. In this case, the treatment variable is the years of university education, and an instrument is usually sought because it is believed that university education is self-selecting. There is probably a correlation between the years of university education and inherent academic ability, and the latter is usually difficult to include in a regeression as there is no widely available proxy. This shouldn't change if we use parental expectations as an instrument; parents' expectations of their child's likelihood of attending university are probably affected by their perception of the child's academic ability.

(b)

A naïve way of assessing the economic return to schooling is to look at the earnings differential between workers with different levels of education. Traditionally, this was thought to overstate the return to education for largely the same reason as (a): higher levels of education could be mostly proxying for higher latent ability. Under this hypothesis, a suitable instrument for years of education should return a smaller coefficient in the earnings function than the OLS estimate. Card [1995] used geographical variation as an instrument for education, and found the opposite: the 2SLS estimate for the coefficient on education was larger than OLS estimates by about 50%. This suggests that the economic return to schooling is *understated* by the earning differential between workers with different levels of education.

One possible reason for this is that there are measurement errors in the levels of education which leads to an attenuation bias. A quick and dirty explanation is that measurement errors in the independent variable, even if unbiased and uncorrelated with the true value, effectively 'stretch' the distribution along one dimension leading to a smaller estimated gradient along that dimension. However, the degree of bias seems too large to be explained by just measurement errors if we take standard estimates of the reliability of survey measures of education.

The other explanation is that this source of exogenous variation does not equally affect all prospective students. There are some in the population who would seek higher education no matter how far the nearest college is, and some who will only attend university if the costs are low enough. The instrumental variable captures the compliers, which are the latter group. There is a good chance that this group is not characteristic of the entire population. For example, they could come from liquidity-constrained households. If the marignal returns to education are higher for this subgroup,

then it means that standard OLS estimates are biased downwards for this particular subgroup. Whether the direction of the bias is still downwards for the entire population is ambiguous. So the evidence from Card [1995] suggests the returns to education could be understated for some groups of workers, although the estimates are imprecise.

ii.

In the same spirit as Card [1995], Angrist and Krueger [1991] (AK) use the season of birth as an instrumental variable for schooling. Children born later in the year are legally required to undergo more years of education than children born earlier, since the legal dropout age is fixed while the age of school entry depends on the child's birthday. At the time, there seemed no reason to think that these seasonal variations would be correlated with any other characteristics captured by the error term in the earnings function. Thus the season of birth seemed a plausible instrument for years of education. The authors found that the 2SLS estimate for the returns to education were close to the OLS estimate, suggesting there is little bias in the latter, although this could simply mean the biases in opposite directions roughly cancel out. (This would actually support the theory that the differences found in Card [1995] are due to differing marginal returns to education across subgroups.)

Buckles and Hungerman [2013] (BH) test the exogeneity assumption often made in studies that use birth month as an instrument. They find that "women giving birth in winter are more likely to be teenagers and less likely to be married or to have a high school diploma". The implication for AK is that there is probably an upward bias in school dropout rates for children born in later months, among other things. If the proportion of students who wish to drop out is not invariant to the birth season, this will bias the estimates for the returns to education. In this case, it is difficult to know which direction the bias points, because the trend found for winter babies includes December and January. In any case, the direction of bias in the 2SLS estimates in AK depends on what we think the association is between certain maternal characteristics and the returns to education.

Children born in the last quarter usually have to wait 1 more year before they can drop out, and tend to be born to unmarried teenage mothers without a high school degree. If we believe children from this subgroup usually do not benefit as much from a mandated additional year of education, and if the maternal trends found by BH are more prevalent in the last quarter than the first, then this will lead to a downward bias in AK's estimates of the returns to education. Of course, there are other possibilities that will bias the results upward as well.

Question 2

i.

This depends on what question the researcher is trying to answer. If the researcher is trying to determine the average earnings of people born to parents at different education levels, to examine persistence of parental characteristics or social mobility, then a 'simple' regression is not too dangerous. However, if the researcher is trying to determine the intergenerational effects of an *exogenous* variation in some parental attribute, for example to inform some policy decision, then the problem of endogeneity is quite relevant. For example, if it is found that the parents' years of education are positively correlated with the child's earnings in the future, a quick claim one might make is that the benefits of higher education are understated since there are intergenerational benefits.

However, it could just be that women with more years of education select into residential areas with more effective school districts, and this drives the earnings differential in the next generation. The relevant policy advice would actually be to improve school districts, not to push everyone into having more years of education (which would work for people in good school districts but not for people who are stuck in bad ones).

ii.

If we know that the timing of implementation between municipalities is not strongly correlated with any other municipal characteristics which might affect children's future earnings, then the variation in years of education associated with the reform could be used as an exogenous variation in parental education. In this case, Black, Devereux, and Salvanes [2005] (BDS) cite both current and previous work which shows "no systematic relationship" between the timing of implementation and several municipal background variables. In any case, they control for such municipality fixed effects so any correlation would not be immediately fatal to the analysis. Migration between municipalities due to the differences in timing of implementation is probably not significant.

Given this dataset, we could perform a two-stage least squares regression to estimate the causal effect of parental education on children's earnings. The first stage would primarily involve regressing parental education levels on a dummy variable indicating whether the parent was affected by the reform. This estimates the degree of variation in parental education 'explained' by the reform. This estimated variation is, in a sense, 'clean'; if our assumptions are correct, the variation 'explained' by the first-stage regression only contains exogenous variation in parental education. The second stage would then involve regressing children's earnings on the predicted values from the first stage regression, in addition to a set of controls such as children's education, year of birth, and municipality characteristics. Provided our assumptions are correct, the coefficient on the predicted values from the first stage should be an unbiased estimate of the effect of parental education on children's earnings.

As it stands, BDS found that performing this analysis for the full sample leads to very imprecise and statistically noisy estimates. This is because the compliers for this treatment are a small subset ofthe population; typically only those with 9 years of education or fewer exhibit any change in their years of education in response to the policy. Restricting their analysis to this subgroup, the 2SLS estimates are more precise but mostly suggest effects close to zero, and "the true causal effect of parental education on child education appears to be weak".

Like with Card [1995], we would have to be wary about whether the marginal effects of parental education are significantly (in the economic sense) different in this subgroup. It may be that people affected by the reform, who were going to drop out as soon as possible anyway, are less able to benefit from the additional education and pass it on to their children through the children's upbringing. As a general rule, this is something that will always affect natural experiments that are based on changes in the years of mandatory education. Still, the results are far from useless. In fact, results that apply to specific subgroups can be said to be more economically relevant than results that show the average effect across the population. The conclusions from BDS can usefully inform policymaking with respect to lower-educated subgroups. If on the other hand we somehow managed to get unbiased estimates for the intergenerational effects of parental education across the whole population, this would lead to a broader but not necessarily clearer picture. Such results might only be useful if we were considering very wide-ranging policies affecting the whole population, and precludes us from devising more targeted policies that reduce the deadweight loss from a one-size-fits-all policy.