Discussion Papers
Statistics Norway
Research department

No. 748
June 2013

Statistics Norway

Statistisk sentralbyrå

Taryn Ann Galloway and Christian N. Brinch

Is the relationship between schooling and disability pension receipt causal?

Discussion Papers No. 748, June 2013
Statistics Norway, Research Department

Taryn Ann Galloway and Christian N. Brinch

Is the relationship between schooling and disability pension receipt causal?

Abstract:
We examine the potential causal effect of years of schooling on the use of public disability pensions by studying the extension of compulsory schooling introduced in Norway in the 1960s. Simple regressions of disability pension receipt on schooling suggest a very strong negative relationship between education and disability pension use, particularly in the lower tail of the educational distribution. Given the strength of this observed relationship, one might suspect that improvements in educational attainment would lower disability receipt and alleviate the public finance burden from such social security benefits. Our analysis of the extension of compulsory education from 7 to 9 years in Norway in the 1960s shows essentially no effects on disability pension use at age 50, with a confidence interval suggesting that at best only a minor part of the observed relationship between schooling and disability pension receipt can be explained by a causal effect of schooling on disability.

Keywords: Education, health, disability

JEL classification: I18, I21, J14, J24

Acknowledgements: We would like to thank Torbjørn Hægeland, Andreas Kostøl and Kjetil Telle for useful comments. While carrying out this research, Brinch has been associated with the centre of Equality, Social Organization, and Performance (ESOP) at the Department of Economics at the University of Oslo. ESOP is supported by the Research Council of Norway.

Address: Taryn Ann Galloway: Statistics Norway: Email: tag@ssb.no

Christian N. Brinch: Statistics Norway; Dept. of Economics, University of Oslo,
Email: cnb@ssb.no

Discussion Papers comprise research papers intended for international journals or books. A preprint of a Discussion Paper may be longer and more elaborate than a standard journal article, as it may include intermediate calculations and background material etc.

Abstracts with downloadable Discussion Papers
in PDF are available on the Internet:
http://www.ssb.no
http://ideas.repec.org/s/ssb/dispap.html

For printed Discussion Papers contact:
Statistics Norway
Telephone: +47 62 88 55 00
E-mail: Salg-abonnement@ssb.no

ISSN 0809-733X
Print: Statistics Norway

Sammendrag

Vi undersøker den kausale effekten av antall skoleår på bruken av uførepensjon ved å studere utvidelsen av obligatorisk skolegang in Norge i 1960-årene. Enkle regresjoner av mottak av uførepensjon på fullførte år med skolegang tilsier en svært sterk negativ sammenheng mellom utdanning og bruk av uførepensjon, spesielt i den nedre delen av utdanningsfordelingen. Gitt styrken i denne observerte sammenhengen, kunne en mistenke at forbedringer i fullførte skoleår skulle lede til lavere mottak av uførepensjon og lette en byrde på offentlige budsjetter gjennom slike trygdeordninger. Vår analyse av utvidelsen av obligatorisk skolegang fra 7 til 9 år i Norge på 1960-tallet viser imidlertid ingen effekt på bruken av uførepensjon for 50-åringer som følge av økt skolegang, med et konfidensintervall som tilsier at i beste fall bare en liten del av den observerte sammenhengen mellom utdanning og uførepensjonsmottak kan forklares med en kausal effekt av skolegang på uførepensjonering.

1 Introduction

The past decade has seen a surge in the literature studying the casual effects of education on health, see Eide and Showalter (2011) and Cutler and Lleras-Muney (2011) for recent reviews. The results from these recent studies are, however, far from conclusive. Using exogenous variation induced by complusory schooling laws, Lleras-Muney (2005) finds a large effect of education on mortality in the US and evidence in Lager and Torssander (2012) also suggests there is some effect in Sweden, but other studies of changes in compulsory schooling have failed to confirm effects on mortility for France (Albouy and Lequien, 2009), the UK (Clark and Royer, 2010) or even in alternative analyses from the US (Mazumder, 2008). However, further studies have found effects of schooling in self-reported health (Silles, 2009), hospitalization (Arendt, 2008), poor health (Oreopoulos, 2007) and hypertension (Powdthavee, 2010), but others fail to find a significant effect on similar outcomes, see e.g., Arendt (2005) for self-reported health and BMI or Jürges et al. (2011) for biomarkers related to cardio-vascular disease.

This current study looks at the possible causal effects of education on use of publicly funded disability pensions (DP) in Norway by exploiting the exogenous variation in schooling brought about by a major comprehensive compulsory schooling reform. In contrast to self-reported health status, which can be subject to bias in reporting and self-assessment, disability pension receipt requires that a person has undergone extensive medical evaluation to determine whether disease or disability prevents him or her from pursuing gainful employment. In all but the most obvious cases of extreme disability, other alternatives, such as re-training or transfer to different tasks or positions, have been attempted, often under the guidance of the social security agency. We also briefly consider mortality, both as a health-related outcome in its own right and as an outcome which might confound results on disability pension use. In looking at public-funded disability benefits, we move the discussion away from direct effects on disease, death or self-perception of “healthiness” and toward an outcome that also reflects the fact that health outcomes - and the manner in which these might be affected by education - are relevant for social security systems. Because of the high costs of publicly funded disability pension programs, the effects of education on use of public disability is relevant for a number of countries. Indeed, in recent years a number of commentators have expressed concern about the high level of spending on disability-related benefits and the high levels of disability pension use in the US as well as many other OECD countries (Burkhauser and Daly, 2002; Autor and Duggan, 2006; OECD, 2010). Norway is noted for having particularly high levels of spending on disability benefits, due to both the high incidence of disability pensioners in the population and a particularly generous level of benefits (OECD, 2006).

The reform studied here extended compulsory education in Norway from 7 to 9 years over a period of several years, predominantly in the 1960s, and increased educational levels for precisely that part of the distribution of educational attainment for which disability pension use is the highest in Norway. If general education has a profound effect on disability pension use and/or marginal effects of education are larger at low levels of education, then one would expect just such a reform to be particularly potent in influencing use of disability benefits. Also, the manner in which the reform was implemented makes it particularly suited to analysis within a quasi-experimental framework, since introduction of the reform at different times in different areas allows for better control of general time trends.

The potential for non-linearities in the relationship between schooling and disability pensions requires that we give careful consideration to how we assess the potential magnitude of the reform effects. In particular, basic results from a linear specification in ordinary least squares regression (OLS) will tend to understate the true magnitude of the relationship between education and DP at lower levels of education (and vice versa at higher levels) if the non-linearities are such that people with lower levels of education are much more likely to be on disability. Thus, given that the reform we analyze primarily affects educational attainment at the lower end of the education distribution, if we were to use the basic OLS results to gain a benchmark estimate of the potential magnitude for improved education (assuming exogeneity of education), then we would be underestimating the potential of the reform to affect DP if non-linearities in marginal effects by educational level exist. This would, in turn, also influence assessment of the extent of possible endogeneity when comparing results from a quasi-experimental analysis (Lochner and Moretti, 2011; Løken et al., 2012). When exploiting the reform as an instrumental variable (IV) with two-stage least squares (2SLS), the IV-estimate of the effect of education implicitly accounts for such non-linearities and thereby adjusts for the varying effects of the reform when influencing educational attainment in different parts of the education distribution. Thus, in order to gain a proper understanding of the magnitude of any estimated effects – as well as evaluate the precision of the IV/2SLS estimates – we need to adjust the baseline OLS estimates to better reflect the relative effectiveness of the reform in influencing educational attainment at different levels.

