

# Signaling and Employer Learning with Instruments

By GAURAB ARYAL AND MANUDEEP BHULLER AND FABIAN LANGE\*

*This paper considers the use of instruments to identify and estimate private and social returns to education within a model of employer learning. What an instrument identifies depends on whether it is hidden from, or transparent (i.e., observed) to, the employers. A hidden instrument identifies private returns to education, and a transparent instrument identifies social returns to education. We use variation in compulsory schooling laws across non-central and central municipalities in Norway to, respectively, construct hidden and transparent instruments. We estimate a private return of 7.9%, of which 70% is due to increased productivity and the remaining 30% is due to signaling.*

*JEL: J24, J31, D83*

*Keywords: signaling, human capital, employer learning, instruments*

Two competing models rationalize the positive relationship between earnings and education that is universally found in data. Since Becker (1962), proponents of the human capital model argue that education increases skills that employers value. By contrast, the job-market signaling model of Spence (1973) posits that education signals differences in innate abilities among workers.<sup>1</sup> Signaling, however, tends to be inefficient because workers expend valuable resources only to signal their abilities. Thus, signaling creates a wedge between private returns and social returns to education.<sup>2</sup>

\* Aryal: Department of Economics, University of Virginia, [aryalg@virginia.edu](mailto:aryalg@virginia.edu). Bhuller: Department of Economics, University of Oslo and Statistics Norway, [manudeep.bhuller@econ.uio.no](mailto:manudeep.bhuller@econ.uio.no). Lange: Department of Economics, McGill University and NBER, [fabolange@gmail.com](mailto:fabolange@gmail.com). We thank Thomas Lemieux, three anonymous referees, Peter Arcidiacono, Leora Friedberg, John Bodian Klopfer, Edwin Leuven, Emily Nix, John Pepper, and participants at the 2018 Cowles Conference in Honor of Joseph Altonji, 2019 Essen Health Conference, 2021 WEAI conference, and seminar participants at University of Alberta, University of Bergen, University of British Columbia, University of Calgary, Deakin University, University of Delaware, Georgetown University, Goethe University Frankfurt, Harris School of Public Policy, London School of Economics and Political Science, Norges Bank, Ohio State University, Ragnar Frisch Centre for Economic Research, University of Rochester, University of Southern California, University of Sussex, Tinbergen Institute, and University of Waterloo for helpful comments and suggestions. This project received generous financial support from the Research Council of Norway through grants 194339, 250516, 267428 and 275123, the Social Science and Humanities Research Council of Canada and the Canada Research Chair program.

<sup>1</sup>For more on signaling and the human capital model, see Wolpin (1977); Tyler, Murnane and Willett (2000); Bedard (2001); Fang (2006); Hopkins (2012); Clark and Martorell (2014); Feng and Graetz (2017) and Arteaga (2018).

<sup>2</sup>We define private returns as the effect of education on individual wages and social returns as the effect of education on individual productivity, and the difference between them is the signaling value. This definition abstracts from productive externalities beyond the employer-employee relationship (Acemoglu and Angrist, 2000; Moretti, 2004), non-production and non-pecuniary benefits (Lochner, 2011; Oreopoulos and Salvanes, 2011), and fiscal externalities and distributional impacts (Stiglitz, 1975).

Education policy requires empirical guidance on how large this wedge is. However, the literature has long recognized the difficulties in separating signaling effects from human capital effects. See Lange and Topel (2006) and the references therein. One way forward is to recognize that if workers use education to signal their abilities at the start of their careers, then over time, employers may also learn about workers' abilities.

Two influential papers, Farber and Gibbons (1996) and Altonji and Pierret (2001) (FG and AP, respectively) use changes across experience in how earnings correlate with schooling and a proxy of unobserved ability (e.g., IQ score) to test employer learning. Following FG and AP, Lange (2007) shows that employers learn fast, and using the first-order conditions that characterize schooling decisions, he identifies an upper bound on how much signaling contributes to the private returns to education over the career. In general, researchers following this line of literature need a valid proxy of unobserved ability. Even then, they can only identify bounds on social returns after imposing strong and unverifiable assumptions.

We add to the set of empirical methods available for distinguishing human capital returns from signaling returns to schooling by asking: what do instrumental variable (IV) estimates of the returns to education identify within the employer learning framework of FG, AP, and Lange (2007)? We show that, under suitable assumptions about the instrument's observability, IV estimates of the causal effects of education on wages point-identify the private and the social returns to education.

Particularly, to make progress in this setting with asymmetric information, we introduce two new concepts related to IVs: a *hidden* IV that employers do not observe and a *transparent* IV that they do. Then we show that, in a general setting where returns to skills can vary with experience, a hidden IV identifies *experience-varying* private returns to education, whereas a transparent IV identifies *experience-varying* social returns to education. Thus, the central message of the paper and its main contribution is to show that what employers know about the IV determines what we can estimate with it. More broadly, this approach can guide the identification strategies in similar contexts where asymmetric information is important.

To identify the signaling returns to education, we need to identify both the private *and* social returns to education. We show that we need to lean more heavily on the model when only a hidden IV is available. In particular, if we assume that the returns to skill are constant across work experience, then we can identify the private and social returns to education. Intuitively, when the productivity returns to education are constant across experience, the private returns, identified using the hidden IV, change only because employers' information changes. Furthermore, with work experience these hidden IV estimates converge to the experience-invariant social return, which is thus also identified. We can then use the gap between these social and private returns to identify the signaling value

of education.<sup>3</sup>

Unsurprisingly, when the returns to skill vary with work experience, a hidden IV alone can not identify the private and social returns to education. However, access to both hidden *and* transparent IVs is sufficient for identification because then we can disentangle the effects of experience-varying productivity on wages from employer learning.<sup>4</sup> Intuitively, we can first rely on how the transparent IV estimates vary with experience to identify the effect of education on productivity –the social returns– across the work experience. Then we can combine these estimates with the hidden IV estimates to separate the social and the private returns to education.

We implement these ideas on a unique dataset of earnings histories between 1967 and 2014 for Norwegian males born between 1950 and 1980. From 1960 to 1975, Norway extended compulsory schooling from 7 years to 9 years, but it did not implement this change on the entire country all at once. Instead, compulsory schooling requirements increased at different times for different municipalities. The differential implementation of this reform provides us with a plausibly exogenous source of variation in schooling attainment.

We use this historical episode to construct two instruments, one of which we interpret as plausibly hidden and the other as plausibly transparent. In particular, to define these two IVs, we rely on an important feature of how the compulsory schooling reform was implemented across municipalities and the local labor markets structure in Norway. In 1960, the Norwegian territory consisted of 732 municipalities, and the timing of reform implementation varied substantially across these municipalities, even within a local labor market.<sup>5</sup> Employment was primarily concentrated in the central municipalities, with employers hiring workers from both central and non-central locations. For workers born and raised in non-central municipalities, it is reasonable to assume that employers were uninformed about the reform’s timing, given that there were a large number of such municipalities, each with a potentially different reform timing.

In contrast, employers were more likely informed about the timing of the reform in the dominant population center within each labor market. Consequently, they observe the reform exposure status for workers who grew up in central locations. Building on this distinction, we interpret our instruments as hidden for those growing up in the non-central municipalities and transparent for those in the central municipalities.

<sup>3</sup>In addition, if we are willing to impose parametric assumptions, such as normality of idiosyncratic shocks in productivity, as in Lange (2007), then we can also identify the speed of employer learning.

<sup>4</sup>In practice, two IVs are seldom available for the same population of workers. To combine estimates from two samples (exposed to hidden IV and transparent IV), we have to assume that the effect of education on productivity varies in the same way by work experience across these two samples. This homogeneity assumption still allows for differences in the overall productivity effect of education across these samples.

<sup>5</sup>Norway is classified into 160 local labor markets. In each market, we define municipalities with the largest population as central municipalities and the rest as non-central. The average population in non-central municipalities was 3,500, and around 40% lived in central municipalities.

Using these data, we first examine how the IV returns to schooling vary across work experience and interpret the patterns through the lens of our model. The hidden IV estimates from the non-central municipalities suggest that the private returns to education are initially high during the first few years in the labor market, but they decline rapidly and stabilize around 5.5% after 15 years of work experience. Thus, our results are in line with those by Lange (2007) who found that employers learn fast. In contrast, the transparent IV estimates from the central municipalities suggest that social returns to education do not vary systematically with experience. In particular, in the transparent IV sample, the social returns to education average at 8.8% across experience levels, close to 10% in the initial year of work experience and 8% after 30 years. Due to this constancy in the social returns, we cannot reject the standard employer learning model with constant returns to skill.

Second, we quantify how much signaling and human capital acquisition contributes to the lifetime returns to education. The signaling value is determined by how quickly employers learn and how strong the signal is, i.e., how large a difference in ability can be inferred based on education. Combining estimates from the transparent and hidden IVs, we determine the internal rate of return to an additional year of education to be 7.9%. Our estimates suggest that 70% of this private return represents the productivity-enhancing effect of education, and 30% represents the signaling effect of education. These estimates are from the model with experience-varying returns to skill. Our conclusions do not change even if we estimate the model, assuming that the returns to skills are constant over experience.<sup>6</sup>

Finally, we consider two extensions of our model. First, we study the identification when it is not clear whether the IV is hidden or transparent. In particular, we consider the case of a *partially-transparent* IV, where the IV is transparent with some probability and hidden with the complementary probability. We show that with experience-invariant returns to skill, a partially transparent IV identifies the lower bounds on the private returns and the signaling value while point-identifying the speed of learning. With experience-varying returns to skill, access to a transparent IV *and* a partially-transparent IV is sufficient to identify the lower bounds on the private returns and the signaling value. Access to a hidden IV *and* a partially-transparent IV, however, is sufficient to point-identify all the parameters under a stronger homogeneity condition on the social returns across samples.

For the second extension, we revisit the assumption about homogeneous returns. Recent studies have provided evidence that the returns can vary across individuals (Card, 1999; Heckman, Lochner and Todd, 2006; Carneiro, Heckman and Vytlacil,

<sup>6</sup>In a companion work Aryal, Bhuller and Lange (2021), we consider an extension to allow a hidden correlate of ability, as traditionally assumed in the employer learning literature, such as AP, FG, Lange (2007) and Arcidiacono, Bayer and Hizmo (2010). In that paper, we report results relying on an IQ test score from the Norwegian Armed Forces to estimate the social and private returns to education. These results are broadly consistent with those in the literature and the IV estimates reported here.

2011). To accommodate heterogeneous returns, we reframe our analysis within the potential outcomes framework, e.g., Imbens and Angrist (1994), with binary (low or high) schooling. We show that our identification results extend to this setting, with a proviso that now a hidden IV identifies the *average* private returns among compliers, and a transparent IV identifies the *average* social returns among compliers.

The rest of our paper proceeds as follows. Section I describes our model and defines the private and the social returns. Section II establishes the identification of the private and the social returns using instruments. Section III presents our data and Section IV contains our empirical findings and robustness exercises. Section V considers extensions and Section VI presents a discussion. Section VII concludes. All additional results are in the Online Appendix.

## I. Model

We present a model of employer learning in a perfectly competitive labor market. Our model extends FG's original formulation, which has become the standard model in the empirical employer learning literature, by allowing the returns to skill to vary with experience.

Let worker  $i$ 's log-productivity with work experience  $t \in \mathbb{T} := \{0, 1, 2, \dots, \infty\}$  be

$$(1) \quad \psi_{it} := \ln \chi_{it} = \lambda_t \times (\beta_{ws} S_i + A_i + \varepsilon_{it}) + H(t),$$

where  $S$  is the years of schooling,  $A$  is the unobserved ability (to both employers and researchers), and  $\varepsilon$  is transient variation in productivity independent of all other variables.<sup>7</sup>

Thus, in our setting, work experience affects productivity through  $H(t)$  and  $\lambda_t \in [0, \infty)$ . Here, the function  $H(t)$  captures how log-productivity commonly varies with work experience across individuals. While we allow  $H(t)$  to be a nonparametric function of  $t$ , we assume that it does not depend on either  $S$  or  $A$ .<sup>8</sup> The parameters  $\{\lambda_t : t \in \mathbb{T}\}$  capture the experience-specific effect of composite individual skill  $(\beta_{ws} S_i + A_i + \varepsilon_{it})$ .<sup>9</sup>

Throughout the paper, we normalize the experience-specific effect at the start

<sup>7</sup>In FG, there are also variables capturing information available to employers but not the researchers. An example of such a variable could be knowledge of a foreign language, typically mentioned in job applicants' résumés and verified. FG also allows for a correlate of ability available to researchers but not employers. These are necessary to derive how the partial correlation between earnings, ability proxy, and schooling changes with experience. For notational convenience, we suppress these variables here. Our results go through unchanged if we allow for them; see the Online Appendix D for more.

<sup>8</sup>In our empirical application, we specify  $H(t, X)$  so that the experience profile can vary flexibly with individual characteristics  $X$  that include dummy variables for birth cohort and childhood municipality.

