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## Abstract

There have been many studies estimating the causal effect of an additional year of education on earnings. The majority employ administrative changes in the minimum school leaving age as the mechanism allowing identification. Here we survey 66 such estimates. However, remarkably, while the majority of these studies find substantial gains from education, a number of well-grounded studies find no effect. The average return from these studies still implies substantial average gains from an extra year of education: an average of 8.5%. But the pattern of reported returns shows clear evidence of publication biases. There is, in particular, large scale omission of studies showing negative return estimates. Correcting for these omitted studies, the implied average causals returns to an extra year of schooling are close to 0.

JEL Codes: I26, J24, N3

Keywords: human capital, returns to education, publication bias

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# 1. Introduction

The general problem in the estimation of the causal effects of schooling is that people of higher ability choose more schooling, so that the observed earnings gains from schooling overestimate the actual returns. A number of strategies have thus been devised to control for ability in estimating the return to schooling, as discussed in Card (1999). Some of those strategies, such as using monozygotic twins to control for ability, despite many high profile publications of such results, have proven to be unsound.[2]

The only seemingly reliable methods to estimate the causal effects of education are thus administrative elements of school systems which cause one cohort of students to get more education than another (Card, 1999, p. 1855).

A puzzle, however, has been that such estimates controlling for ability produce estimated returns to education as high or even higher than those from cross-sectional estimates uncontrolled for abilities. Since abilities are clearly positively correlated with years of education, this is unexpected.

Card reports that the most plausible explanation for this is that "the marginal returns to schooling for certain subgroups of the population – particularly those subgroups whose schooling decisions are most affected by structural innovations in the schooling system – are somewhat higher than the average marginal returns to education in the population as a whole." (Card, 1999, p. 1855).

The estimates of substantial causal returns to schooling imply that the extensive provision of public schooling in high income countries in the modern era should have substantially increased the relative earnings of families lower in social status.

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[2] In appendix 1 we discuss why twin estimates are unreliable.

Estimates of relatively constant returns to schooling across the spectrum of years – primary, secondary and tertiary – suggest that any extensions to schooling will be socially valuable. Thus an OECD study of the private and social returns to tertiary and "post-secondary non-tertiary" education found significant private gains, but also significant public gains that came from increased contribution of taxes to the public treasury (OECD, 2013). Reflecting this thinking there have been huge investments in education in modern societies. In the UK in 2017, for example, total investment in education, was close to 10% of NNP. These estimates also suggest that with appropriate interventions peoples' social outcomes can be substantially changed.

Here we conduct a meta-study of 66 estimates of the returns to education derived from increases of the mandated school-leaving age, or legislation banning child labor under certain ages. The average return in these studies is high at 8.5%. But we find strong evidence in the distribution of reported returns of publication biases. These biases take two forms. Publication only of studies where the return is statistically significantly above 0, and publication of studies only where the estimated return is positive. Correcting for these omitted studies, the implied causal returns to schooling are in fact close to 0.

Evidence of publication biases in these causal estimates comes in two forms. First is a link between the standard error of the estimate and the average size of the estimated return.

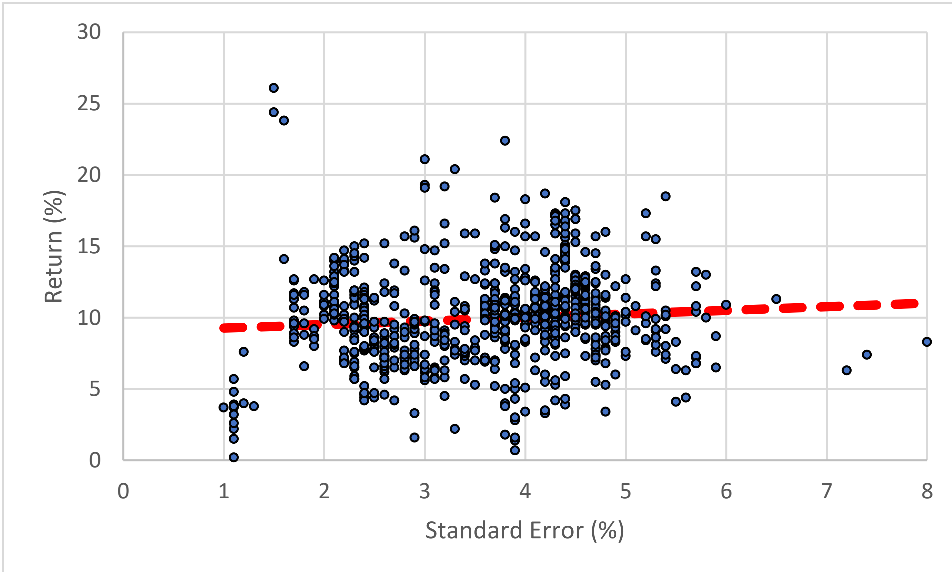



**Figure 1: Gross Returns to Schooling versus Standard Error**
**Source:** Montenegro and Patrinos, 2014, Annex Table 1, columns A and B.

Absent publication biases there should be no connection between the average effect size estimated and the standard error of that estimate. A meta-study of the gross returns to an additional year of schooling (estimated using the Mincer equation which regresses log wage on years of schooling with no controls for ability), for example, reported estimated returns and the standard error of these estimates for 819 estimates (Montenegro and Patrinos, 2014). The average return was 10.1%. Figure 1 plots these estimated returns against the standard error of the estimates with the fitted regression line.[3] As can be seen there is very slight evidence of publication bias in the form of a positive slope of the regression line. However, the slope of this line was not significantly different from 0. Thus there is no clear evidence of publication bias in this literature.

