

**The causal effect of education on chronic
health: Evidence from the UK.**

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Propper, Michael A Shields**

Critical assessment

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1 Introduction

It's generally accepted that poor health conditions are associated with low levels of education, even if in principle it is possible to think about reasons why the relationship could go in the opposite direction (more educated people may have higher income that allows to consume more alcohol or tobacco, for example). Most economists argue that data show a that higher levels of education are associated with better health conditions.

Janke, Johnston, Propper, and Shields (2020) study the causal impact of education on chronic health conditions. Differences in health conditions across education groups may be generated by socioeconomic differences, say, for example, people with more educated parents may have higher probability of receiving a good health care during their childhood, and at the same time, there is correlation with a person's education and her parents' one.

What the authors are interested in analysing, is the amount of causal link between education and health conditions. The relationship between the two variables may be something more than correlation: it's reasonable to think that education directly affects health status. For example, we may explain the causal link by looking at behaviours such as smoking, alcohol consumption, diet habits. It's possible that people with higher educational level are more conscious about the effects of smoking or of a poor diet, hence they tend to have healthier habits.

Studying this causal link is not as easy as it may seem: we can't just compare health status of more educated people with the health status of less educated people. The reason is that these two groups could have very different characteristics in terms of other factors affecting health. If these two groups have on average very different characteristics (other than education), and these characteristics also impact health status, the difference we observe in terms of health between the groups will not only reflect the impact of education on health, but also the impact of the other factors on health.

Sometimes, however, is possible to implement techniques that allow to isolate the effect of one variable on another. Janke er Al. exploit two reforms that took place in September 1972 and during early 1990s that affected British students to isolate the effect of education on health.

They conclude that there is a large association between education and health

conditions, but the correlation is much larger than the causal estimation. The reason being that the same factors (parents' socioeconomic status, ability ...) are determinants of educational attainment and important determinant in health conditions.

Their conclusions are consistent across the two reforms: neither the extra year of schooling led by the 1972 reform nor the rise in education led by the 1990s reform had relevant effect on reduction in chronic conditions for those affected by the reform. The only relevant causal relationship they detect is the one between education and CVD and diabetes.

2 1972 Reform

2.1 Assumption 1

The first step of Janke, Johnston, Propper, and Shields is to exploit the reform implemented the 1st September 1972: after this date the minimum school leaving age in Britain was raised from 15 to 16 years. The authors compare the average value of chronic conditions for those born before the 1st September 1957 (15 years before 1972), hence not affected by the reform, and those born after the 1st September 1957, which are affected by the reform.

Even if in principle comparing health status of people with different levels of education does not provide any evidence of the causal relationship, in this case the comparison could allow to quantify the causal effect, due to the fact that those that were born after September 1st did not choose to attend one more year of education, but instead they were forced to, due to the presence of the law. This provides a reliable research design as long as three main assumptions are satisfied.

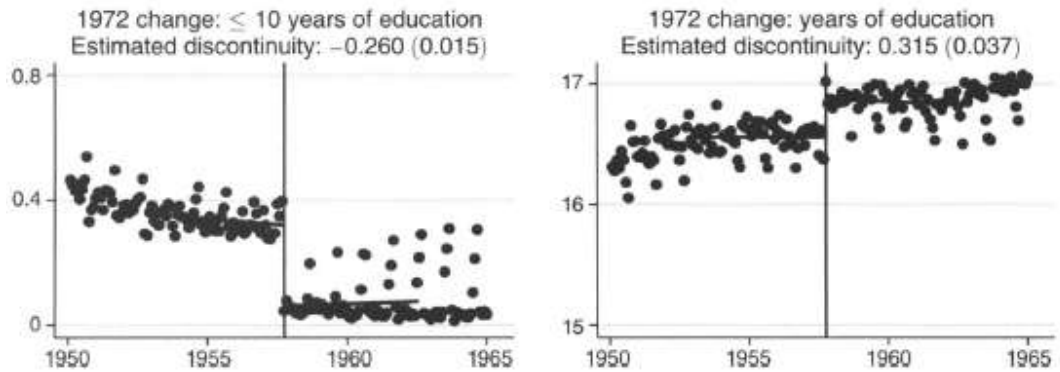


Figure 1: **Source:** Clark and Royer *Effect of Education on Mortality and Health: Evidence from Britain*

Clark and Royer (2013) investigate on the effect of ROSLA (the 1972 reform we are referring to) on educational attainment. In their paper they show that ROSLA decreased the fraction of students that completed ten years of education (leave school when they are 15) by roughly one quarter. In Figure 1 the vertical line denotes the 1957 cohort, the first cohort being affected by that. After 1957 there is an evident reduction of students that complete 10 years of education, and an increase in the average years of education people complete. Their claim is consistent to the one made by the Janke et Al.