<sup>9</sup>The composite skill  $(\beta_{ws} S_i + A_i + \varepsilon_{it})$  and skill returns  $(\lambda_t)$  are assumed to be multiplicatively separable in (1). Relaxing this assumption will in most cases require other functional form assumptions about the productivity effect of ability for the identification. We, however, can allow the productivity effect of schooling to be experience-varying, while the ability effect to be experience-invariant as a special case of the model.

of a career to be one, i.e., at time  $t = 0$ ,  $\lambda_0 = 1$ . So,  $\lambda_t$  is the returns to skill in period  $t > 0$  relative to the start of one's career. When  $\lambda_t$  is constant across  $t$ , i.e.,  $\lambda_t = 1$ , for all  $t \in \mathbb{T}$ , we say that the returns to skill are experience-invariant. When  $\lambda_t$  varies with  $t$ , then the effect of the individual skill component ( $\beta_{ws}S_i + A_i + \varepsilon_{it}$ ) on productivity varies with experience and we say that the returns to skill are experience-variant.

Under perfect competition, workers are paid the conditional expected output, given the information available to the employers. At time  $t$ , besides knowing  $S_i$ , employers also observe the total output ( $\chi_{it}$ ), which is equivalent to observing a signal  $\xi_{it} := A_i + \varepsilon_{it}$  about  $i$ 's productivity. Let  $\mathcal{E}_{it} = (S_i, \xi_i^t)$  denote employers' information about  $i$  in period  $t$  where  $\xi_i^t = \{\xi_{i\tau}\}_{\tau < t}$  is the history of all past signals.<sup>10</sup> So, the wage in period  $t$  is equal to the expected productivity given  $\mathcal{E}_{it}$ , i.e.,  $W_{it} = \mathbb{E}[\chi_{it}|\mathcal{E}_{it}] = \mathbb{E}[\exp(\psi_{it})|\mathcal{E}_{it}]$ .

Next, we make distributional assumptions to simplify the wage equation and keep the employer learning process tractable. In particular, we follow Lange (2007) and assume that  $\varepsilon_{it} \stackrel{i.i.d}{\sim} \mathcal{N}(0, \sigma_\varepsilon^2)$  and  $(S_i, A_i) \stackrel{i.i.d}{\sim} \mathcal{N}(\boldsymbol{\mu}, \Sigma)$ , across workers and across experience. Let  $\sigma_0^2 = \text{Var}(A|S)$  be the conditional variance of  $A$  given  $S$ . Thus  $\exp(\psi_{it})$  in (1) is a lognormal random variable with parameters  $\mathbb{E}[\psi_{it}|\mathcal{E}_{it}]$  and  $v_t := \text{Var}(\lambda_t(A_i + \varepsilon_{it})|\mathcal{E}_{it})$ , so its mean is given as  $\mathbb{E}[\exp(\psi_{it})|\mathcal{E}_{it}] = \exp(\mathbb{E}[\psi_{it}|\mathcal{E}_{it}] + \frac{1}{2}v_t)$ .

Substituting this expression for the mean of  $\exp(\psi_{it})$  in the wage equation gives a simpler form  $W_{it} = \exp(\mathbb{E}[\psi_{it}|\mathcal{E}_{it}] + \frac{1}{2}v_t)$ . Then, taking the log on both sides of this equation, and using (1), we get the log-earnings equation

$$\begin{aligned} (2) \quad \ln W_{it} &= \lambda_t \times (\beta_{ws}S_i + \mathbb{E}[A_i|\mathcal{E}_{it}]) + H(t) + \frac{1}{2}v_t \\ &= \lambda_t \times (\beta_{ws}S_i + \mathbb{E}[A_i|\mathcal{E}_{it}]) + \tilde{H}(t), \end{aligned}$$

where  $\tilde{H}(t) \equiv H(t) + \frac{1}{2}v_t$  collects the terms that vary only with  $t$  but not across the realizations of  $\xi_i^t$ . For notational simplicity, we suppress  $\tilde{H}(t)$  until the empirical analysis. Next, we define the *social returns* to education and the *private returns* to education.

### Social Returns to Education.

Above, we defined  $A_i$  as the component of skill that correlates with but is not caused by schooling. Together with assuming that  $\varepsilon_{it}$  is orthogonal to  $S_i$ , it follows that the coefficient  $\delta_t^{\psi|S} := \lambda_t \times \beta_{ws}$  of  $S$  in (1) is the causal effect of education on productivity. For notational simplicity, letting  $u_{it} = \lambda_t \times (A_i + \varepsilon_{it})$

<sup>10</sup>Throughout the paper, we assume that employers have symmetric information about workers' abilities and past outputs. See Section VI.A for more on this assumption.

and suppressing  $H(t)$  in (1) we get our key equation:

$$(3) \quad \psi_{it} = \delta_t^{\psi|S} \times S_i + u_{it}.$$

Thus, the social returns to education,  $\delta_t^{\psi|S}$ , can vary with work experience.

### Private Returns to Education.

In contrast to the social returns, the *private return* to education is the causal effect of schooling on wages, which captures the fact that besides productivity, schooling also affects (log) wages through employers' expectations about the worker's ability. Thus, the private return to education is the partial derivative of log-wages in (2) with respect to  $S$ , which can vary with work experience. The private returns depend on how  $S$  affects  $\mathbb{E}[A_i|\mathcal{E}_{it}]$ , which in turn depends on the employers' learning process. So, next, we model employers' learning process.

To this end, first, note that the joint normality of  $(S, A)$  implies that the conditional expectation of  $A_i$  given information at  $t = 0$ ,  $\mathbb{E}[A_i|\mathcal{E}_{i0}] = \mathbb{E}[A_i|S_i]$ , is linear in  $S$ , i.e.,

$$(4) \quad A_i = \phi_{A|S} \times S_i + \varepsilon_{A_i|S_i},$$

where  $\varepsilon_{A_i|S_i} := A_i - \mathbb{E}[A_i|S_i]$ .<sup>11</sup> Furthermore, the normality assumptions allow us to write the conditional expectation of ability  $\mathbb{E}[A_i|\mathcal{E}_{it}]$  in linear form as

$$(5) \quad \mathbb{E}[A_i|\mathcal{E}_{it}] = \theta_t \mathbb{E}[A_i|S_i] + (1 - \theta_t) \bar{\xi}_i^t,$$

where  $\bar{\xi}_i^t = \frac{1}{t} \sum_{\tau < t} \xi_{i\tau}$  is the average of signals up to period  $t$  and  $\theta_t = \frac{1-\kappa}{1+(t-1)\kappa} \in [0, 1]$  is the weight on the initial signal ( $S_i$ ) with  $\kappa = \frac{\sigma_0^2}{\sigma_0^2 + \sigma_\varepsilon^2} \in [0, 1]$ . In particular, (5) shows that the conditional expectation of ability at time  $t$  is a weighted average of the conditional expectation of ability at  $t = 0$ , before any additional information about productivity has been received, and the average of all additional signals received up to period  $t$ , i.e.,  $\bar{\xi}_i^t$ .

The weight  $\theta_t$  declines with  $t$  because, as time passes, the accumulated observed output measures become a better predictor of individual productivity differences than  $S$ , which was the information available at  $t = 0$ . The rate at which  $\theta_t$  declines with  $t$  depends on the parameter  $\kappa$  that Lange (2007) refers to as the "speed of learning." The speed of learning governs how quickly information about productivity accumulates in the market. If the signal-to-noise ratio  $\sigma_0^2/\sigma_\varepsilon^2$  is high, then  $\kappa$  will be close to 1, and the market learns quickly about the worker's ability  $A$ . However, irrespective of  $\kappa$ , when workers have spent enough time in the

<sup>11</sup>For much of what follows,  $S$  need not be Gaussian, but it simplifies the exposition of the argument. Without it, equation (4) would represent a linear projection of  $A_i$  on  $S_i$  instead of a conditional expectation.

market and enough signals have been accumulated, the employers will put all weight only on the information accumulated after joining the labor market, i.e.,  $\lim_{t \rightarrow \infty} \theta_t = 0$ .

Thus, schooling can affect expected log-earnings at  $t$  in two different ways: (i) directly because employers use schooling to form expectations about productivity; and (ii) through learning, because schooling affects productivity, which employers learn over time by observing workers' outputs. Substituting (4) and (5) in (2), and using  $\xi_i^t = A_i + \bar{\varepsilon}_i^t$  and the fact that  $\mathbb{E}(A_i|S_i)$  is linear in  $S_i$ , and for notational simplicity making a change of variable  $\tilde{u}_{it} \equiv \lambda_t (1 - \theta_t) (A_i + \bar{\varepsilon}_i^t)$  we get

$$\begin{aligned} (6) \quad \ln W_{it} &= \lambda_t (\beta_{ws} + \theta_t \phi_{A|S}) \times S_i + \lambda_t (1 - \theta_t) (A_i + \bar{\varepsilon}_i^t) \\ &= \delta_t^{W|S} \times S_i + \tilde{u}_{it}. \end{aligned}$$

Therefore, the coefficient of schooling ( $\delta_t^{W|S}$ ) in (6) is the private return to education.

Comparing the private returns in (6) with the social returns in (3) gives us the following crucial relationship between the two:

$$(7) \quad \underbrace{\delta_t^{W|S}}_{\text{private returns}} = \underbrace{\delta_t^{\psi|S}}_{\text{social returns}} + \underbrace{\theta_t}_{\text{weight}} \times \underbrace{\lambda_t \times \phi_{A|S}}_{\text{adjustment term}}.$$

Thus, the private returns  $\delta_t^{W|S}$  differs from the social return  $\delta_t^{\psi|S}$  if the marginal effect of schooling on expected  $A$  based on the information available to firms at  $t$ ,  $\lambda_t \times \phi_{A|S}$ , is non-zero. In fact, the signaling literature (Spence, 1973) assumes that the “adjustment term” in (7) is non-negative, so that education has signaling value, and hence  $\delta_t^{W|S} \geq \delta_t^{\psi|S}$ . However, employers eventually learn everything, as  $\lim_{t \rightarrow \infty} \theta_t = 0$ , so, the wedge between the private and the social returns disappears with work experience, i.e.,  $\lim_{t \rightarrow \infty} \delta_t^{W|S} = \lim_{t \rightarrow \infty} \delta_t^{\psi|S}$ .

## II. Identification

In this section, we study how to identify the social and private returns using IVs. The primary concern in the empirical literature on the returns to education is the ability bias, i.e., education correlates with determinants of productivity that are unobserved to the researcher. IV estimates of the returns to education require that unobserved ability is orthogonal to the IV. Our paper's core methodological message is as follows: to interpret the causal effects of education on wages based on IVs as either private or social returns, we must also take a stand on whether the IV is hidden from, or known to, the employers.

Before we present formal identification arguments, we highlight the intuition behind our approach. To this end, consider an IV that is uncorrelated with individual ability  $A$  except through schooling. The question then is how earnings are related to this instrument and how that relation depends on whether employers



observe the instrument and how informed they are about the ability  $A$ . We contrast two polar cases: first, the start of workers' careers when employers only observe schooling and possibly the instrument, and second, after the worker has spent sufficient time in the market so that  $A$  has been revealed to the employers.

Consider the second case first. Once employers know  $A$ , earnings equal productivity, and any differences in earnings associated with variation in the instrument reflect only the causal effect on productivity through education. It is so because, by assumption, the instrument is unrelated to ability  $A$ . Thus, if we consider workers with sufficiently long work experience, whether the IV is hidden or transparent is immaterial—it identifies the social returns to education.

In the first case, it matters whether the IV is hidden or transparent for workers at the start of their careers. When the instrument is hidden, employers observe only schooling, and any variation in schooling induced by the instrument will therefore lead employers to deduce higher ability. Therefore, hidden IVs identify the causal effect of education on wages but not on productivity, i.e., they identify the private but not the social returns to education.

However, when the IV is transparent, average compensation, conditional on the instrument, will equal expected productivity conditional on the instrument. Any variation in compensation with the instrument status will therefore capture the causal effect of the instrument on productivity. Moreover, the IV is uncorrelated with  $A$ , so the causal effect only runs through education. Transparent instruments thus identify the social returns to education, not the private returns.

The remainder of this section formalizes these intuitions and shows what happens during the learning period between these two polar cases and how having hidden *and* transparent IV is sufficient to identify the signaling value of education.

#### A. Instrumental Variables

Throughout the paper, we maintain the assumption that the IV is orthogonal to unobserved productivity components. For simplicity, we develop the analysis using a binary IV,  $D_i \in \{0, 1\}$ , and we assume that this IV satisfies the following assumption:

ASSUMPTION 1: (*Instrumental Variables*)

- 1) (*Exogeneity*):  $u_{it} \perp D_i$ , where  $u_{it}$  is defined in (3);
- 2) (*Relevance*):  $\mathbb{E}[S_i|D_i = 0] \neq \mathbb{E}[S_i|D_i = 1]$ .