The second evidence of publication bias comes from the non-normal distribution of estimated returns in the case of these causal estimates in the return to schooling. These returns should show a normal distribution absent publication bias. This is shown in Figure 2 for the 819 gross return estimates in the meta-study above.

[3] The regression line was fitted including an indicator for returns in high income countries.

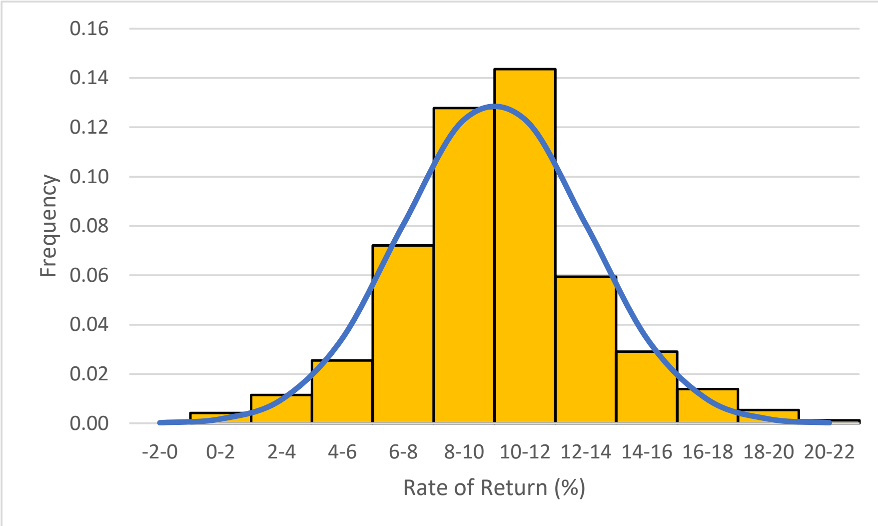



**Figure 2: Distribution of Returns to a Year of School, 1970-2012**

**Notes:** Returns estimated using Mincerian regressions of ln(wage) on years of schooling. The normal curve is that based on the mean and standard deviation of reported returns.

**Source:** Montenegro and Patrinos, 2014, Annex Table 1, column A.

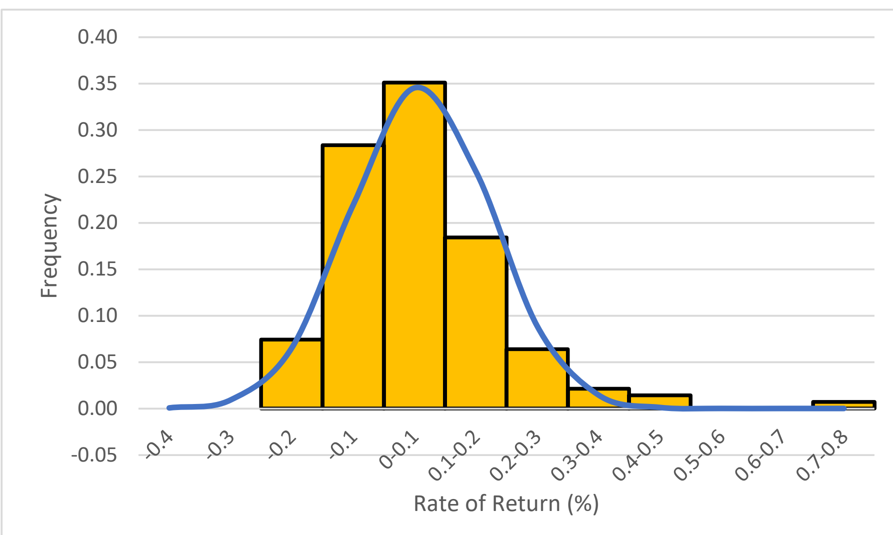



**Figure 3: Distribution of Effect Sizes from Educational RCTs**

**Source:** Lortie-Forgues and Inglis, 2019, figure 1.

As can be seen in figure 2 the estimated returns have close to a normal distribution.[4]

Similarly, figure 3 shows the distribution of effect sizes, measured in standard deviation units, in a meta-study of 141 randomized control trials of educational interventions, with 271 distinct academic outcomes, where the evaluators were independent of those proposing the intervention, and all the study results were published. These RCTs were commissioned by the UK-based Education Endowment Foundation and the U.S.-based National Center for Educational Evaluation and Regional Assistance, which evaluated interventions aimed at improving academic achievement in kindergarten, primary and secondary education. For a trial to be included in the study, the allocation of children to the intervention and control groups had to be random, and the outcome academic in nature (Lortie-Forgues and Inglis, 2019). The mean effect size was surprisingly small – a gain in performance of 0.06 standard deviation units. Here the fit with the normal distribution is not as clear, but note the substantial number of trials which had negative effects on student outcomes.[5]

Thus absent publication bias we would expect the 66 estimates examined here to yield a normal distribution of estimates of returns. We shall see, however, that the causal estimates catalogued here on compulsory schooling show instead just half a normal distribution centered around 0, with implications of severe publication bias. The true implied causal return to education is thus close to 0.

## 2. Criteria for inclusion in the meta-study

In this review we follow two simple criteria for inclusion of papers. These criteria were set before the search for papers began. The papers included had to contain:

(1) Original data analysis with a causal component based on changes in compulsory schooling laws, or in similar legal or institutional changes which changed average years of schooling, such as adoption or modification of a child labor law. We also included cases where accidental

[4] There is sign, however, even in this case of some publication bias. There were no estimates of returns which were negative. If there was symmetry with the upper tail of the estimates, there should have been 6 such estimates. Correcting for this likely omission the average return was 10%.