We are unable to uncover evidence of a causal effect of education on disability pension use by age 50, an age at which we could reasonably expect to find an effect. We can easily reject causal effects of the magnitude suggested by OLS estimates. Furthermore, once we carefully analyze the substantial non-linearities in the relationship between education and DP, even the upper range of our confidence intervals suggests that any causal effect of education is, at best, of a much smaller magnitude than suggested by descriptive evidence or basic OLS. In other words, our results show that, at most, only a very small proportion of the relationship between education and DP use suggested by descriptive or OLS analysis might be causal in nature. The effects of education on mortality are not estimated with the same precision as for disability and we can not rule out effects of the same magnitude as suggested by non-linear OLS at typical confidence levels. However, our results certainly do suggest that the causal effects of education on mortality are not larger than those estimated with OLS, in contrast to the findings in e.g. Lleras-Muney (2005).

The following section provides brief descriptive evidence on the relationship between eduation and use of DP in Norway followed by a discussion of why we might expect to find a causal effect of education on health in general and on disability pension use in particular.

Section 3 offers further details of the Norwegian compulsory schooling reform and describes the data used in this analysis. Section 4 outlines the econometric methods and empirical specifications used in the analysis, including details on the weighting scheme implicit in instrumental variables (IV) estimation. Details on how we take non-linearities in the effect of education into account in our analysis are also provided there. The results are presented in Section 5, and Section 6 concludes.

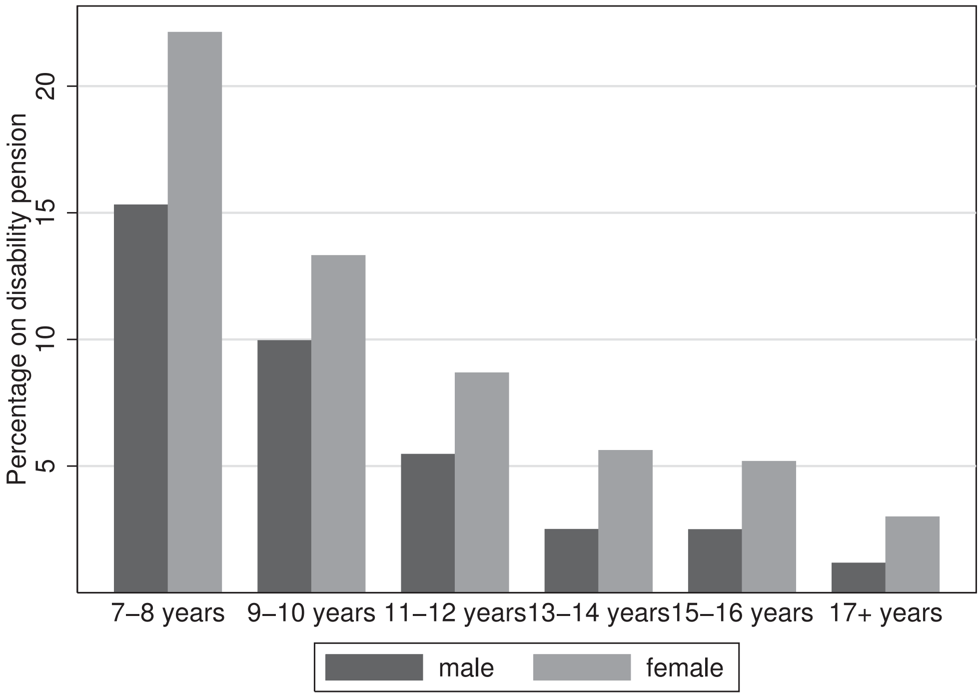
2 Education, health and uptake of disability pension

The Norwegian system of publicly-funded disability pensions guarantees a decent standard of living to persons aged 18-67 who are permanently unable to pursue gainful employment due to illness, injury or ill-health. Individuals must have gone through a careful medical evaluation and appropriate treatments as well as attempted re-training, where possible, before receiving permanent disability benefits. The illness or disability must be of a nature that the person is unable to work more than 49% of full-time. The level of benefits is calculated by means of a complicated formula based on previous earnings and employment. Persons with little previous earnings or employment are granted a minumum pension. During the past couple of decades, roughly 10 % of the working age population (18-66) has been on disability in Norway (**?**.

As documented by Figure 1, a clear and profound difference in DP uptake by educational level exists in Norway for both men and women by age 50. The largest difference appears to be between low levels of education and schooling at the secondary level, whereas decreases in DP use beyond the secondary level, i.e. over 12 years of education, are less pronounced. As many as 15-20% of persons with the lowest levels of education (7 or 8 years) are already on disability by age 50. At levels of education beyond secondary school (13+ years of education), about 5% or less are on disability by age 50. As the figure documents, the relationship between DP and educational attainment appears to be non-linear.

The literature generally distinguishes between three possible explanations for the the ob-

Figure 1: Disability pension use at age 50 by educational attainment. Birth cohorts 1950-1958.



Notes: See Section 3 for further description of the data and population used in analysis.

served relationship between education and health-related outcomes and behaviors: selection, reverse causation and a true causal effect. Selection might work through other factors that affect both educational attainment and DP. Differences in time preferences such that persons who value the future more are both more likely to abstain from current earnings in order to obtain higher education leading to higher earnings in the future and to invest in health-promoting behaviors are a much cited example (Fuchs, 1982). Similarly, individuals with higher earnings ability might find it worthwhile both to obtain higher education and to limit detrimental health outcomes in order to avoid (larger) future earnings losses. Among other factors, Conti et al. (2010) and Conti and Heckman (2010) also consider early childhood endowments that affect both educational and health outcomes. The typically strong relationship between education and health might, however, also reflect a situation in which one of the consequences of poor health is lower educational attainment rather than vice versa Currie (2009). This can easily be the case for certain types of health problems which make it more difficult for an individual to obtain more education.

In discussing the factors and mechanisms which might generate a causal effect of schooling on disability pensions we can, in turn, distinguish between two general types of explanations. The first relates to the more general discussion of how education can create differences in health-related behaviours and outcomes in general, see, for example, Grossman (2006). The second revolves around incentive structures created by the social security system.

Cutler and Lleras-Muney (2010) carefully consider and empirically explore a number of alternative channels by which education might affect health outcomes and health-related behaviors. After considering the evidence in a descriptive analysis, they conclude that there is little evidence that points to the gradient being attributable to differences in price responses, time preference or risk aversion, but there is some evidence for other channels studied. Higher levels of income due to higher education provide better opportunities for purchasing health insurance or health-promoting services as well as imply higher utility associated with living to an older age; their empirical results document that differences in resources do indeed account for some of the relationship between schooling and health. Futhermore, Cutler and Lleras-Muney (2010) suggest that it is not simply differences in specific (factual) health knowledge that can generate the observed education gradient in health - although these also seem to matter - but that differences in cognitive ability or the ability to process and fully understand the context of health-related knowledge is important. Skills attributable to education can be assumed to affect a person's ability to read and understand information about disease, disease control and disease prevention; the ability to understand treatment alternatives and patients' rights; the ability to understand and follow treatment instructions and, finally, understanding of how to take care of one's health, such as with good general nutrition. Controlling for cognitive ability does indeed help to explain part of the education gradient in health in Cutler and Lleras-Muney (2010). Similarly, differences in non-cognitive skills, personality measures and social integration also appear as mediating factors.

Many of these main findings are also echoed in Conti et al. (2010) and Conti and Heckman (2010), who provide a framework by which to distinguish between relevant early childhood endowments when determining the causal part of the relationship between health and education. They show that cognitive, noncognitive and health endowments developed early in life (by age 10) are important factors in educational attainment as well as the generation of health outcomes and behaviors at age 30. With their methods the proportion of various health-related outcomes which can be viewed as causally related to education is also estimated. Their work indicates that, for example, selection into education attributable to factors from early childhood explains more than half of the observed educational gradient in such outcomes as general poor health, depression and obesity while for other health-related behaviors, in particular smoking, the causal effect of education is much more profound.

