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# Signaling and Employer Learning with Instruments

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# ABSTRACT

## Signaling and Employer Learning with Instruments*

The social and the private returns to education differ when education can increase productivity, and also be used to signal productivity. We show how instrumental variables can be used to separately identify and estimate the social and private returns to education within the employer learning framework of Farber and Gibbons [1996] and Altonji and Pierret [2001]. What an instrumental variable identifies depends crucially on whether the instrument is hidden from, or observed by, the employers. If the instrument is hidden then it identifies the private returns to education, but if the instrument is observed by employers then it identifies the social returns to education. Interestingly, however, among experienced workers the instrument identifies the social returns to education, regardless of whether or not it is hidden. We operationalize this approach using local variation in compulsory schooling laws across multiple cohorts in Norway. Our preferred estimates indicate that the social return to an additional year of education is 5%, and the private internal rate of return, aggregating the returns over the life-cycle, is 7.2%. Thus, 70% of the private returns to education can be attributed to education raising productivity and 30% to education signaling workers' ability.

**JEL Classification:** J24, J31, D83

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

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# 1 Introduction

Two competing models rationalize the positive relation between earnings and education that is universally found in data. Ever since Becker [1962], proponents of the human capital model argue that education increases skills that are valued by employers. By contrast, the job-market signaling model of Spence [1973] posits that education signals differences in innate abilities among workers. Signaling, however, is inherently inefficient because workers expend valuable resources just to signal their productivity. Thus, signaling creates a wedge between the private returns and the social returns to education.[1]

Education policy requires empirical guidance on how large this wedge is, which in turn requires us to identify the social returns and the private returns.[2] The difficulties in separating signaling from human capital models, however, have long been recognized in the literature; 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 employers might also update their beliefs, i.e., learn, about workers' abilities over time.

In two influential papers, Farber and Gibbons [1996] and Altonji and Pierret [2001] (henceforth FG and AP, respectively) derive testable predictions of employer learning for how earnings correlate with schooling and a proxy of unobserved ability at different points of the life-cycle. In particular, AP use the Armed Forces Qualification Test (AFQT) score in the NLSY1979 as a proxy for unobserved ability. They show that the variation in earnings with schooling and the AFQT is consistent with employers that statistically discriminate on the basis of schooling and learn about workers' unobserved skills over time. Following their lead, Lange [2007] shows that employers learn fast. Using the first-order condition

[1] There are other reasons why the private and social returns might differ from each other. For instance, there can be productive externalities beyond the employer-employee relationship [Acemoglu and Angrist, 2000; Moretti, 2004], or education might also entail various non-production and non-pecuniary benefits, e.g., reducing crime, improving public health [Lange and Topel, 2006; Lochner, 2011; Oreopoulos and Salvanes, 2011]. While we abstract from these aspects in this paper, comprehensive measures of social returns ideally should aim to account for such spillovers. More broadly, education policy might also be concerned with fiscal externalities, and be motivated by distributional impacts.

[2] For more on signaling and the human capital model, see Wolpin [1977]; Tyler et al. [2000]; Bedard [2001]; Fang [2006]; Hopkins [2012]; Clark and Martorell [2014]; Feng and Graetz [2017] and Arteaga [2018].

that characterizes schooling decisions, Lange [2007] identifies an upper bound on how much signaling contributes to the private returns to education over a life-cycle. Lange [2007] however is unable to point-estimate the private and the social returns to education and to arrive at his bound, he needs to rely on strong behavioral assumptions regarding the costs of education. The literature following AP thus relies heavily on the assumption that the AFQT score is a valid proxy of unobserved ability. Even with this assumption, this literature has so far only been able to deliver bounds on the private and social returns to education under untestable behavioral assumptions.

In this paper, we broaden the empirical basis of the employer learning literature 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 IV estimates of the causal effects of education on wages allow us to point-identify the private and the social returns to education. Unlike Lange [2007], our identification strategy does not rely on the first-order condition for schooling choices and thus does not require specifying the costs and benefits of schooling. And, unlike the extant literature that follows AP, our identification strategy does not require access to a correlate of ability that is unobserved by the employers.

To this end, we present several identification results. First, we show that any conventional IV estimate of the causal effect of education on wages, measured at sufficiently high level of work experience, identifies the causal effect of schooling on productivity. This implies that access to an IV and a repeated cross-section of wages across workers' careers are sufficient to identify the productivity effect of schooling. This interpretation of "long-run" IV estimates follows directly from a limit result in the employer learning model that wages eventually converge to the true productivity because employers eventually learn workers' productivities.

Our second set of results illustrate how central the assumptions about employers' knowledge of the instruments are for interpreting the IV estimates. For that we distinguish between a *hidden* IV and a *transparent* IV. We say that an IV is hidden if it is unobserved by employers, and that it is transparent if it is observed by the employers and priced into the wages.

If the IV is hidden then it identifies the private returns to education at each level of worker experience; and if the IV is transparent then it identifies the social returns to education. For our third result, we show how IV estimates measured at different levels of worker experience identify the speed at which employers learn about workers' productivity.

In summary, within the FG and AP framework of employer learning, a hidden IV is sufficient to (i) point-identify the relative contributions of human capital and signaling in the lifetime returns to education; and (ii) estimate the speed of learning. Any additional information – either because we have access to multiple instruments or because of the availability of an ability correlate like the AFQT – allows testing and/or relaxing functional form assumptions in the employer learning model.

We implement these ideas on a unique dataset consisting of all Norwegian males born between 1950 and 1980, with earnings histories between 1967 and 2014. Between 1960 and 1975, Norway extended compulsory schooling from 7 to 9 years, but not simultaneously in the entire country. Instead, compulsory schooling increased at different times for different municipalities. This reform provides us with a hidden IV based on the local variation in compulsory schooling across birth cohorts. We also observe an ability correlate in the form of a cognitive test administered by the Norwegian military to all male conscripts around the age of 18. This test score is not directly observed by employers.

Using these data, we examine how the IV returns to schooling vary across work experience, and interpret this through the lens of our employer learning model. The returns to schooling start high at around 15% in the first year following graduation, and then decline, rapidly at first and then slowly, until they stabilize to about 5-6%, after approximately 20 years of work experience. These findings are consistent with the hypothesis that employers use past performance to learn about workers' productivity, and the assumption that our IV is hidden from the employers. Like Lange [2007] we find that employers learn fast.

Second, we quantify the contribution of signaling and human capital acquisition to the lifetime returns to education. Our analysis reveals a productivity effect of education of 5%

and a private internal rate of return in lifetime earnings, discounted to the time of schooling choice, of 7.2%. These estimates suggest that 70% of the private returns to schooling, over the life-cycle, represents a productivity-enhancing effect of education and the remaining 30% represents the signaling contribution of education. Thus, we find a non-negligible role for signaling in explaining the positive returns to education estimated in our data.

Third, we compare our OLS estimates of returns to schooling and cognitive test scores with estimates from previous studies that use the NLSY data. The patterns we uncover in the Norwegian data are strikingly similar to those found by FG, AP, Lange [2007], and Arcidiacono et al. [2010] for the NLSY. In Norway, the estimated return to one standard deviation increase in the ability score increases from near zero in the first few years in the labor market to about 7% after around 15 years of experience. The experience pattern in the NLSY with respect to the AFQT is similar, except that the return to a standard deviation increase in the AFQT score converges to approximately 14% after 15 years. Controlling for the interaction between the ability score and experience, we find that OLS estimates of the coefficients on years of schooling decline rapidly from about 10% to about 3% within the first 20 years. Likewise, in the NLSY, the returns decline from about 9% to 6%.[3]

Finally, we consider two important extensions of our model. First, we allow the returns to skill to vary with experience. That is, we allow for differential on-the-job growth in productivity over time across workers with different skills. Identifying the signaling value of education in this model is significantly more challenging because one has to disentangle the effects of time-varying productivity and the effects of time-varying employer learning on log-wages. We show that even in this model, a hidden IV identifies the private returns to education; and a transparent IV identifies the social returns to education. Importantly, we also show that access to *both* hidden and transparent IVs is sufficient to identify the speed

[3]We also find patterns very similar to Arcidiacono et al. [2010] who report that the evidence of employer is concentrated among the less educated. Like them we find that that the association between ability and log-earnings increases with experience only for those with a high school degree or less. Among those with a college degree or more, the returns for a one standard deviation increase in the ability score remain constant at around 6-7% across all years of experience.

of employer learning and the signaling value of education. However, in this more general setting it not possible to identify the speed of learning only from a hidden IV.

Second, we revisit the assumption about homogeneous returns made in the FG and AP framework. In recent decades, the literature on the returns to education [Card, 1999; Heckman et al., 2006a] has provided evidence that the returns can vary across individuals. Following this literature, we reframe our analysis within the potential outcomes framework, e.g., Imbens and Angrist [1994], with a binary schooling outcome such as high or low schooling. We show that similar to our setting with homogeneous returns, even in this framework with heterogeneity, hidden IV identifies the private returns among compliers, and with work experience these returns converge to the social returns; and transparent IV identifies the social returns to education. Further, we also consider the identification of the speed of learning. We show that because the speed of learning itself depends on the level of schooling, the identification relies on specific assumptions about selection to schooling and heterogeneity in employer learning across workers with different schooling.

The rest of our paper proceeds as follows. Section 2 describes the model of employer learning as developed by FG, AP, and Lange [2007] and defines the private and the social returns to education within this structure. Section 3 then discusses identification of the private and the social returns to education in the model of employer learning using instrumental variables. Section 4 presents the data and our empirical setting. Section 5 contains our main empirical findings. We consider extensions to models with time-varying productivity returns and heterogeneous returns, respectively, in Section 6. We conclude in Section 7.

# 2 Model of Employer Learning

In this section, we present the model of employer learning in a perfectly competitive labor market, first proposed by FG and AP. Let worker $i$'s productivity be given by

$$\chi_{it} \quad = \exp\left(\beta_{ws} S_i + \beta_{wq} Q_i + A_i + H(t) + \varepsilon_{it}\right) \quad \equiv \exp(\psi_{it}), \tag{1}$$

where $S$ is the years of schooling, $Q$ is a correlate of ability observed by employers but unobserved by researchers, and $A$ is ability unobserved (to employers and researchers) and possibly correlated with the employer-observed correlates $(S, Q)$. An example of a $Q$ could be knowledge of foreign languages, which is typically mentioned in job applicants' résumés and that can easily be verified. The function $H(t)$ captures how log-productivity varies with experience $t = 0, 1, \ldots, \infty$. While we allow $H(t)$ to be a nonparametric function of $t$, we assume that it does not depend on either schooling or ability.[4] Finally, $\varepsilon_t$ represents time-varying noise in the production process that is independent of all other variables.

To model employer learning we follow Lange [2007] and assume that $\varepsilon_{it} \overset{i.i.d}{\sim} \mathcal{N}(0, \sigma_\varepsilon^2)$ and $(S_i, Q_i, A_i) \overset{i.i.d}{\sim} \mathcal{N}(\boldsymbol{\mu}, \Sigma)$, across workers and across time.[5] Let $\sigma_0^2 = Var(A_i|S_i, Q_i)$ be the conditional variance of $A_i$ given $(S_i, Q_i)$. Besides knowing $(S_i, Q_i)$, every period employers also observe total output $(\chi_{it})$, which is equivalent to observing a signal $\xi_{it} := A_i + \varepsilon_{it}$ about $i$'s productivity. If we let $\mathcal{E}_{it}$ denote employers' information about $i$ in period $t$, then $\mathcal{E}_{it} = (S_i, Q_i, \xi_i^t)$ with $\xi_i^t = \{\xi_{i\tau}\}_{\tau<t}$ as the history of all past signals.[6]

Assuming $(S_i, Q_i, A_i)$ are jointly normal random variables implies that the conditional expectation of $A_i$ given information at $t = 0$, $\mathbb{E}[A_i|\mathcal{E}_{i0}] = \mathbb{E}[A_i|S, Q]$, is linear in $(S, Q)$

$$A_i = \phi_{A|S} S_i + \phi_{A|Q} Q_i + \varepsilon_{A|S,Q}, \tag{2}$$

where $\varepsilon_{A|S,Q} := A_i - \mathbb{E}[A_i|S, Q]$. Under perfect competition workers are paid their expected output, conditional on the information available to the employers. The wage in period $t$ is then equal to the expected productivity conditional on $\mathcal{E}_{it}$, so that $W_{it} = \mathbb{E}[\chi_{it}|\mathcal{E}_{it}] = \mathbb{E}[\chi_{it}|S_i, Q_i, \xi_i^t]$. Taking the expectation of the log of (1) and using the fact that $\exp(A_i + \varepsilon_{it})$

[4] When estimating (Section 4.4) we use $H(t, X_i)$ which allows the experience profile to vary flexibly with individual characteristics that include a full set of dummies for birth cohort and municipality, $X_i$. In Section 6.1 we study the identification of an extension of our model where schooling and experience are nonseparable.

[5] For much of what follows, $S$ need not be Gaussian, but it simplifies the exposition of the argument. Without it, we would work with a linear projection of $A_i$ on $S_i$ and $Q_i$ instead of the Equation (2).

[6] We assume that all employers have symmetric information about workers' ability and past outputs. For assessment of how to test asymmetry among current and potential employers and its effect on labor market outcomes, see, e.g., Kahn [2013], Pinkston [2009], Schönberg [2007] and Waldman [1984], among others.

is log-normal with conditional variance $v_t = Var\left(A_i + \varepsilon_{it} | \mathcal{E}_{it}\right)$, we get

$$\ln W_{it} \quad = \quad \beta_{ws} S_i + \beta_{wq} Q_i + \tilde{H}\left(t\right) + \mathbb{E}\left[A_i | \mathcal{E}_{it}\right], \tag{3}$$

where $\tilde{H}(t) \equiv H\left(t\right) + \frac{1}{2} v_t$ collects the terms that vary only with $t$ but not across the realizations of $\xi_i^t$. For notational simplicity, we suppress $\tilde{H}\left(t\right)$ until our empirical implementation.

We can use the Kalman filter to represent the process by which employers update their expectations $\mathbb{E}\left[A_i | \mathcal{E}_{it}\right]$. It allows us to write the expectation of ability in a simple form as

$$\mathbb{E}\left[A_i | \mathcal{E}_{it}\right] = \theta_t \mathbb{E}\left[A_i | S, Q\right] + \left(1 - \theta_t\right) \bar{\xi}_i^t, \tag{4}$$

where $\bar{\xi}_i^t = \frac{1}{t} \sum_{\tau < t} \xi_{i\tau}$ is the average of signals up to period $t$ and $\theta_t = \frac{1-\kappa}{1+(t-1)\kappa}$ is the weight on the initial signal $(S, Q)$ with $\kappa = \frac{\sigma_0^2}{\sigma_0^2 + \sigma_\varepsilon^2} \in [0, 1]$. In particular, equation (4) shows that the conditional expectation of ability at time $t$ is the weighted average of the expectation at $t = 0$, before any additional information about productivity has been received, and the average of all additional signals up to period $t$ received by the employers.

The weight $\theta_t$ declines with experience $(t)$ because with time, observed measures of productivity become better predictors of productivity than the correlates $(S, Q)$, which were the only information available at $t = 0$. The rate at which $\theta_t$ declines, however, 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, which depends on the information contained in the signals. In particular, if the signal-to-noise ratio is high, i.e., when the variance of noise $(\varepsilon)$ in production is small, so that $\sigma_\varepsilon^2 / \sigma_0^2$ is small, then $\kappa$ will be close to 1, and the market quickly learns the ability $A$. But, irrespective of $\kappa$, after a sufficiently long work experience employers will put all weights on the new information, i.e., $\lim_{t \to \infty} \theta_t = 0$, and the initial productivity correlates will become less important determinants of earnings.