[5] Given that these were RCTs, independently evaluated, we would expect that the effect size found would be, as in figure 1, largely independent of the standard error. However, presumably just by chance, there was a significant positive association between the effect size and the standard error.

features such as month of birth, under existing schooling laws, led to differential years of schooling by birth month.

(2) An adult outcome for the affected children which can be translated into an estimate of the earnings effect of a change in schooling quantity.

The criteria were chosen to avoid correlational studies, as well as repetitions of the same analysis. They are not restrictive, in order to include as many relevant papers as possible. Thus we also included compulsory schooling reforms that were accompanied by changes in the structure of the education system, such as in Sweden in the 1950s where a compulsory schooling extension was accompanied by abolishing tracking based on grades (Meghir and Palme 2005). The assumption here is that the effects of changes in the structure of education were a random shock with respect to changes of time in school. The focus on compulsory education laws or child labor laws means that the results here concern just primary and secondary education.

Both published papers and working papers were included. You could argue that this is misleading since we identify publication bias as a major distortion in these estimates. Publication bias can also exist, however, in terms of what papers researchers choose to complete and publicize as working papers. We do show, however, that the results are robust to the exclusion of working papers.[6]

The papers in the review were found by using general searches on Google Scholar and other similar platforms. The searches included "compulsory schooling laws", "effects of education", "returns to education", etc. Many papers were also found using literature lists from the initial batch of papers we identified, as well as other review studies. We can't confirm that we have found every relevant paper, but it is likely we have identified most, when looking at literature lists and other reviews.

Some papers report multiple different outcomes: for men versus women, or for multiple extensions of compulsory education. In each case we included all the independently estimated effects in the source.

[6] Results excluding working papers and papers from "less prestigious journals" (proxied by journal impact factor) can be replicated using the do-file in the online appendix.

Though most of the papers included below utilize compulsory schooling laws that extend the time children must spend in school, there is a lot of variation in the mechanics of these extensions. Though most extensions added an additional year to compulsory school attendance, Turkey in 1997 increased years of compulsory schooling from 5 to 8 years (Torun 2018). The typical effect of extensions of compulsory schooling was a sharp discontinuity in educational attainment across one or more years, as figure 4 shows for the Turkish reform.

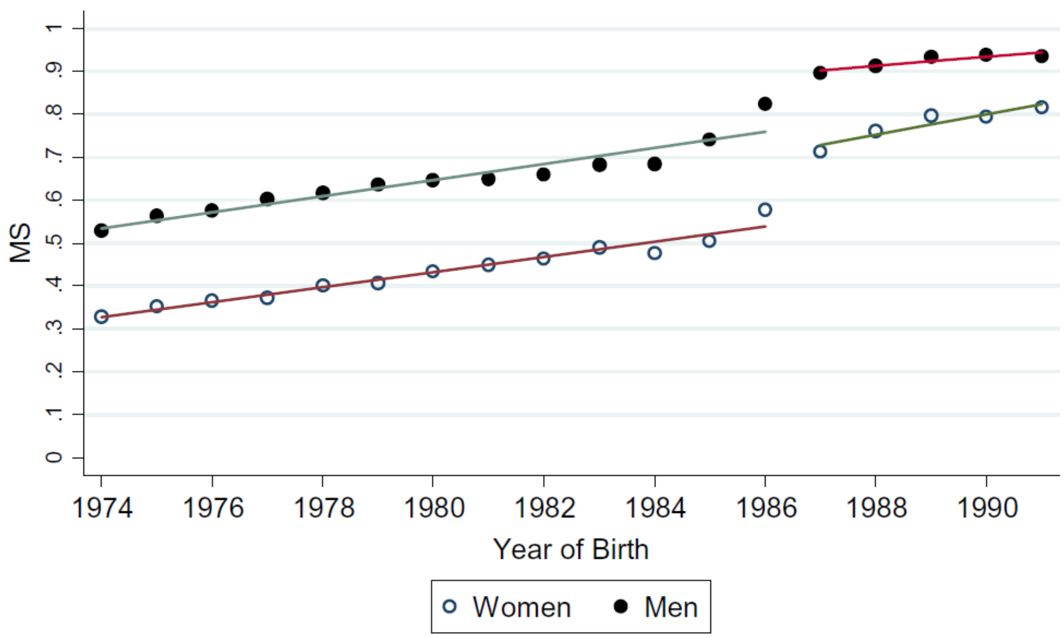



**Figure 4: Educational Attainment and Compulsory Schooling Extensions, 1986**
**Notes:** MS indicates fraction of each birth cohort completing middle school.
**Source:** Torun 2018, figure 2.

An important prerequisite for this method to work is that compulsory schooling laws are implemented and significantly affect years of schooling. This is the case for all the schooling laws included in this review. Some papers, however, do have reforms that are relatively weak instruments in some specifications (with first stage F<10) (e.g., Buscha and Dickson 2012, Clay et al. 2012, Fang et al. 2012).

The econometric logic of these laws is that they affect children that would otherwise have left school as soon as legally possible. They thus measure the effects of additional education for a pool of children of the same average ability. The effects are estimated in different ways, despite the common logic of comparing unaffected and affected cohorts. Most utilize IV-estimation, while others use difference-in-differences or regression discontinuity estimates. No matter the estimation method, these laws primarily affect children that would otherwise have left school as early as possible, and don't give us any certain information on the effects of schooling on children motivated to attend school beyond whatever the minimum leaving age is. In IV terminology they are therefore a LATE effect for children affected by the reforms, not the whole population of school attending children.