Under Assumption 1, the Wald estimate in period  $t$  gives

$$(8) \quad \text{plim } \hat{b}_t^{IV} := \frac{\mathbb{E}[\ln W_{it}|D_i = 1, t] - \mathbb{E}[\ln W_{it}|D_i = 0, t]}{\mathbb{E}[S_i|D_i = 1, t] - \mathbb{E}[S_i|D_i = 0, t]},$$

which is one of the main identifying equations that we use throughout the paper.<sup>12</sup>

For workers with sufficiently long work experience, an IV identifies the social returns to education. In particular, for workers with sufficiently long work experience,  $\lim_{t \rightarrow \infty} \theta_t = 0$  so, with a slight abuse of notation, we get  $\lim_{t \rightarrow \infty} \ln W_{it} = \psi_{i\infty}$ , and because  $S$  does not vary with  $t$ , from (8) we get

$$(9) \quad \text{plim} \left( \lim_{t \rightarrow \infty} \hat{b}_t^{IV} \right) = \frac{\mathbb{E}[\psi_{i\infty}|D_i = 1] - \mathbb{E}[\psi_{i\infty}|D_i = 0]}{\mathbb{E}[S_i|D_i = 1] - \mathbb{E}[S_i|D_i = 0]} = \lim_{t \rightarrow \infty} \delta_t^{\psi|S},$$

where the second equality follows from Assumption 1-(1). Thus the part of the productivity,  $\psi_{it}$ , not caused by schooling,  $S$ , is orthogonal to the instrument,  $D$ . So, with sufficient work experience, i.e.,  $t \rightarrow \infty$ , the IV identifies the causal effect of schooling on productivity. Intuitively, in the long run, everything about a worker's ability is revealed to the employers, and thus, knowledge of the IV itself becomes irrelevant for wage setting. It is only through its effect on schooling, which affects productivity that the IV affects wages in the long run. In the case of intermediate work experience, i.e.,  $t < \infty$ , what  $D$  identifies depends on whether it is hidden from, or known to, the employers.

#### HIDDEN AND TRANSPARENT INSTRUMENTS.

To focus on the role of observability, we distinguish between two types of IVs depending on whether they are observed and priced-in by the employers. A *hidden* instrument is unobserved by the employers and therefore does not enter into wage-setting beyond its effect on schooling. We use the superscript  $\mathfrak{h}$  in  $D^{\mathfrak{h}}$  to refer to a hidden IV. In particular, a hidden IV,  $D^{\mathfrak{h}}$ , satisfies the following assumption:

**ASSUMPTION 2:** (*Hidden Instrument*) For all  $i$ ,  $D_i^{\mathfrak{h}} \notin \mathcal{E}_{it}$  which implies that  $\ln W_{it} \perp D_i^{\mathfrak{h}} | (S_i, \xi_i^t)$ .

Assumption 2 is conceptually different from Assumption 1-(1). Assumption 1-(1) asserts that the IV is conditionally independent of the determinants of productivity not caused by schooling, whereas Assumption 2 captures the idea that wages do not depend on the instrument  $D^{\mathfrak{h}}$ , given the information  $\mathcal{E}_t$ . So,  $\ln W_{it} = \mathbb{E}[\psi_i | \mathcal{E}_{it}, D_i^{\mathfrak{h}}] = \mathbb{E}[\psi_i | \mathcal{E}_{it}]$ .

In many settings, Assumption 2 is a natural assumption. The clearest examples relate to field experiments of interventions that incentivize some students to take more schooling (treatment) and not others (control). In these cases, whether a

<sup>12</sup>We are also implicitly assuming stable unit treatment value assumption (SUTVA), e.g., Assumption 1 in Angrist, Imbens and Rubin (1996) is valid. This assumption holds that a treated unit's potential outcomes are not affected by other units' exposure to the instrument. Signaling models are inherently subject to general equilibrium effects since workers' schooling decisions and employers' inference problems, central to these models, depend on the population distributions of skills and schooling. Also see related discussion on general equilibrium effects in Section VI.

student is in the treatment or the control group is typically not known to employers. Other examples of plausibly hidden instruments from quasi-experimental settings include (i) the interaction of draft lottery number and year of birth in Angrist and Krueger (1992); (ii) the interaction of policy intervention, family background, and season of birth in Pons and Gonzalo (2002); (iii) parents' education and the number of siblings in Taber (2001); and (iv) the elimination of student aid programs interacted with an indicator for a deceased father in Dynarski (2003). Besides these, many studies also exploit interactions of birth year and location of birth with locally implemented policy reforms, e.g., Duflo (2001) and Meghir and Palme (2005), which are similar to our IV.

In contrast to the *hidden* instrument, we say that an instrument is a *transparent* instrument if it satisfies Assumption 1, but is known to the employers and “priced-in” the wages. We use the superscript  $t$  in  $D^t$  to refer to a transparent IV.

**ASSUMPTION 3:** (*Transparent Instrument*) Employers observe  $D_i^t$  and  $\ln W_{it} = \mathbb{E}[\psi_{it} | \mathcal{E}_{it}, D_i^t]$ .

Assumption 3 implies that  $D^t$  is not orthogonal to wages, conditional on schooling and other controls. From Assumption 1, however, we know that  $D^t$  satisfies the exclusion restriction with respect to productivity  $\psi$ . So, while  $D^t$  violates the exclusion restriction for wages and thus does not identify the causal effect of schooling on wages (i.e., the private return), it identifies the causal effect of schooling on productivity (i.e., the social return). We formalize these intuitions in the following two subsections.

Examples of instruments used in the literature that are plausibly transparent are (i) tuitions at two- and four-year state colleges in Kane and Rouse (1995); (ii) a dummy for being a male aged 19-22 from Ontario in Lemieux and Card (2001); (iii) local labor market conditions in Cameron and Heckman (1998); Cameron and Taber (2004) and Carneiro, Heckman and Vytlačil (2011); (iv) change in minimum school-leaving age in the U.K. from 14 to 15 in Oreopoulos (2006); and (v) the distance to the college in Card (1993); Kane and Rouse (1995); Kling (2001) and Cameron and Taber (2004).

In practice, the distinction between hidden and transparent instruments can and often will be blurry. Thus, researchers need to be explicit about their assumptions about employers' information regarding the IV when interpreting the IV estimates.

#### PRIVATE AND SOCIAL RETURNS.

Next, we show that a hidden IV identifies the private returns to education. As discussed earlier, the intuition behind this is as follows. When employers observe a worker with more years of schooling, they do not know that some have higher schooling because of the IV and not because of higher abilities. Thus, workers who are affected by the hidden IV are, on average, paid more relative to the others because Assumption 1-(2) implies that the former group has, on average, more

years of schooling than the latter group. Then Assumption 1-(1) implies that the effect of the IV on the wages is only through schooling, thereby identifying the causal effect of schooling on earnings.

In particular, with a hidden IV,  $D_i^h$ , that satisfies Assumptions 1 and 2, the numerator in (8) for  $D_i^h = d, d \in \{0, 1\}$  becomes

$$\mathbb{E} [\ln W_{it} | D_i^h = d, t] = \mathbb{E} [\beta_{ws} S_i + \mathbb{E} [A_i | S_i, \xi_i^t] | D_i^h = d, t].$$

Here,  $\ln W_{it}$  does not directly depend on  $D_i^h$  because the employers do not use  $D_i^h$  when setting the wages. Instead,  $D_i^h$  affects  $\ln W_{it}$  indirectly via  $(S_i, \xi_i^t)$  that makes up the information  $\mathcal{E}_{it}$  about productivity available to the employers in period  $t$ . Then (4) allows us to express the conditional log-wages given above as

$$\mathbb{E} [\ln W_{it} | D_i^h = d, t] = (\beta_{ws} + \theta_t \phi_{A|S}) \mathbb{E} [S_i | D_i^h = d] = \lambda_t (\delta^{\psi|S} + \theta_t \phi_{A|S}) \mathbb{E} [S_i | D_i^h = d].$$

Subtracting the conditional log-wages at  $D_i^h = 0$  from the conditional log-wages at  $D_i^h = 1$  and taking the probability limit of (8) gives

$$\begin{aligned} (10) \quad \text{plim } \hat{b}_t^{IV^h} &= \frac{\mathbb{E} [\ln W_{it} | D_i^h = 1, t] - \mathbb{E} [\ln W_{it} | D_i^h = 0, t]}{\mathbb{E} [S_i | D_i^h = 1] - \mathbb{E} [S_i | D_i^h = 0]} \\ &= \lambda_t \times (\delta^{\psi|S} + \theta_t \phi_{A|S}) = \delta_t^{W|S}. \end{aligned}$$

Next, we show that transparent IV identifies the social returns to education. Intuitively, employers know the economy's structure and know that the instrument is orthogonal to productivity except for its effect on schooling. Because employers pay the expected product conditional on their information, any average wage difference between treated and untreated populations reflects average productivity differences between the two groups. Since, by Assumption 1, all productivity differences between groups are causal, the transparent IV using wages also identifies the social return to education.

Therefore, if the employers are informed about the instrument, the IV estimate of returns to education is a consistent estimate of the productivity effect of education on earnings, i.e.,

$$\mathbb{E} [\ln W_{it} | D_i^t = d, t] = \mathbb{E} [\delta^{\psi|S} S_i + u_{it} | D_i^t = d, t] = \lambda_t \times \delta^{\psi|S} \times \mathbb{E} [S_i | D_i^t = d],$$

where the first equality follows from (3) and the second equality from Assumption 1. Using this expression for the conditional log-wages in (8) and taking the

probability limit gives

$$(11) \quad \begin{aligned} \text{plim } \hat{b}_t^{IV^t} &= \frac{\mathbb{E} [\ln W_{it} | D_i^t = 1, t] - \mathbb{E} [\ln W_{it} | D_i^t = 0, t]}{\mathbb{E} [S_i | D_i^t = 1] - \mathbb{E} [S_i | D_i^t = 0]} \\ &= \lambda_t \times \delta^{\psi|S} = \delta_t^{\psi|S}. \end{aligned}$$

Hence the Wald estimator for a transparent IV,  $D^t$ , identifies the social returns to education at all  $t$ , i.e.,  $\text{plim } \hat{b}_t^{IV^t} = \delta_t^{\psi|S}$ .

In summary, access to a hidden IV is sufficient to identify the experience-varying private returns to education, and access to a hidden IV is sufficient to identify the experience-varying social returns to education. The two of them together identify the signaling value of schooling and the speed of employer learning, even when the returns to skills vary with experience. Next, we show that if we have access to only a hidden IV, we can still identify the signaling value of education, but it requires the additional assumption that the returns to skills are experience-invariant.

#### B. Experience-Invariant Returns to Skill

With a hidden IV, setting  $\lambda_t = 1$  in (12) gives

$$(12) \quad \text{plim } \hat{b}_t^{IV^h} = \delta^{\psi|S} + \theta_t \phi_{A|S}.$$

Comparing (12) with the private returns defined in (7), we conclude that with experience-invariant returns to skill, at every work experience level  $t$ , the hidden IV identifies the private returns to education, i.e.,  $\text{plim } \hat{b}_t^{IV^h} = \delta^{W|S}$ . Furthermore, from (12) we can see that as the worker gets more experience, i.e.,  $t \rightarrow \infty$ ,  $\lim_{t \rightarrow \infty} \theta_t = 0$  and so  $\text{plim } \hat{b}_t^{IV^h}$  converges to the social return to education,  $\delta^{\psi|S}$ . Thus, having access to a hidden IV and a repeated cross-section of wages across workers' careers is sufficient to identify the productivity effect of education, i.e., the social returns to education.<sup>13</sup> Using private and social returns, we can identify the signaling value of education.

Besides the private and the social returns, a hidden IV also identifies the speed of learning  $\kappa$ . Let  $b_0^{IV^h} = \delta^{\psi|S} + \phi_{A|S}$  denote the private return to education at  $t = 0$  and  $b_\infty^{IV^h} = \delta^{\psi|S}$  denote the social return to education. We can then re-write the IV estimate in (12) at experience  $t$  as a weighted average of private and social returns, as

$$(13) \quad \text{plim } \hat{b}_t^{IV^h} = \theta_t \times b_0^{IV^h} + (1 - \theta_t) \times b_\infty^{IV^h}.$$

Then, using the IV estimates for  $t = 0, \dots, T$ , for a large  $T$  we can estimate the RHS parameters using the non-linear least squares (NLLS). This means that we

<sup>13</sup>Although three periods suffice to identify the model, data from additional periods improve the precision of the estimates.

can “solve” for  $\{b_0^{IV}, b_\infty^{IV}, \kappa\}$  from  $\hat{b}_t^{IV^h}$  for  $t = 0, \dots, T$ , and once we know  $\kappa$  we can determine  $\theta_t$  for each  $t$ .

Similarly with a transparent IV, setting  $\lambda_t = 1$  in (14) gives

$$(14) \quad \text{plim } \hat{b}_t^{IV^t} = \delta^{\psi|S}.$$

Hence the Wald estimator for a transparent IV,  $D^t$ , identifies the social returns to education at all  $t$ , i.e.,  $\text{plim } \hat{b}_t^{IV^t} = \delta^{\psi|S}$ . Unlike the hidden IV, however, access to a transparent IV is not sufficient to identify the signaling value of education or the speed of learning.

### C. Experience-Varying Returns to Skill

Next, we consider the case when the returns to skill vary with experience. Like the experience-invariant case, a hidden IV identifies the private returns to education, and a transparent IV identifies the experience-varying social returns to education. When the returns to skill vary with experience, then the social returns also vary with experience. Thus, to identify the speed of learning, we have to separately identify the effect of employers’ learning from the variation in the returns to the skill with experience.

In this case, access to only one IV, hidden or transparent, is insufficient to identify the speed of learning. If we have access to both hidden and transparent IVs and if  $\lambda_t$  satisfies homogeneity (defined below), we can identify the speed of learning. Our approach to identifying the model with experience-varying returns to skill relies on combining estimates from (10) and (11). However, in practice, two IVs are seldom available for the same sample. So, to combine estimates from different samples, we need homogeneity.