### Social and Private Returns to Education

Next, we define *social returns* and *private returns* to education. To that end, note that the coefficient $\beta_{ws}$ in Equation (1) is not the causal effect of education on productivity, but it is instead only the "partial" causal effect of schooling, holding the employer-observed ability correlate, Q, and unobserved ability, $A$, fixed. Schooling, however, can causally affect both $Q$ and $A$, so the "total" causal effect of schooling on productivity also includes the (indirect) effect on productivity mediated through $(Q, A)$. We refer to this total causal effect as the *social returns* to education. Education moreover also affects wages through employers' expectations about the ability of a worker, and these expectations can change over the life cycle. We define *private returns* to be the expected earnings increase from an additional year of schooling evaluated at the beginning of a life-cycle.

To formalize these two measures of returns, we need new notations and a simplifying assumption. For a random variable $Y$, let $\delta^{Y|S}$ denote the causal effect of $S$ on $Y$ and let $\tilde{Y}$ denote the part of $Y$ that is not caused by schooling $S$ but may correlate with $S$. More formally, we assume that the conditional expectation of $Y$ is additively separable and linear in $S$. Furthermore, we let there be a linear causal relationship between $S$ and $(Q, A)$, i.e.,

$$Q_i = \delta^{Q|S} S_i + \tilde{Q}_i; \quad \text{and} \quad A_i = \delta^{A|S} S_i + \tilde{A}_i. \tag{5}$$

Then, substituting $(Q, A)$ from the above equations into Equation (1), we obtain

$$\psi_{it} \quad = \quad \underbrace{\left(\beta_{ws} + \beta_{wq}\delta^{Q|S} + \delta^{A|S}\right)}_{:=\delta^{\psi|S}} S_i + \underbrace{\beta_{wq}\tilde{Q}_i + \tilde{A}_i + \varepsilon_{it}}_{:=u_{it}} \equiv \delta^{\psi|S} S_i + u_{it}. \tag{6}$$

The first term, $\delta^{\psi|S}$, in (6) is the total causal effect of schooling on productivity, or the social return to education, and it captures the causal effect (direct and indirect) on other ability components $(Q, A)$. Thus (6) shows that an extra year of schooling increases $Q$ by $\delta^{Q|S}$ units, which in turn raises productivity by $\beta_{wq}$, and it also raises ability $A$ by $\delta^{A|S}$ units.

Consider now the private returns to education. Schooling can affect expected log-earnings at $t$ in three different ways: (i) directly, because employers use schooling to form expectations about productivity; (ii) indirectly, because schooling may impact $Q$ observed by employers (as shown above in Equation (5)); and (iii) through learning, because schooling affects productivity, which employers learn over time by observing workers' outputs.

Substituting (2) and (4) in (3), and using $\bar{\xi}_i^t = \frac{1}{t}\Sigma_{\tau<t}\left(A_i + \varepsilon_{i\tau}\right) = A_i + \bar{\varepsilon}_i^t$ and that $\mathbb{E}(A_i|S_i, Q_i)$ is linear and separable in $S_i$ and $Q_i$ we get log-earnings as $\ln W_{it} = \left(\beta_{ws} + \theta_t \phi_{A|S}\right) S_i + \left(\beta_{wq} + \theta_t \phi_{A|Q}\right) Q_i + (1-\theta_t)\left(A_i + \bar{\varepsilon}_i^t\right)$. Again using (5) in the above equation, we obtain

$$\begin{aligned}
\ln W_{it} &= \left(\beta_{ws} + \theta_t \phi_{A|S}\right) S_i + \left(\beta_{wq} + \theta_t \phi_{A|Q}\right)\left(\delta^{Q|S} S_i + \tilde{Q}_i\right) + (1-\theta_t)\left(\delta^{A|S} S_i + \tilde{A}_i + \bar{\varepsilon}_i^t\right) \\
&= \underbrace{\left(\beta_{ws} + \beta_{wq}\delta^{Q|S} + \delta^{A|S} + \theta_t(\phi_{A|S} + \phi_{A|Q}\delta^{Q|S} - \delta^{A|S})\right)}_{:=\delta_t^{W|S}} S_i \\
&\quad + \underbrace{\left(\beta_{wq} + \theta_t \phi_{A|Q}\right)\tilde{Q}_i + (1-\theta_t)\left(\tilde{A}_i + \bar{\varepsilon}_i^t\right)}_{:=\tilde{u}_{it}} = \delta_t^{W|S} S_i + \tilde{u}_{it}. \qquad (7)
\end{aligned}$$

The coefficient of schooling, $\delta_t^{W|S}$, in Equation (7) is the private return to education. Comparing this coefficient with the coefficient in Equation (6), we get the following relationship:

$$\underbrace{\delta_t^{W|S}}_{\texttt{private returns}} = \underbrace{\delta^{\psi|S}}_{\texttt{social returns}} + \underbrace{\theta_t}_{\texttt{weight}} \underbrace{\left(\phi_{A|S} + \phi_{A|Q}\delta^{Q|S} - \delta^{A|S}\right)}_{\texttt{adjustment term}}. \qquad (8)$$

Thus the private returns $\delta_t^{W|S}$ differs from the social return $\delta^{\psi|S}$ if the effect of schooling on expected $A$ based on the information available to firms, which is captured by $(\phi_{A|S} + \phi_{A|Q}\delta^{Q|S})$, differs from the causal effect of schooling on unobserved ability $\delta^{A|S}$. The signaling literature assumes that this "adjustment term" is non-negative, so that education has signaling value, which in turn implies that $\delta_t^{W|S} \geq \delta^{\psi|S}$. For instance, in Spence [1973], the ability is fixed and does not vary with $S$, i.e., $\delta^{A|S} = 0$, implying that the adjustment term is positive. While we think that the adjustment term being non-negative is consistent with the theory of signaling, in a general model like ours, whether employers' expectations of worker productivity overshoot or undershoot the causal effect of schooling on ability is as an empirical question. This wedge between private and social returns, however, disappears with work experience, i.e., $\lim_{t\to\infty}\delta_t^{W|S}=\delta^{\psi|S}$ because $\lim_{t\to\infty}\theta_t=0$.

# 3 Identification

In this section, we study the identification of the social and private returns to education. We show how commonly used estimators (e.g., ordinary least squares and instrumental variables) are related to the private returns to education, the social returns to education, and the speed of learning defined above. We begin by considering least-squares projections of log-earnings on education over the life-cycle and then consider how one might proceed if additional information is available in the form of: (i) a correlate of ability not observed by the employers (e.g., AFQT score); and/or (ii) an instrument for schooling. We show that what IV identifies depends crucially on whether the IV is observed by employers.

## 3.1 Bias in the OLS

Begin by considering the regression of log-earnings on years of schooling for any given level of experience. Using Equation (7), we can derive the probability limit of the OLS estimate of the coefficient for schooling, evaluated at experience $t$, to be

$$\texttt{plim}\ \hat{b}_t^{OLS}=\underbrace{\delta_t^{W|S}}_{\texttt{private returns}}+\underbrace{\left(\beta_{wq}+\theta_t\phi_{A|Q}\right)\frac{\text{cov}(\tilde{Q},S)}{\text{var}(S)}+(1-\theta_t)\frac{\text{cov}(\tilde{A},S)}{\text{var}(S)}}_{\texttt{omitted variable bias}}. \tag{9}$$

The OLS estimate of the schooling coefficient is a biased estimate of the experience-specific private return $\delta_t^{W|S}$ because the omitted ability components $(\tilde{Q},\tilde{A})$ correlate with, but are not *caused* by, schooling. This omitted ability bias is the main reason why researchers rely on IV(s) to identify the returns to education. The magnitude of bias depends on the speed of learning, $\kappa$, which determines the weight $\theta_t$ employers put at time $t$ on the initial signal.

Now, let us consider what happens to this bias as workers accumulate work experience, i.e., as $t \to \infty$. Taking the limit in (9) and using $\lim_{t\to\infty} \delta_t^{W|S} = \delta^{\psi|S}$ from (8) we get

$$\texttt{plim}\left(\lim_{t\to\infty} \hat{b}_t^{OLS}\right) = \underbrace{\delta^{\psi|S}}_{\texttt{social returns}} + \underbrace{\frac{\operatorname{cov}\left(\beta_{wq}\tilde{Q} + \tilde{A}, S\right)}{\operatorname{var}(S)}}_{\texttt{remaining bias}}. \tag{10}$$

Thus, even after employers observe a long history of outputs, the bias does not disappear. We conclude that the OLS does not identify the private or the social returns to education.

## 3.2 Exploiting a Hidden Correlate of Ability

Now suppose that we have access to a correlate of ability, denoted by $Z$, and suppose $Z$ is unobserved by employers. We refer to this as a "hidden" correlate of ability. Furthermore, suppose that $A_i = \beta_{Az} Z_i + \eta_i, \eta_i \perp Z_i$ so that $\eta_i$ represents the productivity component observed by neither researchers nor employers.[7] Substituting $A_i$ in Equation (1) gives $\chi_{it} = \exp\left(\beta_{ws} S_i + \beta_{wq} Q_i + \beta_{Az} Z_i + \eta_i + H(t) + \varepsilon_{it}\right) = \exp(\psi_{it})$, and following the steps from Section B in [Lange, 2007], we can show that

$$\mathbb{E}\left[\ln W_{it} | S, Z, t\right] = \theta_t \mathbb{E}\left[\ln W_{i0} | S, Z\right] + (1-\theta_t)\mathbb{E}\left[\ln W_{i\infty} | S, Z\right], \tag{11}$$

where $W_{i0}$ is the wage received in period $t = 0$ and $W_{i\infty}$ is the wage received at $t \to \infty$, when enough information has been revealed so that worker productivity is known in the market. The linearity of (11) allows us to estimate the speed of learning, $\kappa$, by projecting log-earnings on $(S, Z)$ across different work experience levels, $t$, because the weight $\theta_t$ depends only on $\kappa$. Thus, the regression coefficients of log-earnings on $(S, Z)$ converge from their $t = 0$ value to their $t = \infty$ value at a rate that depends only on $\kappa$, thereby identifying $\kappa$.

The projection coefficients obtained from estimating (11) across different experience lev-

[7] $\beta_{wq}$ in Equation (1) accounts for variation in productivity with $Q$. Therefore, omitting $Q$ in the projection of $A_i$ on $Z_i$ simply represents a normalization.

els, however, do not identify the causal effect of $S$ or $Z$ on productivity. These coefficients are biased (even when $t \to \infty$) because $(S, Z)$ can be correlated with the omitted variables $(Q, \eta)$. Thus, while we can identify $\kappa$ if we have a hidden correlate of ability, we cannot identify the private or the social returns to education without additional information.

### 3.3 Instrumental Variables

Next, suppose that we have access to a binary instrument variable $D_i \in \{0, 1\}$. In other words, suppose $D_i$ satisfies the following standard assumptions for a valid IV.

**Assumption 1.** *Instrumental Variables*

1. *(Exogeneity): $u_{it} \perp D_i$, where $u_{it}$ is defined in (6).*
2. *(First Stage): $\mathbb{E}[S_i|D_i = 0] \neq \mathbb{E}[S_i|D_i = 1]$.*

Under Assumption 1, for a binary instrument $D_i$, in period $t$ we get

$$\texttt{plim}\ \hat{b}_t^{IV} \quad := \quad \frac{\mathbb{E}\left[\ln W_{it}|D_i = 1, t\right] - \mathbb{E}\left[\ln W_{it}|D_i = 0, t\right]}{\mathbb{E}\left[S_i|D_i = 1, t\right] - \mathbb{E}\left[S_i|D_i = 0, t\right]}. \tag{12}$$

Furthermore, using the fact that $S$ is constant across $t$ and $\lim_{t\to\infty} \ln W_{it} = \psi_i$, we get

$$\texttt{plim}\ \left(\lim_{t\to\infty} \hat{b}_t^{IV}\right) = \frac{\mathbb{E}\left[\psi_i|D_i = 1\right] - \mathbb{E}\left[\psi_i|D_i = 0\right]}{\mathbb{E}\left[S_i|D_i = 1\right] - \mathbb{E}\left[S_i|D_i = 0\right]} = \delta^{\psi|S}, \tag{13}$$

where the second equality follows from Assumption 1-(1), which implies that the part of the productivity, $\psi_i$, not caused by schooling, $S$, is orthogonal to the instrument, $D$. Thus, as $t \to \infty$ the IV identifies the causal effect of schooling on productivity. In other words, the IV estimate of returns to education at sufficiently high levels of experience is a consistent estimator of the causal effect of schooling on productivity.

Note that this identification strategy is valid *irrespective* of what the employers know about $D$. Heuristically, in the long run everything about a worker's ability is revealed to the employers, and thus knowledge of the instrument itself has become irrelevant for wage setting. For intermediate work experience (i.e., $t < \infty$), however, what the IV identifies depends on whether or not $D$ is hidden from the employers. To determine how the information of employers affects the interpretation of the IV estimates, we distinguish between *hidden* and *transparent* instruments next.

### Hidden Instrument

We begin with a setting where the instrument is unobserved by the employers, i.e., when $D^{\mathfrak{h}}$ is a hidden IV. We use the superscript $\mathfrak{h}$ to refer a hidden IV.

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

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

The numerator in Equation (12) for a binary, hidden IV, $D_i^{\mathfrak{h}}$, is

$$\begin{aligned} \mathbb{E}\left[\ln W_{it} | D_i^{\mathfrak{h}} = 1\right] - \mathbb{E}\left[\ln W_{it} | D_i^{\mathfrak{h}} = 0\right] &= \mathbb{E}\left[\beta_{ws} S_i + \beta_{wQ} Q_i + \mathbb{E}\left[A_i | S_i, Q_i, \xi_i^t\right] | D_i^{\mathfrak{h}} = 1, t\right] \\ &\quad - \mathbb{E}\left[\beta_{ws} S_i + \beta_{wQ} Q_i + \mathbb{E}\left[A_i | S_i, Q_i, \xi_i^t\right] | D_i^{\mathfrak{h}} = 0, t\right] \end{aligned}$$

where $\ln W_{it}$ does not directly depend on $D_i^{\mathfrak{h}}$ because it is not used by the employers in the wage setting. The IV, $D_i^{\mathfrak{h}}$, affects $\ln W_{it}$ only indirectly by affecting $(S_i, Q_i, \xi_i^t)$ that makes up the information $\mathcal{E}_{it}$ used by employers to infer productivity. From Assumption 1 and Equation (5) we get $\mathbb{E}\left[Q_i | D_i^{\mathfrak{h}}, S_i\right] = \delta^{Q|S} S_i + \tilde{Q}_i$. Using that with Equation (2), the fact that $D_i^{\mathfrak{h}} \perp \tilde{Q}_i$, and simplifying further gives

$$\begin{aligned} \mathbb{E}\left[\ln W_{it} | D_i^{\mathfrak{h}}, t\right] &= \left(\left(\beta_{ws} + \beta_{wQ}\delta^{Q|S}\right) + \theta_t\left(\phi_{A|S} + \phi_{A|Q}\delta^{Q|S}\right) + (1-\theta_t)\,\delta^{A|S}\right)\mathbb{E}\left[S_i | D_i^{\mathfrak{h}}\right], \\ &= \left(\delta^{\psi|S} + \theta_t\left(\phi_{A|S} + \phi_{A|Q}\delta^{Q|S} - \delta^{A|S}\right)\right)\mathbb{E}\left[S_i | D_i^{\mathfrak{h}}\right]. \end{aligned}$$

Then, taking the probability limit, we get

$$\texttt{plim } \hat{b}_t^{IV^\natural} = \frac{\mathbb{E}\left[\ln W_{it}|D_i^\natural = 1\right] - \mathbb{E}\left[\ln W_{it}|D_i^\natural = 0\right]}{\mathbb{E}\left[S_i|D_i^\natural = 1\right] - \mathbb{E}\left[S_i|D_i^\natural = 0\right]} = \delta^{\psi|S} + \theta_t \left(\phi_{A|S} + \phi_{A|Q}\delta^{Q|S} - \delta^{A|S}\right). \quad (14)$$

Comparing Equation (14) with the private returns defined in Equation (8), we can conclude that, for every work experience level $t$, the hidden IV identifies the private returns to education, i.e., $\texttt{plim } \hat{b}_t^{IV^\natural} = \delta_t^{W|S}$. Further, we note that $\texttt{plim } \hat{b}_t^{IV^\natural}$ converges to the social return to education, $\delta^{\psi|S}$, as $t \to \infty$. 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 return, as well as, the signaling value of education.