Since many papers had multiple estimates, the following criteria were used to select those to include here:

- When multiple estimates were given with no reported "primary model" (e.g., different controls, bandwidths in RD), we average all causal estimates and their standard errors.
- When a preferred causal estimate was stated by the authors, this is the estimate we report.
- If there are multiple datasets in a paper, we focus on the dataset with most observations.
- When both hourly wage and weekly earnings were reported, we report the effect on hourly wages.
- If men and women have separate estimates, each is included separately.
- If the combined sample and only male returns are reported, but not those for women, these reported estimates are taken as two independent estimates.

We identified 44 papers.[7] They focused on a wide variety of countries, however there is a clear bias towards developed western democracies, especially the UK and US. Specifically, there are 12 papers focusing on the UK, 7 on the US, 3 on Sweden, 3 on Germany, 3 on Australia, and 1 or 2 on a few other western democracies, while 2 utilize across country samples within Europe. There are only 9 papers that focus on countries outside the western world, specifically Turkey, China, Hong Kong, Indonesia, Taiwan, Jordan, and Argentina.

Most papers (40) focus on reforms that were implemented 1945-2000. Few papers (4) focus on the period before 1945, with the earliest reforms being from 1880.

[7] A list of all papers is reported in appendix 2.

The total number of reforms in the included papers are 38, most of them 1- or 2-year extensions to minimum school leaving age.[8] Most reforms are the focus of a specific paper, but some reforms are merely one of many in cross-country samples. Many reforms are used in multiple papers, especially reforms from the United Kingdom as well as the Turkish school reform of 1997 that extended compulsory schooling years from 5 to 8.[9]

## 3. Causal Returns to Education

With these procedures the average percentage gain in earnings from an additional year of schooling was 8.5% for 66 independent estimates, from 44 papers.[10] [11]

We consider three possible publication biases in this literature. The first is the greater likelihood of an estimate being published when it is statistically significantly different from zero. This will cause biased estimates in two ways. Results below the standard level of significance will remain unpublished. But the second is that authors will be potentially induced by such publication bias to engage in "p-hacking." They will have an incentive to engage in specification search to find the specification, and data treatment, that creates a result that is significant at the conventional 5% or 1% levels.

Third is the greater likelihood of an estimate being published when the return is greater than 0. No-one believes it plausible that the causal effect of education is negative. Thus authors may simply abandon studies that produce a negative estimate, or journals will reject such estimates as obviously incorrect. One effect this produces is that if an estimate incorporates a programming or other error, it is much more likely to be detected if it leads to a negative implied rate of return to education than when it produces a positive return.

[8] Some papers utilize variations in multiple law changes across different states in the US (e.g., Angrist and Krueger 1991, Acemoglu & Angrist 2000), Canada (Oreopoulos 2006b), and Australia (Powdthavee et al. 2013). Each of these batches of laws are counted as one reform in the above count.
[9] A very brief description of all reforms is available in appendix 3.
[10] The average return was 8.6% from 77 estimates, where not all were independent, again from 44 papers.
[11] Note, however, that of 77 estimates in total only 43 (56%) were statistically significant at the 5% level.