**ASSUMPTION 4:** (*Homogeneous Experience-Varying Returns to Skill*) Let  $\lambda_t$  be homogenous across the hidden IV and transparent IV samples, for each  $t \in \mathbb{T}$ .

Assumption 4 requires that the two samples exposed to hidden and transparent IVs have the same experience-varying returns to skills. This assumption, however, does not impose any restriction on the experience-invariant components of social returns, i.e., the parameter  $\delta^{\psi|S}$  in (10) and (11), so that the overall social returns can be different across the samples. We will discuss the validity of this assumption in our setting later.

The identification then proceeds in the following steps. First, from (11) at  $t = 0$  we can identify  $\text{plim } \hat{b}_0^{IV^t} = \delta^{\psi|S}$ . Then, substituting  $\delta^{\psi|S}$  in (11) identifies  $\lambda_t = \frac{\text{plim } \hat{b}_t^{IV^t}}{\text{plim } \hat{b}_0^{IV^t}}$  for  $t > 0$ . Once we have identified the parameters  $\{\lambda_t : t > 0\}$ , then under Assumption 4 we can use these parameters with  $D^h$  in (10) to identify  $\{b_0^{IV^h}, b_\infty^{IV^h}, \kappa\}$  in the same way as we identified the private returns using  $D^h$  in (13). We can either estimate the parameters sequentially, by following the identification steps, or jointly-estimate the parameters from (10) and (11).

### III. Data and Empirical Setting

In this section, we first describe our data sources, how we construct our sample, and the key variables. Then we describe the Norwegian compulsory schooling reform that we use to construct hidden and transparent IVs. Finally, we discuss the empirical model specifications.

#### A. Data Sources and Sample Construction

We use several registry databases maintained by Statistics Norway to construct a rich longitudinal dataset containing all Norwegian males from 1967 to 2014. We observe each of these individuals' demographic information (e.g., birth cohort and childhood municipality of residence) and socio-economic information (e.g., years of education and annual earnings). Notably, the dataset also includes a unique personal identifier, which allows us to follow individuals' earnings over time and merge our dataset with the information on IQ test scores from the Norwegian Armed Forces.

The Norwegian earnings data have several advantages over those available in most other countries. First, there is no attrition from the original sample other than the natural attrition due either to death or out-migration. Second, the data pertain to all residents and are not limited to some sectors or occupations. Third, we can construct long earnings histories to estimate the returns to education at each year of labor market experience.

We restrict our sample to Norwegian males born between 1950 and 1980, and our sample includes several cohorts for whom we observe earnings for most of their adult life.<sup>14</sup> We restrict the sample to males because females in these cohorts had comparatively lower labor force participation, more variation in hours worked over the life-cycle, and the military IQ test scores are available only for males born after 1950. We further exclude immigrants and Norwegian males with missing information on schooling, childhood municipality of residence, IQ test score, or "exposure" to the compulsory schooling reform. Applying these restrictions, we retain 732,163 Norwegian males.

Our primary outcome variable is the natural logarithm of pre-tax annual labor earnings.<sup>15</sup> Although hourly wages would have been an ideal outcome to measure, they are unavailable for most of our sample period. We thus restrict our sample to workers with earnings above the SGA threshold (which was U.S. \$10,650 in 2015) to minimize variation in earnings induced by variation in working hours.<sup>16</sup> We are left with 718,237 males forming a panel comprising 14,746,755 person-year observations. On average, an individual works full-time for approximately 20.5

<sup>14</sup>In our annual income panel data from 1967 to 2014, we observe the oldest cohort (1950) between the ages 17 and 64 and the youngest cohort (1980) up to age 34.

<sup>15</sup>These earnings exclude income from capital and self-employment. They also exclude unconditional cash transfers from the government, e.g., social and economic assistance, housing assistance, child allowance.

<sup>16</sup>In Section IV.C, we show that our results are robust to varying the earnings threshold.

years. We use this sample in our empirical analysis, but the sample is unbalanced across work experience. We observe earnings for 579,984 individuals at the start, i.e., experience  $t = 0$ , and 190,900 individuals after thirty years, i.e., experience  $t = 30$ .<sup>17</sup>

### B. Measures of Schooling and IQ

Our primary variable of interest is years of schooling, corresponding to the highest level of completed education. This variable comes from Statistics Norway's Education Register. Statistics Norway uses reports on educational attainment submitted directly by the relevant educational establishments to determine each individual's schooling years. Thus the chance of misreporting is slight. Using the years of schooling variable and the age at the start of each year, we measure potential experience as age minus years of schooling minus school starting age.<sup>18</sup>

We also have information on conscripts' IQ test scores from the Norwegian Armed Forces. In Norway, military service was compulsory for all able males in the birth cohorts in our sample. Before each male entered the service, the military assessed his medical and psychological suitability. Most eligible Norwegian males in our sample took this test around their 18<sup>th</sup> birthday. We use the IQ test score that is a composite unweighted mean from three speeded tests—arithmetics, word similarities, and figures.<sup>19</sup>

In Figure 1, we show the average and conditional density of IQ for each year of schooling between 7 and 21 years. This figure illustrates two striking patterns in our data worth noting. First, the measures of IQ and schooling are strongly correlated, with a correlation of almost 0.5. Second, sharp increases in the average IQ score occur around the entry years of college (13/14 years) and master's degree (16/17), with more gradual increases at other schooling stages. This pattern could be due to substantial ability-related (psychic) costs or particular requirements for enrolling in higher education in Norway.<sup>20</sup> This strong correlation in our data suggests that schooling predicts ability, satisfying a necessary condition for schooling to have a signaling value in our setting.

<sup>17</sup>Death or migration contribute little to the decline in the number of observations between  $t = 0$  and  $t = 30$ . The main reason the number of observations declines is because the sample ends in 2014. Thus, we can follow the first cohort (1950) across all ages from 17 to 64, while we can only follow the last cohort (1980) until they are 34 years old. In Section IV.C, we show results for alternative samples, e.g., by limiting the analysis to cohorts 1950–1965 and focusing on the early part of the workers' careers, for which the panel is balanced.

<sup>18</sup>We measure age of individuals at the start of each year. We then follow their earnings from the first calendar year after they graduate. We do not use earnings from the year of their graduation because most individuals would be in school for some time during the said calendar year. As discussed above, we drop observations for the years when workers have either low earnings or are unemployed because we restrict our sample to workers with earnings above the SGA threshold.

<sup>19</sup>Arithmetic and word tests mirror respective tests in the Wechsler Adult Intelligence Scale, and the figures test is comparable to the Raven Progressive Matrix test, see Sundet, Barlaug and Torjussen (2004), and Thrane (1977).

<sup>20</sup>Public education in Norway is meritocratic (Kirkeboen, Leuven and Mogstad, 2016). Students with higher GPAs in high school are more likely to select into fields with high demand and have higher IQ scores.



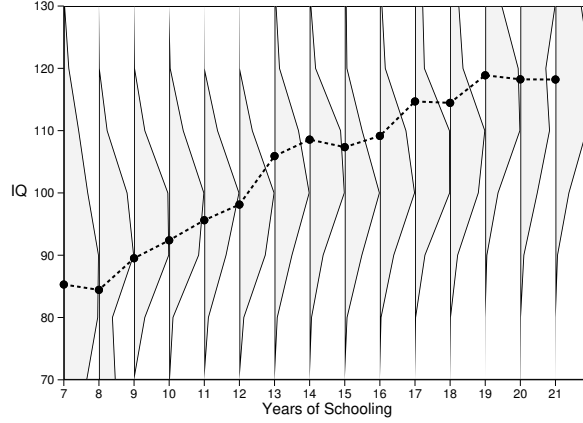


Figure 1. : Conditional Probability Density of IQ Test Scores on Schooling.

*Note:* The sample consists of Norwegian males born 1950-1980 observed in earnings data over the years 1967-2014 with years of potential experience between 0 and 30 years with annual earnings above 1 SGA threshold ( $N=14,746,755$ ). The IQ test score along the y-axis is the residual from a regression of the raw IQ test score on birth cohort dummies, removing secular changes over time, and further standardized to have a mean of 100 and a standard deviation of 15. The black dotted line plots the average IQ test score by individuals' years of schooling, while the shaded areas plot the conditional probability density of IQ.

### C. The Compulsory Schooling Reform

Between 1960 and 1975, Norway enacted a school reform that increased the minimum required schooling from 7 to 9 years. Different municipalities, which are the most decentralized local administration, implemented this reform in different years. Thus, for more than a decade, the length of compulsory schooling required of Norwegian children depended on their birth year and municipality of residence at age 14, henceforth, the childhood municipality. We use the timing differences across municipalities induced by the reform's staggered implementation as an IV for school years. For more on this reform, see Black, Devereux and Salvanes (2005).<sup>21</sup>

For 672 out of the 732 municipalities in 1960, we can date the reform's imple-

<sup>21</sup>This compulsory schooling reform in Norway has been used previously, albeit in different contexts, by Monstad, Propper and Salvanes (2008); Aakvik, Salvanes and Vaage (2010); Machin, Salvanes and Pelkonen (2012), and Bhuller, Mogstad and Salvanes (2017). However, besides increasing the minimum required schooling, the law also standardized curricula to improve schooling quality, as noted by Black, Devereux and Salvanes (2005). This change in quality may raise some concern about whether an IV based on this reform satisfies the exclusion restriction (Assumption 1-(1)). Although we do not show these results for brevity, our model can incorporate this effect and provide a sharp prediction about the bias. In particular, suppose we assume that the causal effect of  $D$  on  $A$  is linear and additively separable, i.e.,  $A_i = \delta^{A|D} D_i + \tilde{A}_i$ , such that  $\delta^{A|D} > 0$  is the effect of the reform on school quality (in terms of log-productivity). Then we can show that the estimates of the private and social returns have the same bias,  $\lambda_t(\delta^{A|D}/\varkappa)$ , where  $\varkappa > 0$  is the effect of the reform on years of schooling. As the difference between the private and the social return equals the signaling value, the bias term would cancel out, and the signaling value estimate would remain unchanged despite a violation of the exclusion restriction.

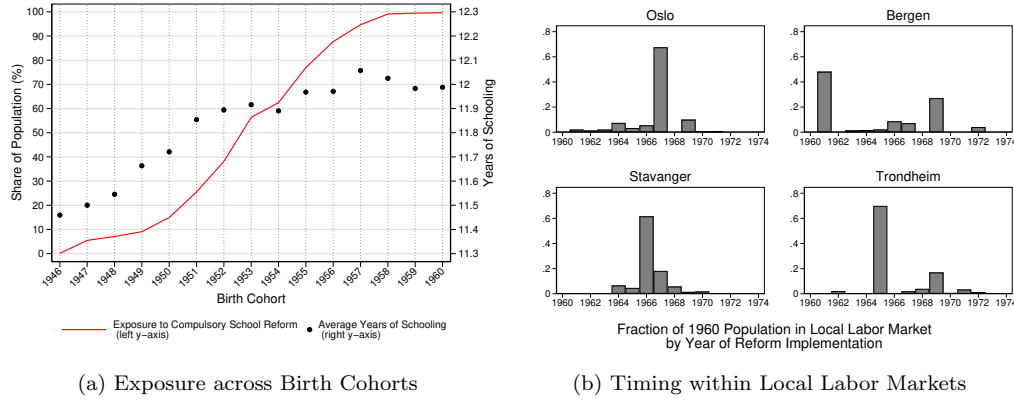


Figure 2. : Compulsory School Reform Across Birth Cohorts and Local Markets.

*Note:* The red line in plot (a) shows the cohort-specific share of population exposed to the compulsory school reform, while the black dots indicate the average years of schooling for Norwegian male cohorts born 1946-1960. Plot (b) shows the fraction of 1960 population in the four biggest local labor markets (concentrated around the four major cities) by the year of reform implementation. Using the 1960 classification of municipalities, there were 40 municipalities in the Oslo region, 27 municipalities in the Trondheim region, and 25 municipalities each in the Bergen and Stavanger regions. The variation in the timing of reform within local labor markets (LLMs) is due to variation in the timing of reform across municipalities within LLMs.

mentation using historical records. This information is missing for the remaining 60 municipalities Monstad, Propper and Salvanes (2008). As shown in Figure 2, there is considerable variation in the fraction of birth cohort exposed to the reform (Figure 2-(a)) and in the timing of reform even within local labor markets (Figure 2-(b)). In particular, panel (a) shows that nobody born before 1946 and everybody born after 1960 were required to attend school for nine rather than seven years.

Figure 2-(b) shows that there is considerable variation even within the four largest local labor markets (the four most significant metropolitan areas in Norway). For instance, Oslo city's municipality, which in 1960 accounted for two-thirds of the Oslo labor market region population, implemented the reform in 1967. In contrast, the reform's timing varied between 1961 and 1971 across the remaining population living in one of the other 39 municipalities.<sup>22</sup>

As shown in Section II, to separately identify the private and the social returns to education in a model with experience-invariant returns to skill, the instrument should satisfy the standard IV assumption (Assumption 1) and also be a hidden instrument (Assumption 2). For an instrument to be called a hidden instrument in our setting, it must mean that employers are not informed about the interaction between a worker's birth cohort and the timing of compulsory school reform in

<sup>22</sup>We use the classification of Norway into 160 local labor markets based on geographic commuting patterns constructed by Gundersen and Juvkam (2013). On average, each market has five municipalities.

the worker's childhood municipality. On the contrary, for an instrument to be transparent (Assumption 3), the employer must be informed about the worker's birth cohort and the timing of reform in the worker's childhood municipality.