2.2 Assumption 2

For a successful comparison there must not be systematic difference among those affected and not affected by the reform. In other words, for their conclusion to be valid, we need to assume that people born some months before the 1st September 1957 are on average equal to people born some months after: individuals close to the cut-off are exchangeable, with respect to observed and unobserved characteristics. If this assumption is satisfied, we could attribute a potential discontinuity at the threshold to the causal effect of compulsory schooling changes.

Clearly, we cannot test the groups being equal in terms of unobserved factors. However, we could in principle falsify this assumption by looking

for any pre-existing difference between the cohorts affected and those not affected by the reform. In order to investigate for the presence of any pre-existing difference one could look for predetermined characteristics at birth, like number of stillborn births, cohort size, infant mortality. If we do not have any balance in terms of observed factors, we should not expect the unobserved factors to be equal as well.

It goes without saying that the closest to the threshold we are, the more the assumption seems reasonable. There are no particular reasons to think a priori that these values should be different, but if they do, we can think that the cohorts are not comparable.

In practice, to assess exchangeability around the cut-off, we could look at plots of the distribution of baseline characteristics as a function of month of birth.

Damon Clark and Heather Royer (2006) study the same reform and provide data on cohort characteristics.

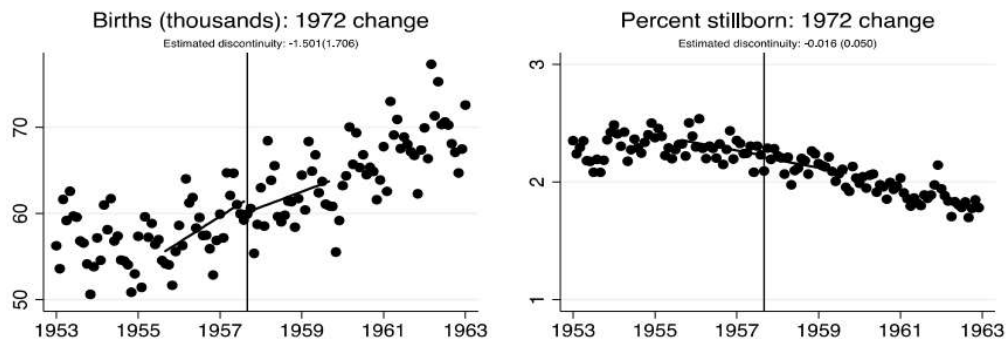


Figure 2: **Source:** Clark and Royer *Online Appendix-Effect of Education on Mortality and Health:Evidence from Britain*

In the Appendix of their paper Clark and Royer provide useful evidence that the baseline characteristics of the cohort affected and not affected by the reform do not present significant discontinuity. This makes less uncomfortable when it comes to assume that the cohort are similar also in terms of unobserved factors.

2.3 Assumption 3

Finally, the third relevant assumption is that the effect of the reform on the probability of having a chronic condition only goes through the increased school attainment, and there is no other effect of the reform on the probability of showing a chronic condition in the following years.

Avendano, de Coulon, Naflyan (2020) find that the 1972 reform led to an increase in the percentage of people reporting having mental health condition (an increase of 0.8 %). In particular the ROSLA increased the prevalence of depression or anxiety. They use an IV strategy to conclude that the additional year of schooling induced by ROSLA increased the probability to report a mental health condition by 3.9 percentage points.

In other words, they argue that there are reasons why those who had to stay in school against their willingness until the age of 16 may have worse mental health conditions than those that left school at 16 by choice. This may suggest that being in school against one's willingness may increase the chance of mental health disorder. They present evidence that suggest that forcing students to stay in school may led to psychological and emotional cost, which could have consequences on mental health.

It follows that the third assumption mentioned may be violated as the change in the schooling law may affect health through channels other than increased educational attainment. In particular, the treated group is exposed to worse mental health conditions.

3 1990 Reform

The second reform exploited is the large rise in British educational attainment that occurred around 1990. The rise in educational attainment resulted as a consequence a number of educational policy changes which occurred in the same period. According to Janke et Al. the policies affected cohorts born in a short window from 1970 to around 1976.

The main reason for looking at this reform is to investigate on the impact of one additional year of education not only when this happens at the minimum school leaving age, but also at higher levels of education. The second reform they study is much different, for two reasons. First it does not involve scholars only aged the minimum dropout age (16 at that time). Second, it's not compulsory, so people are incentivised to stay more years at school, but

not obliged to do so. Hence, this allows to generalise the results previously obtained.

3.1 Assumption 1

As before, the first underlying assumption for the natural experiment to work is that after 1990 the likelihood of a student to attend school for one more year is actually greater than before 1990.