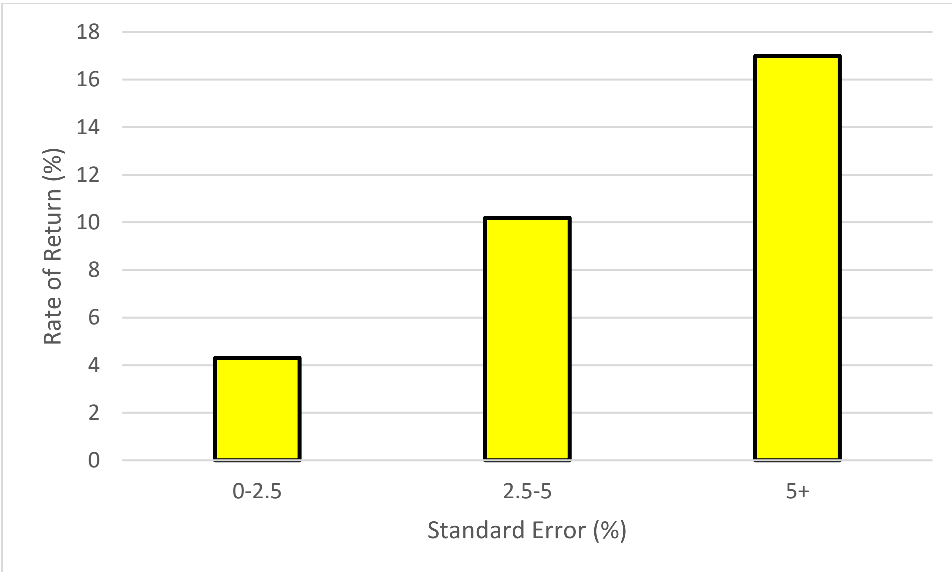



**Figure 5: Rate of Return versus Standard Error**

**Notes:** N = 59.

**Source:** Meta-study dataset ("Clark_Nielsen_returns_to_education.dta" in online appendix).

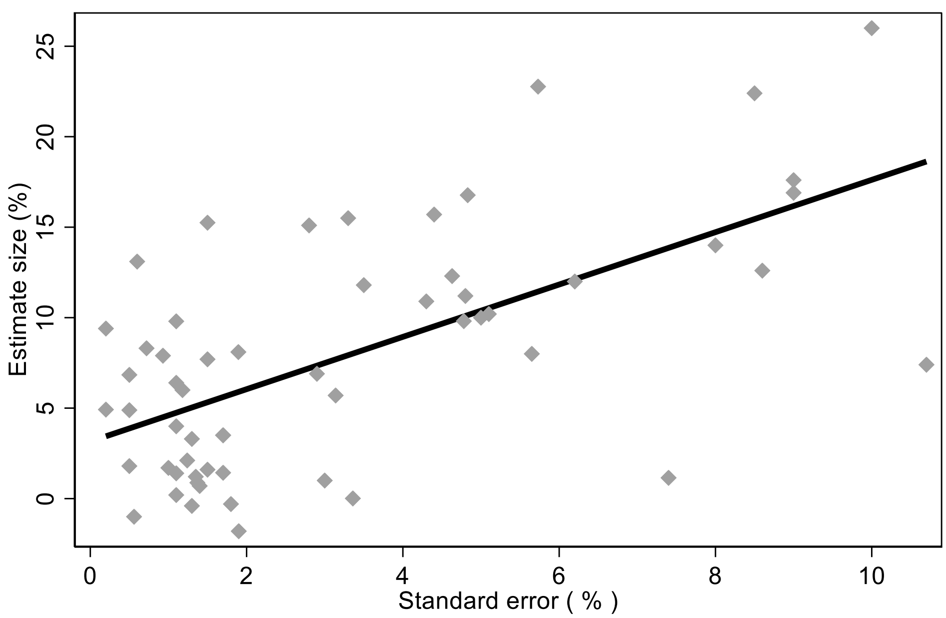



**Figure 6: Rate of Return versus Standard Error**

**Notes:** N = 57. Two outliers with standard errors above 20% omitted.

**Source:** Meta-study dataset ("Clark_Nielsen_returns_to_education.dta" in online appendix).

There is clear evidence of the first publication bias in the data. As figure 5 shows there was a much greater likelihood of an estimate being published when it was statistically significantly different from 0. The average estimated return rises substantially where the standard error is larger. If all estimates were being published independent of the estimated return there would be no such gradient.

Figure 6 shows the individual estimates of returns versus standard errors for 57 independent estimates where the standard error was less than 20%, omitting two outliers.[12] [13] The linear fit to this data suggests that at a standard error of 0, the measured rate of return would be around 3%, as opposed to the 8.5% observed on average. Thus just this first source of publication bias raises the returns estimated for education by 5%.

44 IV estimates had a corresponding OLS estimate. In 28 of the 44 cases (64%) the IV estimate was bigger. The IV estimates thus average 9.5% compared to 7.0% for the OLS estimates for the same population. As Card (1999) noted, the IV estimates tend to be as large or larger than the OLS estimate, despite the OLS estimates being upward biased by the selection into education of those of higher ability. As noted above, Card explained this though a higher return to education among those affected by extensions of compulsory schooling than among those choosing additional schooling. But this implies that the estimated return to education using compulsory schooling will reveal little about the return to education for most educational investments.

A more likely explanation for why the IV estimates here are larger than the OLS estimates would again be publication bias. The IV estimates will have larger standard errors than OLS estimates. If the papers they appear in only get published where the IV estimate is statistically different from 0, then there will be a selection towards higher IV estimates that does not apply to the OLS estimates, helping explain the higher IV returns.

There was a larger average effect of 13.3% in poor to middle income countries (e.g., China, Indonesia, Thailand, Jordan, Turkey, Argentina) compared to 6.8% for high income countries. But the average standard error was much bigger for the poorer countries (5.7%) compared to

[12] Note that 7 other estimates are not included due to some papers not reporting standard errors.
[13] This can also be visualized as a funnel plot. A funnel plot of the same data can be seen in appendix 4. Our conclusions remain unchanged, when observing the data in this way.

high income (3.3%). Potentially then the higher observed returns in poor to middle income countries could be the result just of publication biases in favor of statistically significant results.

There is, however, no sign in response to this first bias of further bias through p-hacking. To test for this we looked at a bandwidth of 0.2 around a t-statistic of 1.96 and 2.58 (5% and 1% confidence intervals). What was the relative numbers of t-statistics in this band above and below 1.96 and 2.58? The answer is 5 above versus 4 below, for 1.96 and 2 versus 2 for 2.58. Thus there is no clear sign of p-hacking in the results.

The most significant source of publication bias, however, is the clear sign that negative estimates of returns to education are not being published, or even appearing in working papers. This publication bias is very visible in figure 7, which shows the distribution of returns in the meta study, as well as the best fitting normal distribution to the set of positive returns.

This best fitting curve was found by a grid search on normal distributions with varying mean and standard deviations, where the criteria was the minimum sum of squared errors in each 5% return interval between the observed frequency and the normal distribution. The best fitting normal distribution, as shown, had a mean of 0.2% and standard deviation of 12.1%. As can be seen for values above 0 this fits very well the observed return distribution. Thus the observed data conform well to a normal distribution with most observations below 0 omitted.

Note also that of the six negative estimates, the largest was -1.8%, and four were above -1%. Also, three of the six estimates were from papers that have significant positive effects in other subsamples or datasets. There are only two papers out of 44 with standalone negative estimates of the return to schooling.

In figure 7, note that we expect that the estimated returns were substantially upward biased, based on figure 5. But if we corrected the estimates for that upward bias, for example, by reducing all those above 0 by some fixed proportion, then this would just produce another normal distribution centered close to 0.