While health surely plays a role in eligibility for disability, health status may not be the only determinant of disability uptake. Type of occupation can influence the extent to which an individual is able to remain employed when struck by disease or injury. Futhermore, earnings replacement rates in social security systems generally create different incentives depending on work history, especially with varying degrees of incapacitation. Both occupation and earnings can themselves be affected by education. In particular, it seems likely that higher educational attainment makes it more likely that a person is able to avoid manual labor, which decreases the chance of many types of physical injury and illness. Jobs with higher status and more job control may also reduce psychological strain and stress. Such factors are documented to be associated with lower disability uptake in Norway (Krokstad et al., 2002).

Replacement rates within the benefits system decrease with earning in Norway as well as many other countries (OECD, 2010). If education increases earnings, higher earnings imply lower replacement rates, and lower replacement rates affect incentives for disability uptake, then education will also affect disability through such pecuniary channels. The effects of incentives on disability pension uptake has been previously studied by Parsons (1980), and these original strong results have been debated, see e.g. the exchange between Bound (1989, 1991) and Parsons (1991). More recent quasi-experimental evidence suggests some effect of incentives on disability pension uptake, see e.g. Gruber (2000) or Chen and van der Klaauw (2008) for analyses of data from Canada and the US or Brinch (2009) and Kostøl and Mogstad (2012) for analyses of the Norwegian disability pension program studied here.

Previous studies document the effectiveness of the Norwegian compulsory schooling reform in raising education levels, primarily at the lower tail of the education distribution (Aakvik et al., 2010). Also, the reform has been shown to generate returns to education, i.e. have a causal effect on earnings (Aakvik et al., 2010), and to affect the cognitive abilities of men in early adulthood, as measured by the cognitive ability ("IQ") test from the universal draft assessment for the Norwegian military (Brinch and Galloway, 2012). [1] Thus, given the particularly large difference in disability uptake when moving from low to middle levels of education and the documented effectiveness of the reform in affecting outcomes and behaviors

[1] The effect of the reform on other outcomes is also discussed in a number of other papers, including Black et al. (2008); Monstad et al. (2008) on female fertility and Black et al. (2005) on intergenerational transmission of human capital.

that appear relevant for understanding disability uptake, there would have been good reason to expect the reform to also have had an effect on disability pension use.

3 Empirical strategy and Data

The schooling reform studied in this paper was introduced in different municipalities in Norway at different times over the course of many years, primarily in the 1960s. The reform raised compulsory schooling from 7 years to 9 years. Almost 20 percent of the pupils completed less than 9 years of schooling prior to the reform. In addition to increasing the number of years pupils stayed in school, the reform may also have affected the quality of education. Prior to the reform, two different types of educational institutions existed at the middle school level, i.e. beyond the compulsory 7-year basic education. *Realskole* was academically oriented and prepared students for further education at the upper secondary level. *Framhaldsskole* offered basic practical or vocational education, mostly in one-year courses. Both schooling types were not offered in all municipalities and travelling distances in many communities would have made education beyond the compulsory level prohibitively expensive for many youths in rural Norway. We can assume that pupils who would have attended the non-academic schools prior to the reform were able to receive high-quality education under the reform regime, since the reform provided them with education at a higher level closer to home. Further details on both the refom and the data used in this analysis can be found in Brinch and Galloway (2012). [2]

The key advantage of this reform for research purposes is that it was implemented in different municipalities in different years, giving rise to difference-in-difference identification of the effects of reform. In addition, because essentially all municipalities introduced the reform at some point, the standard limitation of difference-in-difference that we need to assume parallell counterfactual trends for treatment and control groups is somewhat alleviated. Since all municipalities are in both treatment and control groups (albeit at different times),

[2] Extensive narratives on the reform as well as educational structure in Norway at the time are available in Norwegian in Telhaug (1982) and Myhre (1992).

idiosyncratic trends are not a problem unless these trends are not linear in nature. The timing of the reform in different municipalities was not explicitly randomized, but a clear objective was to ensure that the early years of the reform (referred to as a "trial" at the time) encompassed diverse municipalities. There is little evidence suggesting correlations between characteristics of the municipalities and timing of the reform (Lie, 1973). There are many similarities between this reform and a compulsory schooling reform introduced somewhat earlier in Sweden, see Holmlund (2008), used for analysing the effect of education on health outcomes in Lager and Torssander (2012) and Meghir et al. (2012).

This project utilizes data from a number of different comprehensive administrative registers encompassing the entire resident population of Norway. A person's place of residence at age 14, i.e. when he or she would have started 8th grade under the new educational system, is used to establish whether or not he/she would have been offered middle school education under the old or new educational system; such data on municipality of residence is available starting in 1964, and for this reason we focus on cohorts born after 1949. Data on highest level of educational attainment at age 30 was taken from the Norwegian national educational database (NUDB) and the detailed (six-digit) educational coding includes information on both level and type of schooling completed[3]. The data allow us to distinguish between the different types of pre-reform schooling (7-year compulsory primary school, *framhaldsskole,* and r*ealskole)* and the new type of post-reform compulsory middle school education. There was a change of practice for registration of education lengths of 11 years (some education at the secondary level) for cohorts born after 1958, so we choose to exclude birth cohorts born after 1958. In other words, we are able to study outcomes for birth cohorts born 1950-1958 and cover therefore reform implementation in all but the very small minority of municipalities that introduced the reform either before 1964 or after 1972.

[3]The basis for the data on educational obtainment in NUDB starts with the 1970 census. From 1974 onward updated information on educational participation and attainment has then been collected directly from (all) eductional institutions in the country. Due to the "missing" data from 1971-1973, complete information on educational attainment was first available after corrections and updating could take place in conjunction with the 1980 census.

As a result of a major restructuring of municipalities during the early 1960s, a large number of municpialities merged (or were split up) and the total number of municipalities was reduced dramatically. In some cases, a municipality which had already introduced the reform merged with another community that had not yet implemented the reform. The merging or splitting up of municipalities forced us to exclude all persons in the affected municipalities from further analysis except in rare cases when reform timing and municipal mergers were compatible with respect to identifying reform timing. Furthermore, information on implementation of the reform in Oslo exhibited some inconsistencies, i.e. the appearance then disappearance of a number of new types of middle schools several years before full reform implementation in the capital city (Statistics Norway, 1964). Altogether, such "institutional" difficulties forced us to exclude roughly 20 % of municipalities (in existence in 1964) and 22 % of persons in the relevant birth cohorts. For another 18 % of individuals in the relevant cohorts (27 % of municipalities), the patterns exhibited in the data were insufficient to pinpoint the timing of the reform. This would be generally be the case for very small municipalities–of which there were many in Norway at the time–where the educational choices of just a small handful of students would have been enough to mask any sharp change in educational attainment due to the reform. Altogether, we were able to identify reform timing for a majority (roughly 60 percent) of the 1950-1958 birth cohorts. Since there was some indication of the last pre-reform cohorts having been affected by the pending reform, we consider the last pre-reform cohort in each municipality as partially treated and exclude them from the analysis. For further details on pinpointing and validating reform years for different municipalities, consult Brinch and Galloway (2012).[4]

Given that disability pension use can, in many cases, be related to severe health problems, there is a danger that mortality might introduce non-random selection in the population for analysis. For this reason, our main outcome of interest will be a variable indicating whether a person is on disability or deceased by age 50, but we also present separate results for DP

[4]The data on the reform are exactly the same in this study and in Brinch and Galloway (2012).