Besides the private and the social returns, a hidden IV also identifies the speed of learning $\kappa$. To see this, note that we can express the probability limit of the hidden IV estimator as $\texttt{plim } \hat{b}_t^{IV^\natural} = \theta_t \times b_0^{IV} + (1-\theta_t) \times b_\infty^{IV}$, where $b_0^{IV}$ is the private return to education at $t = 0$, $b_\infty^{IV}$ is the social return to education, and $\theta_t = \frac{1-\kappa}{1+(t-1)\kappa}$ is the weight defined in Equation (4). Using the IV estimates for all $t = 0, \ldots, T$, we can estimate the RHS parameters using the non-linear least squares method. Heuristically, we can "solve" for $\{b_0^{IV}, b_\infty^{IV}, \kappa\}$ from $\hat{b}_t^{IV^\natural}$ for $t = 0, \ldots, T$, and once we know $\kappa$ we can determine $\theta_t$ for each $t$.

In many settings, Assumption 2 is a natural assumption. The clearest examples relate to field experiments that provide subsidies or information that induce higher school enrollment. In these cases, whether a student is in the control or treatment group is typically not known to the (potential) employers. Some examples of hidden instruments from the empirical literature in quasi-experimental settings include (i) the interaction of draft lottery number and year of birth in Angrist and Krueger [1992]; (ii) the interaction of a policy intervention, family background and season of birth in Pons and Gonzalo [2002]; (iii) parents' education and 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.

#### Transparent Instrument

We say that an instrumental variable is *transparent* if it is known to the employers and is thus "priced-in" the wages. In other words, if the IV, $D^{\mathfrak{t}}$, is transparent, it is included in the information set of the employers, but it is still a valid IV because it satisfies Assumption 1. Let $\tilde{\mathcal{E}}_{it} := \mathcal{E}_{it} \cup \{D_i^{\mathfrak{t}}\}$ be the new set of information employers have about $i$ in $t$.

**Assumption 3.** *(Transparent Instrument) Employers observe $D_i^{\mathfrak{t}}$ so that* $\ln W_{it} = \mathbb{E}[\psi_{it}|\tilde{\mathcal{E}}_{it}]$.

By Assumption 1, we have that transparent instruments satisfy the exclusion restriction with respect to productivity $\psi$. Assumption 3, however, implies that the instrument is used in wage setting and thus will not be orthogonal to wages conditional on schooling and other controls. So if $D^{\mathfrak{t}}$ is transparent, it violates the exclusion restriction for wages and thus does not estimate the causal effect of schooling on individual wages (which is the private return), but it estimates the social returns (the effect on productivity). To see the intuition as to how transparent IV identifies the social returns to education, consider two workers $i \neq j$, who have the same abilities and past outputs but different realizations of the instrument. Suppose $D_i^{\mathfrak{t}} = 1$ but $D_j^{\mathfrak{t}} = 0$ and $S_i > S_j$. $D^{\mathfrak{t}}$ is transparent, so employers can deduce that $S_i > S_j$ because of $D^{\mathfrak{t}}$ and and not because of $A$. So if $\ln W_i \geq \ln W_j$ then this wage difference can be attributed to the productivity effect of schooling. 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.,

$$\begin{aligned}
\mathbb{E}\left[\ln W_{it}|S_i, D_i^{\mathfrak{t}}\right] &= \mathbb{E}\left[\delta^{\psi|S} S_i + \tilde{\psi}|S_i, D_i^{\mathfrak{t}}\right] = \delta^{\psi|S} \times S_i; \\
\mathbb{E}\left[\ln W_{it}|D_i^{\mathfrak{t}}\right] &= \delta^{\psi|S}\mathbb{E}\left[S_i|D_i^{\mathfrak{t}}\right].
\end{aligned}$$

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

Examples of instruments used in the literature that are more likely to be transparent than not 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 et al. [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].

# 4 Data and Empirical Setting

In this section, we first describe our data sources, sample construction and the key variables utilized in our analysis. Then we describe the Norwegian compulsory schooling reform that we utilize as a source of exogenous variation in educational attainment to construct IV estimates of the returns to education in log-earnings at each year of experience. Finally, we discuss the empirical specifications motivated by the discussion in Section 3.

## 4.1 Data Sources and Sample Construction

Our empirical analysis uses several registry databases maintained by Statistics Norway. These databases allow us to construct a rich longitudinal dataset containing records for all Norwegian males from 1967 to 2014. We observe demographic information (e.g., cohort of birth and childhood municipality of residence) and socio-economic information (e.g., years of schooling and annual earnings) for these individuals. Importantly, the dataset also includes a unique personal identifier which allows us to follow individuals' earnings across time. The personal identifier also allows us to merge information on IQ test scores for males from the Norwegian Armed Forces to our dataset.

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 natural attrition due to either death or out-migration. Second, our earnings data pertain to all individuals, and are not limited to some sectors or occupations. Third, we can construct long earnings histories that allow us estimate the returns to education at each year of labor market experience.

We restrict our sample to Norwegian males born between 1950 and 1980, including several cohorts with earnings observed over a wide-range of labor market experiences.[8] We restrict the sample to males because the military IQ test scores are not available for females. We further exclude immigrants as well as Norwegian males with missing information on either of the following variables, including years of schooling, childhood municipality of residence, IQ test score, or exposure to the compulsory schooling reform. Applying these restrictions we retain a sample consisting of 732,163 Norwegian males born between 1950 and 1980.

Our primary outcome variable is the natural logarithm of pre-tax annual labor earnings.[9] To limit variation in earnings across labor market experience due to the intensity of hours of work, we focus only on full-time workers who are defined as having annual labor earnings (adjusted for wage inflation) above the substantial gainful activity threshold (henceforth, SGA) as defined by the Norwegian Social Security System.[10] Restricting the sample to full-time employed males, we retain $718,237$ individuals–thus most males are recorded having a full-time employment spell at least once–and a panel data set comprising $14,746,755$ person-year observations. On average an individual is thus observed working full-time for 20.5 years. This sample is utilized in the empirical part of our analysis. Note that this sample is unbalanced: we have earnings for $579,984$ individuals in the initial year of work experience and for $190,900$ individuals in the $30^{th}$ year.

[8] In our annual income panel data from 1967 to 2014, we observe the oldest cohort (1950) between ages 17 and 64 and the youngest cohort (1980) up to age 34.

[9] This measures excludes income from self-employment, capital income or unconditional cash transfers such as social economic assistance, housing assistance, child allowance, etc.

[10] The ideal outcome variable would be an hourly wage rate, however, this measure is not available for most of our sample period. By conditioning our sample to workers with earnings above the SGA threshold, we try to limit variability in earnings due to differences across individuals and over time in working hours. In 2015, the SGA threshold was USD 10,650. Estimates using alternative thresholds are available upon request.

## 4.2 Measures of Schooling and IQ Test Scores

The first key regressor of interest is years of schooling corresponding to the highest level of completed education. This variable is taken from Statistics Norway's Education Register and it is based on the educational attainment reports submitted by educational establishments directly to Statistics Norway, which minimizes the chance of misreporting. Using the years of schooling variable and the age at the start of each year, we construct a measure of potential labor market experience as age minus years of schooling minus school starting age.[11]

Our second regressor of interest is the IQ test score accessed from the Norwegian Armed Forces. In Norway, military service was compulsory for all able males in the birth cohorts we study. Before each male entered the service, his medical and psychological suitability was assessed. Most eligible Norwegian males in our sample took this test around their $18^{th}$ birthday. The IQ test score we use is a composite unweighted mean from three speeded tests–arithmetics, word similarities, and figures.[12]

Figure 1 displays the average and the conditional density of IQ for each year of schooling between 9 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 high school (10 years) and college (13/14 years), with more gradual increases at later stages of schooling. This pattern could be due to substantial ability-related (psychic) costs for enrolling in high school or selective entry requirements enforced in the entry to higher education in Norway.[13]

[11] We measure age at the start of each year in order to follow individuals' earnings from the first calendar year ($t \geq 0$) after the year of their graduation. We don't use earnings from the year of their graduation, since most individuals would be enrollment in school at some point during that calendar year. As discussed above, since we also restrict our sample to full-time observations, individuals working less than full-time are dropped for years in which they work part-time or are non-employed and appear in the sample in the remaining years. Similarly, individuals performing military service do not enter the sample in service years.

[12] The arithmetic test mirrors the test in the Wechsler Adult Intelligence Scale (WAIS), the word test is similar to the vocabulary test in WAIS, and the figures test is comparable to the Raven Progressive Matrix test. See Sundet et al. [2004] and Thrane [1977] for details.

[13] As documented in Kirkeboen et al. [2016], Norway has a system where access to public higher education is based on merit, and it is administered through a centralized admissions process. Students with higher

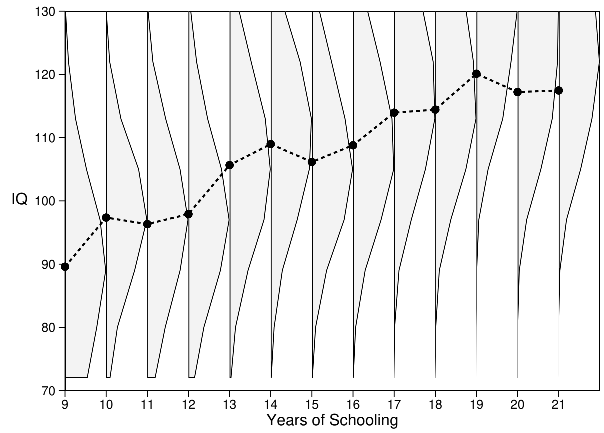



Figure 1: The Conditional Probability Density of IQ Test Scores on Years of Schooling.
*Note:* The sample consists of Norwegian males born 1950-1980 observed in earnings data over years 1967-2014 with years of potential experience between 0 and 30 years with annual earnings above 1 SGA threshold (N=14,746,755). The IQ test score along the y-axis is 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.

Arguably, Norway is an interesting setting to assess employer learning and the signaling value of education for several reasons. First, the strong correlation between schooling and ability test scores in our data suggests that schooling may predict ability, satisfying a necessary condition for schooling to have a signaling value. Second, the Norwegian Armed Forces do not provide certified information to conscripts containing their personal ability test scores (e.g., diplomas), making it infeasible for conscripts to disclose this information in a verifiable and credible manner in a job search process.[14] It is thus reasonable to assume that employers do not observe the ability test scores from military conscription. Using the correlation between the military IQ test scores and earnings across experience, researchers can thus infer the process of employer learning. As discussed above, we allow for the possibility that other correlates (as captured by $Q$ in Section 2) of applicants' ability could be revealed in the job application process. Finally, most cohorts in our sample entered the labor market before the arrival of online recruitment tools in the early 2000s, which might have altered the way in

GPAs from high school can thus more easily select into fields with high demand, and these students may also have higher IQ test scores in military conscription.

[14] There is no legal obligation on the Norwegian Armed Forces to communicate the results of ability tests to conscripts undergoing such testing, and thus many conscripts may not even be familiar with their own test scores. This further limits the possibility of voluntary disclosure of ability test scores by job applicants and it is also not a common practice that Norwegian employers request such information from job seekers.

which employers tended to screen or recruit workers.

### 4.3 The Compulsory Schooling Reform

Between 1960 and 1975 Norway enacted a compulsory schooling reform that increased the minimum required schooling from 7 to 9 years. This reform was implemented by different municipalities–the lowest level of local administration–in different years. Thus, for more than a decade, Norwegian schools were divided into two separate systems, where the length of compulsory schooling depended on the birth year and the municipality of residence at age 14, which we refer to as the childhood municipality. We use the timing differences across municipalities, induced by the staggered implementation of the reform, as our instrumental variable for school years. For more on the reform see Black et al. [2005].[15]

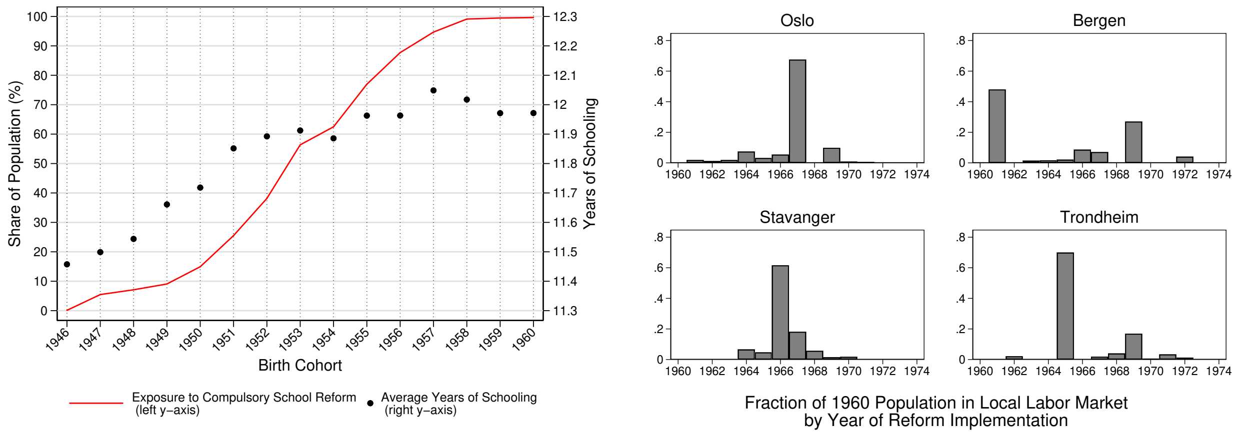
Historical records provide information about the year in which the reform was implemented for 672 out of the 732 municipalities in 1960. This information is missing for the remaining 60 municipalities [Monstad et al., 2008]. As shown in Figure 2, there is considerable variation in the fraction of birth 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 was subjected to 9 years of compulsory schooling law, whereas everyone born after 1960 was affected by the new law.

Figure 2-(b) shows that there is considerable variation even within the four largest local labor markets (the four biggest metropolitan areas in Norway). For instance, the municipality of Oslo city, which accounted for two-thirds of the population in the Oslo labor market region in 1960, implemented the reform in 1967, whereas the timing of the reform varied between 1961 and 1971 across the remaining population living in one of the other 39 municipalities.[16]

As discussed in Section 3, to separately identify the private and the social returns to education, the instrument should satisfy the standard IV assumptions (Assumption 1) and

[15] This compulsory schooling reform in Norway has been used previously, albeit in different contexts, by Monstad et al. [2008]; Aakvik et al. [2010]; Machin et al. [2012], and Bhuller et al. [2017].

[16] 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 5 municipalities.