While the information on a worker's birth cohort and residence is readily available for the employer (e.g., from worker's résumé), the employer may not know the year when the law was implemented in the worker's childhood municipality. In contrast to compulsory schooling laws legislated centrally in many countries or by the states in the U.S., the Norwegian compulsory school reform was decentralized and was implemented at the local municipal level. As indicated by Figure 2-(b), this decentralized implementation leads to substantial variation in the reform's timing across municipalities. These features related to the implementation of the reform, combined with the structure of local labor markets in Norway, provide exogenous variation in schooling that can be utilized either as a hidden or a transparent instrument.

On the one hand, the substantial variation in reform timing illustrated in Figure 2 implies that retrieving information on reform exposure for each applicant would be onerous and costly for the employers. Information on the timing of compulsory schooling reform for all municipalities was, until recently, not readily available in any public databases. Therefore, for the 1946-1960 cohorts, graduating in an era long before the Internet, this information would not have been easily traceable for employers. These institutional features suggest that it is reasonable to assume that exposure to the reform can serve as a hidden IV.

On the other hand, employers have incentives to remain informed about significant changes to their local labor market's education system. When major cities or population centers implemented such schooling reforms, newspapers and other media were more likely to report, and employers more likely to learn. We thus believe it is more plausible that compulsory schooling regulations pertaining to the residents of the central municipalities were understood and priced into wages by employers. Based on this reasoning, we construct a plausibly hidden instrument and a plausibly transparent instrument by partitioning our analytical sample into two, based on the relative size of workers' childhood municipality in their local labor markets.

To construct a transparent instrument, we include workers who grew up in population centers, defined as municipalities with the largest population in each local labor market, and assume that employers are informed about their reform exposure status. We refer to this sample as the *transparent IV sample*. It includes 295,488 individuals and 6,048,776 person-year observations, i.e., 41% of the entire sample.

Similarly, to construct a hidden instrument, we include workers who grew up in non-central municipalities, defined as the complement of central municipalities in each local labor market. We assume that employers are uninformed about these workers' reform exposure status. We refer to this sample as the *hidden IV sample*, and it includes 422,749 individuals and 8,697,979 person-year observations, i.e.,

59% of the entire sample.

By relying on central and non-central municipalities to construct transparent and hidden IVs, respectively, we assume that (1) firms everywhere can verify a worker’s education level, (2) firms know when the most populous municipality in the local labor market implemented a compulsory schooling reform, and (3) firms do not know when compulsory schooling reforms were implemented in the remaining municipalities in the local labor market. As illustrated in Figure 2 the non-central municipalities are typically very small, and the timing of the reform varies widely across them. Furthermore, the timing of compulsory schooling reform across municipalities was not readily available in online databases until recently. These features support our assumptions. It would have been onerous and costly for the employers to retrieve the information on the timing of the reforms for employees stemming from small municipalities.

Finally, as discussed in Section II.C, to identify the model with experience-varying returns to skill, we also need to maintain homogeneity of experience-varying skill returns across samples (Assumption 4). In our context, this implies that the dynamic path of skill returns is identical across individuals who grew up in central and non-central municipalities. One justification for this assumption is that there existed substantial “leaving the nest” migration in early adulthood such that while the childhood municipality in which an individual grew does determine whether an individual was exposed to the compulsory schooling reform, this does not predict the labor market conditions that the individual faced as an adult. Indeed, we find that almost 47% of people had migrated out of their childhood municipality by age 30. Furthermore, the vast majority of these migrated from non-central to central municipalities. These features may imply that the assumption of homogeneity of experience-varying skill returns across those who grew up in central and non-central municipalities could be reasonable in our context. Naturally, the plausibility of this assumption should be assessed in the particular context considered.

#### D. Empirical Specifications

Our empirical approach estimates the IV specification using 2SLS. In particular, at each experience  $t$ , the second stage projects log-earnings ( $\ln W_{it}$ ) on control variables ( $X_i$ ) as well as the projected schooling ( $\hat{S}_i$ ) from the first stage, i.e.,

$$(15) \quad \ln W_{it} = a_t^{IV^\tau} + b_t^{IV^\tau} \hat{S}_i + e_t^{IV^\tau} X_i + u_{it}^{IV^\tau}.$$

By estimating (15) separately for each  $t$  and  $\tau$ , we obtain parameters that vary freely across experience and samples. The control variables include a full set of dummies for birth cohort and childhood municipality, and  $\tau \in \{\mathfrak{h}, \mathfrak{t}\}$  indicates whether we use the hidden IV or the transparent IV sample. The first-stage

equation is

$$(16) \quad S_i = o^{\tau} + \varkappa^{\tau} D_i^{\tau} + n^{\tau} X_i + v_i^{\tau},$$

where the binary instrument  $D_i^{\tau} \in \{0, 1\}$  is equal to 1 if  $i$  was “exposed” to compulsory schooling reform and 0 otherwise. We record individual  $i$  as exposed if the reform was implemented in  $i$ ’s childhood municipality when  $i$  was 14 years old or younger.<sup>23</sup>

We use the childhood municipality indicators to control for unobservable determinants of earnings or schooling fixed at the municipality level. By adding these indicators, we can compare individuals who grew up in the same municipality but were exposed to different compulsory schooling requirements. Thus, we exploit variation in their schooling stemming from differential exposure to the compulsory schooling law. By including birth cohort indicators, we control for aggregate changes in schooling and earnings across cohorts.

Our parameters of interest are  $\{b_t^{IV^h}, b_t^{IV^t}\}$ , the coefficients on schooling at experience  $t = 0, 1, \dots$ , resulting from the hidden IV and the transparent IV, respectively. We maintain the assumptions that (i) conditional on  $X$ , both instruments  $\{D^h, D^t\}$  satisfy Assumption 1, and as discussed in Section III.C, that in our setting (ii)  $D^h$  satisfies the hidden IV Assumption 2 and  $D^t$  satisfies the transparent IV Assumption 3.<sup>24</sup>

As discussed in Section II.B, under experience-invariant returns to skill and maintaining Assumption 2,  $\hat{b}_t^{IV^h}$  converges to the experience-invariant social return to education,  $\delta^{\psi|S}$ , as  $t \rightarrow \infty$ , and moreover,  $\hat{b}_t^{IV^h}$  provides a consistent estimate of the private return at  $t$ ,  $\delta_t^{W|S}$ , for any  $t < \infty$ . Thus, we can estimate the social returns to education as  $\hat{b}_{t \rightarrow \infty}^{IV^h}$ , and use the rate at which  $\hat{b}_t^{IV^h}$  converges to  $\hat{b}_{\infty}^{IV^h}$  to estimate the speed of learning,  $\kappa$ .

For the model with experience-varying returns to skill and maintaining Assumption 2 on  $D^h$  and Assumption 3 on  $D^t$ , we can recover the private returns  $\delta_t^{W|S}$  for the hidden IV sample from  $\hat{b}_t^{IV^h}$  and the experience-varying social returns  $\delta_t^{\psi|S}$  for the transparent IV sample from  $\hat{b}_t^{IV^t}$ . As discussed in Section II.C, using  $\hat{b}_t^{IV^t}$ , we can also recover the experience-varying skill component  $\hat{\lambda}_t$  under location normalization  $\lambda_0 = 1$ . And, finally, maintaining Assumption 4 of homogeneity of  $\lambda_t$  across the two samples, we can infer the speed of employer learning,  $\kappa$ , and the experience-invariant part of the social returns  $\delta^{\psi|S}$  for the hidden IV sample.

A challenge to identifying returns to schooling based on (15) is that individuals

<sup>23</sup>Unlike (15), the coefficient  $\varkappa^{\tau}$  of the instrument  $D^{\tau}$  in the first-stage equation do not change with  $t$  because  $D^{\tau}$  and  $S$  are experience-invariant. However, with an unbalanced panel and separate estimations by experience, the first-stage estimates of  $\varkappa^{\tau}$  can vary with  $t$ . In practice, estimates of  $\varkappa^{\tau}$  are stable across the experience range that we consider despite differences in the sample composition by experience.

<sup>24</sup>The reform timing is also uncorrelated with baseline municipality characteristics (Bhuller, Mogstad and Salvanes, 2017).

who grew up in different municipalities could have had different growth rates in schooling and earnings even in the absence of compulsory schooling reform. Following Bhuller, Mogstad and Salvanes (2017), we also test the stability of our first-stage and IV estimates to the inclusion of extrapolated linear and quadratic municipality-specific trends in educational attainment estimated using data on pre-reform cohorts as additional controls. We refer to estimates based on (15)–(16) as obtained from the baseline specification and estimates that we get after controlling for municipality-specific trends.

Finally, note that by estimating (15) separately for each  $t$ , we also allow the work experience to interact with observable characteristics  $X_i$ , further weakening the functional form assumption embedded in (1). In particular, in (15) we have specified  $H(t, X_i) = a_t^{IV^r} + e_t^{IV^r} X_i$ , where coefficients  $a_t^{IV^r}$  and  $e_t^{IV^r}$  can vary flexibly by experience, and thus flexibly capture both a common experience profile and its interactions with  $X_i$ .

#### IV. Empirical Results

This section contains our main empirical results, beginning with IV estimates by work experience for both the hidden and the transparent IV samples. These allow us to identify both the private and the social returns to education. We use these to quantify the contribution of signaling to the returns to education before we provide results from sensitivity analyses.

##### A. IV Estimates of the Returns to Education

In Table 1 column (1), we display the estimates of (16)—the first-stage effect of our compulsory schooling reform instrument on years of schooling—for the entire sample. This estimate indicates that exposure to compulsory schooling reform increased completed schooling by 0.237 years. Columns (2) and (3) show the same first-stage estimates for the hidden IV and the transparent IV samples, respectively. All three first-stage coefficients are of similar magnitude and precisely estimated, confirming that the IV relevance condition is satisfied in all samples and that weak IV bias is not a concern for our analysis.

We now turn our attention to the second-stage IV estimates of (15) for the hidden IV sample. Figure 3-(a) displays the hidden IV estimates at different work experiences. Under the assumption that this IV is hidden from the employers, this provides consistent estimates of private returns to schooling at each year of work experience. The estimates suggest initially high private returns to schooling, followed by a steep decline during the first five years of work. Then, the returns gradually stabilize and approach 5.5% for those with 15 or more years of work experience.

Next, we turn to the transparent IV estimates of (15). These estimates are displayed in Figure 3-(b), and they correspond to the social returns to schooling across the experience. In contrast to the hidden IV estimates in Figure 3-(a), the

Table 1—: First-Stage Estimates on Years of Schooling.

	Full Sample	Hidden IV Sample	Transparent IV Sample
	(1)	(2)	(3)
Instrument:			
<i>Exposure to Compulsory Schooling Reform</i>	0.237 <sup>***</sup> (0.025)	0.228 <sup>***</sup> (0.034)	0.240 <sup>***</sup> (0.032)
Controls:			
Municipality Fixed Effects	✓	✓	✓
Cohort Fixed Effects	✓	✓	✓
F-statistic (instrument)	87.7	45.7	55.5
Sample Mean Years of Schooling	12.36	12.27	12.50
Standard Deviation Years of Schooling	2.50	2.46	2.56
Number of Observations	14,746,755	8,697,979	6,048,776

*Note:* The full sample (column 1) consists of Norwegian males born in 1950-1980 observed any time in earnings data over 1967-2014 with years of potential experience between 0 and 30 years and annual earnings above 1 SGA threshold (N=14,746,755). The hidden IV sample (column 2) further drops individuals who grew up in the municipality with the largest population size in each of the 160 labor market regions in Norway (N=8,697,979), while the transparent IV sample (column 3) retains only individuals who grew up in the municipality with the largest population size in each labor market (N=6,048,776). All estimations include fixed effects for birth cohort and childhood municipality. We cluster the standard errors at the local labor market region. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

transparent IV estimates do not decline with experience and are generally flatter across work experience. These estimates are indeed consistent with constant social returns, and, in turn, we fail to reject the standard model of employer learning with experience-invariant returns to skill.<sup>25</sup>

The transparent IV estimates in Figure 3-(b) imply that the average social return (across experience) is 8.8%. This social return is higher than the long-term value that the hidden IV estimates, as shown in Figure 3-(a) converge towards, indicating that the experience-invariant part of the social returns is not homogeneous across the transparent IV and the hidden IV samples. While we do not explore the sources of such heterogeneity in social returns, our identification approach allows differences in the experience-invariant part of the social returns across workers from central and non-central locations.<sup>26</sup>

<sup>25</sup>Formally, we use  $\hat{\lambda}_t = (\hat{b}_t^{IV^t} / \hat{b}_0^{IV^t})$ , and test the hypothesis that for every  $t \in \mathbb{T}_{>0}$  the ratio  $\hat{\lambda}_t = 1$ . The F-statistic from this joint test is 0.9, which means we cannot statistically reject the hypothesis of constant social returns. The  $\hat{\lambda}_t$  estimates and the associated 90% confidence intervals are presented in the Online Appendix, Figure A.1. By comparison, the F-statistic for the joint test of constant hidden IV estimates is 3.3, and we can easily reject the hypothesis that the private returns are constant over experience.