Table 1: Descriptive statistics

Birth cohort	1950	1951	1952	1953	1954	1955	1956	1957	1958
Full population									
Education length	11.911	12.002	12.068	12.129	12.176	12.230	12.273	12.405	12.461
(stand. dev.)	(2.582)	(2.581)	(2.548)	(2.542)	(2.513)	(2.487)	(2.434)	(2.387)	(2.373)
Disability, age 50	0.101	0.101	0.104	0.104	0.105	0.100	0.100	0.094	0.091
Full disability	0.076	0.076	0.076	0.077	0.076	0.074	0.077	0.073	0.072
Dead by age 50	0.041	0.039	0.039	0.039	0.038	0.038	0.039	0.038	0.037
Female	0.487	0.485	0.487	0.489	0.491	0.488	0.490	0.490	0.489
# observations	57.243	55.480	57.253	57.734	57.245	58.020	59.047	57.699	57.902
Sample for analysis									
Education length	11.890	11.993	12.065	12.114	12.182	12.207	12.274	12.395	12.459
(stand. dev.)	(2.568)	(2.557)	(2.534)	(2.524)	(2.522)	(2.474)	(2.439)	(2.391)	(2.374)
Disability, age 50	0.105	0.104	0.106	0.106	0.107	0.104	0.101	0.096	0.093
Full disability	0.079	0.078	0.078	0.078	0.078	0.077	0.078	0.074	0.074
Death by age 50	0.041	0.039	0.039	0.039	0.039	0.038	0.038	0.036	0.037
Female	0.485	0.484	0.485	0.487	0.490	0.488	0.492	0.487	0.488
Reform	0.163	0.307	0.430	0.502	0.575	0.693	0.806	0.877	0.932
# observations	32.633	32.012	33.275	33.713	33.484	34.073	34.856	34.322	34.263

alone (where death is classified as not being on DP) and mortality alone, i.e. having died by age 50, in order to evaluate whether mortality might be a major issue influencing results. Data on deaths were taken from the populations registers available from Statistics Norway.

Table 1 presents the basic descriptive statistics on the full population of cohorts being studied as well as the subset of individuals for whom we could identify the timing of the reform and were therefore included in the analysis. We see that the there is little difference in key variables between the full population and the sample used in analysis.

4 Econometric methods

4.1 Difference-in-differences and linear instrumental variables

In general, we are interested in studying the effect of (years of) schooling on binary outcome y_i, which in this paper is an indicator for DP use, mortality or a combined DP/mortality outcome variable. This relationship is specified in terms of a linear regression linear probability model (LPM):

$$y_i = x_i^{'}\beta + \gamma s_i + \epsilon_i \tag{1}$$

where $y_i = 1$ if the individual receives disability benefits (and/or is dead) and zero otherwise, ϵ_i is an error term, x_i is a vector of relevant covariates, including a constant term, β is a vector of parameters to be estimated, and s_i is years of schooling such that the parameter γ captures the estimated effect of education on disability pension use. The main issue in this analysis is whether and/or to what extent schooling affects DP. In particular, if schooling is correlated with other factors that affect disability (and/or death), then s_i as specified in (1) would be endogenous with respect to such outcomes and ordinary least squares (or similar) estimators of γ will be inconsistent.

One way to address such an endogeneity problem is to find an instrumental variable (IV), r_i, which is correlated with education but otherwise uncorrelated with the error term in (1), conditional on the vector x_i. We specify

$$s_i = x_i\theta + \rho r_i + \upsilon_i \tag{2}$$

where υ_i is another error term, and ρ a parameter capturing the relationship between schooling and the instrument. In such a case, we can then apply an appropriate method, such as indirect least squares, to estimate γ. More specifically, inserting (2) into (1) yields

$$y_i = \alpha + \gamma\theta + x_i^{'}\beta + \gamma\rho r_i + \gamma\upsilon_i + \epsilon_i = \kappa + x_i^{'}\beta + \lambda r_i + \zeta_i \tag{3}$$

with $\kappa = \alpha + \gamma\theta$,$\lambda = \gamma\rho$, and $\zeta = \gamma\upsilon_i + \epsilon_i$. Thus, the final estimate of γ can be obtained by dividing the $\hat{\lambda}$ estimated from OLS in equation (3) by the $\hat{\rho}$ estimated from OLS in equation (1).

In this analysis, the vector x_i includes indicators for time and municipality of residence (at age 14). Treatment (reform) status is the instrument, r_i, and is determined by year of birth in relation to timing of the reform in each individual's municipality of residence at age 14, i.e. $r_i = 1$ if it is reasonable to assume, based on birth cohort, place of residence at age 14 and timing of the reform, that a person would have attended 8th grade under the new schooling system, and $r_i = 0$ otherwise. Estimation of equation (3) provides the difference-in-differences estimate of the effect of the reform on DP, $\hat{\lambda}$, and estimation of equation (2) results in the difference-in-differences estimate of the effect of the reform on schooling, $\hat{\rho}$. The IV estimate is then the ratio of these two effects. To the extent the reform might also have increased the quality (and not only the quantity) of education obtained, the IV estimate would violate the typical exclusion restriction implicit in IV-estimation leading to upward bias in the magnitude of the effect of years of education on DP.

4.2 The implicit estimand of linear IV for nonlinear relationships

Descriptive evidence on the relationship between education and DP (see Figure 1) gives us reasonable grounds to suspect that the effect of years of schooling is non-linear, i.e. that the magnitude of the marginal effect of increased education varies over level of education. As discussed in Lochner and Moretti (2011) and Løken et al. (2012), OLS and IV estimates entail different linear combinations of the margin-specific effects at different levels of education. In OLS that linear combination is a form of weighted average of the marginal effects at different margins. IV is, instead, a linear combination of margin-specific effects with coefficients that can be estimated from the data. Intuitively, the weights for OLS reflect the entire distribution of education in the population under analysis, whereas the linear combination coefficients for IV reflect the margins at which the instrument (reform) shifts educational attainment. In the presence of non-linear effects, these different linear combinations in OLS and IV can lead to different estimates of the magnitude of the effect of interest even in the *absence* of endogeneity problems in OLS.

In the following we briefly show how the IV and OLS estimands can be expressed as linear combinations of marginal effects based on similar discussions in Lochner and Moretti (2011) and Løken et al. (2012). We further proceed to describe how we construct benchmark comparison measures for the IV estimators that converge to the same limit as these estimators under the assumption of exogeneity of education. Because OLS is a linear combination of such marginal effects the benchmark measure reweights the original OLS estimates to take explicit account of the margins affected by the reform we study.

To understand the weighting scheme of an OLS estimate in the case of a non-linear relationship, define indicator variables $\delta_{is} = 1\{s_i > s\}$ for discrete levels of education, $s = s_{min}, \ldots, s_{max}$. The indicator function $1\{\}$ takes the value 1 if the argument is true and the value 0 if the argument is false. It now follows that

$$s_i = s_{min} + \sum_{j=s_{min}}^{s_{max}} \delta_{ij}. \tag{4}$$

and the general non-linear relationship between education and disability can be written as

$$y_i = \alpha + x_i^{'}\beta + \Sigma_{j=s_{min}}^{s_{max}} \gamma_j \delta_{ij} + \epsilon_i. \tag{5}$$

To see how the OLS estimator of γ can be written as a weighted average of the marginal effects, γ_j, we first define $s_i^* = s_i - \hat{\phi} x_i$, where $\hat{\phi}$ is the OLS estimate of the linear regression of s_i on x_i. The OLS estimator of γ can now be expressed as the empirical analog of

$$\gamma_{OLS} = \frac{Cov(s_i^*, y_i)}{Var(s_i^*)} \tag{6}$$

Inserting y_i from equation (5), we find that

$$\gamma_{OLS} = \sum_{j=s_{min}}^{s_{max}} \gamma_j \frac{Cov(s_i^*, \delta_{ij})}{Var(s_i^*)} + \frac{Cov(s_i^*, \epsilon_i)}{Var(s_i^*)}, \tag{7}$$

where the latter term is zero under the exogeneity assumption. Hence, the model that specifies the expected outcome as a linear function of s estimates a weighted average of the marginal effects of s on the expected outcome.

