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SIGNALING AND EMPLOYER LEARNING WITH INSTRUMENTS

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ABSTRACT

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

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1 Introduction

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

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

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

We 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

¹For 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 \[2018\]](#).

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

learning framework of FG, AP, and Lange [2007]? We show that, under suitable assumptions about the instrument’s observability, IV estimates of the causal effects of education on wages point-identify the private and the social returns to education. Unlike Lange [2007], our identification strategy does not rely on the first-order conditions for schooling choices, and thus, does not require specifying costs and benefits of schooling. Also, unlike the existing empirical literature that uses the employer learning framework, our identification strategy does not require access to an ability-correlate that employers cannot observe.

This paper’s central message is that what employers know about the instrument determines the interpretation of the IV estimates. To make this point, we distinguish between *hidden* instruments that employers do not observe and *transparent* instruments that they do. We then present several identification results based on this distinction. Most importantly, we show that a hidden instrument identifies the private returns to education whereas a transparent instrument identifies the social returns to education.

We establish these results while relaxing the assumption that log-productivity is additively separable in individual skills and experience, which is an assumption maintained by AP, FG, and Lange [2007], and most studies following their approach. We instead allow the returns to skills to vary flexibly with experience. In this context, we show that a hidden IV identifies the experience-varying private returns, and a transparent IV identifies the experience-varying social returns. To identify both, private *and* social returns to education, and thus, the signaling value of education, there are two ways to proceed. One way is to assume that the returns to skill do not vary with work experience. Another way is for the researcher to have access to both a hidden and a transparent instrument.

Consider first the case when the returns to skill are assumed to be constant across experience. Then several additional identification results follow. Most importantly, we show that access to a hidden IV and a repeated cross-section of wages across workers’ careers are sufficient to identify the private and social returns, and the speed of learning. Intuitively, when the returns to skill are constant across work experience, changes in the hidden IV esti-

mates across work experience arise only from the changes in employers' information. The IV estimates, however, gradually converge to the experience-invariant social returns, and these changes in private returns identify the speed of learning and the signaling value of education.

Next, consider the second case where the returns to skill vary with work experience. Identifying this model requires disentangling the effects of experience-varying productivity on wages from employer learning. Not surprisingly, data requirements are more demanding in this case. In particular, we show that access to a hidden IV *and* a transparent IV is sufficient to identify the speed of learning under a homogeneity condition.³ Intuitively, we can rely on how the transparent IV estimates vary with experience to identify the productivity effects of individual skills across experience. We can then combine these estimates with the hidden IV estimates to isolate the effects of employers' learning about individual skills.

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

We use this historical episode to construct two instruments, one of which we interpret as plausibly hidden and the other as plausibly transparent. In particular, to define these two IVs we rely on an important feature of how the compulsory schooling reform was implemented across municipalities, and the local labor markets structure in Norway. In 1960, the Norwegian territory consisted of 732 municipalities, and the timing of reform implementation varied substantially across these municipalities, even within a local labor market.⁴ Employ-

³In practice, two IVs are seldom available for the same population of workers. To combine estimates from two samples (exposed to hidden IV and transparent IV), we have to assume that the effect of education on productivity varies in the same way by work experience across these two samples. This homogeneity assumption still allows for differences in the overall productivity effect of education across these samples.

⁴Norway is classified into 160 local labor markets. In each of those markets, we define municipalities with the largest population as central municipalities and the rest as non-central. The average population in non-central municipalities was 3,500, and around 40% of the population lived in central municipalities.

ment was primarily concentrated in the central municipalities, with employers hiring workers from both central and non-central locations. For workers born and raised in non-central municipalities, it is reasonable to assume that employers were uninformed about the reform's timing, given that there were a large number of such municipalities each with a potentially different reform timing. In contrast, employers were more likely informed about the timing of the reform in the dominant population center within each labor market. Consequently, they observe the reform exposure status for workers who grew up in central locations. Building on this distinction, we interpret our instruments to be hidden for those growing up in the non-central municipalities and transparent for those growing up in the central municipalities.