<sup>26</sup>Notably, Assumption 4 requires that the *experience-varying* component of returns to skill is identical across samples. However, this assumption does not restrict the *experience-invariant* component of social

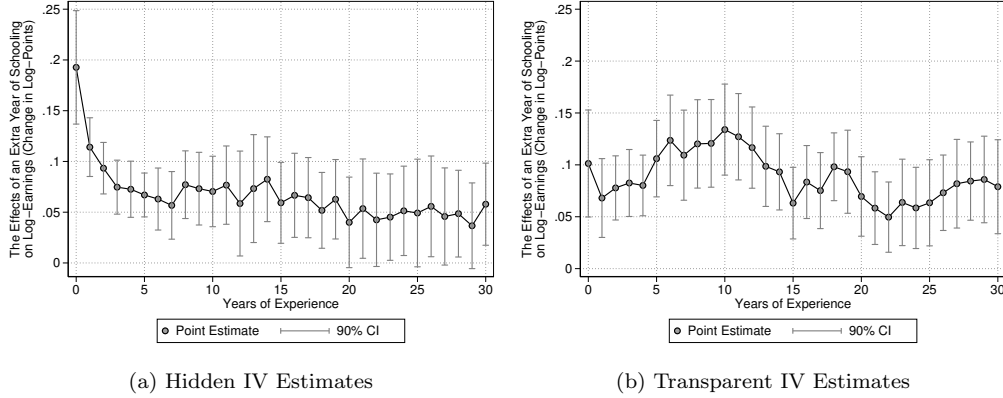


Figure 3. : Hidden and Transparent IV Estimates of the Returns to Schooling.

*Note:* The estimation sample consists of Norwegian males born 1950-1980 observed in earnings data over years 1967-2014 with years of potential experience between 0 and 30 years and annual earnings above 1 SGA threshold ( $N=14,746,755$ ). The hidden IV sample (plot (a)) further drops individuals who grew up in the municipality with the largest population size in each of the 160 labor market regions in Norway ( $N=8,697,979$ ), while the transparent IV sample (plot (b)) retains only individuals who grew up in the municipality with the largest population size in each labor market ( $N=6,048,776$ ). Plots (a) and (b) display IV estimates from separate estimations of (15) for each year of experience using the hidden and the transparent IV samples. All estimations include fixed effects for birth cohort and childhood municipality. Standard errors are clustered at the local labor market region. Vertical bars denote the 90% confidence intervals.

### B. The Speed of Learning and the Signaling Value of Education

We now use the hidden IV estimates and the transparent IV estimates, shown in Figure 3 to determine the private and social returns to schooling, the speed of employer learning, and the signaling value of education. We first present estimates that allow for experience-varying returns to skill, constructed by combining estimates from the hidden and the transparent IVs, as discussed in Section II.C. We report the NLLS estimates in Table 2 columns (1)-(2), based on sequential (i.e., estimate (11) before (10)) and joint estimations (i.e., estimate (11) and (10) jointly), respectively. Consistent with Figure 3-(a), in column (1), we estimate an initial private return ( $b_0^{IV^h}$ ) of 19.8%, a limit return ( $b_\infty^{IV^h}$ ) of 5.5% and a speed of learning ( $\kappa$ ) estimate of 0.505, which implies rapid learning on the part of employers. In particular, this estimate of  $\kappa$  implies that after the first five years of employment, employers put 16.4% weight on the initial signal they received from workers. As we can see from panel B, column (1), after 15 years, this weight declines further to 6.1%. Comparing across columns (1)-(2), we find that our estimates are robust to whether we sequentially or jointly estimate (10)-(11).

returns. Such heterogeneity in social returns could, for instance, exist due to differences in inputs factor in the production of human capital during childhood across workers who grew up in central and non-central locations (e.g., child-care access, parental resources, see discussions in Cunha and Heckman (2007)).



Table 2—: IV Estimates of the Speed of Employer Learning, and Initial and Limit Returns to Schooling.

Model Specifications:	Experience-Varying Skill Returns		Experience-Invariant Skill Returns
	<i>Sequential</i>	<i>Joint</i>	
	<i>Estimation</i>	<i>Estimation</i>	
	(1)	(2)	(3)
A. Parameters of Interest:			
Speed of Learning $\kappa$	0.505 <sup>***</sup> (0.107)	0.550 <sup>***</sup> (0.126)	0.532 <sup>***</sup> (0.058)
Initial Return $b_0^{IV^h}$	0.198 <sup>***</sup> (0.015)	0.195 <sup>***</sup> (0.012)	0.192 <sup>***</sup> (0.010)
Limit Return $b_\infty^{IV^h}$	0.055 <sup>***</sup> (0.006)	0.055 <sup>***</sup> (0.008)	0.050 <sup>***</sup> (0.003)
Average Return $b^{IV^h}$	0.064 <sup>***</sup> (0.006)	0.066 <sup>***</sup> (0.004)	0.067 <sup>***</sup> (0.005)
Average Return $b^{IV^t}$	0.088 <sup>***</sup> (0.004)	0.089 <sup>***</sup> (0.002)	—
B. Weight on Initial Signal:			
$\theta_t$ at $t = 5$	16.4%	14.1%	15.0%
$\theta_t$ at $t = 10$	8.9%	7.6%	8.1%
$\theta_t$ at $t = 15$	6.1%	5.2%	5.5%
C. Internal Rate of Return:			
Private IRR	0.079	0.076	0.072
Social IRR	0.054	0.055	0.050
Signaling Value	31.6%	27.6%	30.6%
D. Present Discounted Value:			
Private PDV	438,650	451,254	447,791
Social PDV	290,321	314,690	297,338
Signaling Value	33.8%	30.3%	33.6%

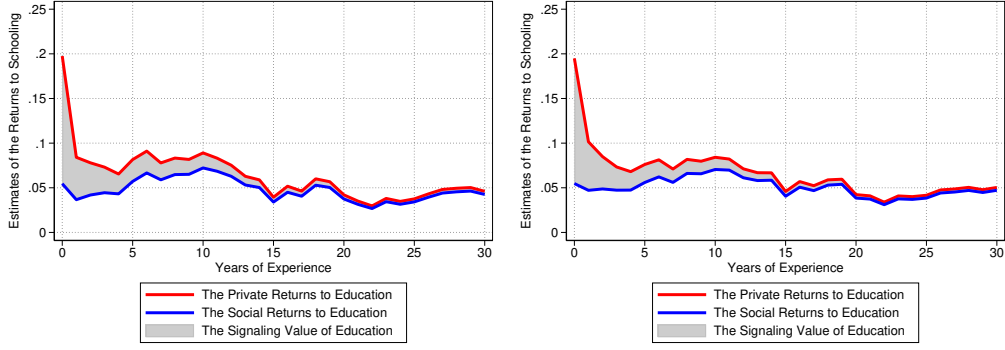
*Note:* The estimation sample consists of Norwegian males born 1950-1980 observed in earnings data over years 1967-2014 with years of potential experience between 0 and 30 years and annual earnings above 1 SGA threshold (N=14,746,755), partitioned in a hidden IV sample (N=8,697,979) and a transparent IV sample (N=6,048,776) as discussed in Section III.C. Panel A provides estimates of key model parameters for different model specifications. Columns (1)-(2) provide parameter estimates for the specification with experience-varying returns to skill, which combine the hidden, and the transparent IV estimates plotted in Figure 3(a)-(b). In the sequential estimation approach (column (1)), we first estimate the  $\hat{\lambda}_t$  profile based on the transparent IV estimates using (11) under location normalization  $\lambda_0 = 1$ , and then insert  $\hat{\lambda}_t$  in (10) and solve for other model parameters using the non-linear least-squares estimation. In the joint estimation approach (column (3)), we jointly solve for all model parameters and  $\lambda_t$  using (10)-(11) by non-linear least-squares estimation. Column (3) provides parameter estimates for the specification with experience-invariant returns to skill, which are based solely on the hidden IV estimates plotted in Figure 3(a) and constructed using non-linear least-squares estimation of (10) with  $\lambda_t = 1$ . Panel B provides the implied weight  $\theta_t$  on the initial signal corresponding to each estimate of the speed of employer learning  $\kappa$ . Using the estimates reported in Panel A for each model specification, Panel C provides the implied internal rates of private and social returns (IRR), while Panel D provides the implied present discounted value (PDV) of the lifetime earnings return from an additional year of schooling. The PDV estimates are reported in 2015 Norwegian Kroner (1USD  $\approx$  8NOK) and are calculated using an interest rate of 2.3%, which corresponds to the average real interest rate on deposits and loans in Norway over the period 1967-2010 (Bhuller, Mogstad and Salvanes, 2017). \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

In addition to estimates of our key model parameters ( $b_0^{IV^h}, b_\infty^{IV^h}, \kappa$ ), we also report the average return ( $b^{IV^t}$ ) implied by the transparent IV estimates, and similarly, the average return ( $b^{IV^h}$ ) implied by the hidden IV estimates across years of experience 0 to 30. The average return for the transparent IV sample is estimated to be 8.8%, compared to an average return of 6.4% and a limit return of 5.5% for the hidden IV sample.

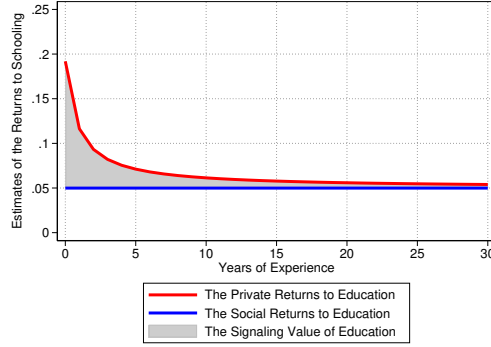
Next, we present estimates under the assumption of experience-invariant returns to skill following the results in Section II.B. In Table 2, panel A, column (3), we display the estimates of initial private returns ( $b_0^{IV^h}$ ), limit returns ( $b_\infty^{IV^h}$ ) and the speed of learning ( $\kappa$ ) obtained solely from the hidden IV estimates shown in Figure 3-(a) based on NLLS estimation. In this case, we estimate an initial return of 19.2%, a limit return of 5%, and a speed of learning estimate of 0.532, which again implies rapid learning on the part of employers. Comparing columns (1)-(3), we find that imposing experience-invariant returns to skill does not materially change our conclusions regarding the speed of learning and the evolution of returns. This finding is consistent with the estimates in Figure 3-(b), based on which we could not statistically reject the null hypothesis of constant social returns across the experience.

Using the estimates in Table 2, we now determine the signaling value of education under different model specifications. We proceed by first graphically illustrating the private and the social returns at each  $t$  that are implied by each set of model parameters presented in Table 2. In Figure 4-(a) we display the private returns (red line) and social returns (blue line) based on the estimates in Table 2, column (1), from a sequential estimation approach. In Figure 4-(b), we similarly display the returns estimates in Table 2, column (2), from a joint estimation. Both of these estimates allow experience-varying returns to skill ( $\hat{\lambda}_t$ ) based on the point estimates presented in the Online Appendix, Figure A.1. By comparison, in Figure 4-(c), we display the returns estimates from Table 2, column (3), where we impose constant returns to skill. In each sub-figure, we also provide the signaling value of education at each  $t$ , illustrated by the gap between the implied private and social returns to education.

Figure 4 highlights that a sizable fraction of the private returns that accrue during the early stages of workers' careers are attributable to signaling. To provide a summary measure of the contribution of signaling for the private returns to education over the workers' careers, we calculate the *internal rate of return* (IRR) for an additional year of schooling implied by our estimates. Following Heckman, Lochner and Todd (2006), we define IRR as the discount rate that equalizes the present discounted value of earnings over the career for different choices of schooling. The IRR captures that schooling benefits that accrue early in workers' careers are more valuable. Even with fast employer learning, one could therefore expect a sizable signaling value as the signaling returns occur primarily early in



(a) Experience-Varying Returns to Skill (Sequential) (b) Experience-Varying Returns to Skill (Joint)



(c) Experience-Invariant Returns to Skill

Figure 4. : Private Returns, Social Returns and Signaling Value of Education.

*Note:* The private and social returns plotted in the red and the blue lines, respectively, are constructed using the estimates in Table 2, columns (1)–(3). The difference between the private and social returns provides the signaling value of education.

workers' careers.<sup>27</sup>

Using the experience-specific returns estimates in Figure 4-(a), we estimate the private IRR to be 7.9%, see Table 2 panel C, column (1). Thus the private IRR is 2.5 percentage points greater than the social returns at 5.4%. Based on these estimates, we attribute 70% of the private return to education, raising workers' productivity and 30% to signaling. In columns (2)-(3), we perform the same calculation based on the estimates of the returns in Figure 4, (b)-(c), and again

<sup>27</sup>Formally, the private IRR  $r$  solves  $\sum_{t=0}^T \frac{\hat{W}(S+1,t)}{(1+r)^{t+1}} - \sum_{t=0}^T \frac{\hat{W}(S,t)}{(1+r)^t} = 0$ , where, for simplicity, we set  $\hat{W}(S+1,t) = \hat{W}(S,t)(1 + b_t^{IV^h}) = \bar{W}(t)(1 + b_t^{IV^h})$ ,  $\bar{W}(t)$  denotes average earnings for male workers at experience  $t$ , and we use a fixed length of work career  $T = 40$ . In this calculation, we rely solely on the opportunity cost of schooling to compute the IRR, and abstract from tuition and psychic costs of schooling and any earnings while in school. For  $t > 31$ , we assume that  $\theta_t = 0$  and  $\lambda_t = 1$ .

find a signaling component of a similar magnitude.