With similar reasoning, the estimator of γ obtained with the aid of instrument r can be expressed as the empirical analog of

$$\gamma_{IV} = \frac{Cov(r_i^*, y_i)}{Cov(r_i^*, s_i)}, \tag{8}$$

where $r_i^* = r_i - \hat{\tau} x_i$, where $\hat{\tau}$ is the OLS estimate of the linear regression of r_i on x_i. Inserting from equation (5), we find

$$\gamma_{IV} = \sum_{j=s_{min}}^{s_{max}} \gamma_j \frac{Cov(r_i^*, \delta_{ij})}{Cov(r_i^*, s_i)} + \frac{Cov(r_i^*, \epsilon_i)}{Cov(r_i^*, s_i)}, \tag{9}$$

where the latter term is zero under the assumption that the instrument is exogenous. Equation (9) shows that the IV estimate is a linear combination of marginal effects, γ_j, with the linear combination coefficients defined as $Cov(r_i^*, \delta_{ij})/Cov(r_i^*, s_i)$. By comparing with equation (6) it is clear that in a case with non-linear marginal effects the coefficients used to construct the linear combination of marginal effects in IV differs from the corresponding coefficients used in OLS except in the special case where the variable is an instrument for itself.

Note that the linear combination coefficients for the IV estimate are actually straightforward to estimate by means of the empirical moments in (9). If the true data generating process is non-linear, OLS and IV will generally not lead to the same estimates, even in the case of exogeneity of schooling. In order to properly assess exogeneity of schooling (as well as interpret changes in the magnitude of the estimates from the different methods in general) one needs to create a relevant OLS benchmark for comparison with the IV results and to do so one must obtain the level-specific OLS estimates of γ_j, estimate the linear combination coefficients for IV and then adjust the OLS estimate accordingly.

By appropriately re-adjusting the OLS estimates to be more comparable to the IV estimate, we are essentially answering the question: "What change in expected outcomes would we predict from an average one-year increase in schooling in the population when the increase in schooling was concentrated at certain levels and we assume that education levels (as observed in our data) are exogenous with respect to DP when estimating OLS?"

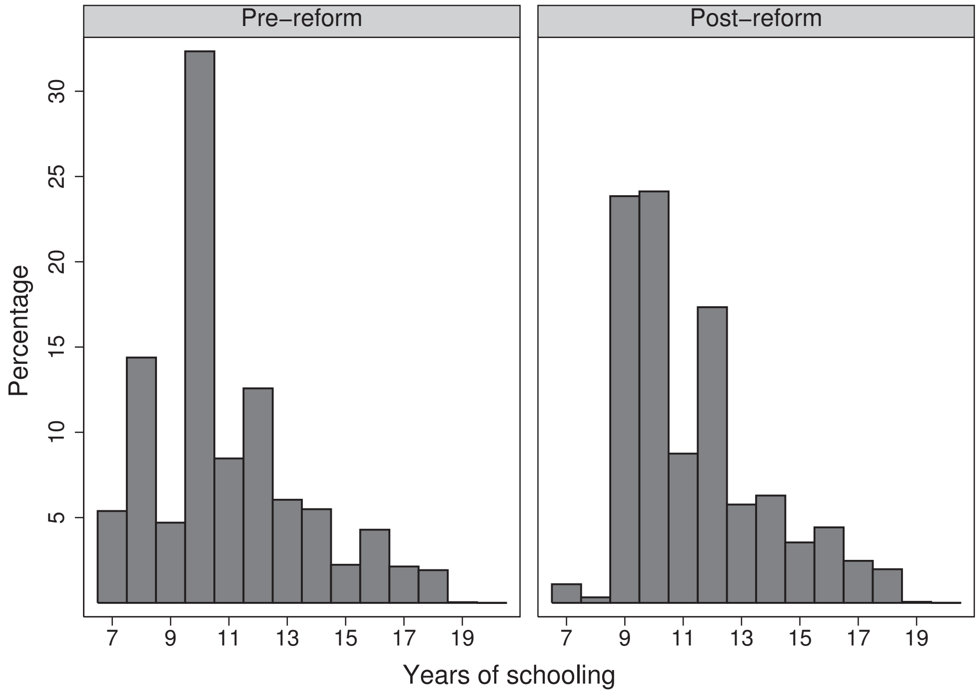
The most straightforward manner in which to estimate γ_j is using dummy variables for each level of education in OLS, see equation (5), but certain features of the Norwegian data and educational system make this a bit more tricky. As Figure 2 documents, the pre-reform educational system mainly led to educations of lengths 7 for completed elementary school, 8 for completed *framhaldsskole*, 10 for complete *realskole* and 12 for completed secondary school. Using the data to predict the effects of changing education from 8 to 9 and from 9 to 10 years is therefore difficult, because the data points at 9 years for the pre-reform period are rare and most likely consist of a mix of persons who take unusual forms of education (such as at private middle schools), attend the new middle school (perhaps in a neighboring town) before it was available in their own area, migrate or are simply subject to data misclassification and/or input error.

The paucity of observations with 9 years of education prior to the reform and 7 and 8 years of schooling after the reform also has implications for the comparison of OLS and IV estimates. If we pool pre- and post-reform data in OLS estimation, then the marginal effect estimated for the margin from 8 to 9 years of schooling would largely reflect variation introduced as a result of the reform, since observations with 8 years of education would largely come from pre-reform data and observations with 9 years of education would largely come from post-reform data. A simple solution to this difficulty is to base OLS estimates on pre-reform data only and to employ some form of local smoothing in estimating marginal effects at different educational levels. To this end, we estimate level-specific marginal effects based on quadratic splines with knots at every *second* year of schooling on pre-reform data when estimating marginal effects at different levels with OLS.

Formally, quadratic splines are accommodated within a multiple regression framework by introducing the following variables in the regression equation

$$s_{ij} = \delta_{is}(s_i - j)^2, j = 7, 9, 11, 13, 15, 17, 19, \tag{10}$$

Figure 2: Highest completed education, pre- and post-reform



where the choices for j are termed the knots. Conceptually, the expected outcome is now specified to be a function of s that is continuous, continuously differentiable everywhere, locally quadratic, but with shifts in the second order derivatives at the knots. Since s is discrete in the data, the main purpose is to impose a reasonably smooth function without imposing unnecessary additional restrictions.

5 Results

5.1 Linear model results

Even though we suspect that linearity of effects does not hold in our case, we first present basic results from a linear framework as a useful baseline specification based on standard methods. Table 2 provides OLS estimates on the effect of years of schooling on the three outcomes mortality, disability and combined mortality/disability for men and women separately and together. If we were willing to assume that length of schooling is exogenous

Table 2: Linear OLS estimates of the effect of years of schooling on mortality and disability

Gender	All	Men	Women
Mortality	-0.005	-0.007	-0.003
	(-0.006,-0.005)	(-0.008,-0.007)	(-0.003,-0.002)
Disability	-0.020	-0.018	-0.024
	(-0.021,-0.020)	(-0.019,-0.017)	(-0.025,-0.023)
Disability/mortality	-0.026	-0.025	-0.026
	(-0.027,-0.025)	(-0.026,-0.024)	(-0.027,-0.025)
Municipality fixed effects	x	x	x
Cohort fixed effects	x	x	x
Gender control	x	n.a.	n.a.
# observations	275.500	141.281	134.219

in OLS analysis, the table would suggest that the effects of schooling on DP are strongly negative and highly statistically significant for the combined disability/mortality measure as well as mortality and disability separately. One additional year of schooling is associated with a 2.5 percentage point lower probability of DP/mortality. The effect of education on mortality is larger for men than women, whereas the effect of education on DP alone is larger for women. The effect of on the combined DP/mortality outcome is roughly the same for men and women. Note that the effects suggested by the OLS estimates are rather large in relative terms, with effects of one extra year of education reducing mortality at age 50 by about 15 percent and disability use by about 20 percent.