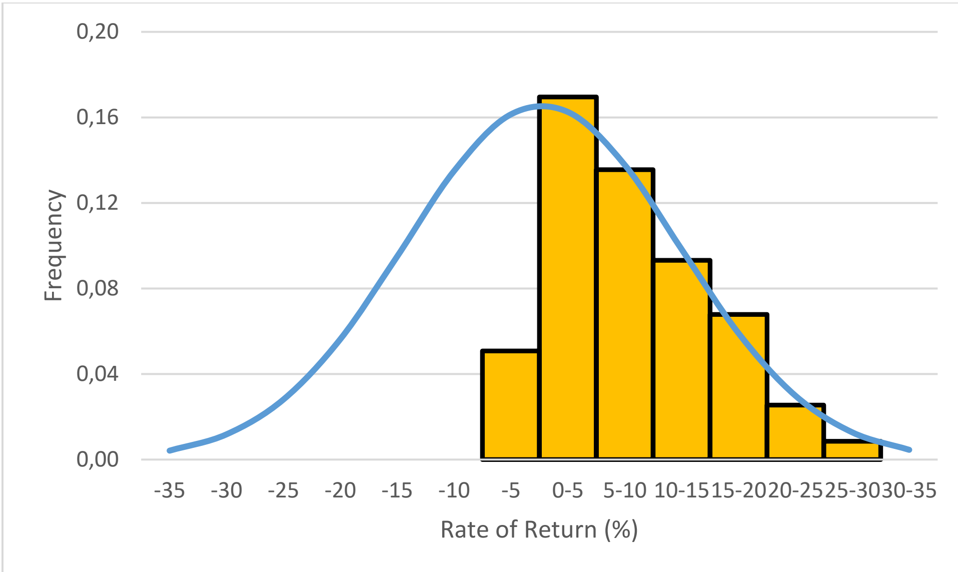



**Figure 7: Relative Frequency of Estimated Rates of Return fitted to Normal Distribution**

**Notes:** The figure shows 65 rates of return to an extra year of education reported in the meta-study versus the best fitting normal distribution of returns for reported returns greater than 0%. One outlier observation with a return greater than 50% was omitted.

**Source:** Meta-study dataset ("Clark_Nielsen_returns_to_education.dta" in online appendix).

An issue with arguing that the conducted meta-study is evidence of current issues of publication bias is the inclusion of older papers. Could these results simply be driven by old research that was less methodologically rigorous? The evidence makes this conclusion unlikely. Average returns are similar for new and old papers, for example papers published in 2014-2023 averaged a 7.6% return (36 estimates) while papers in 1991-2013 averaged only modestly higher at 10.2% (23 estimates).[14] Also, the presence of negative estimates is similar with 3 in 2014-2023, and 3 in 1991-2013. However, the most convincing evidence is that among new papers we still observe a strong positive relationship between the size of estimates and standard errors, and the distribution is close to a normal distribution truncated around 0.[15] This is unfortunately not a story of a lack of methodological rigor in the past, but of ongoing publication bias in the present.

[14] If 2010 is taken as the cutoff instead, the averages are 8.9% for new papers and 6.9% for old papers.
[15] See the do-file in the online appendix for replication.

We also investigated whether there is a relationship between effect sizes and number of citations. Do large and significant positive returns to education in a paper elicit more citations of that paper? This supplementary test can reveal if large positive returns also garner more attention after publication. Once we control for year of publication, a dummy indicator of the paper that is considered the pioneering study (Angrist and Krueger, 1991), and use the log of citations to reduce the influence of outliers, we find no relationship between the estimated effect size and citation frequency. Specifically, an estimate of -0.54 (P = 0.678), meaning a very modest decrease in citations per each 1 percentage point increase in returns to a year of schooling. We also tested if there was a relationship between citation frequency and the size of standard errors. Our expectation was that papers producing more precise estimates should be judged more favorably and garner more citations. Similarly, there was no relationship here with an estimate of -3.76 (P = 0.293). Neither bigger estimated returns, nor more precise estimates, garner more citations.

Thus, whatever is driving the clear publication biases for estimates of the return to schooling, it is not that estimates with smaller effect sizes once published are regarded as dubious by the research community.

## Conclusion

In an earlier meta-study of returns to education Ashenfelter, Harmon, and Oosterbeek, 1999 report also finding evidence of publication bias with IV estimates of the returns to a year of schooling. They thus report a significant positive correlation between the estimated effect size and the standard error. Despite the very strong correlation between the return estimate and the standard error their method of correction for publication bias, however, reduces the average IV return estimate from 8.6% to only 8.1%, an inconsequential amount (Ashenfelter, Harmon, and Oosterbeek, 1999, Table 5).

In this study we report estimates of the causal effects of education on earnings that are close to that earlier study: an 8.5% gain in earnings for each year of education.[16] Also in this meta-study of 66 such estimates we find clear evidence of very significant publication biases. Studies passing the filter of publication were more likely to have coefficients statistically

[16] If we exclusively include IV estimates it would be a 10.3% gain from 52 estimates.

significantly greater than 0. This finding is in line with recent experimental evidence showing a ”null result penalty”, where insignificant results are perceived as less publishable, and as being of lower quality and importance during the publication process (Chopra et al., 2024). Further there is strong evidence that studies with returns estimated at less than 0 were not being published. Correcting just the first publication bias through looking at the average return on the studies with the lowest standard errors would suggest an average causal return to education of no more than 3%, compared to the reported 8.5%. But independent of this, correcting for the omission of returns less than 0 implies an overall causal return to education of very close to 0%.