(a) Exposure across Birth Cohorts

(b) Timing within Local Labor Markets

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

also be a hidden instrument (Assumption 2). An implication of the latter assumption in our setting is that employers are not informed about the interaction between a worker's birth cohort and the timing of compulsory school reform in the worker's municipality of childhood.

For two reasons we think this assumption is reasonable in our setting. First, in contrast to compulsory schooling laws legislated centrally in many countries or by the states in the U.S. states, the timing of the implementation of the Norwegian compulsory school reform was decentralized and decided at the local municipal level. This decentralized implementation is consistent with our data, e.g., Figure 2-(b) that displays substantial variation in the timing of the reform even within local labor markets. Within local labor markets, there are high rates of commuting and mobility. This means that to know whether or not an individual was treated, an employer not only would have to know the exact date of implementation for each municipality, but would have to determine the childhood municipality of each worker (or job applicant). While it might be easier to discern the place of residence and birth year, from the CV, say, determining the childhood municipality would be difficult and expensive, if not impossible.

Second, even if employers had information on each applicant's birth year and childhood municipality, retrieving information on exposure to compulsory school reform for each applicant would still be onerous and costly. The information on the timing of compulsory reform was until recently not readily available in online public databases.[17] Therefore, for the 1946-1960 cohorts, graduating in an era long before the internet, this information would not have been easily traceable for employers.

Even though we do not directly test the hidden instrument assumption, to substantiate the identifying assumption that our IV is indeed hidden, we also restrict our analytical sample in Section 5 by *excluding* workers who grew up in the municipality with the largest population in each local labor market. Heuristically, by focusing on the subset of remaining workers, for whom it is plausible to assume that the employers are uninformed about the timing of reform in their childhood municipality and consequently their reform exposure status, we argue that the hidden instrument assumption is likely to be satisfied in our setting. This restricted – and our preferred – sample retains 422,749 individuals and 8,697,979 person-year observations, which is 59% of the full sample. For completeness, we also present results from the IV analysis for the full sample retaining individuals from the main municipality.

## 4.4 Empirical Specifications

### 4.4.1 Instrumental Variable Specification

Our first approach uses an instrumental variables (IV) specification, where we regress log-earnings on schooling and control variables $X$ at each experience $t$:

$$\ln W_{it} = a_t^{IV} + b_t^{IV} S_i + e_t^{IV} X_i + u_{it}^{IV}, \tag{15}$$

where $\ln W_{it}$ and $S_i$ are log-earnings and years of schooling, respectively, and $X_i$ is a vector of control variables, including a full set of dummies for birth cohort and childhood municipality.

[17] Previously, Monstad et al. [2008] tracked various historical documents and databases to construct information on the timing of reform for 672 out of 732 municipalities.

The IV specification consists of the second-stage Equation (15) and the first-stage equation

$$S_i = r + dD_i^{\natural} + nX_i + w_i, \tag{16}$$

where the binary instrument $D_i^{\natural} \in \{0, 1\}$ is equal to 1 if the individual was exposed to the new schooling law, and 0 otherwise and $X$ is as before a full set of dummies for birth cohort and childhood municipality. An individual $i$ is coded to be exposed if the reform had been implemented in $i$'s childhood municipality of residence by the time he had turned 14.[18]

We estimate the system of Equations (16) and (15) by 2SLS, separately for each year of experience, $t$.[19] We use the childhood municipality indicators to control for unobservable determinants of earnings or schooling fixed at the municipality level. By conditioning on these indicators, we can compare individuals that grew up in the same municipality, yet were born in different years, and therefore exploit variation in their schooling stemming from differential exposure to compulsory schooling requirements. By adding the birth cohort indicators, we moreover control for aggregate changes in schooling and earnings across cohorts.

Our parameter of interest is $b_t^{IV}$, the coefficient on years of schooling at experience $t$. We maintain the assumptions that (i) conditional on $X$, $D^{\natural}$ satisfies Assumption 1, and as discussed in Section 4.3, that in our setting (ii) $D^{\natural}$ satisfies the hidden IV Assumption 2.[20] As discussed in Section 3.3, under Assumption 2, $\hat{b}_t^{IV^{\natural}}$ converges to the social return to education, $\delta^{\psi|S}$, as $t \to \infty$, and moreover, $\hat{b}_t^{IV^{\natural}}$ 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 $\hat{b}_{t\to\infty}^{IV^{\natural}}$, and use the rate at which $\hat{b}_t^{IV^{\natural}}$ converges to $\hat{b}_{\infty}^{IV}$ to estimate the speed of learning, $\kappa$.

[18] At that time the school starting age in Norway was 7 years, and before the reform the critical age at which a pupil would be required to take two additional years of schooling was 14 years. Cohorts with ages 14 years or less at the time of school reform would be required to take the two additional years, while all cohorts aged above 14 at the time the new law went into effect would not.

[19] Unlike Equation (15), there is no experience subscript $t$ attached to the $d$ coefficient on our instrument $D^{\natural}$ in the first-stage equation because both compulsory schooling reform exposure status $D^{\natural}$ and schooling $S$ are time-invariant variables. However, with an unbalanced panel and separate estimations by experience, the first-stage estimates of $d$ will be allowed to vary by $t$. In practice, estimates of $d$ are very stable across the experience range that we consider despite differences in the sample composition by experience.

[20] The reform timing is also uncorrelated with baseline municipality characteristics [Bhuller et al., 2017].

A challenge to identification of returns to schooling based on Equation (15) would be that individuals growing up in different municipalities could have had different growth in schooling and earnings even in the absence of a compulsory schooling reform (i.e., differential trends across treatment units). Following Bhuller et al. [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 education attainment and lifetime earnings estimated using data on pre-reform cohorts as additional controls. We refer to estimates based on Equations (16) and (15) as obtained from the baseline specification, and estimates that we get after further controlling for municipality-specific trends as coming from the trends specification.

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

### 4.4.2 OLS Specification Using a Hidden Correlate

As discussed above, we can estimate the speed of learning $\kappa$ using the IV estimates as well as the estimates relying on the IQ test score as a hidden correlate (Section 3.2). This latter approach requires projecting log-earnings on schooling $S$, IQ score, $Z$, and other control variables, $X$, at different work experience level $t$:

$$\ln W_{it} = a_t^{OLS} + b_t^{OLS} S_i + c_t^{OLS} Z_i + e_t^{OLS} X_i + u_{it}^{OLS}. \tag{17}$$

Under the assumptions that schooling does not independently enter $H(t, X)$ and that $Z$ is unobserved in the market, we can use the regression estimates of $\{b_t^{OLS}, c_t^{OLS}\}$ to obtain two estimates of the speed of learning $\kappa$. See Lange [2007] for further details.

It is well known that log-earnings tend to be nonlinear in schooling. Thus, we cannot simply compare the OLS estimates and IV estimates that we get from Equations (15) and (17). Comparing OLS and the IV estimates in the presence of non-linearities can be misleading simply because the OLS and the IV estimates weigh different marginal returns to schooling differently. We can, however, construct weighted OLS estimates that are comparable to the IV estimates by first estimating the fully non-linear model in OLS and then weighting the marginal returns using the weights that correspond to the IV estimator. This re-weighting procedure ensures that the OLS estimates are obtained from the same support of schooling distribution as the IV estimates and thus allows us to compare estimates of the speed of learning across estimators in the presence of non-linearities. We refer to these re-weighted OLS estimates as IV-weighted OLS estimates and denote them by $\{b_t^{WOLS}, c_t^{WOLS}\}$.[21]

# 5 Main Results

This section contains our main empirical results. To begin, we present the IV estimates of returns to education over work experience and use these estimates to determine the speed of learning. We then use the same IV estimates for our main contribution, which is to provide estimates of the private and social returns to education. The gap between these two returns represents our estimate of the contribution of signaling to the return to education. Finally, we present OLS estimates that use the IQ test score as a hidden correlate of ability.

## 5.1 IV Estimates of the Returns to Education

Table 1 column (1) displays the first-stage estimate of the effect of our compulsory schooling reform instrument on years of schooling, as defined in Equation (16), for the full sample. This estimate indicates that exposure to compulsory schooling reform increased completed schooling by 0.237 years. The partial F-statistic is approximately 88, which means that weak instrument bias is not a concern for our analysis.

[21] We follow Angrist and Imbens [1995]; Løken et al. [2012] and Mogstad and Wiswall [2016], and provide additional details on the re-weighting procedure in the Appendix Section A.1.

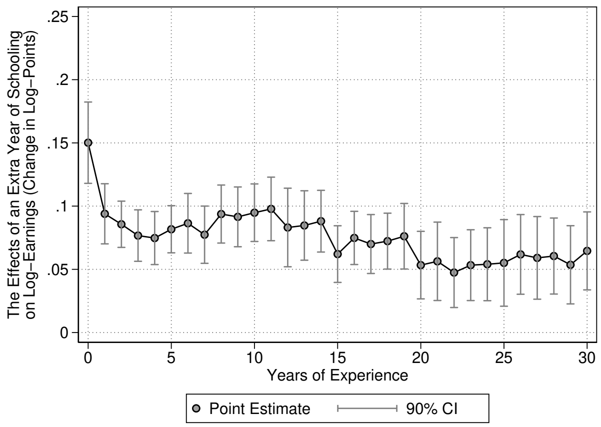
Table 1: First-Stage Estimates on Years of Schooling.

| | Full Sample | | Preferred Sample | |
|---|---|---|---|---|
| | Baseline Specification (1) | Trends Specification (2) | Baseline Specification (3) | Trends Specification (4) |
| Instrument: | | | | |
| *Exposure to Compulsory Schooling Reform* | 0.237*** | 0.209*** | 0.228*** | 0.209*** |
| | (0.025) | (0.034) | (0.034) | (0.040) |
| Municipality Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Cohort Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Municipality-Specific Trends | | ✓ | | ✓ |
| F-statistic (instrument) | 87.7 | 37.9 | 45.7 | 27.7 |
| Sample Mean Years of Schooling | 12.36 | 12.36 | 12.27 | 12.27 |
| Standard Deviation Years of Schooling | 2.50 | 2.50 | 2.46 | 2.46 |

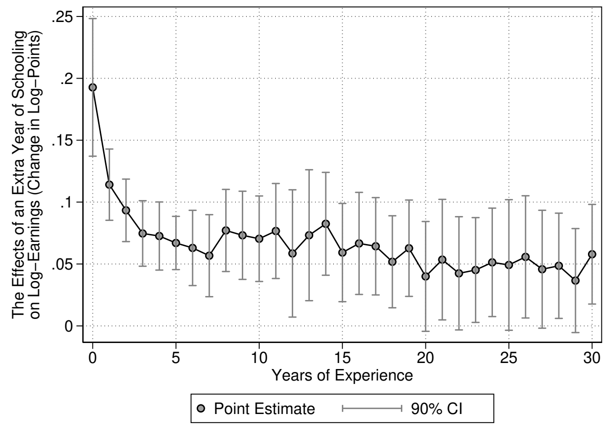
*Note:* The full estimation sample consists of Norwegian males born in 1950-1980 observed any time in earnings data over years 1967-2014 with years of potential experience between 0 and 30 years and annual earnings above 1 SGA threshold (N=14,746,755). The restricted estimation sample further drops individuals 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). All estimations include fixed effects for birth cohort and childhood municipality. The trends specifications in columns (2) and (4) further also controls for linear and quadratic municipality-specific trends estimated using data on all pre-reform cohorts born 1930 or later and extrapolated to all post-reform cohorts, separately for each municipality. Standard errors are clustered at the local labor market region (160 groups). * $p < 0.10$, ** $< 0.05$, *** $p < 0.01$.

As described above, how we interpret the IV estimates depends crucially on whether the IV is hidden or transparent. We are more confident that the IV is hidden when we restrict ourselves to the variation across small municipalities that surround the core of large urban agglomerations. Our preferred estimates therefore derive variation from a sample that excludes those born in the largest municipalities in the different labor markets. These estimates are in column (3), and we can see that the effect of our IV on education is unchanged. We repeat these two estimation exercises including municipality-specific trends (columns 2 and 4), and find that although the absolute effect is smaller, the conclusion does not change.

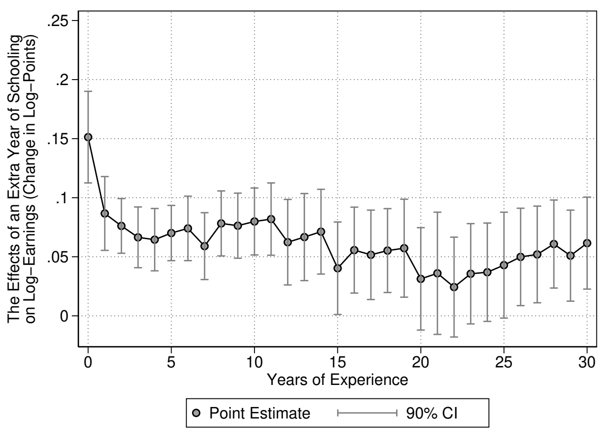
We now turn our attention to the second-stage IV estimates in Equation (15). Figure 3-(a) displays the IV estimates for the full sample, and these coefficients represent the private returns to schooling, at each year of work experience. Similarly, Figure 3-(b) displays the IV estimates for the restricted sample, and Figures 3-(c) and 3-(d) display the IV estimates for each of these two samples with municipality-specific trends, respectively.



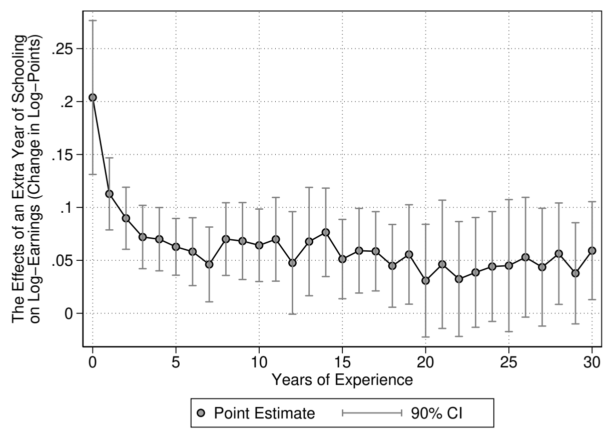

(a) Full Sample–Baseline Specification




(b) Preferred Sample–Baseline Specification




(c) Full Sample–Trends Specification




(d) Preferred Sample–Trends Specification

Figure 3: IV Estimates of the Returns to Schooling by Year of Experience.

*Note:* The full 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 restricted estimation sample further drops those growing up in the municipality with largest population size in each the 160 labor market regions in Norway (N=8,697,979). Plots (a) and (b) display IV estimates from separate estimations of Equation (15) for each year of experience using the two samples, while plots (c) and (d) further control for municipality-specific trends. All estimations include fixed effects for birth cohort and childhood municipality. Standard errors are clustered at the local labor market region (160 groups). The 90% confidence intervals corresponding to each point estimate are displayed as vertical bars.