As an alternative way to measure the private and social returns, we show in Table 2 panel D the present discounted value (PDV) of lifetime returns to an additional year of schooling. As for using the IRR, a comparison of these PDVs estimates also leads us to conclude that about a third of the private returns are attributable to signaling.<sup>28</sup>

### C. Robustness and Additional Results

In this subsection, we assess whether our findings are robust to allowing for differential trends and using different sample selection criteria. Additionally, we also briefly discuss estimation results following Lange (2007) using the workers' IQ as a hidden correlate of the ability.

#### DIFFERENTIAL TRENDS.

As discussed in Section III.D, differential trends across individuals who grew up in different municipalities, even in the absence of the compulsory schooling reform, pose a challenge to identifying returns to schooling based on (15). Following Bhuller, Mogstad and Salvanes (2017), we verify that our estimates do not change when we include extrapolated linear and quadratic municipality-specific trends. The results are presented in the Online Appendix Tables A.1 and A.2. We can see that both the first and the second-stage IV estimates of key model parameters are robust to the inclusion of differential trends.<sup>29</sup> Similarly, from Figure A.2, we can see that the experience-specific IV estimates are also robust to differential trends.

#### SAMPLE SELECTION.

Next, we consider the sensitivity of our results to using alternative samples. As described in Section III.A, our estimation sample consists of male workers born 1950–1980, and we retain yearly observations for each of the years when a worker has annual earnings above the SGA threshold. By construction, this sample is unbalanced because our earnings data are limited to calendar years 1967–2014 (data availability) and because some individuals could have been unemployed or worked part-time in some years (partial employment). Due to data availability, the unbalancedness is more severe for younger cohorts as earnings towards the latter part of these workers' careers are not measured by 2014.

<sup>28</sup>To determine the PDV of private lifetime returns, we calculate  $\sum_{t=0}^T \frac{\hat{W}(S+1,t)}{(1+r^m)^{t+1}} - \sum_{t=0}^T \frac{\hat{W}(S,t)}{(1+r^m)^t}$ , where the discount rate  $r^m$  is set equal to the average real interest rate of 2.3% on deposits and loans in Norway from 1960 to 2010 (Bhuller, Mogstad and Salvanes, 2017).

<sup>29</sup>Indeed, based on the estimates in Table A.2 that allow for differential trends we cannot reject that the experience-invariant part of the social returns is equal across the transparent IV and the hidden IV samples.

To address concerns related to having an unbalanced sample in our baseline, we proceed in two ways. In the first case, we retain our baseline sample of male workers born 1950–1980 but use yearly observations of only up to 10 years of work experience in our estimation. In the second case, we restrict our sample to male workers born 1950–1965 and use observations up to 30 years of work experience, as in our primary analysis. In both of these alternative estimations, the sample is substantially more balanced over time.<sup>30</sup>

The results from these estimations are presented in the Online Appendix Table A.3, columns (2)-(3) and in Figure A.3-(b). These results show that our returns estimates are robust to using more balanced samples with fewer observations. Furthermore, in Table A.3, column (4), and Figure A.3-(c), we report estimation results when we extend our sample to male workers born 1940-1980.<sup>31</sup> Our findings are also robust to using this extended sample.

#### EARNINGS THRESHOLD.

In the absence of data on hourly wage rates for most of our sample period, in our baseline estimation, we limited the analysis to individuals with annual labor earnings above the substantial gainful activity (SGA) threshold. This restriction is likely to reduce variability in earnings due to differences in working hours across workers. In the Online Appendix Table A.3, columns (5)-(7) and Figure A.3-(d)-(f), we provide estimates where we vary the annual earnings threshold to be either 50%, 75% or 150% of the SGA threshold. These estimates again indicate that our findings are robust to using alternative earnings thresholds to select a sample of workers with little variation in working hours.

#### DEFINITION OF THE HIDDEN AND TRANSPARENT IV SAMPLES.

As discussed in Section III.C, to construct the hidden and transparent IVs, we rely on partitioning our sample into two subsamples, based on the relative size of workers' childhood municipality in their local labor markets. We assign workers who grew up in municipalities with the largest population in their respective local labor market to the transparent IV sample, while the remaining workers, those who grew up in non-central municipalities, are part of the hidden IV sample.

We now assess how sensitive our results are to these choices by changing the definition of the hidden IV sample (non-central municipalities) to be further limited by the number of residents living in each municipality at the onset of the

<sup>30</sup>In our baseline hidden IV sample of male workers born 1950–1980, we include 422,749 individuals and 8,697,979 person-year observations, among whom we observe earnings for 344,547 in the initial year of work experience, 355,602 in the 10<sup>th</sup> year, and 116,385 in the 30<sup>th</sup> year. In the sample of males born 1950–1965, we include 213,206 individuals and 5,326,336 person-year observations, among whom we observe earnings for 173,465 in the initial year of work experience, 179,027 in the 10<sup>th</sup> year, and 116,385 in the 30<sup>th</sup> year.

<sup>31</sup>The reason we restricted our baseline sample to birth cohorts born 1950–1980 was that the military IQ test scores are not available for those born before 1950. In Table A.3, column (4), we drop this requirement.

compulsory schooling reform in 1960. In the Online Appendix Table A.3, columns (8)-(10) and Figure A.3-(g)-(i), we provide estimates where we vary the population thresholds for being included as non-central municipalities to be either less than 5,000, 10,000 or 25,000 residents. These estimates again indicate that our findings are robust to using alternative sample definitions.

#### CAUSAL EFFECT OF EDUCATION ON IQ.

In other related studies, (e.g., Brinch and Galloway, 2012; Carlsson et al., 2015), researchers have provided causal estimates of education on measures of workers' ability. In a similar spirit, we regress the standardized IQ test score as a function of years of schooling, using the compulsory schooling reform as an instrument for schooling and present the results in Online Appendix Table A.4. As mentioned in Section III.B, we use an IQ test score from the Norwegian Armed Forces available for conscripts born 1950–1980. These IV estimates show a strong effect of schooling on IQ: an additional year of schooling at age 18 *causes* approximately 1/4 of a standard deviation increase in IQ.<sup>32</sup> Thus, schooling increases worker ability, which we expect to positively correlate with workers' productive skills. A back-of-the-envelope calculation suggests that the schooling-induced increase in IQ results in 3.2% higher earnings, which is almost 60% of the 5.4% social return estimate presented in Table 2, column (1).<sup>33</sup> While we are cautious in interpreting these estimates as “direct” evidence of a productivity return to education, they suggest that part of social returns to education in (3) could be attributed to the causal effect of education on workers' abilities.

### V. Extensions

#### A. Partially-Transparent Instrument

So far, we assumed that researchers could determine whether each individual worker was treated by a hidden or a transparent IV. We now consider an extension that accommodates the situation when researchers are unable to determine the type of IV each worker was exposed to. This is relevant, as in many settings, the distinction between hidden and transparent IVs can be blurry. To operationalize this idea, assume that an IV was transparent for a fraction  $\rho$  of workers, and

<sup>32</sup>The IQ test score is standardized to have a mean of zero and a standard deviation equal to one. Using the same compulsory schooling reform for Norway, Brinch and Galloway (2012) documented that an additional year of schooling increased IQ scores measured at age 19 by 3.7 points. Their IQ test score is scaled to have a mean of 100 and a standard deviation of 15. In terms of magnitude, their estimate also corresponds to 1/4 of a standard deviation increase in IQ. Carlsson et al. (2015) also document similar results for Sweden.

<sup>33</sup>For this calculation, we use the estimated 1/4 of a standard deviation increase in IQ from an additional year of schooling in the Online Appendix Table A.4, and an OLS regression of log-wages on IQ, controlling flexibly for experience, which shows that a standard deviation increase in IQ is associated with 12.8% higher earnings. For the purpose of this exercise, we interpret each coefficient estimates as causal, and conclude that a schooling-induced increase in IQ leads to 3.2% ( $\approx 12.8\% \times 1/4$ ) higher earnings.

hidden for the remaining fraction  $(1 - \rho)$ . Researchers do not know this fraction  $\rho$ , nor do they know the type of IV each worker was exposed to. In this situation, we say that we have access to a “partially-transparent” instrument. We summarize the main identification results in this setting and leave details to the Online Appendix B.

In this setting, the conditional expectation of log wages becomes a weighted average of the conditional expectation of log wages given hidden and transparent IVs, respectively. When the returns to skill are constant, a partially-transparent IV identifies a lower bound on the private returns to education. Nevertheless, as with the hidden IV, after long enough work experience, this lower bound converges to the social returns. We thus identify the speed of learning and can provide a lower bound for the signaling value. When the returns to skill vary with work experience, we can identify lower bounds on the private returns and the signaling value if we have access to a transparent *and* a partially-transparent IV. To point-identify all parameters, it suffices to have access to a hidden IV in addition to the partially transparent IV if we also strengthen Assumption 4 and instead assume that the social returns are the same across the two IV samples.

### B. Heterogeneous Returns

There is increasing evidence that returns to education are heterogeneous across workers, see Card (1999); Heckman, Lochner and Todd (2006) and Carneiro, Heckman and Vytlačil (2011), among others. So, it is important to understand how the heterogeneity affects our identification results. To this end, we consider heterogeneous returns in the Neyman-Rubin potential outcomes framework and study identification with a binary (low or high) schooling choice. We present all the details in the Online Appendix C, and for brevity, present a summary of our main findings.

In this environment, both the social returns to education and the private returns to education become worker-specific. So, we have to work with the averages across an appropriate subset of workers, which we refer to as the *average social returns* and the *average private returns*, respectively. We know that an IV identifies the local average treatment effect for those induced by the IV to switch their schooling status (Imbens and Angrist, 1994). In particular, if the IV satisfies an appropriate monotonicity condition, it identifies the treatment effect on the compliers, and hence, the causal effect of schooling on workers who obtain “low” schooling in the absence of the IV but obtain “high” schooling when “exposed” to the IV.

We contribute to this literature by showing that even with heterogeneous returns, a hidden IV identifies the average private returns to education among the compliers, and a transparent IV identifies the average social returns among the compliers. Under our maintained assumption of conditional normality of productivity given schooling, where the mean (but not the variance) of productivity depends on schooling, we can use the hidden IV to identify the speed of learning and the signaling value of education for compliers if the returns to skill are

experience-invariant. Moreover, if the returns to skill vary with experience, but if they are homogeneous across compliers of the hidden IV and the transparent IV (Assumption 4), then having access to the two IVs is again sufficient to identify the speed of learning.

## VI. Discussion

We have shown that interpreting the returns to education from IV estimates requires assumptions on what employers know (or do not know) about the instruments. We have done so within a tractable framework, setting aside important issues to convey our core message. In this section, we briefly touch upon a few of these issues, but our discussion here is brief and at best may serve as a marker of topics for future research.

### A. Asymmetric Employer Learning

Throughout our paper, we have assumed symmetric learning across employers. Symmetry in learning implies that all current and future employers have the same information about an employee. While there has been some empirical support in favor of symmetric learning, at least for employees with low education (Schönberg, 2007), there is also empirical evidence obtained from wage changes across jobs that incumbent employers have an information advantage (Gibbons and Katz, 1991; Kahn, 2013), which in turn can lead to the different speed of learning and different wage profiles; see Pinkston (2009). The learning process becomes significantly more complicated when the information is asymmetric across the employers and when workers and employers act strategically based on this asymmetry. Thus, it is challenging to determine implications for social and private returns in such settings.

### B. Market Power

We also assume that employers perfectly compete for workers, so that workers are paid their expected productivity based on the information available to employers. While convenient, this assumption is at odds with new research suggesting that employers have market power, see Manning (2021) for a review. However, such monopsony power in itself does not imply a systematic bias in our estimates of the social and private returns to education. When employers have market power, they will mark-down wages so that for the marginal worker, expected productivity exceeds the wage. Estimates of the returns to education are, however, based on relative wages across schooling levels. For instance, if the mark-downs are proportional to the expected productivity then they “cancel-out” when we estimate log-linear earnings equations.

This same argument does not apply when mark-downs of wages relative to the expected productivity vary systematically with education. Such patterns could result from differences across educational groups in the elasticity of labor supply



and workers' ability to capture rents. While there is little (if any) evidence on whether the elasticity of labor supply varies systematically with education, it is plausible that labor supply is less elastic for specialized labor, and education generally supports specialization. This inverse relation between education and elasticity suggests that mark-downs increase with education. On the other hand, more educated workers may bargain more effectively and have better abilities to compete for rents. This positive relation between education and bargaining power, in turn, suggests that mark-downs decrease with education. The relative strength of these (and other) factors and how they affect mark-downs across education is at present is not well understood.

### *C. Information and Assignment*

In our framework, the social returns to education are equal to the direct increase in workers' skills. However, information revealed during schooling might also affect the process of allocating workers to jobs in which they have a comparative advantage. If education improves workers' assignment to employers, this represents an additional channel through which education raises productivity by improving allocative efficiency. A key question then is to whom do these social returns accrue? In a perfectly competitive market, potential employers bid for employees' services based on workers' expected contribution. The highest bidder will employ the workers in their highest productivity job, however, the wages depend on the information revealed during assignment. Therefore it is also not clear to whom the social returns would accrue. More generally, while this mechanism is interesting and could potentially shed light on wage differentials across workers and firms, this mechanism is also not well understood.

