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Econ 201

Final Project

Econ 201 – Referee Report

This paper attempts to identify the effect of parental education on child and parental health. The authors do this by exploiting an exogenous shock on education reform that mandated an additional year of schooling. Identifying what causes deficient child health is important because other studies have examined the impact of child health on adult development and have found a negative effect meaning that poor adult outcomes can be linked to early child development. A variety of causal mechanisms have been proposed that explain how parental education levels can affect the health of their children. Many authors have argued that these relationships are spurious.

The Education Act of 1944 in England and Wales was a policy aimed at allocating students to different schooling categories to meet state needs. In 1947, the Act raised the minimum school-leaving age from 14-15. This increased the number of students in secondary schools. The 1934 cohort was the first to be affected. The author shows that mothers and fathers on average received more schooling by a few months. The reform was successful at limiting the number of students dropping out at age 14 and increased the number of students dropping out at age 15. The number of dropouts returns to normal for ages 16 and up. The early life health data is taken from The National Child Development Study which tracks 17,000 babies born in Great Britain from ages week 3-9 on March 1958. This dataset contains information on parents and children about health and education and other background characteristics. They are then tracked on six other occasions to monitor activities and acquire data on various covariates. In addition, the authors gathered information on the mother and their medical records.

The identification strategy used is a 2SLS fuzzy RD method because cohorts before and after the reform should be roughly similar and that leaving school doesn't only depend upon the school reform. The RD method can be used to compare cohorts near 1934 and estimate the local average treatment effect of child health outcomes between cohorts. The first stage uses a dummy variable regression indicating whether the mother or father finished school. This is then used to determine how average schooling length is effected by the exogenous policy shock. The estimated coefficient is then used in the second stage regression equation as a determinant of the child's health outcome.

The author finds a positive relationship between health indicators for new born children and parental education near the lower end of the educational distribution. However, for later in life health indicators there are very small associations and significance tests are not robust to changing the sample size or restricting the sample range. Also, results tend to be different for boys and girls. In general, there is no causal impact of parental education on child health for a variety of health indicators after controlling for covariates although there is correlation as indicated in other articles as well. Some general problems are listed below:

- Selection bias of dropouts
- Exclusion assumption of instrumental variable approach
- No clear demarcation between treatment and non-treatment group (ie: too fuzzy of a regression discontinuity, too much treatment non-compliance)
- External Validity (lack of generalizability to different contexts with different data) (perhaps with different treatments and different cutoffs we may see different results) (only seems valid within this very narrow range of 7 months additional schooling)
- Sensitivity of coefficients to choice of parametric or non-parametric specification (recommend checking functional form with multiple specifications and check the sensitivity of the results) (linear, quadratic, cubic, local linear etc.) (add interaction terms)
- Density tests near the cutoff
- Assumes that distribution of outcome variable doesn't change over time of fixed-effects

Generally, I am very skeptical of designs that assume cohorts are comparable even though being separated by one year. The main assumption is that the two cohorts (1934 and the year prior) are comparable but we cannot support this assumption in any scientific way because students may be very different on unobservables that are positively or negatively related to the outcome of interest. What the authors purport is that dropouts in the previous cohort are like dropouts in the 1934 cohort. Presumably they do share many characteristics that warrant comparison but we can never actually assess if there are unobserved variables that are related to the outcome that differ between the two groups (different socio-economic circumstances). In other words, this design is not "as good as random" at the threshold and an alternative design implementing a matching strategy might be worth pursuing.

There is no way we can determine if the exclusion assumption is satisfied. Perhaps being treated is related to a health outcome. I suggest using regressing health outcomes on the instrument to see if there is any relation. In addition, we should include information whether the inclusion assumption is met (if the school reform was strongly correlated with the decision to go to school). This can explain why the RD design is as fuzzy as it is. Perhaps the instrument is weak and it is biasing our results.

I think the biggest drawback of this paper is that it lacks any alternative functional form which if considered, can change the results. In the above bullet point I suggest a couple of considerations for the authors. In addition, density testing near the cutoff can help us determine if there is any selection into treatment group. Lastly, I do not see any consideration of using fixed effects in the response variable even considering the authors are using longitudinal data.

There may also be errors in measurement by the way the data was acquired for later in life health indicators but since the health indicators were measured by a physician these seem to not be of much concern. My main concern is how robust our estimations are to the construction of the outcome variables and the input variables. If we slightly changed the definition or criteria for what is considered a "specific health outcome" then how much will these results change? The authors have many outcome measures that attempt to measure "health" but these may be subject to categorical problems. Many of the measures are indicators of health, but not health itself. Perhaps if we redefine health in another fashion we might find different results. For example,

chronic condition, mental condition, and acute condition are constructed by combinations of health indicators. If we have a strong basis for redefining how a certain health condition should be constructed how much will the results change? This may be something to consider in a second paper perhaps because this could be time consuming.