All four panels exhibit point estimates that suggest high initial returns to schooling, followed by a relatively steep decline during the first 5 years of work. Then, the returns gradually stabilize and approach 5-6% for those with 15 years or more of work experience. These patterns are consistent with employers learning about workers' ability. Moreover, these estimates also indicate that employers did not fully price in the variation in schooling that is induced by the variation in compulsory schooling reform exposure, across cohorts and

Table 2: IV Estimates of the Speed of Employer Learning, Initial Value and Limit Value.

| | Full Sample | | Preferred Sample | |
|---|---|---|---|---|
| | (1) Years of Schooling | (2) Years of Schooling | (3) Years of Schooling | (4) Years of Schooling |
| Speed of Learning $\kappa$ | 0.447*** | 0.490*** | 0.532*** | 0.565*** |
| | (0.127) | (0.110) | (0.058) | (0.055) |
| Initial Value $b_0^{IV}$ | 0.145*** | 0.148*** | 0.192*** | 0.204*** |
| | (0.013) | (0.014) | (0.010) | (0.010) |
| Limit Value $b_\infty^{IV}$ | 0.063*** | 0.048*** | 0.050*** | 0.045*** |
| | (0.004) | (0.004) | (0.003) | (0.003) |
| Weight $\theta_t$ on Initial Signal: | | | | |
| at $t = 5$ | 19.8% | 17.2% | 15.0% | 13.3% |
| at $t = 10$ | 11.0% | 9.4% | 8.1% | 7.1% |
| at $t = 15$ | 7.6% | 6.5% | 5.5% | 4.9% |
| Municipality Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Cohort Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Municipality-Specific Trends | | ✓ | | ✓ |

*Note:* The full 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 estimates plotted in Figure 3(a) for the full estimation sample are used to construct the corresponding IV estimates of speed of learning, initial value and limit value in columns (1)-(2). The estimates in columns (3)-(4) are similarly based on the estimates plotted in Figure 3(b) for a restricted estimation sample in which the municipality with largest population size in each of the 160 labor market regions in Norway is dropped (N=8,697,979).
* p < 0.10, ** < 0.05, *** p < 0.01.

municipalities, which is consistent with our hidden IV assumption.

Table 2 displays the estimates of initial private returns to education ($b_0^{IV}$), social returns to education ($b_\infty^{IV}$) and speed of employer learning ($\kappa$) obtained using the coefficient estimates shown in Figure 3 based on the non-linear least squares method discussed in Section 3.3. Comparing the estimates across columns (1)-(4), we can see that the estimates are robust with respect to sample restrictions and controls for municipality-specific trends. In particular, we cannot reject that the equality of the speed of learning estimates across columns (1)-(4). The point estimates of $\kappa$ are between 0.447 and 0.565, which imply very rapid learning on the part of employers. More precisely, our preferred estimate of the speed of employer learning at 0.532 in Table 2-(3) implies that already after the first five years of employment, employers put only 15% weight on the initial signal they received from the worker, and after 15 years of employment history this weight further declines to 5.5%.

## 5.2 The Signaling Value of Education

Next, we use the estimates from Table 2 to determine the signaling value of education. Our employer learning model implies that the limit return to education $b_{\infty}^{IV}$ is the social returns to education. The experience-specific IV estimates directly represent the private returns to education. Using estimates of model parameters in Table 2, we can also construct estimates of the private returns at each $t$. In Figure 4 we display the private and social returns based on the estimates from Table 2-(3), as well as the experience-specific IV estimates. The scatter plot displays the IV estimates obtained from the preferred sample in Figure 3-(b), and the horizontal blue line is the estimated social return to education $b_{\infty}^{IV}$ from Table 2-(3) at 5%.

In order to determine the signaling value of education we also need the private *internal rate of return* (IRR) for an additional year of schooling. The private IRR is defined as the discount rate that equates present discounted value of earnings over the career for different choices of schooling. Using the experience-specific IV estimates of the private returns to education (the scatter plot in Figure 4), we estimate the private IRR to be 7.2%.[22] The private IRR is 2.2 percentage points greater than the social returns to schooling at 5%. From these estimates, we conclude that 70% of the private return to education can be attributed to education raising the productivity of workers and 30% to the signaling value of education.

Alternatively, we can use the estimates of $b_0^{IV} = 0.192$, $b_{\infty}^{IV} = 0.05$ and $\kappa = 0.532$ from Table 2-(3) directly to calculate the private IRR at each $t$, corresponding to the red line in Figure 4. Imposing this learning process and assuming a career length of 40 years, we obtain an estimate of the private IRR of 7.2%. This estimate of the private IRR is identical to the estimate we obtained using the experience-specific IV estimates, and so in both cases we calculate that 30% of the private return to education can be attributed to signaling.

As an additional evidence that education has an effect on a measure of workers' ability, we

[22] For $t > 31$ and beyond retirement age, we assume that experience-specific IV estimates also equal 5%.

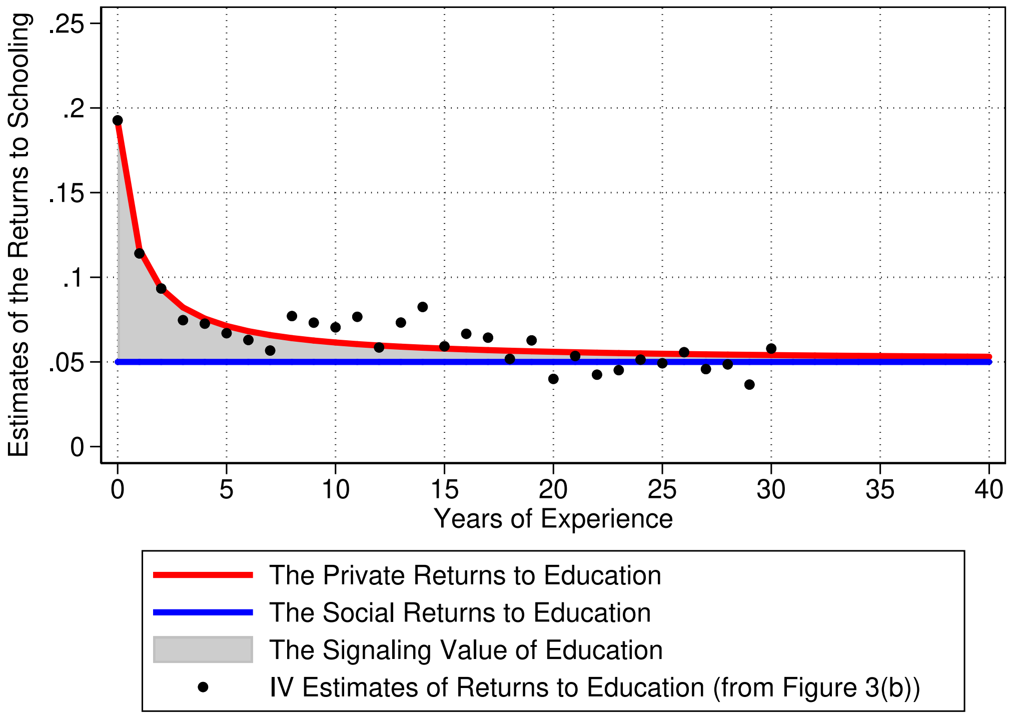



Figure 4: The Private and Social Returns to Education.
*Note:* The private and social returns to education plotted in the red and the blue lines, respectively, are constructed using the estimates in Table 2-(3). The scatter plot of IV estimates of returns to education uses the estimates displayed in Figure 3-(b).

also estimated specifications with the standardized IQ test score as the dependent variable and years of schooling, instrumented using the compulsory schooling reform, as the main independent variable. As presented in Table 3, these IV estimates show strong effects of schooling on IQ, with an additional year of schooling at age 18 causing around 1/4 of a standard deviation increase in IQ.[23] This evidence shows that schooling increases a measure of worker ability, which is expected to be highly correlated with workers' productive skills. While we are cautious in interpreting this as 'direct' evidence on a productivity return to education, these results may indicate that part of the social return to education (see Equation (6)) could arrive through a causal effect on workers' ability.

Finally, we can also compare the estimate of the social returns to education of 5% with

[23] Our IQ test score measure is standardized to have mean zero and a standard deviation (SD) 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 measure is scaled to have a mean of 100 and a SD of 15. In terms of magnitude, their estimate also corresponds to 1/4 of a SD increase in IQ. Carlsson et al. [2015] also document similar results for Sweden.

Table 3: IV Estimates of Years of Schooling on Standardized IQ Test Scores.

| | Full Sample | | Preferred Sample | |
|---|---|---|---|---|
| | Baseline Specification (1) | Trends Specification (2) | Baseline Specification (3) | Trends Specification (4) |
| Reduced Form: | | | | |
| *Exposure to Compulsory Schooling Reform* | 0.041*** | 0.036*** | 0.047*** | 0.037*** |
| | (0.009) | (0.013) | (0.014) | (0.016) |
| IV Estimates: | | | | |
| *Years of Schooling at Age 18* | 0.265*** | 0.235*** | 0.318*** | 0.258** |
| | (0.055) | (0.074) | (0.075) | (0.099) |
| Municipality Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Cohort Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Municipality-Specific Trends | | ✓ | | ✓ |

*Note:* The estimation sample consists of Norwegian males born in 1950-1980 observed any time 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 restricted estimation sample 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). The outcome in each regression is an IQ test score, which is standardized to have a mean of zero and a standard deviation equal to one. All estimations include fixed effects for birth cohort and childhood municipality. The trends specifications in columns (2) and (4) further also controls for municipality-specific trends estimated using data on all pre-reform cohorts born 1930 or later and extrapolated to all post-reform cohorts, separately for each municipality. Standard errors are clustered at the local labor market region (160 groups).
* $p < 0.10$, ** $< 0.05$, *** $p < 0.01$.

a standard Mincer returns to education – that is the (time constant) coefficient on years of schooling in a non-interacted specification controlling for a flexible experience profile. This comparison indicates how much the social returns differ from the observed average differences in earnings in the population and is of interest since the Mincer coefficient is a very commonly used indicator of the value of education. We find that the Mincer coefficient is at 6.8%, exceeding the social returns by 1.8 percentage points.

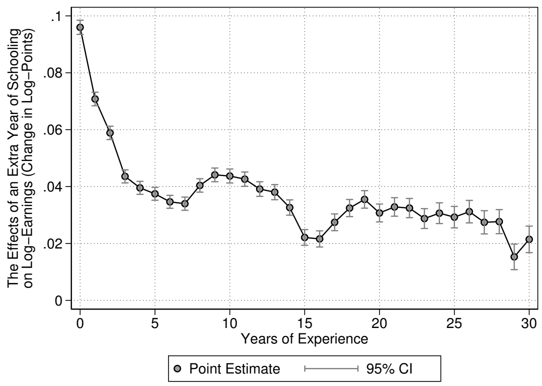
### 5.3 OLS Estimates Using a Hidden Correlate of Ability

Next, we present results from the OLS specification that uses a correlate of ability that is observed by us but not by the employers. We begin by presenting IV-weighted OLS estimates of Equation (17), for each year of work experience, using the standardized IQ test score as the hidden correlate of ability, and after controlling for municipality and cohort fixed effects. Figures 5-(a) and 5-(b) display the IV-weighted OLS estimates of returns to schooling, $b_t^{WOLS}$, and returns to IQ, $c_t^{WOLS}$, respectively. The estimates of $b_t^{WOLS}$ decline rapidly in the first few years before stabilizing, and the estimates of $c_t^{WOLS}$ increase with experience, rapidly at first, and then slowly until stabilizing after 15 years.

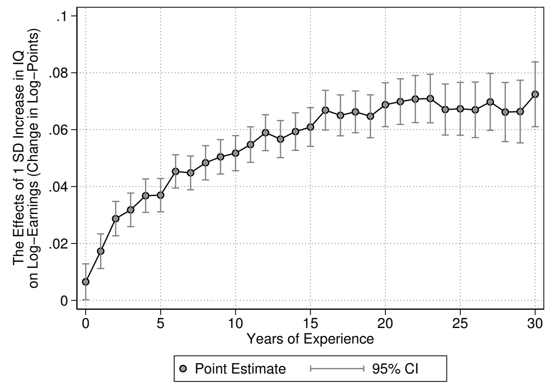
Comparing Figures 5-(a) and 3-(a) we can see that the IV-weighted OLS and the IV estimates of returns to schooling reflect a similar pattern although these two estimators use different sources of variation. It is also noteworthy that the patterns in the returns to schooling and IQ over the workers' careers are surprisingly similarly to those found in the NLSY using the AFQT score. For instance, Figure 1 in Lange [2007] indicates that the returns to schooling decline early in the career and the returns to IQ score increase rapidly before converging to stable, long-run, values after a few years.

Relatedly, Arcidiacono et al. [2010] also use the NLSY data and find that the returns to the AFQT score increases with experience for those with a high school degree or less, and for those with a college degree the returns to the AFQT score are constant over the life-cycle. This led them to conclude that a college degree has a direct role in revealing ability. Such a mechanism could be at play if employers are better informed about the differences in cognitive ability among those with and without a college degree, possibly because they observe transcripts, field of study, reference letters and students have additional work experience, e.g., internships.

When we perform the same analysis as Arcidiacono et al. [2010], i.e., split our estimation sample in two groups –one with at most a high school degree and other with a college degree– we find similar results in our data. Figures 5-(c) and 5-(d) display the OLS estimates of returns to schooling and IQ, respectively, for the first sample and Figures 5-(e) and 5-(f) show the corresponding estimates for the second sample. We can see that the returns to IQ increase with experience but only for those with at most high school degree, and for those with a college degree the returns to IQ are constant at around 6-7% across all years of experience. This pattern is consistent with a college degree revealing a worker's ability also

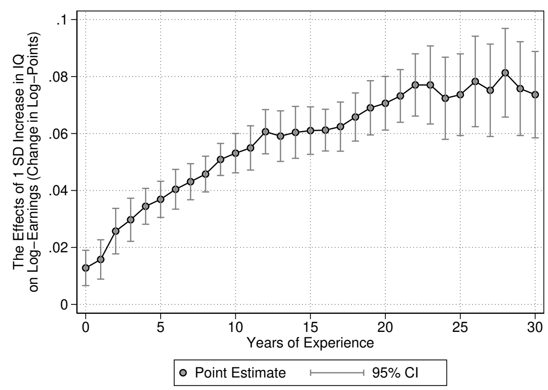



(a) Returns to Schooling: IV-Weighted OLS



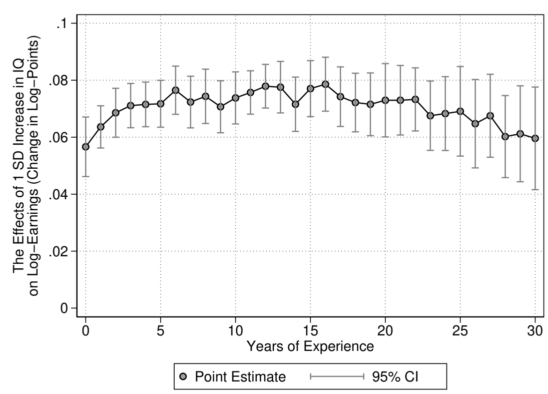

(b) Returns to IQ: IV-Weighted OLS


(c) Returns to Schooling: High School or Below




(d) Returns to IQ: High School or Below


(e) Returns to Schooling: College or University




(f) Returns to IQ: College or University

Figure 5: OLS Estimates of the Returns to Schooling and IQ.

*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).

in the Norwegian labor market.

Next, we compare the IV-weighted OLS estimates to the OLS estimates we obtained from separate estimations for the two groups of workers differentiated by their education attainment. Comparing the estimates of returns to schooling in Figures 5-(a) and 5-(c) and returns to IQ in Figures 5-(b) and 5-(d), we confirm that the IV-weighted OLS estimates are similar to the OLS estimates for workers with a high school degree or less. This is reassuring because the IV-weighted OLS estimates must put substantially more weight on marginal returns in the lower end of the schooling distribution, as shown in Appendix Section A.1. In contrast, estimates for college educated workers display a very different pattern.