Table 3 provides difference-in-difference estimates of the effect of the reform on education as well as DP and mortality outcomes. As detailed in Section 5.1, an IV estimate with the educational reform can be obtained by taking the ratio of the reform effect on DP and the reform effect on education. These IV estimates, along with the predicted effect of the reform in the case when years of schooling is exogenous are also presented in Table 3. The main result from Table 3 is that none of the estimates of the effects of education are significantly different from zero at a 5 % significance level. To assess the precision of these null results, we therefore present results with 95 percent confidence intervals around the estimated effects

Table 3: Quasi-experimental estimates

Gender	All	Men	Women
Difference-in-differences estimates of effect of:			
Reform on years of schooling	0.222	0.183	0.262
	(0.175,0.268)	(0.124,0.242)	(0.199,0.325)
Reform on mortality	-0.000	-0.002	0.001
	(-0.003,0.002)	(-0.006,0.003)	(-0.002,0.004)
Reform on disability	0.002	0.002	0.003
	(-0.002,0.007)	(-0.004,0.008)	(-0.004,0.010)
Reform on mortality/disability	0.002	0.000	0.004
	(-0.003,0.007)	(-0.007,0.008)	(-0.004,0.011)
IV estimates of effect of:			
Years of schooling on mortality	-0.002	-0.009	0.004
	(-0.013,0.010)	(-0.033,0.016)	(-0.007,0.015)
Years of schooling on disability	0.011	0,011	0.010
	(-0.009,0.031)	(-0.021,0.044)	(-0.017,0.037)
Years of schooling on mortality/disability	0.009	0.003	0.012
	(-0.013,0.030)	(-0.036,0.043)	(-0.015,0.040)
Municipality fixed effects	x	x	x
Cohort fixed effects	x	x	x
Gender	x	n.a.	n.a.
# observations	275.500	141.916	134.521

Notes: Confidence intervals in parentheses based on robust standard errors clustered on the municipality level.

and turn our attention to gleaning what we can from these confidence intervals.

However, before turning to that task, we can note that alternative specifications of the quasi-experimental analysis yield similar results. In particular, Table 4 reports results from several of the most important alternative specifications relevant in this context. A typical difference-in-difference analysis focuses on one area that experiences some type of change or "reform" and another - somehow similar - area that does not and compares the change in the affected area over time with the concurrent change in the unaffected area. The main identifying assumption of this approach is, therefore, that the trend in the two areas would have been similar in the absence of the reform. In many cases, this assumption can be questioned based on possible reasons why one area implemented (or experienced) some type of notable change. In other words, the simple fact of one area introducing a reform when the other did not may itself be strong indication of the two areas being quite different, in ways that are difficult to observe. In the case of the reform studied here, *all* Norwegian municipalities do eventually introduce the reform, so that argument in its simplest form does not pose an immediate obvious threat to the identification set-up in this paper. However, there can be a threat to the identifying assumption in this paper if the *timing* of the reform was somehow correlated with differing *trends* in educational attainment and/or DP/mortality and we are unable to sufficiently account for this. We can address this issue in a number of different ways, the two most important of which are presented in Table 4. The results presented under "Alt 1" use only those municipalities that introduced the reform in the period 1951-1958, since other municipalities implicitly only serve as controls (for time trends) in our analysis. If early or late reform municipalities differ greatly in trends in educational attaintment, DP or mortality over cohorts, then allowing these to function as part of the "control group" in the analysis can bias results. The results presented under "Alt 3" in Table 4 specifies and estimates a separate linear trend for each municipality, i.e. allows for flexibility in the relevant time trend for each municipality. Finally, Table 4 also reports results (under "Alt 2") with the last pre-reform cohort included in the analysis, since these are otherwise excluded from analysis. As expected, this gives a somewhat weaker first stage estimate (the reform effect on schooling).

5.2 Nonlinear effects of years of schooling

In the section above we established that the effects of education on disability estimated with an IV approach were not statistically different from zero. However, the increased statistical uncertainty implicit in the IV-approach can make it difficult to interpret that result as clear evidence of education truly having no (large) effect on DP. In this section, we would therefore like to gain a deeper understanding of the extent to which the results presented here shed light on the possible extent (or limitations on the extent) to which education affects DP use. As elucidated in Sections 4.2 this requires that we also consider possible non-linearities in the effect of education in a manner akin to that implied by the IV approach. In estimating

Table 4: Robustness of quasi-experimental estimates

	Baseline	Alt 1	Alt 2	Alt 3
Effect of reform on schooling	0.222	0.253	0.203	0.272
	(0.024)	(0.026)	(0.020)	(0.029)
Effect of reform on disability/mortality	0.002	0.003	0.002	0.002
	(0.002)	(0.003)	(0.002)	(0.003)
Effect of years of schooling on disability/mortality	0.009	0.013	0.009	0.005
	(0.011)	(0.012)	(0.011)	(0.012)
# observations	275.500	205.456	302.631	275.500
Gender	x	x	x	x
Cohort fixed effect	x	x	x	x
Municipality fixed effects	x	x	x	x
Municipality specific linear trends				x
Including immediate pre-reform cohort			x	
Including only municipalities with treatment 1951-1958		x		

Notes: Robust standard errors clustered on the municipality level in parantheses.

and presenting results on the relevance of different linear combination coefficients in OLS and IV estimates, this section will also document that the reform shifted educational attainment precisely in that part of the educational distribution where we might expect to also find the largest effects of education. This, in turn, affects the baseline which is needed to properly assess the (range of) effect sizes which can be reasonably ruled out based on this analysis. Such assessment can take place for both difference-in-difference estimates of the effect of the reform on disability/mortality and IV estimates of the effect of (a year of) schooling on disability/mortality.

We saw in Section 4.2 that IV estimates a specific linear combination of marginal effects with coefficients that can be estimated from the data in the case of a non-linear relationship between the (potentially) endogenous variable and the outcome. Table 5 presents the different coefficients used in the linear combination of marginal effects implicit in IV and OLS estimation. When looking at the IV coefficients, we see that the effect of the reform was heavily concentrated on the margin going from 8-9 years, since that coefficient was, by far, the largest. There is also clearly some “weight” placed on the 7-8 margin. Furthermore, the coefficient for the 9-10 margin was negative. This means that some individuals who would

Table 5: Linear combination coefficients in IV and OLS for different levels of education

Education level (margin)	IV	OLS
7-8 years	0.185	0.018
8-9 years	0.788	0.045
9-10 years	-0.183	0.107
10-11 years	0.091	0.157
11-12 years	0.073	0.161
12-13 years	0.036	0.143
13-14 years	0.025	0.125
14-15 years	0.005	0.098
15-16 years	-0.002	0.078
16-17 years	-0.012	0.044
17-18 years	-0.005	0.022
18-19 years	0.000	0.001
19-20 years	-0.000	0.000

Notes: See equations (6) and (9).

have obtained 10 years of education in the old system, only obtained 9 years of education after the reform. Since it would have been natural for some students to end education after completing *realskole* under the old system, it is reasonable to expect that 10 years of education was a fairly common level of educational attainment prior to the reform. (See also Figure 2.) After the reform, 10 years of education would have corresponded to completing one year of upper secondary education, which would not be such a natural point for ending education. Hence, the situation documented in Table 5 is not surprising. From Table 5 we can also note that the reform induces some people to achieve education beyond 10 years, as some of the coefficients for the 10-11, 11-12, 12-13 and 13-14 margins are small but positive. After that, the coeffcients are largely negligible. In other words, Table 5 gives us little reason to suspect that the reform influenced educational obtainment beyond the secondary level. Finally, note that the OLS linear combination coefficients are very different from the IV weights. OLS emphasizes the margins from 9-14 years of education and gives very little weigh to the 8-9 margin. Hence, that alone would lead us to expect that there might be quite large differences between OLS and IV results.