### *D. General Equilibrium Effects*

Many instruments rely on large interventions that change educational attainment in the population. These interventions are likely to engender general equilibrium effects, because among other reasons, changes in educational attainment in the population imply movements along production isoquants. Besides, signaling model predicts that the equilibrium wage function changes if interventions change the population wide joint distribution of education and ability. Moreover, because the wage function affects schooling decisions, the signaling aspect of education generates additional feedback from the wage to education decisions.<sup>34</sup>

While we abstract from such equilibrium effects, they may affect our analysis. First, our analysis applies best to instruments that affect small population groups (relative to the economy) and are thus unlikely to generate equilibrium effects. Thus, the ideal setting for an empirical application along the lines of our approach would be an experiment that treats only a small subset of the population and

<sup>34</sup>See Lang and Kropp (1986) and Bedard (2001) for evidence on such feedback effects.

sidesteps the concern about equilibrium effects.<sup>35</sup>

Second, specific to our context, the compulsory schooling reform was gradually implemented throughout Norway between 1960 and 1975. As the reform increasingly applied to the whole country, it is possible that the wage function could have changed due to equilibrium effects. These changes might have interacted with experience so that the variation in the returns to education estimates across different experience levels might confound changes in the wage function over time. Accounting for such effects is beyond the scope of this paper.

## VII. Conclusion

Education policy hinges on the estimates of private and social returns to education, but these returns are difficult to estimate separately. In this paper, we determine conditions under which instruments allow us to separately identify the private and social returns to education within the context of an employer learning model.

We distinguish between two types of IVs: hidden IVs and transparent IVs, where the former are unobserved by employers and thus not directly priced in the wages, while the latter are observed by employers and correctly factored in the wages. We show that a hidden IV identifies the private returns to education. A transparent IV, by contrast, identifies the social returns to education at each level of work experience. If the returns to skill do not vary with experience, then a hidden IV identifies the private and the social returns to education as well as the speed of learning. When returns to skill do vary with experience, access to both a hidden and a transparent IV suffices to identify these parameters. Using data from Norway, we estimate a private internal rate of return to education of 7.9%, of which 70% is due to increased productivity and 30% to signaling. Our estimates also suggest that employers learn workers' abilities quickly.

We believe that IV estimates can provide useful information on the debate between proponents of the human capital and signaling models of the returns to education. To do so requires taking a stand on how informed employers in the market are about the instrument used for identification. We hope that future researchers reporting IV estimates of the returns to education will consider how informed employers are about the IVs and what this means for interpreting their estimates.

<sup>35</sup>In practice, estimating effects of education interacted with experience using IVs requires large datasets. A complementary approach is to use cross-country variation in educational attainment to identify social return to education, including any general equilibrium effects. For this approach to work, however, we need strong exogeneity assumptions. We refer the interested reader to Lange and Topel (2006).

## REFERENCES

- Aakvik, Arild, Kjell G. Salvanes, and Kjell Vaage.** 2010. "Measuring Heterogeneity in the Returns to Education in Norway Using Educational Reforms." *European Economic Review*, 54(4): 483–500.
- Acemoglu, Daron, and Joshua Angrist.** 2000. "How Large Are Human-Capital Externalities? Evidence from Compulsory Schooling Laws." *NBER Macroeconomics Annual*, 15: 9–59.
- Altonji, Joseph G., and Charles R. Pierret.** 2001. "Employer Learning and Statistical Discrimination." *Quarterly Journal of Economics*, 116(1): 313–350.
- Angrist, Joshua D., and Alan B. Krueger.** 1992. "Estimating the Payoff to Schooling Using the Vietnam-Era Draft Lottery." *NBER Working Paper 4067*.
- Angrist, Joshua D., Guido W. Imbens, and Donald B. Rubin.** 1996. "Identification of Causal Effects Using Instrumental Variables." *Journal of the American Statistical Association*, 91(434): 444–455.
- Arcidiacono, Peter, Patrick Bayer, and Aurel Hizmo.** 2010. "Beyond Signaling and Human Capital: Education and the Revelation of Ability." *American Economic Journal: Applied Economics*, 2(4): 76–104.
- Arteaga, Carolina.** 2018. "The Effect of Human Capital on Earnings: Evidence from a Reform in Columbia's Top University." *Journal of Public Economics*, 157: 212–225.
- Aryal, Gaurab, Manudeep Bhuller, and Fabian Lange.** 2021. "Employer Learning and Heterogeneity in Wage Profiles." *Working Paper*.
- Becker, Gary S.** 1962. "Investment in Human Capital: A Theoretical Analysis." *Journal of Political Economy*, 70(5): 9–49.
- Bedard, Kelly.** 2001. "Human Capital versus Signaling Models: University Access and High School Dropouts." *Journal of Political Economy*, 109(4): 749–775.
- Bhuller, Manudeep, Magne Mogstad, and Kjell G. Salvanes.** 2017. "Life-Cycle Earnings, Education Premiums, and Internal Rate of Return." *Journal of Labor Economics*, 35(4): 993–1030.
- Black, Sandra E., Paul J. Devereux, and Kjell G. Salvanes.** 2005. "Why the Apple Doesn't Fall Far: Understanding Intergenerational Transmission of Human Capital." *American Economic Review*, 95(1): 437–449.
- Brinch, Christian N., and Taryn A. Galloway.** 2012. "Schooling in Adolescence Raises IQ Scores." *Proceedings of the National Academy of Sciences of the United States of America*, 109(2): 425–430.

- Cameron, Stephen V., and Christopher R. Taber.** 2004. "Estimation of Educational Borrowing Constraints Using Returns to Schooling." *Journal of Political Economy*, 112(1): 132–182.
- Cameron, Stephen V., and James J. Heckman.** 1998. "Life Cycle Schooling and Dynamic Selection Bias: Models and Evidence for Five Cohorts of American Males." *Journal of Political Economy*, 106(2): 262–333.
- Card, David.** 1993. "Using Geographic Variation in College Proximity to Estimate the Return to Schooling." *NBER Working Paper 4483*.
- Card, David.** 1999. "The Causal Effect of Education on Earnings." In *Handbook of Labor Economics*. Vol. 3, , ed. Orley Ashenfelter and David Card, Chapter 30, 1801–1863. Elsevier.
- Carlsson, Magnus, Gordon B. Dahl, Björn Öckert, and Dan-Olof Rooth.** 2015. "The Effect of Schooling on Cognitive Skills." *Review of Economics and Statistics*, 97(3): 533–547.
- Carneiro, Pedro, James J. Heckman, and Edward J. Vytlacil.** 2011. "Estimating Marginal Returns to Education." *American Economic Review*, 101(6): 2754–2781.
- Clark, Damon, and Paco Martorell.** 2014. "The Signaling Value of a High School Diploma." *Journal of Political Economy*, 122(2): 282–318.
- Cunha, Flavio, and James Heckman.** 2007. "The Technology of Skill Formation." *American Economic Review*, 97(2): 31–47.
- Duflo, Esther.** 2001. "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment." *American Economic Review*, 91(4): 795–813.
- Dynarski, Susan M.** 2003. "Does Aid Matter? Measuring the Effect of Student Aid on College Attendance and Completion." *American Economic Review*, 93(1): 279–288.
- Fang, Hanming.** 2006. "Disentangling the College Wage Premium: Estimating a Model with Endogenous Education Choices." *International Economic Review*, 47(4): 1151–1185.
- Farber, Henry S., and Robert Gibbons.** 1996. "Learning and Wage Dynamics." *Quarterly Journal of Economics*, 111(4): 1007–1047.
- Feng, Andy, and Georg Graetz.** 2017. "A Question of Degree: The Effects of Degree Class on Labor Market Outcomes." *Economics of Education Review*, 61: 140–161.

- Gibbons, Robert, and Lawrence Katz.** 1991. "Layoffs and Lemons." *Journal of Labor Economics*, 9: 351–380.
- Gundersen, Frantz, and Dag Juvkam.** 2013. "Inndelinger i senterstruktur, sentralitet og BA-regioner." *NIBR - rapport 2013:1 (in Norwegian)*.
- Heckman, James J, Lance J Lochner, and Petra E Todd.** 2006. "Earnings Functions, Rates of Return and Treatment Effects: The Mincer Equation and Beyond." *Handbook of the Economics of Education*, 1: 307–458.
- Hopkins, Edward.** 2012. "Job Market Signaling of Relative Position, or Becker Married to Spence." *Journal of the European Economic Association*, 10(2): 290–322.
- Imbens, Guido W., and Joshua D. Angrist.** 1994. "Identification and Estimation of Local Average Treatment Effects." *Econometrica*, 62(2): 467–475.
- Kahn, Lisa B.** 2013. "Asymmetric Information between Employers." *American Economic Journal: Applied Economics*, 5(4): 165–205.
- Kane, Thomas J., and Cecilia Elena Rouse.** 1995. "Labor-Market Returns to Two- and Four-Year Colleges." *American Economic Review*, 85(3): 600–614.
- Kirkeboen, Lars J., Edwin Leuven, and Magne Mogstad.** 2016. "Field of Study, Earnings, and Self-Selection." *Quarterly Journal of Economics*, 131(3): 1057–1111.
- Kling, Jeffrey R.** 2001. "Interpreting Instrumental Variables Estimates of the Returns to Schooling." *Journal of Business and Economic Statistics*, 19(3): 358–364.
- Lange, Fabian.** 2007. "The Speed of Employer Learning." *Journal of Labor Economics*, 25(1): 1–35.
- Lange, Fabian, and Robert Topel.** 2006. "The Social Value of Education and Human Capital." *Handbook of the Economics of Education*, 1: 459–509.
- Lang, Kevin, and David Kropp.** 1986. "Human Capital versus Sorting: The Effects of Compulsory Attendance Laws." *Quarterly Journal of Economics*, 101(3): 609–24.
- Lemieux, Thomas, and David Card.** 2001. "Education, Earnings, and the 'Canadian G.I. Bill'." *Canadian Journal of Economics*, 34(2): 313–344.
- Lochner, Lance.** 2011. "Chapter 2 - Nonproduction Benefits of Education: Crime, Health, and Good Citizenship." In *Handbook of the Economics of Education*. Vol. 4, , ed. Eric A. Hanushek, Stephen Machin and Ludger Woessmann, 183–282. Elsevier.

- Machin, Stephen, Kjell G. Salvanes, and Panu Pelkonen.** 2012. "Education and Mobility." *Journal of the European Economic Association*, 10(2): 417–450.
- Manning, Alan.** 2021. "Monopsony in labor markets: a review." *ILR Review*, 74(1): 3–26.
- Meghir, Costas, and Mårten Palme.** 2005. "Educational Reform, Ability, and Family Background." *American Economic Review*, 95(1): 414–424.
- Monstad, Karin, Carol Propper, and Kjell G. Salvanes.** 2008. "Education and Fertility: Evidence from a Natural Experiment." *Scandinavian Journal of Economics*, 110(4): 827–852.
- Moretti, Enrico.** 2004. "Estimating the Social Return to Higher Education: Evidence from Longitudinal and Repeated Cross-Sectional Data." *Journal of Econometrics*, 121(1-2): 175–212.
- Oreopoulos, P.** 2006. "Estimating Average and Local Average Treatment Effects of Education when Compulsory Schooling Laws Really Matter." *American Economic Review*, 96: 152–175.
- Oreopoulos, Philip, and Kjell G. Salvanes.** 2011. "Priceless: The Nonpecuniary Benefits of Schooling." *Journal of Economic Perspectives*, 25(1): 159–184.
- Pinkston, Joshua C.** 2009. "A Model of Asymmetric Employer Learning with Testable Implications." *Review of Economic Studies*, 76(1): 367–394.
- Pons, Empar, and Maria Teresa Gonzalo.** 2002. "Returns to Schooling in Spain: How Reliable Are IV Estimates?" *Labour*, 16(4): 747–770.
- Schönberg, Uta.** 2007. "Testing for Asymmetric Employer Learning." *Journal of Labor Economics*, 25(4): 651–691.
- Spence, Michael.** 1973. "Job Market Signaling." *Quarterly Journal of Economics*, 87(3): 355–374.
- Stiglitz, Joseph E.** 1975. "The Theory of "Screening," Education, and the Distribution of Income." *American Economic Review*, 65(3): 283–300.
- Sundet, Jon Martin, Dag G. Barlaug, and Tore M. Torjussen.** 2004. "The End of the Flynn Effect? A Study of Secular Trends in Mean Intelligence Test Scores of Norwegian Conscripts During Half a Century." *Intelligence*, 32: 349–362.
- Taber, Christopher R.** 2001. "The Rising College Premium in the Eighties: Return to College or Return to Unobserved Ability?" *Review of Economic Studies*, 68(3): 665–691.

- Thrane, Vidkunn C.** 1977. "Evneprøving av utskrivingspliktige i Norge 1950-53." *Arbeidsrapport nr. 26, INAS (in Norwegian)*.
- Tyler, John H., Richard J. Murnane, and John B. Willett.** 2000. "Estimating the Labor Market Signaling Value of the GED." *Quarterly Journal of Economics*, 115(2): 431-468.
- Wolpin, Kenneth I.** 1977. "Education and Screening." *American Economic Review*, 67(5): 949-958.