Using the IV-weighted OLS estimates of returns to schooling and IQ displayed in Figures 5 (a)-(b), i.e., $\{b_t^{WOLS}, c_t^{WOLS}\}_{t=0}^T$, we can construct additional estimates of the speed of employer learning. As before, from Equation (11) we know that $\{b_t^{WOLS}, c_t^{WOLS}\}_{t=0}^T$ satisfy

$$b_t^{WOLS} = \theta_t b_0^{WOLS} + (1-\theta_t)\, b_\infty^{WOLS} \quad \text{and} \quad c_t^{WOLS} = \theta_t c_0^{WOLS} + (1-\theta_t)\, c_\infty^{WOLS}, \tag{18}$$

where $\{b_0^{WOLS}, c_0^{WOLS}\}$ are the projection coefficients of log-earnings on schooling and ability at the start of a career, and $\{b_\infty^{WOLS}, c_\infty^{WOLS}\}$ are the projection coefficients that would be observed once productivity of individuals was fully revealed in the market. Similarly, using estimates displayed in Figures 5 (c)-(f), we can also construct separate estimates of speed of learning for workers with a high school degree or less and for workers with a college degree.[24]

Table 4 displays estimates of the speed of employer learning, $\kappa$, the initial returns to schooling and IQ, $(b_{s,0}^{WOLS}, b_{z,0}^{WOLS})$, and the limit returns to schooling and IQ, $(b_{s,\infty}^{WOLS}, b_{z,\infty}^{WOLS})$, that we obtained using non-linear least squares for the three sets of OLS estimates of returns to schooling and IQ displayed in Figure 5. Estimates in each panel in Table 4 correspond to one of the three sets of estimates in Figure 5. As noted by Lange [2007], $\kappa$ is over-identified when a hidden correlate of ability is available, so we can construct two different estimates of $\kappa$ based on the OLS estimates of returns to schooling and the OLS estimates of returns to IQ, respectively. If the same learning process drives how schooling and IQ coefficients evolve with worker experience then these two estimates of $\kappa$ should be identical.

[24] Unlike the IV estimates, parameters $\{b_0^{WOLS}, c_0^{WOLS}, b_\infty^{WOLS}, c_\infty^{WOLS}\}$ lack a meaningful interpretation.

Table 4: OLS Estimates of the Speed of Employer Learning, Initial Value and Limit Value.

| | Two Values of $\kappa$ | | One Value of $\kappa$ | |
|---|---|---|---|---|
| | (1) Years of Schooling | (2) IQ Test Score | (3) Years of Schooling | (4) IQ Test Score |
| | A. Full Sample – IV-Weighted OLS | | | |
| Speed of Learning $\kappa$ | $0.386^{***}$ | $0.127^{***}$ | $0.214^{***}$ | |
| | (0.045) | (0.023) | (0.029) | |
| Initial Value ($b_0^{WOLS}, c_0^{WOLS}$) | $0.096^{***}$ | $0.007^{**}$ | $0.084^{***}$ | -0.001 |
| | (0.004) | (0.004) | (0.004) | (0.004) |
| Limit Value ($b_\infty^{WOLS}, c_\infty^{WOLS}$) | $0.024^{***}$ | $0.086^{***}$ | $0.019^{***}$ | $0.077^{***}$ |
| | (0.002) | (0.004) | (0.002) | (0.003) |
| | B. Compulsory/High School Sample – Standard OLS | | | |
| Speed of Learning $\kappa$ | $0.333^{***}$ | $0.065^{***}$ | $0.104^{***}$ | |
| | (0.034) | (0.010) | (0.015) | |
| Initial Value ($b_0^{OLS}, c_0^{OLS}$) | $0.091^{***}$ | $0.012^{***}$ | $0.074^{***}$ | $0.006^{**}$ |
| | (0.003) | (0.002) | (0.003) | (0.003) |
| Limit Value ($b_\infty^{OLS}, c_\infty^{OLS}$) | $0.030^{***}$ | $0.111^{***}$ | $0.017^{***}$ | $0.097^{***}$ |
| | (0.001) | (0.006) | (0.003) | (0.004) |
| | C. College/University Sample – Standard OLS | | | |
| Speed of Learning $\kappa$ | 0.061 | 0.788 | 0.115 | |
| | (0.056) | (0.603) | (0.080) | |
| Initial Value ($b_0^{OLS}, c_0^{OLS}$) | $0.067^{***}$ | $0.056^{***}$ | $0.069^{***}$ | $0.068^{***}$ |
| | (0.003) | (0.004) | (0.003) | (0.002) |
| Limit Value ($b_\infty^{OLS}, c_\infty^{OLS}$) | $0.033^{***}$ | $0.071^{***}$ | $0.039^{***}$ | $0.072^{***}$ |
| | (0.008) | (0.001) | (0.004) | (0.002) |
| Municipality Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Cohort Fixed Effects | ✓ | ✓ | ✓ | ✓ |

*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 estimates of speed of learning, initial values of returns to schooling and IQ, and limit values of returns to schooling are obtained from non-linear least squares estimations on the experience-specific returns to schooling and IQ presented in Figure 5.
* $p < 0.10$, ** $< 0.05$, *** $p < 0.01$.

Alternatively, we can restrict the estimate of $\kappa$ using the returns to schooling to be the same as the $\kappa$ using the returns to IQ. We implement both methods and present the estimation results in Table 4. Estimates that allow for differential learning are in columns (1)-(2) and the estimates that impose a common learning process are in columns (3)-(4).

Consistent with what we found in Figure 5, here too we find similar estimates of employer learning, initial returns to schooling and IQ and limit returns to schooling and IQ across panels A and B in Table 4. As earlier, in panel A we show results using the IV-weighted OLS estimates, while in panel B we use OLS estimates for workers with a high school degree or below. Moreover, the estimates of returns to schooling and speed of employer learning in panels A-B, column (1), which allow for a differential learning process across schooling and IQ, are also comparable to the corresponding IV estimates we presented in Table 2, column (1). At conventional levels, we cannot reject the equality of the speed of learning $\kappa$ across these OLS estimates and the IV estimates presented earlier.

There are however two striking differences among the estimates in Table 4. First, comparing estimates of the speed of learning $\kappa$ across columns (1)-(2), we can reject the assumption of a common learning process for schooling and IQ over worker experience.[25] This result suggests that a standard assumption made in the OLS approach that uses a hidden correlate of ability to identify the speed of employer learning is violated in our context. Second, we do not find any evidence of employer learning for workers with a college degree as shown in panel C in Table 4, unlike the results shown in panels A-B.

# 6 Extensions

In this section, we consider two important extensions of our model. First, we consider an environment where the returns to skill can vary with experience. The standard employer learning model assumes that log-productivity is additively separable in experience and education. As a consequence, the social return is constant over time, and the only reason for private returns to vary over time is employer learning. Now, if there is differential on-the-job growth in productivity across workers with different skills [Arcidiacono et al., 2010], then the social returns will also vary with experience. Second, we acknowledge that there is increasing evidence that returns to education are heterogeneous (Card [1999]; Heckman et al. [2006a];

[25] In Section 6.2, we consider an environment with heterogeneous returns to education and discuss implications for differential employer learning across schooling choices in a potential outcomes framework.

Carneiro et al. [2011]; among others). We therefore consider an environment with heterogeneous returns to education and define private and social returns within a potential outcomes framework. For each of these two model extensions, we then determine data requirements and assumptions that are sufficient for identification of key parameters of interest.

## 6.1 Time-Varying Returns

Here, we generalize of our model by allowing the returns to skill to vary flexibly with experience. We show that having access to hidden IV allows identifying the private returns to education, and with transparent IV we can identify the (time-varying) social returns to education. If we have both, hidden and transparent, IVs then that is sufficient to identify key model parameters, including the signaling value of education and the speed of learning. Only a hidden IV, however, is insufficient to determine the speed of learning.

To this end, we start by extending the log-productivity in (1) to

$$\psi_{it} = \lambda_t \times [\beta_{ws} S_i + \beta_{wq} Q_i + A_i] + H(t) + \varepsilon_{it}, \tag{19}$$

where $\lambda_t < \infty$ is an experience-specific slope parameter, allowing the effects of workers' composite ability, $(\beta_{ws} S_i + \beta_{wq} Q_i + A_i)$, on log-productivity to vary flexibly with $t$.[26]

Following the same steps that lead to (6) and (7), while suppressing $H(t)$ for notational convenience, we can express log-productivity as $\psi_{it} = \lambda_t \delta^{\psi|S} S_i + u_{it}$, and log-wages as $\ln W_{it} = \lambda_t \delta_t^{W|S} S_i + \tilde{u}_{it}$, where $\delta^{\psi|S}$ and $\delta_t^{W|S}$ are defined as in (6) and (7), respectively. As can be seen, both private returns to education, $\frac{\partial \ln W_{it}}{\partial S} = \lambda_t \delta_t^{W|S}$, and the social returns to education, $\frac{\partial \psi_{it}}{\partial S} = \lambda_t \delta^{\psi|S}$, vary over time. Nonetheless, the relationship between the two remains as in (8). In particular, the private return is the sum of social return and an adjustment term, which now depends on both $\theta_t$ and $\lambda_t$:

$$\lambda_t \delta_t^{W|S} \;\; = \;\; \lambda_t \delta^{\psi|S} + \lambda_t \theta_t \left( \phi_{A|S} + \phi_{A|Q} \delta^{Q|S} - \delta^{A|S} \right), \tag{20}$$

[26] Our specification is similar to Equation (2) in Arcidiacono et al. [2010].

such that the adjustment term $\lambda_t\theta_t\left(\phi_{A|S}+\phi_{A|Q}\delta^{Q|S}-\delta^{A|S}\right)$ vanishes with $t$ because $\lambda_t<\infty$, and $\lim_{t\to\infty}\theta_t=0$. Thus, for workers with long work experience $\lim_{t\to\infty}\frac{\partial \ln W_{it}}{\partial S}=\lim_{t\to\infty}\frac{\partial \chi_{it}}{\partial S}$.

Now, in order to identify the signaling value of education, we first have to separately identify the effect of employer learning $\theta_t$ and the effect of time-varying productivity returns $\lambda_t$ on log-wages at each $t$. Let $D_i^{\mathfrak{h}}\in\{0,1\}$ denote a hidden IV that satisfies Assumptions 1 and 2, and let $D_i^{\mathfrak{t}}\in\{0,1\}$ denote a transparent IV that satisfies Assumptions 1 and 3. As before, $D^{\mathfrak{h}}$ identifies the private returns to education:

$$\texttt{plim}\ \hat{b}_t^{IV^{\mathfrak{h}}} \quad = \quad \frac{\mathbb{E}\left[\ln W_{it}|D_i^{\mathfrak{h}}=1,t\right]-\mathbb{E}\left[\ln W_{it}|D_i^{\mathfrak{h}}=0,t\right]}{\mathbb{E}\left[S_i|D_i^{\mathfrak{h}}=1,t\right]-\mathbb{E}\left[S_i|D_i^{\mathfrak{h}}=0,t\right]}=\lambda_t\times\delta_t^{W|S}. \tag{21}$$

Similarly, if we have only a transparent instrument then we can identify the social returns

$$\texttt{plim}\ \hat{b}_t^{IV^{\mathfrak{t}}} \quad = \quad \frac{\mathbb{E}\left[\ln W_{it}|D_i^{\mathfrak{t}}=1,t\right]-\mathbb{E}\left[\ln W_{it}|D_i^{\mathfrak{t}}=0,t\right]}{\mathbb{E}\left[S_i|D_i^{\mathfrak{t}}=1,t\right]-\mathbb{E}\left[S_i|D_i^{\mathfrak{t}}=0,t\right]}=\lambda_t\times\delta^{\psi|S}. \tag{22}$$

But if we also want to identify the speed of learning and the time-varying component of the productivity $\lambda_t$, just having a hidden IV is not enough. Heuristically, comparing (13) and (21) we can see that because the social return now varies with time, hidden IV is insufficient to identify the signaling value of education, even with $t\to\infty$. So to identify the signaling value we have to separately identify the learning process from the time-varying returns to education due to differential productivity growth. And for that transparent IV is sufficient.

From equations (21) and (22) for any $t$, we get

$$\texttt{plim}\ \Delta_{IV,t}:=\frac{\texttt{plim}\ \hat{b}_t^{IV^{\mathfrak{h}}}-\texttt{plim}\ \hat{b}_t^{IV^{\mathfrak{t}}}}{\texttt{plim}\ \hat{b}_t^{IV^{\mathfrak{t}}}}=\frac{\delta_t^{W|S}-\delta^{\psi|S}}{\delta^{\psi|S}}. \tag{23}$$

Evaluating (23) at $\{t,t'\neq t\}$, simplifying by using (20) and taking their ratios gives $\frac{\texttt{plim}\ \Delta_{IV,t}}{\texttt{plim}\ \Delta_{IV,t'}}=\frac{\theta_t}{\theta_{t'}}$, which using the definition of $\theta_t$ identifies the speed of learning as

$$\kappa=\frac{\texttt{plim}\ \Delta_{IV,t'}-\texttt{plim}\ \Delta_{IV,t}}{(t-1)\times\texttt{plim}\ \Delta_{IV,t}-(t'-1)\times\texttt{plim}\ \Delta_{IV,t'}}. \tag{24}$$

Thus, changes in the relative difference between estimates of the private and the social returns to education across two time periods can identify the speed of employer learning.

For empirical implementation, we can use $(t', t) = (0, 1)$. Once we make the location normalization $\lambda_0 = 1$, we recover the time-invariant part of the social return using transparent IV on log-wages, at the initial period from (22), i.e., $\texttt{plim}\ \hat{b}_0^{IV^{\mathfrak{t}}} = \delta^{\psi|S}$. From the definition of $\theta_t$ we know that $\theta_0 = 1$, so we can also identify the time-invariant part of the adjustment term using the difference between estimates of returns to education using a hidden and a transparent IV at the initial period from (20), i.e., $\texttt{plim}\ \hat{b}_0^{IV^{\mathfrak{h}}} - \texttt{plim}\ \hat{b}_0^{IV^{\mathfrak{t}}} = (\phi_{A|S} + \phi_{A|Q}\delta^{Q|S} - \delta^{A|S})$. Once we have the estimates, $(\hat{b}_0^{IV^{\mathfrak{h}}}, \hat{b}_1^{IV^{\mathfrak{h}}}, \hat{b}_0^{IV^{\mathfrak{t}}}, \hat{b}_1^{IV^{\mathfrak{t}}})$, we can plug them in (24) to first identify $\kappa$. Additionally, with estimates $\hat{b}_t^{IV^{\mathfrak{h}}}$ for $t > 1$, we can identify each component in (20).

Finally, as a corollary of the above identification results, we note that a transparent IV can help us test the log-separability assumption embedded in the standard models of employer learning, including ours in Section 2 by exploiting the following features. Importantly, using a transparent IV alone we can identify the (time-varying) social returns to education at each $t$, however, this is not sufficient to identify the speed of learning, or the signaling value of education. In contrast, under the log-separability of education and experience, a hidden IV is sufficient for identification of key model parameters, as we showed in Section 3.

## 6.2 Heterogeneous Returns

While we have only considered homogeneous returns to education, there is an increasing recognition in the empirical literature that the returns can be heterogeneous across workers. The advantages of having homogeneous returns in our employer learning model were that it allowed us to keep the model tractable, and provide intuitive and informative characterization of its empirical content. For instance, we were able to capture the learning process, the social returns and the private returns, all with only one parameter each, and were also able to compare our estimates and synthesize results across different studies and approaches. These benefits notwithstanding, it is important to consider how our identification strategies would change if the returns were heterogeneous. In Appendix Section A.2 we analyze this question using the Neyman-Rubin potential outcomes framework with binary schooling choices, e.g., high or low schooling. For brevity, here we only present a summary of our main findings.