There are a large number of other papers on extensions of compulsory education that measure outcomes such as employment rates, higher education attainment, mortality rates, health, fertility, and criminality. However, for none of these outcomes were there sufficient numbers of papers with a given outcome such that we could carry out the same analysis as above. However, given the publication biases we observe with earnings, we suspect that papers that show increased education leading to lower employment, less university education, higher mortality, worse health, and increased criminality are all likely to have faced similar problems with publication bias.

## Appendix 1 – Twin Studies

A popular idea, for example, in the 1990s and 2000s was to use monozygotic (identical) twins as the ability control. See, for example, Amin (2011), Ashenfelter and Krueger (1994), Ashenfelter and Rouse (1998), Behrman and Rosenzweig (1999), Behrman, Rosenzweig, and Taubman (1994), Bonjour et al. (2003), Isacsson (1999, 2004), Miller, Mulvey, and Martin (1995, 2006), Rouse (1999), Zhang, Liu, and Yung (2007).

Such estimates using the difference in schooling across such twins versus their difference in earnings were as high as the raw cross-sectional returns to education, suggesting an absence of significant ability selection into education. However, such estimates do not control for ability. For despite their genetics being the same, monozygotic twins differ in academic abilities, which is what explains why they sometimes have different years of schooling. Even within the same family environment there is significant variation in phenotypes even with the same genotype.

## Appendix 2 – List of studies in meta-study

The estimates in the meta-analysis are taken from the following 44 papers:

Angrist and Krueger 1991, Harmon and Walker 1995, Acemoglu and Angrist 2000, Meghir and Palme 2005, Oreopoulos 2006, Oreopoulos 2006b, Leigh and Ryan 2007, Oosterbeek and Webbink 2007, Pischke 2007, Pischke and Wachter 2008, Brunello et al. 2009, Aakvik et al. 2010, Devereux and Hart 2010, Oreopoulos and Salvanes 2011, Buscha and Dickson 2012, Clay et al. 2012, Fang et al. 2012, Dickson 2013, Grenet 2013, Powdthavee et al. 2013, Parinduri 2014, Stephens and Yang 2014, Alzúa et al. 2015, Buscha et al. 2015, Chib and Jacobi 2016, Kamhöfer and Schmitz 2016, Bhuller et al. 2017, Dolton and Sandi 2017, Torun 2018, Delaney and Devereux 2019, Eble and Hu 2019, Albarrán et al. 2020, Fischer et al. 2020, Liwinski 2020, Zhang 2020, Clay et al. 2021, de New et al. 2021, Domnisoru 2021, Patrinos et al. 2021, Fischer et al. 2022, Clark 2023, Hicks and Duan 2023, Korwatamasakul 2023, Li 2023.

For more information on estimates from specific papers see the Excel or dta. file in the online appendix.

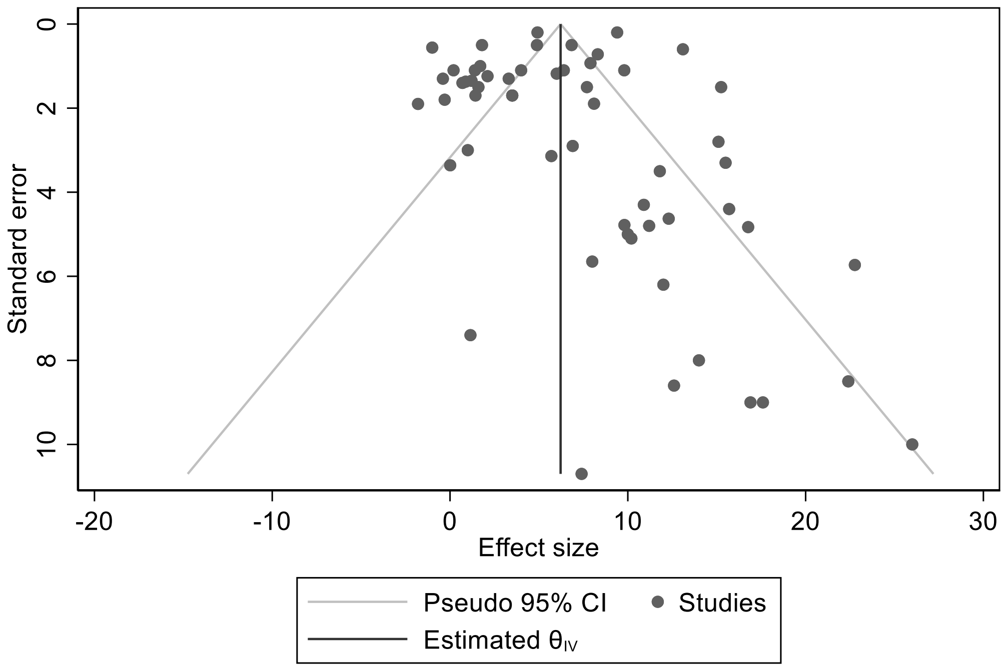
## Appendix 3 – List of reforms in meta-study