Table 6 first presents the different (non-linear) marginal OLS effects based on quadratic splines for 2-year intervals. The marginal effects are clearly decreasing in level of education for all three outcomes. The effects are strongest for the lowest educational levels and seem to disappear altogether for education beyond 14 year.

Table 7 shows our benchmark comparison measures derived form the coefficients in Table 5 and the marginal effects from Table 6. The OLS results originally presented in Table 2 differ from the OLS results presented as “OLS benchmark” in Table 7 for two reasons: the smoothing assumptions imposed by the quadratic spline used in the estimation of the marginal effects and the restriction of estimation to pre-reform data in the estimation of the benchmark. We see that the orginal OLS results differ little from the re-adjusted OLS benchmark results.

The IV benchmark measure reweights the marginal coefficients in Table 6 based on the IV linear coefficients in Table 5 and can be interpreted as the target we would expect our IV to estimate if we were willing to assume that education was exogeneous when estimating OLS. We see that these measures are twice as high as OLS for the disability measure and even higher relative to OLS for the measures involving mortality. This reflects the fact that the reform we use as instrument affected the educational distribution precisely along the margins where the effects of education on outcomes might be expected to be the strongest.

The 95 percent confidence interval for the effect of education on disability/mortality with IV ranges from roughly -0.013 to 0.030. Thus, with such a conservative choice of confidence limits, the data cannot rule out that one additional year could have caused a 1.3 percentage point decrease in disability/mortality. Similarly the lower confidence limit of the effect of education an disability alone is a decrease of about 1 percentage point. Comparing these confidence limits with the benchmark comparison estimates in Table 7 indicates that any causal effects of education on disability pension use revealed by the exogenous variation in education generated by the reform is unlikely to be more than 30 percent of the magnitude suggested by the appropriately re-adjusted benchmark comparisons. The mortality results from the quasi-experimental analysis are not sufficiently precise to rule out effects of the same

Table 6: Estimates of marginal effects of years of schooling based on quadratic splines within OLS

Margin	Mortality	Disability	Mortality / Disability
7-8 years	-0.021 (0.003)	-0.031 (0.005)	-0.053 (0.005)
8-9 years	-0.013 (0.001)	-0.038 (0.001)	-0.051 (0.002)
9-10 years	-0.005 (0.001)	-0.037 (0.002)	-0.042 (0.002)
10-11 years	0.003 (0.001)	-0.028 (0.002)	-0.024 (0.002)
11-12 years	0.002 (0.001)	-0.021 (0.001)	-0.019 (0.002)
12-13 years	-0.010 (0.002)	-0.016 (0.002)	-0.026 (0.003)
13-14 years	-0.011 (0.001)	-0.009 (0.002)	-0.020 (0.002)
14-15 years	-0.003 (0.002)	0.000 (0.003)	-0.002 (0.004)
15-16 years	-0.001 (0.002)	0.001 (0.003)	-0.000 (0.003)
16-17 years	-0.007 (0.004)	-0.006 (0.006)	-0.014 (0.007)
17-18 years	-0.004 (0.005)	-0.005 (0.007)	-0.008 (0.008)
18-19 years	0.009 (0.021)	0.006 (0.032)	0.016 (0.037)
19-20 years	-0.034 (0.138)	-0.014 (0.213)	-0.049 (0.245)

Table 7: Comparing estimates with benchmarks

	Mortality	Disability	Mortality/Disability
OLS (from Table 2)	-0.005	-0.020	-0.026
(95 pct. conf. int.)	(-0.006,-0.005)	(-0.021,-0.020)	(-0.027,-0.025)
OLS benchmark	-0.004	-0.017	-0.022
(95 pct. conf. int.)	(-0.005,-0.004)	(-0.018,-0.017)	(-0.023,-0.021)
IV (from Table 3)	-0.002	0.011	0.009
(95 pct. conf. int.)	(-0.013,0.010)	(-0.009,0.031)	(-0.013,0.030)
IV benchmark	-0.013	-0.034	-0.047
(95 pct. conf. int.)	(-0.011,-0.015)	(-0.037,-0.031)	(-0.051,-0.043)

magnitude as the benchmark comparison estimates with a 95 percent confidence level.

6 Concluding discussion

If the observed statistical relationship between education length and the propensity to become a disability pensioner was largely driven by a causal effect of education on disability, we would expect that the compulsory schooling reform described in this paper would have been a potent means for reducing disability use. We would also expect to be able to uncover evidence of a large causal effect of education on DP use from the quasi-experimental analysis of such a reform. In fact, we were unable to uncover any statistically significant effect of the reform and, as a result, are also unable to provide conclusive evidence that general education at roughly the middle school level has an effect on DP use. When we take non-linearities in the effect of education into account to provide an appropriate benchmark comparison based on properly weight non-linear OLS results, we see that the results presented in this study largely rule out causal effects of education on DP/mortality larger than 30% of the effect suggested by basic OLS results. In other words, we can conclude that, at most, a little over one-quarter of the relationship between education and DP and/or mortality observed in basic OLS results might be due to the causal effect of education on DP use, with point estimates pointing towards no effect at all. Hence, the relationship between education and DP generally observed in descriptive analyses appears to be mostly driven by other explanations, such as selection effects or third factors which affect both educational attainment and DP use.

From a policy perspective, this means that we would be unwise to assume that broad interventions for raising the general level of schooling can make more than a very small dent in aggregate DP use. However, these results should not be misinterpreted as indicating that educational interventions can *never* have an effect on DP. More correctly, this study suggests that broad, *non-targeted* education policies at roughly the middle school level are not alone sufficient to influence outcomes such as DP. There is little evidence to suggest that the additional skills and/or knowledge from an extra year or two of general schooling at roughly age 15 would have been enough – or of a nature – to greatly impact a person's disability status far in the future.

It may also seem puzzling that the educational reform used in this analysis has previously revealed causal effects of education on both earnings (Aakvik et al., 2010) and on general cognitive ability ("IQ") (Brinch and Galloway, 2012) but does not generate an effect on disability use. Based on previous studies (Conti and Heckman (2010); Cutler and Lleras-Muney (2010)) it seems reasonable to assume that both earnings and cognitive ability are important factors in generating health outcomes. However, other studies suggest that DP use is strongly associated with job features, such as job control and high physical demands (manual work)

(Krokstad et al., 2002), which might not change so dramatically with an additional year or two of education, due, perhaps, to general equilibrium effects. In other words, the increase in schooling and/or cognitive ability generated by the increase in education due to the reform might not be enough to change other important elements of a person's job situation or shift a person's relative position in the labor market, even if it does improve cognitive skills, make individuals more productive and lead to slightly higher earnings.

References

Aakvik, A., K.G. Salvanes, and K. Vaage (2010) "Measuring heterogeneity in the returns to education using an education reform," *European Economic Review*, Vol. 54, No. 4, pp. 483–500.

Albouy, V. and L. Lequien (2009) "Does compulsory education lower mortality?" *Journal of Health Economics*, Vol. 28, No. 1, pp. 155–168.

Arendt, J.N. (2005) "Does education cause better health? A panel data analysis using school reforms for identification," *Economics of Education Review*, Vol. 24, No. 2, pp. 149–160.

