

Comment on Oddbjørn Raaum, Kjell G. Salvanes, and Erik Ø. Sørensen: The Impact of a Primary School Reform on Social Stratification: A Norwegian Study of Neighbor and School Mate Correlations

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Do the characteristics of the neighborhoods where children grew up feed on to their adult outcomes? This question is of great concern to parents and policy analysts. Questions about the importance of “neighborhood effects”, “peer group effects”, and “social interactions” have also received considerable amount of interest in recent research. It is to this growing literature that the paper contributes.

For reasons outlined by Manski (1993), it is very difficult to estimate a causal neighborhood effect. In the normal setting, it is, e.g., hard to disentangle individual self-selection into neighborhoods with certain characteristics from the effect of the neighborhood itself. Estimating the neighborhood effect presumably requires some experimental—either quasi or actual—variation. Now, as the title makes clear, the authors are not claiming to estimate causal effects—rather they are estimating the neighborhood correlation.¹

The paper starts off from two facts: (i) the neighborhood correlation is significantly lower for cohorts born in 1955-65 than for cohorts born in 1945-55; (ii) the comprehensive (9-year) school was introduced in the 1960s in Norway. So, the main question asked in the paper is whether this school reform, which increased mandatory schooling from 7 to 9 years, is the reason for the lower neighborhood

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¹ For the most part of the paper they make the distinction between causal effects and correlations clear. However, in the first paragraph of the paper I think they don't. They argue that resources have a modest impact on student achievement, citing papers that are susceptible to bias because of reverse causality, while community characteristics are important in explaining educational attainment. The substance of the latter argument comes from papers that explicitly estimate correlations. Reverse causality presumably inflates the estimate of the neighborhood correlation, while the bias is negative when it comes to the effect of resources. Therefore, I think this is an unfair comparison.

correlation among the cohorts born in 1955-65. It is a question about the effect of the reform.

I am no expert on the Norwegian school system, although I suspect that the Swedish and Norwegian systems are pretty similar. To put the authors' work in perspective, I begin by reviewing the Swedish evidence on the comprehensive school reform and educational attainment across cohorts in Sweden. Then I ask whether reform assignment was exogenous in Norway. The answer to this question is important, since to estimate the effect of the reform it is crucial that the assignment was independent of the unobserved characteristics that influence educational attainment. The final section concludes.

1. The Swedish experience

The comprehensive school reform in Sweden was introduced in much the same way as the Norwegian reform. However, the decision was taken almost ten years earlier, i.e., in 1950 rather than 1959. Prior to 1962, when the reform was finally implemented all over the country, the reform was "piloted" in different municipalities starting in 1949. The reform was thus gradually implemented in different parts of the country. One of the reasons for the gradual implementation was to aid scientific evaluation. Unfortunately, evaluation has rarely preceded reforms of the Swedish education system since.²

Meghir and Palme (2003) have examined the consequences of the Swedish comprehensive school reform for educational attainment and earnings. Table 1, which shows the impact of the reform on educational attainment, is taken from their paper. It compares the attainment of males and females born in 1948 growing up in reformed and non-reformed municipalities, respectively. The table presents two sets of estimates of the reform. The first is the simple difference between individuals growing up in reformed and non-reformed municipalities; see columns (1) and (3). The second is the difference between reformed and matched non-reformed municipalities.

Table 1 shows that, on average, the reform mostly had the "mechanical" effect of pushing individuals from 7-8 years of basic school

² Since then, I know of only one case where reforms have been piloted prior to nationwide implementation. This pertains to the extension of vocational tracks at the upper secondary level by an additional year in the early 1990s. Ekström (2003) uses the pilot scheme conducted prior to the nationwide implementation to estimate the value of a third year of vocational education.

(folkskola) to 9-10 years of comprehensive school (grundskola) or junior secondary school (realskola). There are no significant “knock-on” effects to levels beyond comprehensive or junior secondary school; see columns (2) and (4).

Table 1 may, however, mask differential effects by family background, since it presents the effects averaged over all family backgrounds. Meghir and Palme, therefore, go on to present separate estimates by father’s education. They focus on children where the father has only basic education (85 percent of the sample). There is some, albeit weak, evidence that the reform had a bigger impact on the children of unskilled fathers.³

Table 1. The impact of the reform on educational attainment

	Males		Females	
	(1) Change	(2) Change (Matched)	(3) Change	(4) Change (Matched)
Education level				
Basic school (7-8 yrs.)	-0.197 (0.011)	-0.110 (0.024)	-0.165 (0.009)	-0.109 (0.021)
Comprehensive/Junior secondary (9-10 yrs.)	0.124 (0.010)	0.101 (0.025)	0.116 (0.010)	0.082 (0.020)
More than compre- hensive/Junior second- ary	0.073 (0.013)	0.009 (0.016)	0.049 (0.013)	0.028 (0.017)
Years of education	0.70 (0.081)	0.274 (0.115)	0.61 (0.079)	0.215 (0.130)

Notes: Standard errors in parentheses. Columns headed “Change” presents the difference in educational attainment between individuals growing up in reformed and non-reformed municipalities. Columns headed “Change (Matched)” shows the difference between reformed and matched non-reformed municipalities.

Source: Meghir and Palme (2003), Tab. 7.

The estimates of the effects of the reform on earnings also suggest that children of low-educated fathers were the prime gainers from the educational intervention. Based on these two sets of findings the authors conclude that it is quite likely that the reform had an effect on intergenerational income mobility. In other words, the authors argue

³ The estimate, with males and females pooled together, is that education increased by 0.32 years for individuals with low-skilled fathers, which should be compared to 0.25 years of schooling for the entire sample. The difference between these two estimates is not statistically significant, however.

the reform reduced the importance of families in determining educational attainment.