As before, we start by allowing workers' productivity to depend on whether they obtain high or low schooling. We now define the social returns to be the differences in potential productivities across different choices of schooling, and similarly, define the private returns to be the differences in potential wages across different choices of schooling. Thus, both measures of returns become worker-specific, and if we aggregate these returns across appropriate subset of workers, we can define the *average social returns* and the *average private returns*, or the *social returns for the treated* and the *private returns for the treated*, respectively.

Since Imbens and Angrist [1994] it is well-known that with heterogeneous returns an IV identifies the local average treatment effect for those who are induced by the IV to switch their schooling status. In particular, if the IV satisfies an appropriate monotonicity condition then it identifies the treatment effect on the compliers, and hence, the causal effect of schooling on workers who do not obtain schooling in the absence of the IV but obtain schooling when "exposed" to the IV. We add to this result by showing that a hidden IV identifies the private returns on the compliers, and that a transparent IV identifies the social returns on the compliers. Furthermore, we show that even with heterogeneity private returns converge to the social returns, as employers learn. And thus, for compliers with finite years of experiences, the hidden IV identifies the private returns, and for those with long experience it identifies the social and private returns.

The identification of the speed of learning, however, poses additional challenges, and requires additional assumptions. The reason for this difficulty is that now, with heterogeneous returns, the speed of learning can depend on the level of schooling, and the interpretation of the earnings profile depends on whether we assume that it is easier for employers to learn the abilities of those who have high schooling or of those who have low schooling. For instance, all else equal, a worker with unusually high skills can have higher returns associated with a schooling level, and about whom employers learn (and reward) quickly. To make progress, we assume that the conditional distribution of productivity, given schooling is normal.

We show that the hidden IV estimates of returns to schooling, at each experience level, can be expressed as a weighted linear combination of these schooling level-specific learning parameters, where the weights depend on the extent of selection on unobserved productivity, among the compliers. We can use this system of linear equations to identify the weights and the speed of learning, as long as there is some selection between unobservable ability and school choice. Heuristically, the selection plays the role of a rank condition, which is necessary to "invert" a system of linear equations to identify unknown coefficients. For instance, if there was no selection and the schooling decisions were uncorrelated with unobserved ability, then this would lead to "bunching" in schooling and we will fail to infer the effect of employers learning on wages. When there is selection, and if we assume that the speed of learning for higher schooling level is faster than for low schooling and if this difference in speed is constant over life-cycle, then we can identify the speed of learning.

Thus, our main results about the identification private and social returns under homogeneity carry through to a model with heterogeneous returns to education, with a caveat that under latter we can only estimate average (private or social) returns for the compliers. The learning process, however, differs substantially across these two regimes. In particular we show that if the compliers are positively selected, then the private returns include a positive information return, assuming that the speed of learning increases with schooling, as in Arcidiacono et al. [2010]. So, the identification of the learning parameters becomes more involved when learning differs across schooling levels.

# 7 Conclusion

Education policy hinges on the estimates of private and social returns to education, but these returns are notoriously difficult to estimate separately. In this paper, we determine conditions under which instrumental variables 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 the employers and correctly factored in the wages. We show that hidden IV identifies the private returns to education. If log-wage profiles are additively separable in experience and schooling, hidden IV also help identify the social returns to education. A transparent IV, by contrast, identifies the social returns to education at each level of work experience. Building on this distinction between hidden and transparent IVs, we propose a strategy to identify the returns to education that can be attributed to job market signaling.

Using data from Norway we estimate that the causal effect of schooling on productivity, i.e., the social return to schooling, is 5% and the private return is 7.2%. The difference between the two is attributable to the signaling value of education. In other words, we estimate that 70% of the total private returns to education accrues to human capital and 30% accrues to signaling. Our estimates also suggest employers learn workers' ability quickly.

We conclude this paper by pointing out a few shortcomings of our model. First, our baseline specification of the employer learning model following Farber and Gibbons [1996] and Altonji and Pierret [2001] assumes that the log-productivity is additively separable in schooling, ability, and experience. While this assumption naturally emerges from various formulations of the human capital model (e.g., the Ben-Porath model), it is still restrictive. Moreover, the data patterns that are taken as evidence of employer learning [Lange, 2007] are also compatible with other calibrations of human capital models [Kaymak, 2014]. While we have taken steps (see Section 6.1) towards addressing these shortcomings, more work is needed in this area, especially in settings where researchers have access to *both* hidden and transparent instruments. In such settings, researchers can apply our identification results to estimate the speed of learning and the signaling value of education.

Secondly, throughout our paper we have maintained an assumption of symmetric learning across employers. In other words, our analysis assumes that switching jobs does not affect the speed of learning. This 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 from wage changes across jobs that incumbent employers have an information advantage [Gibbons and Katz, 1991; Kahn, 2013], which in turn can lead to different speed of learning and different wage profiles; see Pinkston [2009]. The learning process becomes significantly more complex when there is asymmetric learning, and when workers and employers act strategically based on this asymmetry. In such settings, it is thus quite challenging to determine the implications for social and private returns to education.

Having noted these limitations, we do believe that instrumental variable estimates can provide useful information on the debate between proponents of the human capital and signaling models of the returns to education. We hope that future researchers estimating instrumental variable estimates of the returns to education will, as a matter of course, discuss how informed employers are about the instruments and what this implies for interpreting the instrumental variable estimates that are provided.

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# Appendix

## A.1 Non-Linear Returns to Education

When the true relationship between log-earnings and schooling is non-linear, the marginal effects of schooling on log-earnings differ across the support of schooling distribution. In such settings, comparisons of OLS and IV estimates are complicated because linear OLS and IV estimators typically identify different weighted averages of the marginal effects of schooling.[27] It is, however, possible to re-weight margin-specific OLS estimates and construct IV-weighted OLS estimates that are comparable to the IV estimates.

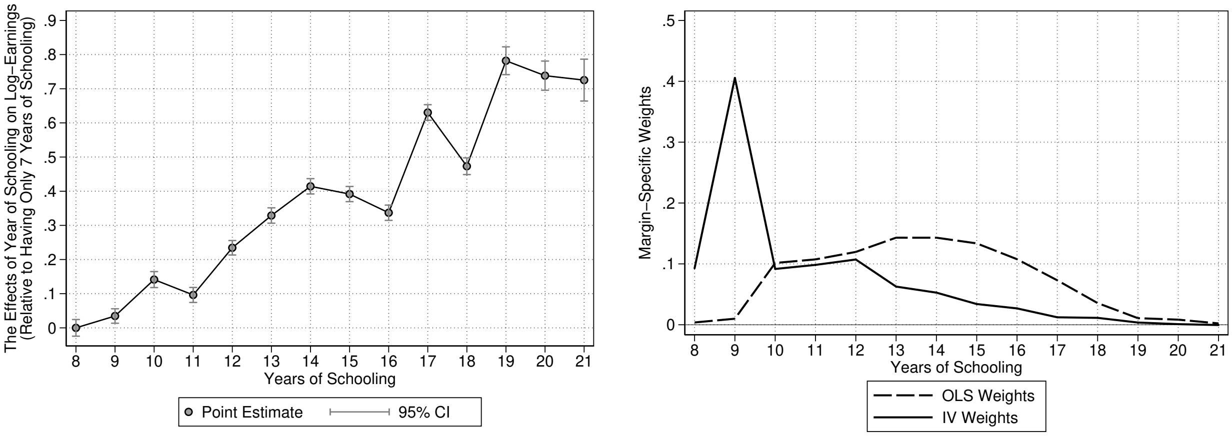
Let $b_t^{WOLS}$ denote such IV-weighted OLS estimates. Consider the following non-linear relationship between log-earnings and schooling:

$$\begin{aligned} \ln W_{it} &= a_t^{OLS} + \sum_{s=8}^{21} \gamma_{s,t}^{OLS} \times \mathbb{1}(S_i \geq s) + c_t^{OLS} Z_{it} + e_t X_i + u_{it}^{OLS}; \\ b_t^{WOLS} &= \sum_{s=8}^{21} \gamma_{s,t}^{OLS} \times \pi_s^{IV}; \qquad \pi_s^{IV} = \frac{\text{cov}\left(\mathbb{1}(S_i \geq s), D_i\right)}{\text{cov}\left(S_i, D_i\right)}, \end{aligned} \tag{A.1}$$

where $\mathbb{1}(S_i \geq s)$ is an indicator for having at least $s$ years of schooling, and $D_i \in \{0, 1\}$ is a binary instrument which equals 1 if individual $i$ was exposed to the compulsory schooling reform. The parameter $b_t^{WOLS}$ is a weighted sum of margin-specific OLS estimates $\gamma_{s,t}^{OLS}$ from a non-linear relationship between schooling and log-earnings, using weights $\pi_s^{IV}$ that mimic the variation exploited by the IV estimator. Intuitively, the IV estimates emphasize the marginal effects of schooling for those that are most affected by the instrument.

Using the specification in Equation (A.1), we can thus construct IV-weighted OLS estimates of returns to education. There are two components that determine the differences between these estimates and the standard linear OLS estimates. One is the extent of non-linearity in the margin-specific OLS estimates $\gamma_{s,t}^{OLS}$, and the other one is the differences

[27] See, e.g., discussions in Angrist and Imbens [1995]; Angrist and Krueger [1999]; Heckman et al. [2006b]; Løken et al. [2012] and Mogstad and Wiswall [2016].




(a) Non-Linear Returns to Schooling (b) Margin-Specific OLS and IV Weights

Figure A.1: Non-Linear Returns to Schooling and Margin-Specific OLS and IV Weights.
*Note:* Panel (a) plots OLS estimates of returns to schooling at 10-20 years of experience from a specification with dummies for each year of schooling, controlling for cohort and childhood municipality fixed effects, and flexible time trends. The estimation sample consists of Norwegian males born 1950-1980 observed in earnings data over years 1967-2014 with years of experience between 10 and 20 years and annual earnings above 1 SGA threshold. The estimates show the returns to each year of schooling relative to 7 years of compulsory schooling. Panel (b) plots the margin-specific OLS and IV weights at each year of schooling.

between the IV weights $\pi_s^{IV}$ and the corresponding OLS regression weights. In Figure A.1-(a), we display the (stacked average of) estimates $\gamma_{s,t}^{OLS}$ at experiences $t = (10, .., 20)$ for each additional year of schooling relative to 7 years of compulsory schooling, which illustrates the non-linear relationship between years of schooling and log-earnings in our data.

Next, in Figure A.1-(b), we display the margin-specific IV weights $\pi_s$ that are used to obtain estimates $b_t^{WOLS} = \sum_s \gamma_{s,t}^{OLS} \pi_s^{IV}$. We also display the margin-specific weights for a standard linear OLS regression, and as expected these weights differ substantially from the IV weights. In particular, the IV places substantially more weight on the marginal effects of schooling in the lower end of the schooling distribution. This is consistent with the compulsory schooling reform instrument triggering changes in schooling attainment mainly at the lower end of the schooling distribution.

## A.2 Heterogeneous Returns to Education

In this section, we extend our model to allow heterogeneous returns to education, and determine conditions under which we can use IVs to identify key model parameters. Using the binary potential outcomes framework of Neyman-Rubin, let us assume that schooling takes two values, $S_i \in \{0,1\}$, where $S_i = 1$ (respectively, 0) denotes a higher (respectively, lower) level of schooling. A worker $i$ is characterized by a random vector of potential outcomes $\{\{\psi_{0,i}, Q_{0,i}\}, \{\psi_{1,i}, Q_{1,i}\}\}$, where $\psi_{S,i}$ denotes the time-invariant component of $i$'s productivity and $Q_{S,i}$ is the skill correlate observed by employers, given $i$'s choice of $S_i$. Thus, the differences in abilities $A_i$ in (1) are now subsumed in the two potential outcomes $\{\psi_{0,i}, \psi_{1,i}\}$.

Furthermore, without loss of generality we can normalize the latent variable (hidden correlate) $Q_{S,i}$ to be in productivity units, and express $i$'s productivity with schooling $S_i$ as

$$\psi_{S,i} = Q_{S,i} + \tilde{\psi}_{S,i}; \quad \tilde{\psi}_{S,i} \perp Q_{S,i}, \tag{A.2}$$

by projecting $\psi_{S,i}$ onto $Q_{S,i}$, for those with $S_i = S$, and normalizing the coefficient of $Q_{S,i}$ to be one and treating $\tilde{\psi}_{S,i}$ as the "residual." Thus, we can treat $\tilde{\psi}_{S,i}$ as the productivity differences across workers that are unobserved by the employers, which they learn about using $i$'s past outputs that are noisy measures of worker productivity. In particular, using the potential outcomes defined above, we can express realized productivity $\psi_{i,t}$ at time $t$ as:

$$\psi_{i,t} \;\; = \;\; S_i \times \left[Q_{1,i} + \tilde{\psi}_{1,i} + \varepsilon_{1,i,t}\right] + (1 - S_i) \times \left[Q_{0,i} + \tilde{\psi}_{0,i} + \varepsilon_{0,i,t}\right] + H(t), \tag{A.3}$$

where $\varepsilon_{S,i,t} \overset{i.i.d}{\sim} \mathcal{N}\left(0, \sigma^2_{\varepsilon,S}\right)$ are mean-zero "noise" in the production process that are independent of the model primitives, with possibly schooling dependent variances. For notational ease, we suppress $H(t)$ in the following.[28]

Worker $i$ knows his potential outcomes $\{\psi_{0,i}, Q_{0,i}, \psi_{1,i}, Q_{1,i}\}$, but employers only observe

[28]Note that productivity $\psi_{i,t}$ is expressed in levels and not in logs, because with this level of generality it is easier to work in levels. And as in Section 2, we maintain that $S$ and $H(t)$ are additively separable.

$(S_i, Q_{S,i}, \psi_i^t)$, where $\psi_i^t = \{\psi_{i,\tau}\}_{\tau<t}$, and $(Q_{S,i}, \psi_i^t)$ are observed only for $S_i = S$. Note that observing $\psi_{i,t}$, conditional on $S_i$ and having observed $Q_{S,i}$, is informationally equivalent to observing $\xi_{S,i,t} = \tilde{\psi}_{S,i} + \varepsilon_{S,i,t}$, i.e., a noisy measure of workers' unobserved ability. We can thus denote the employers' information set by $\mathcal{E}_{i,t}^S = (S_i, Q_{S,i}, \xi_{S,i}^t)$, where $\xi_{S,i}^t = \{\xi_{S,i,\tau}\}_{\tau<t}$. Wages are set equal to the expected productivity, conditional on information $\mathcal{E}_{i,t}^S$, i.e.,

$$\begin{aligned} W_{i,t} &= \mathbb{E}\left[\psi_{i,t}\middle|\mathcal{E}_{i,t}^S\right] = S_i \times \underbrace{\left[Q_{1,i} + \mathbb{E}\left[\tilde{\psi}_{1,i}\middle|\mathcal{E}_{i,t}^1\right]\right]}_{:=W_{1,i,t}} + (1-S_i) \times \underbrace{\left[Q_{0,i} + \mathbb{E}\left[\tilde{\psi}_{0,i}\middle|\mathcal{E}_{i,t}^0\right]\right]}_{:=W_{0,i,t}} \\ &= S_i \times W_{1,i,t} + (1-S_i) \times W_{0,i,t}, \end{aligned} \tag{A.4}$$

where the second equality follows from the definition $\psi_{i,t}$ in (A.3) and the independence and zero-mean properties of $\varepsilon_{S,i,t}$, and $W_{S,i,t}$ denotes potential wage outcomes for different $S_i$. We can define the social returns and the private returns to schooling for $i$, respectively, as

$$\delta_i^{\psi|S} := \psi_{1,i} - \psi_{0,i} = (Q_{1,i} - Q_{0,i}) + \left(\tilde{\psi}_{1,i} - \tilde{\psi}_{0,i}\right); \tag{A.5}$$

$$\delta_{i,t}^{W|S} := W_{1,i,t} - W_{0,i,t} = (Q_{1,i} - Q_{0,i}) + \mathbb{E}\left[\tilde{\psi}_{1,i}|\mathcal{E}_{i,t}^1\right] - \mathbb{E}\left[\tilde{\psi}_{0,i}|\mathcal{E}_{i,t}^0\right]. \tag{A.6}$$

Note that both the social returns $\delta_i^{\psi|S}$ and the private returns $\delta_{i,t}^{W|S}$ are individual-specific. The *average social returns* and *average private returns* are then the population averages of (A.5) and (A.6), respectively, while measures such as *social returns for the treated* and *private returns for the treated* are averages across the corresponding populations.