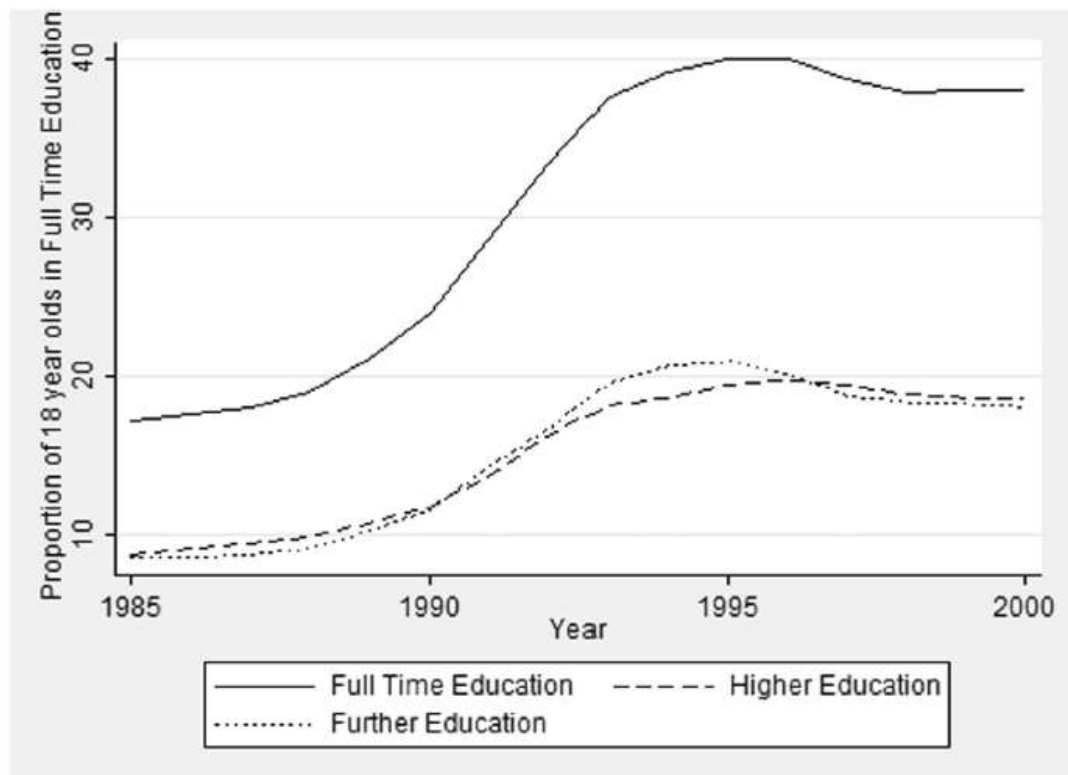


Figure 3: **Source:** James - *Health and education expansion*

The graph taken from James (2015) shows how education level changed from 1985 to 1995.

It shows how the proportion of 18 year olds in full time education rapidly expanded, starting from mid 1980s. James specifies that when considering post compulsory schooling and higher education, the increase was from 17%

in 1985 to 40% in 1995. James also highlights how the average leaving age is about 17 for the 1969 (16 year olds in 1985) rising, throughout the period of expansion, up to 17.7.

It's clear that we cannot say that the set of policies implemented through the years did not have effect on the relevant population. Instead, the policies reached the difficult goal of incentivizing school attainment. The concern is that the change happened during a wider time-window than the one considered by Janke et Al. Even if James highlights that that the expansion is greatest for the cohorts between 1972 and 1975, the effect on school attainment started during 1985

3.2 Assumption 2

The second assumption to be satisfied is that there are no pre-existing differences between the cohort for which the reform applied and those for which the education reform did not apply. According to the authors, the set of policy changes increased the educational attainment of cohorts born from 1970 to 1976. Hence, they compare students born in this window to those born before 1970 (According with the graph in the article, up to 1966).

In order to assess the validity of this assumption, would be interesting to specify a set of features that characterize a priori a cohort, then looking for the average values of these characteristic for the cohort taken into account.

3.3 Assumption 3

Finally, the reform must not have any other direct effects on the outcomes. The probability of having a chronic condition must be the same for people affected and not affected by the policy, a part from the fact of having higher education.

While the 1972 reform only involved an increase in the minimum school dropout age, along the 1990s, as highlighted by the authors, there were a collection of new policies that incentivized young people to attend school, and as they say, there were multiple drivers of this expansion.

The concern is, then, that the changing introduced by the reform may not be just in terms of minimum leaving age, but that that key elements of the school system could be affected.

According to James (2015) the education expansion that took place in the early 90s was the result of two causes: the change in the exam system and in the way higher education has been supplied. The change in the exam system led to a generalised improvement in the results (that's what may have encouraged students to stay in school).

Again, we must assume that the effect of the policy on the outcome only goes through additional years of education. But this reform comprised several changes to the school system. We cannot defend it for being *ceteris paribus* when lowering school entry age.

4 Conclusion

Tests run by Janke, Johnston, Propper, and Shields (2020) to study the causal impact of education on chronic health conditions do not show significant causal impact of additional schooling on most chronic health conditions. Despite the research design implemented has already been used in the literature, they do not provide much information on whether cohort health effects exist and are correlated with the across-cohort growth in educational attainment, upon which the validity of the study relies. As a consequence, it is difficult to be certain that no unobserved trends contribute to the results found.

5 References

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