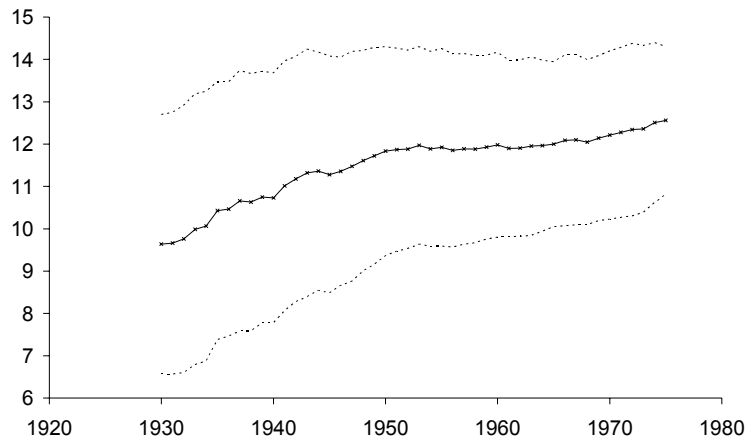
Thus, the comprehensive school reform primarily benefited those from poorer socio-economic backgrounds. It is my impression that many educational interventions since the comprehensive school reform have had this objective. The motive for intervention has often been to reduce the importance of family background in determining educational choice; see the comprehensive review in Erikson and Jonsson (1993). In fact, efficiency as well as equity arguments have been put forward for such policies.

Whether such policies have been successful or not is another matter. But it is clear that the standard deviation of the schooling distribution has decreased substantially over time. This is illustrated in Figure 1, which shows mean years of schooling, along with the standard deviation of the schooling distribution, in the cohorts born 1930-1975. A closer examination of the evolution of years of schooling at different points of the distribution reveals that the increase in the mean, as well as the reduction of the standard deviation, is mainly driven by the evolution in the lower tail of the distribution: over time, years of schooling has increased much more in the lower tail of the distribution than in the remainder of the distribution. Over the 20-year-period spanned by the cohorts born 1930 to 1950, average years of schooling increased by 2.2 years; concomitantly, schooling at the 10th percentile increased by 2.4 years. Between the cohorts born in 1950 and 1970 there was a small increase in average years of schooling. Nevertheless, the standard deviation of the schooling distribution continued to decrease. In sum, throughout the 45-year-period spanned by these cohorts, average years of schooling increased. The reduction of the variance of the schooling distribution is at least as striking. The reduction of the variance has primarily been achieved by an increase in educational attainment in the lower tail of the schooling distribution. If educational policies contribute to this picture—and it is hard to imagine that they don't—it seems that they have been designed to lift the lower tail of the distribution.

I do not know of any Swedish studies examining whether educational policies have affected the importance of neighbourhoods for educational attainment. Nevertheless, if one is interested in the effect of such policies, it seems natural to first examine whether educational reform has changed the importance of families in educational choice. Figure 4, which I think is the most intriguing figure in the paper,

shows that the association between the schooling of the parent and child has been reduced over time. Does the comprehensive school reform contribute to this pattern? To me this is the most interesting question to ask. After all, the families living in a particular neighbourhood to a large extent define the characteristics of it.

Figure 1. Years of schooling for cohorts born 1930-1975.



Notes: The dashed lines show the mean \pm 1 standard deviation. Years of schooling have been imputed from data on educational attainment.

Source: LINDA 2000; see Edin and Fredriksson (2000) for a description of LINDA.

2. Was reform assignment exogenous?

To estimate the effect of the reform, we want reform assignment to be independent of the unobserved individual and municipality characteristics that influence educational attainment. We know that the assignment of reform status was not random. But was it random conditional on observed characteristics? The authors argue that there is little evidence for systematic allocation of the reform to municipalities. Nevertheless, Figure 1 in the paper raises concerns. It shows that the reform was implemented first in municipalities where parental education was high, while communities where parental education was low implemented the reform last. The education of the father in the cohort born in 1956 was around 0.8 years higher in reformed municipalities in comparison to non-reformed ones. Whether such differ-

ences signify differences in unobserved skills is impossible to tell. But to my mind they cause concern.

Meghir and Palme (2003) implement a matching procedure to form comparable pairs. This procedure relies on selection on observables, i.e., that we can observe everything that is relevant for reform status. This is probably a realistic assumption in their case, since they have access to large set of ability indicators that would be unobserved in the usual data sets. As shown in Table 1, matching makes a substantial difference to the conclusions about the effects of the reform. For instance, the simple comparison between reformed and non-reformed municipalities suggests that there was a significant increase in education beyond the comprehensive and junior secondary level for males; see column (1). However, there is effectively no change when doing the matched comparison; see column (2). It is an open question whether such changes in results apply also to the Norwegian case. However, the similarity of the comprehensive school reforms in Sweden and Norway suggests that these concerns may have to be taken seriously.

3. Concluding remarks

The main message I have wished to convey is that the effect of educational policies may vary by socio-economic background. In particular, I think that policies have the greatest potential for individuals from poor socio-economic backgrounds. The studies of class size that are based on experimental variation substantiate this claim. For instance, Krueger (1999) finds that class size reductions improve student performance more among students with a weaker family background. Owing to this reasoning, I would have been very interested in an analysis of whether the reform reduced the importance of family background. I think the authors share this interest. In fact, one of the authors is currently involved in work along these lines; see Black et al. (2003).

If we think that educational policies have the greatest impact on individuals from poor family backgrounds, then maybe we should expect policies to have a greater impact in poor neighborhoods. Did the effect of the comprehensive school reform vary by neighborhood characteristics? The answer to this question will have to await future research.

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