## Instrumental Variables

To understand what a binary instrument identifies, we proceed analogous to Imbens and Angrist [1994]. Let $S_i(D_i)$ denote schooling conditional on $D_i \in \{0,1\}$, and define compliers as $\mathbb{C} \equiv \{i|S_i(1) = 1, \text{and } S_i(0) = 0\}$ and defiers as $\mathbb{D} \equiv \{i|S_i(1) = 0, \text{and } S_i(0) = 1\}$. Similarly, we can define always-takers to be $\mathbb{A} \equiv \{i|S_i(1) = 1, \text{and } S_i(1) = 1\}$ and never-takers to be $\mathbb{N} \equiv \{i | S_i(1) = 0, \text{and } S_i(0) = 0\}$. Then, as before, the Wald estimator gives

$$\texttt{plim}\ \hat{b}_t^{IV} \quad := \quad \frac{\mathbb{E}[W_{i,t}|D_i = 1] - \mathbb{E}[W_{i,t}|D_i = 0]}{\mathbb{E}[S_i|D_i = 1] - \mathbb{E}[S_i|D_i = 0]}. \tag{A.7}$$

As $D$ satisfies monotonicity we get $\Pr(\mathbb{D}) = 0$, so we can write (A.7)'s denominator as

$$\begin{aligned}
\mathbb{E}[S_i|D_i = 1] - \mathbb{E}[S_i|D_i = 0] &= (\mathbb{E}[S_i|D_i = 1, \mathbb{A}] - \mathbb{E}[S_i|D_i = 0, \mathbb{A}]) \times \Pr(\mathbb{A}) \\
&+ (\mathbb{E}[S_i|D_i = 1, \mathbb{N}] - \mathbb{E}[S_i|D_i = 0, \mathbb{N}]) \times \Pr(\mathbb{N}) \\
&+ (\mathbb{E}[S_i|D_i = 1, \mathbb{C}] - \mathbb{E}[S_i|D_i = 0, \mathbb{C}]) \times \Pr(\mathbb{C}) \\
&+ (\mathbb{E}[S_i|D_i = 1, \mathbb{D}] - \mathbb{E}[S_i|D_i = 0, \mathbb{D}]) \times \Pr(\mathbb{D}) = \Pr(\mathbb{C}). \quad \text{(A.8)}
\end{aligned}$$

As in Section 3.3, we consider two types of binary instruments: a hidden IV, $D_i^{\mathfrak{h}} \in \{0, 1\}$, and a transparent IV, $D_i^{\mathfrak{t}} \in \{0, 1\}$. With a hidden IV, we also know that $i$'s wage conditional on employer information $\mathcal{E}_{i,t}^S$ does not depend on the IV itself. Thus, for $D_i^{\mathfrak{h}}$, we have

$$\begin{aligned}
\mathbb{E}[W_{i,t}|D_i^{\mathfrak{h}} = 1] - \mathbb{E}[W_{i,t}|D_i^{\mathfrak{h}} = 0] &= \Big(\mathbb{E}[W_{i,t}|D_i^{\mathfrak{h}} = 1, \mathbb{C}] - \mathbb{E}[W_{i,t}|D_i^{\mathfrak{h}} = 0, \mathbb{C}]\Big) \times \Pr(\mathbb{C}) \\
&= \Big(\mathbb{E}[W_{1,i,t}|D_i^{\mathfrak{h}} = 1, \mathbb{C}] - \mathbb{E}[W_{0,i,t}|D_i^{\mathfrak{h}} = 0, \mathbb{C}]\Big) \times \Pr(\mathbb{C}) \\
&= (\mathbb{E}[W_{1,i,t}|\mathbb{C}] - \mathbb{E}[W_{0,i,t}|\mathbb{C}]) \times \Pr(\mathbb{C}) = \mathbb{E}[\delta_{i,t}^{W|S}|\mathbb{C}] \times \Pr(\mathbb{C}). \quad \text{(A.9)}
\end{aligned}$$

The first equality follows from the law of total expectation, the second from the definition of a complier and substituting for the potential outcomes from (A.4), the third from the properties of a hidden IV, and the last from the definition of private returns in (A.6). Using (A.8) and (A.9) in (A.7) with a hidden IV shows that $\texttt{plim}\ \hat{b}_t^{IV^{\mathfrak{h}}} = \mathbb{E}\left[\delta_{i,t}^{W|S}|\mathbb{C}\right]$, i.e., the Wald estimator using a binary hidden IV identifies the private returns to education for compliers.

Next, we consider the identification with transparent IV, $D_i^{\mathfrak{t}} \in \{0, 1\}$. Wages equal expected productivity given employer information $(\mathcal{E}_{i,t}^S, D_i^{\mathfrak{t}})$, i.e., $W_{i,t} = \mathbb{E}[\psi_{i,t}|\mathcal{E}_{i,t}^S, D_i^{\mathfrak{t}}]$ and $\mathbb{E}[W_{i,t}|D_i^{\mathfrak{t}}] = \mathbb{E}[\mathbb{E}[\psi_{i,t}|\mathcal{E}_{i,t}^S, D_i^{\mathfrak{t}}]|D_i^{\mathfrak{t}}] = \mathbb{E}[\psi_{i,t}|D_i^{\mathfrak{t}}]$, which follows from the law of total expectation. Conditional on $D_i^{\mathfrak{t}}$, the average compensation equals the average product, and hence

$$\begin{aligned}\mathbb{E}\left[W_{i,t}|D_i^{\mathfrak{t}}=1\right]-\mathbb{E}[W_{i,t}|D_i^{\mathfrak{t}}=0] &= \mathbb{E}[\psi_{i,t}|D_i^{\mathfrak{t}}=1]-\mathbb{E}[\psi_{i,t}|D_i^{\mathfrak{t}}=0]=\mathbb{E}[\psi_{1,i}-\psi_{0,i}|\mathbb{C}]\times \Pr(\mathbb{C}) \\ &= \mathbb{E}[\delta_i^{\psi|S}|\mathbb{C}]\times \Pr(\mathbb{C})\,. \qquad (A.10)\end{aligned}$$

Using (A.8) and (A.10) in (A.7) with a transparent IV shows that $\texttt{plim}\ \hat{b}^{IV^{\mathfrak{t}}}=\mathbb{E}[\delta_i^{\psi|S}|\mathbb{C}]$, i.e., the Wald estimator using a binary transparent IV identifies the (constant) social return to education for compliers. Therefore, as in the case with homogeneous returns, a transparent IV identifies the social returns and a hidden IV identifies the private returns to education, albeit now these returns are estimated only for the compliers. Note that this identification result requires fewer assumptions than under homogeneous returns.

As a corollary to the above identification results, we note the following. In settings where researchers have access to *both* a hidden IV and a transparent IV it is also possible to identify a signaling value of education using $\texttt{plim}\ \hat{b}_t^{IV^{\mathfrak{h}}}$ and $\texttt{plim}\ \hat{b}^{IV^{\mathfrak{t}}}$, as long as compliers associated with each IV have identical average potential outcomes. The latter assumption allows us to extrapolate outcomes from one set of compliers to the other, and thus, identify a local signaling value of education. If, however, we have access to only a hidden IV, then to apply the same idea as we proposed earlier for homogeneous returns in (14), we have to verify if $\lim_{t\to}\texttt{plim}\ \hat{b}_t^{IV^{\mathfrak{h}}}=\mathbb{E}(\delta_i^{\psi|S}|\mathbb{C})$, which in turn requires us to model the speed of learning.

## The Speed of Learning

Next, we consider the identification of the speed of learning by determining how quickly information about workers is revealed in the market and reflected in their compensations. But unlike with homogeneous returns, with heterogeneous returns, the speed of learning will depend on schooling and on the selection between schooling and unobserved ability. So, to identify these speeds of learning across schooling choices, additional assumptions are needed.

One such assumption is about the conditional distribution of productivity, given $S_i$ that arises in equilibrium. Even though we do not model workers' schooling decisions, to model the learning process of the employers we need the productivity distribution. Let the unobserved components of productivity, conditional on schooling $S$, follow a bivariate normal distribution, i.e., $\left(Q_{S,i}, \tilde{\psi}_{S,i}\right) \sim \mathcal{N}\left((\mu_{Q,S}, \mu_{\psi,S})^{\top}, \Sigma\right)$, where $\Sigma$ is the diagonal vairance-covairance matrix with conditional variances $\sigma^2_{Q,S}$ and $\sigma^2_{\psi,S}$, and from (A.2) $Cov(Q_{S,i}, \tilde{\psi}_{S,i}|S) = 0$.[29] This assumption is strong because it is imposed directly on the outcomes of a model where workers sort into schooling levels based on their abilities, and not only on the primitives.

Recall that wages are set equal to expected productivity conditional on employers' information set $\mathcal{E}^S_{i,t} = (S_i, Q_{S,i}, \xi^t_{S,i})$, where $\xi^t_{S,i} = \{\xi_{S,i,\tau}\}_{\tau<t}$. Using the potential wage outcomes defined in (A.4), and the Kalman property as in (4), we can thus express wages as

$$W_{S,i,t} = Q_{S,i} + \mathbb{E}\left[\tilde{\psi}_{S,i}|\mathcal{E}^S_{i,t}\right] = Q_{S,i} + \theta_{S,t} \times \mu_{\psi,S} + (1-\theta_{S,t}) \times \bar{\xi}^t_{S,i}, \tag{A.11}$$

where for a schooling level $S$, $\theta_{S,t} := \frac{1-\kappa_S}{1+(t-1)\kappa_S}$ is the weight that characterize the learning process, and $\kappa_S := \frac{\sigma^2_{\psi,S}}{\sigma^2_{\psi,S}+\sigma^2_{\varepsilon,S}}$ is the speed of learning parameters, and $\bar{\xi}^t_{S,i} = \frac{1}{t}\sum_{\tau<t}\xi_{S,i,\tau}$ is the average of signals up to period $t$. So unlike with homogenous returns in (4) the speed of learning depends on $S$. Taking the conditional expectation of (A.11) for $(S_i, Q_{S,i}, \tilde{\psi}_{S,i})$ gives

$$\begin{aligned}
\mathbb{E}[W_{S,i,t}|S_i, Q_{S,i}, \tilde{\psi}_{S,i}] &= Q_{S,i} + \theta_{S,t} \times \mu_{\psi,S} + (1-\theta_{S,t}) \times \mathbb{E}\left[\bar{\xi}_{S,i,t}|S_i, Q_{S,i}, \tilde{\psi}_{S,i}\right] \\
&= Q_{S,i} + \theta_{S,t} \times \mu_{\psi,S} + (1-\theta_{S,t}) \times \tilde{\psi}_{S,i} \\
&= Q_{S,i} + \mu_{\psi,S} + (1-\theta_{S,t}) \times \left(\tilde{\psi}_{S,i} - \mu_{\psi,S}\right).
\end{aligned} \tag{A.12}$$

Thus, the wages for a worker with schooling $S$ depend on the mean values of $\left(Q_S, \tilde{\psi}_S\right)$, and on the product of $(1-\theta_{S,t})$, which captures employers' learning up to $t$ and $\left(\tilde{\psi}_{S,i} - \mu_{\psi,S}\right)$, which is the deviation in the unobserved productivity from the schooling specific mean. Those with high ability will have large $\left(\tilde{\psi}_{S,i} - \mu_{\psi,S}\right) > 0$, and are rewarded more for their

[29] For a given $S$, $(\mu_{Q,S}, \mu_{\psi,S}, \sigma^2_{Q,S}, \sigma^2_{\psi,S})$ characterize realized outcomes and not the potential outcomes.

ability than those with lower ability, but only after the market has learned their abilities.

Substituting (A.6), (A.9) and (A.12) in (A.7) we can express the hidden IV estimate as

$$\begin{aligned}
\texttt{plim}\ \ \hat{b}_t^{IV^\natural} &= \mathbb{E}\left[(Q_{1,i}-Q_{0,i})+(\mu_{\psi,1}-\mu_{\psi,0})+(1-\theta_{1,t})\left(\tilde{\psi}_{1,i}-\mu_{\psi,1}\right)-(1-\theta_{0,t})\left(\tilde{\psi}_{0,i}-\mu_{\psi,0}\right)\Big|\mathbb{C}\right] \\
&= \underbrace{\mathbb{E}\left[(Q_{1,i}-Q_{0,i})+(\mu_{\psi,1}-\mu_{\psi,0})\,|\mathbb{C}\right]}_{c_0}+(1-\theta_{1,t})\underbrace{\mathbb{E}\left[\left(\tilde{\psi}_{1,i}-\mu_{\psi,1}\right)|\mathbb{C}\right]}_{c_1} \\
&\quad -(1-\theta_{0,t})\underbrace{\mathbb{E}\left[\left(\tilde{\psi}_{0,i}-\mu_{\psi,0}\right)|\mathbb{C}\right]}_{c_2}\equiv c_0+(1-\theta_{1,t})\,c_1-(1-\theta_{0,t})\,c_2,
\end{aligned}$$

where $\theta_{0,t},\theta_{1,t}$ are known functions of $t$ and of the signal-to-noise ratios that depend on $S_i=0$ and $S_i=1$, respectively. Note that as $\lim_{t\to\infty}\texttt{plim}\ \ \hat{b}_t^{IV^\natural}=c_0+c_1-c_2=\mathbb{E}((Q_{1,i}-Q_{0,i})+(\tilde{\psi}_{1,i}-\tilde{\psi}_{0,i})\big|\mathbb{C})=\mathbb{E}(\delta_i^{\psi|S}|\mathbb{C})$, the hidden IV estimate of the private returns can be used to identify the social returns among the set of compliers with long work-experience, and the returns to signaling at $t$ as the difference ($\texttt{plim}\ \ \hat{b}_t^{IV^\natural}-\lim_{t\to\infty}\texttt{plim}\ \ \hat{b}_t^{IV^\natural}$).

So, in order to identify the speed of learning we have to identify $\{\kappa_0,\kappa_1,c_0,c_1,c_2\}$ from $\{\hat{b}_t^{IV^\natural}\}_{t=0}^T$ where $T$ is sufficiently large. We conclude this section by noting the limits to the ability to identify learning parameters using a hidden IV. In particular, we note that the the speed of learning is not globally identified, regardless of the selectivity in the sample. That is if there is no selectivity in schooling, and so $\mathbb{E}\left[\tilde{\psi}_{0,i}-\mu_{\psi,0}\right]=\mathbb{E}\left[\tilde{\psi}_{1,i}-\mu_{\psi,1}\right]$, then the IV estimates of the returns to schooling is not sufficient to identify the speed of learning parameters. Second, consider a value of the parameter vector $(c_0,c_1',c_2',\kappa_0',\kappa_1')$ as well its permutated version $(c_0,c_2',c_1',\kappa_1',\kappa_0')$. Both of these values are consistent with $\{\hat{b}_t^{IV^\natural}\}_{t=0}^T$. This suggests that we can identify the parameters up to a permutation of the parameters.