| Nr. | Reform | Paper(s) |
|---|---|---|
| **1** | Argentina, MYOS extension, 7 to 9 years, in 1993 | Alzúa et al. 2015 |
| **2** | Australia, MSLA extension, 14 to 15 years, in 1945 (adoption until 1960/1963) | De New et al. 2021 |
| **3** | Australia, multiple across state variations and changes in CSLs | Leigh and Ryan 2007, Powdthavee et al. 2013 |
| **4** | Austria, MSLA extension, 14 to 15 years, in 1962/66 | Brunello et al. 2009, Albarrán et al. 2020 |
| **5** | Belgium, MSLA extension, 14 to 18 years, in 1983 | Brunello et al. 2009 |
| **6** | Canada, multiple across state variations and changes in CSLs | Oreopoulos 2006b |
| **7** | China, MYOS extension, 5 to 6 years, in late 1970's | Eble and Hu 2019 |
| **8** | China, 9 year compulsory education established, in 1986 (staggered rollout) | Fang et al. 2012 |
| **9** | Czechia/Slovakia, MYOS, 8 to 9 years, in 1960 | Albarrán et al. 2020 |
| **10** | Denmark, MYOS extension, 7 to 9 years, in 1971 | Albarrán et al. 2020 |
| **11** | Finland, MSLA extension, 13 to 16 years, in 1972-1977 (staggered rollout) | Brunello et al. 2009 |
| **12** | France, MSLA extension, 15 to 16 years, in 1967 | Grenet 2013, Albarrán et al. 2020, Domnisoru 2021 |
| **13** | Germany, MYOS extension, 8 to 9 years, in 1949-1970 (staggered rollout across states) | Pischke and Wachter 2008, Brunello et al. 2009, Kamhöfer and Schmitz 2016 |
| **14** | Germany, reform change leading to shorter school year, in 1966-1967 | Pischke 2007 |
| **15** | Greece, MSLA extension, 12 to 15 years, in 1972 | Brunello et al. 2009, Albarrán et al. 2020 |
| **16** | Hungary, MYOS extension, 8 to 10 years, in 1961 | Albarrán et al. 2020 |
| **17** | Indonesia, school term length increase, in 1978-1979 | Parinduri 2014 |
| **18** | Ireland, MSLA extension, 14 to 15 years, in 1972 | Brunello et al. 2009 |
| **19** | Italy, MSLA extension, 11 to 14 years, in 1963 | Brunello et al. 2009, Albarrán et al. 2020 |
| **20** | Jordan, MYOS extension, 9 to 10 years (also reconstruction of secondary schooling), in 1998 | Hicks and Duan 2023 |
| **21** | Malta, MYOS extension, 8 to 10 years, in 1972 | Albarrán et al. 2020 |
| **22** | Netherlands, MSLA extension, 15 to 16 years, in 1975 | Brunello et al. 2009, Oosterbeek and Webbink 2007, Albarrán et al. 2020 |

| | | |
|---|---|---|
| **23** | Northern Ireland, MSLA extension, 14 to 15 years, in 1957 (related to 1947 United Kingdom reform) | Oreopoulos 2006a, Devereux and Hart 2010 |
| **24** | Norway, MYOS extension, 7 to 9 years, in 1960-1975, also change of curriculum, etc. | Aakvik et al. 2010, Bhuller et al. 2017 |
| **25** | Poland, MYOS extension, 7 to 8 years, in 1966 | Albarrán et al. 2020 |
| **26** | Poland, MYOS extension, 8 to 9 years, in 1999 | Liwinski 2020 |
| **27** | Portugal, MYOS extension, 4 to 6 years, in 1964 | Albarrán et al. 2020 |
| **28** | Spain, MSLA extension, 12 to 14 years, in 1970 | Brunello et al. 2009, Albarrán et al. 2020 |
| **29** | Sweden, extension of term length from 34.5/36.5 to 39 weeks in 1939 | Fischer et al. 2020 |
| **30** | Sweden, broad reform including MYOS extension (plus abolishing tracking), 8 to 9 years, in 1948-1953 (staggered rollout) | Meghir and Palme 2005, Fischer et al. 2020, Fischer et al. 2022, |
| **31** | Sweden, MYOS extension, 7 to 8 years, in 1941-1962 (staggered rollout) | Brunello et al. 2009 |
| **32** | Taiwan, MYOS extension, 6 to 9 years, in 1968 | Zhang 2020 |
| **33** | Thailand, MYOS extension, 4 to 6 years, in 1978 | Korwatamasakul 2023 |
| **34** | Turkey, MYOS extension, 5 to 8 years, in 1997 | Torun 2018, Patrinos et al. 2021 |
| **35** | United Kingdom, MSLA extension, 14 to 15 years, in 1947 | Harmon and Walker 1995, Oreopoulos 2006a, Devereux and Hart 2010, Chib and Jacobi 2016, Dolton and Sandi 2017, Clark 2023 |
| **36** | United Kingdom, reform change leading to children born in certain months to have more compulsory schooling and other changes, 1963 | Dolton and Sandi 2017 |
| **37** | United Kingdom, MSLA extension, 15 to 16 years, in 1972 | Buscha and Dickson 2012, Dickson 2013, Buscha et al. 2015, Dolton and Sandi 2017, Delaney and Devereux 2019, Albarrán et al. 2020 |
| **38** | United States, multiple across state variations and changes in CSLs | Angrist and Krueger 1991, Acemoglu and Angrist 2000, Oreopoulos and Salvanes 2011, Clay et al. 2012, Stephens and Yang 2014, Clay et al. 2021, Li 2023 |

**Notes:** The specified date is the year of implementation if reported (otherwise the year of the reform). Papers report either MSLA = minimum school leaving age or MYOS = minimum years of schooling.

**Appendix 4 – Funnel plot (alternative visualization of figure 6)**




**Notes:** 2 outliers with standard errors above 20% removed.
**Source:** Meta-study dataset ("Clark_Nielsen_returns_to_education.dta" in online appendix).

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