——— (2008) "In sickness and in health - Till education do us part: Education effects on hospitalization," *Economics of Education Review*, Vol. 27, No. 2, pp. 161–172.

Autor, D.H. and M.G. Duggan (2006) "The Growth in the Social Security Disability Rolls: A Fiscal Crisis Unfolding," *Journal of Economic Perspectives*, Vol. 20, No. 3, pp. 71–96.

Black, S.E., P.J. Devereux, and K.G. Salvanes (2005) "Why the Apple Doesn't Fall Far: Understanding Intergenerational Transmission of Human Capital," *The American Economic Review*, Vol. 95, No. 1, pp. 437–449.

——— (2008) "Staying in the Classroom and out of the maternity ward? The effect of compulsory schooling laws on teenage births*," *The Economic Journal*, Vol. 118, No. 530, pp. 1025–1054.

Bound, J. (1989) "The Health and Earnings of Rejected Disability Insurance Applicants," *American Economic Review*, Vol. 79, No. 3, pp. 482–503.

——— (1991) "The Health and Earnings of Rejected Disability Insurance Applicants: Reply," *American Economic Review*, Vol. 81, No. 5, pp. 1427–1434.

Brinch, C. (2009) "The effect of benefits on disability uptake," *Statistics Norway Discussion papers*, No. 576.

Brinch, C.N. and T.A. Galloway (2012) "Schooling in adolescence raises IQ scores," *Proceedings of the National Academy of Sciences*, Vol. 109, No. 2, pp. 425–430.

Burkhauser, R.V. and M.C. Daly (2002) "Policy watch: US disability policy in a changing environment," *The Journal of Economic Perspectives*, Vol. 16, No. 1, pp. 213–224.

Chen, S. and W. van der Klaauw (2008) "The work disincentive effects of the disability insurance program in the 1990s," *Journal of Econometrics*, Vol. 142, No. 2, pp. 757–784.

Clark, D. and H. Royer (2010) "The Effect of Education on Adult Health and Mortality: Evidence from Britain," *NBER Working Paper*, No. 16013.

Conti, G. and J.J. Heckman (2010) "Understanding the Early Origins of the Education–Health Gradient," *Perspectives on Psychological Science*, Vol. 5, No. 5, pp. 585–605.

Conti, G., J. Heckman, and S. Urzua (2010) "The education-health gradient," in *American Economic Review: Papers and Proceedings*, Vol. 100, pp. 234–238.

Currie, J. (2009) "Healthy, wealthy, and wise: Socioeconomic status, poor health in childhood, and human capital development," *Journal of Economic Literature*, Vol. 47, No. 1, pp. 87–122.

Cutler, D.M. and A. Lleras-Muney (2010) "Understanding differences in health behaviors by education," *Journal of Health Economics*, Vol. 29, No. 1, pp. 1–28.

——— (2011) "Education and health: insights from international comparisons," *NBER Working Paper*, No. 17738.

Eide, E.R. and M.H. Showalter (2011) "Estimating the relation between health and education: What do we know and what do we need to know?" *Economics of Education Review*, Vol. 30, No. 5, pp. 778–791.

Fuchs, Victor R. (1982) *Time Preference and Health: An Exploratory Study*, Chicago: University of Chicago Press.

Grossman, M. (2006) "Education and nonmarket outcomes," *Handbook of the Economics of Education*, Vol. 1, pp. 577–633.

Gruber, J. (2000) "Disability Insurance Benefits and Labor Supply," *Journal of Political Economy*, Vol. 108, No. 6, pp. 1162–1183.

Holmlund, H. (2008) "A researcher's guide to the Swedish compulsory school reform," *Centre for the Economics of Education, London School of Economics and Political Science*.

Jürges, H., S. Reinhold, and M. Salm (2011) "Does schooling affect health behavior? Evidence from the educational expansion in Western Germany," *Economics of Education Review*, Vol. 30, No. 5, pp. 862–872.

Kostøl, A. and M. Mogstad (2012) "How financial incentives induce disability insurance recipients to return to work," *IZA Discussion Paper*, No. 6702.

Krokstad, S., R. Johnsen, and S. Westin (2002) "Social determinants of disability pension: a 10-year follow-up of 62 000 people in a Norwegian county population," *International journal of epidemiology*, Vol. 31, No. 6, pp. 1183–1191.

Lager, A.C.J. and J. Torssander (2012) "Causal effect of education on mortality in a quasi-experiment on 1.2 million Swedes," *Proceedings of the National Academy of Sciences*, Vol. 109, No. 22, pp. 8461–8466.

Lie, S.S. (1973) "Regulated social change: A diffusion study of the Norwegian comprehensive school reform," *Acta Sociologica*, pp. 332–350.

Lleras-Muney, A. (2005) "The relationship between education and adult mortality in the United States," *Review of Economic Studies*, Vol. 72, No. 1, pp. 189–221.

Lochner, L. and E. Moretti (2011) "Estimating and Testing Non-Linear Models Using Instrumental Variables," *NBER Working Paper*, No. 17039.

Løken, K.V., M. Mogstad, and M. Wiswall (2012) "What Linear Estimators Miss: The Effects of Family Income on Child Outcomes," *American Economic Journal: Applied Economics*, Vol. 4, No. 2, pp. 1–35.

Mazumder, B. (2008) "Does education improve health? A reexamination of the evidence from compulsory schooling laws," *Economic Perspectives*, Vol. 32, No. 2.

Meghir, C., M. Palme, and E. Simeonova (2012) "Education, health and mortality: Evidence from a social experiment," *NBER working paper*, No. 17932.

Monstad, K., C. Propper, and K.G. Salvanes (2008) "Education and Fertility: Evidence from a Natural Experiment*," *The Scandinavian Journal of Economics*, Vol. 110, No. 4, pp. 827–852.

Myhre, R (1992) *Development of the Norwegian school (in Norwegian)*: Oslo: Didakta.

OECD (2006) *Sickness, Disability and Work: Breaking the Barriers. Norway, Poland and Switzerland.*: OECD Publishing.

——— (2010) *Sickness, Disability and Work: Breaking the Barriers: a Synthesis of Findings Across OECD Coutnries*: OECD.

Oreopoulos, P. (2007) "Do dropouts drop out too soon? Wealth, health and happiness from compulsory schooling," *Journal of Public Economics*, Vol. 91, No. 11-12, pp. 2213–2229.

Parsons, D. O. (1980) "The Decline in Male Labor Force Participation," *Journal of Political Economy*, Vol. 88, No. 1, pp. 117–134.

——— (1991) "The Health and Earnings of Rejected Disability Insurance Applicants: Comment," *American Economic Review*, Vol. 81, No. 5, pp. 1419–1426.

Powdthavee, N. (2010) "Does education reduce the risk of hypertension? Estimating the biomarker effect of compulsory schooling in England," *Journal of Human Capital*, Vol. 4, No. 2, pp. 173–202.

Silles, M.A. (2009) "The causal effect of education on health: Evidence from the United Kingdom," *Economics of Education Review*, Vol. 28, No. 1, pp. 122–128.

Statistics Norway (1964) "Educational statistics 1963-1964. Primary and continuation schools. Vol. 1 (in Norwegian)."

Telhaug, A.O. (1982) *Norwegian School Development after 1945 (in Norwegian)*: Oslo: Ad Notam Gyldendal.

B

Return to:
Statistisk sentralbyrå
NO-2225 Kongsvinger

From:
Statistics Norway

Postal address:
PO Box 8131 Dept
NO-0033 Oslo

Office address:
Kongens gate 6, Oslo
Oterveien 23, Kongsvinger

E-mail: ssb@ssb.no
Internet: www.ssb.no
Telephone: + 47 62 88 50 00

ISSN 0809-733X

Statistisk sentralbyrå
Statistics Norway

Design: Siri Boquist