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

Second, we quantify how much signaling and human capital acquisition contributes to the lifetime returns to education. The signaling value is determined by how quickly employers learn and how strong the signal is, i.e., how large a difference in ability can be inferred based on education. Combining estimates from the transparent and hidden IVs, we determine the internal rate of return to an additional year of education to be 7.9%. Our estimates suggest that 70% of this private return represent a productivity-enhancing effect of education, and 30% represent the signaling effect of education. These estimates are from the model with

experience-varying returns to skill. Our conclusions do not change even if we estimate the model under the assumption that the returns to skills are constant over experience.

Finally, we consider two extensions of our model. First, we study the identification when the distinction between a hidden IV and a transparent IV is unclear. In particular, we consider the case of a *partially-transparent IV*, where the IV is transparent with some probability and hidden with the complementary probability. We show that with experience-invariant returns to skill, a partially-transparent IV identifies the lower bounds on the private returns and the signaling value while point-identifying the speed of learning. With experience-varying returns to skill, access to a transparent IV *and* a partially-transparent IV is sufficient to identify the lower bounds on the private returns and the signaling value. Access to a hidden IV *and* a partially-transparent IV, however, is sufficient to point-identify all the parameters under a stronger homogeneity condition on the social returns across samples.

For the second extension, we revisit the assumption about homogeneous returns. Recent studies have provided evidence that the returns can vary across individuals (Card [1999]; Heckman et al. [2006a]; Carneiro et al. [2011]). To accommodate heterogeneous returns, we reframe our analysis within the potential outcomes framework, e.g., Imbens and Angrist [1994], with binary (low or high) schooling. We show that our identification results extend to this setting, with a proviso that now a hidden IV identifies the *average* private returns among compliers, and a transparent IV identifies the *average* social returns among compliers.

The rest of our paper proceeds as follows. Section 2 describes our model and defines the private and the social returns. Section 3 establishes the identification of the private and the social returns using instruments. Section 4 presents our data and Section 5 contains our empirical findings and robustness exercises. Section 6 considers extensions and Section 7 presents a discussion. Section 8 concludes. All additional results are in the Online Appendix.

2 Model of Employer Learning

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

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

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

where S is the years of schooling, A is the ability that is unobserved (to both employers and researchers), and ε is transient variation in productivity independent of all other variables.⁵

In our setting, work experience affects productivity in two ways: through $H(t)$ and through $\lambda_t \in [0, \infty)$. The function $H(t)$ captures how log-productivity varies with work experience commonly across individuals. While we allow $H(t)$ to be a nonparametric function of t , we assume that it does not depend on either S or A .⁶ The parameters $\{\lambda_t : t \in \mathbb{T}\}$ capture the experience-specific effect of composite individual skills. Throughout the paper, we normalize the initial effect to be one, i.e., $\lambda_0 = 1$. When λ_t is constant across t such that $\lambda_t = 1$, for all $t \in \mathbb{T}$, we say that the returns to skill are experience-invariant. When λ_t varies with t , then the effect of the individual skill component $(\beta_{ws} S_i + A_i + \varepsilon_{it})$ on productivity varies with experience and we say that the returns to skill are experience-variant.

To model employer learning in a parsimonious manner we follow Lange [2007] and assume that $\varepsilon_{it} \stackrel{i.i.d}{\sim} \mathcal{N}(0, \sigma_\varepsilon^2)$ and $(S_i, A_i) \stackrel{i.i.d}{\sim} \mathcal{N}(\boldsymbol{\mu}, \Sigma)$, across workers and across experience. Let $\sigma_0^2 = \text{Var}(A|S)$ be the conditional variance of A given S . Besides knowing S_i , every period, employers also observe the total output (χ_{it}) , which is equivalent to observing a signal

⁵In FG, there are also variables capturing information available to employers but not the researchers. An example of such a variable could be knowledge of a foreign language, which is typically mentioned in job applicants’ résumés and verified. FG also allows for a correlate of ability available to researchers but not employers. These are necessary to derive how the partial correlation between earnings, ability proxy, and schooling change with experience. For notational convenience, we suppress these variables here. Our results go through unchanged if we allow for them, see Online Appendix E for more.

⁶In our empirical application, we specify $H(t, X)$ so that the experience profile can vary flexibly with individual characteristics X that include dummy variables for birth cohort and childhood municipality.

$\xi_{it} := A_i + \varepsilon_{it}$ about i 's productivity. Let $\mathcal{E}_{it} = (S_i, \xi_i^t)$ denote employers' information about i in period t where $\xi_i^t = \{\xi_{i\tau}\}_{\tau < t}$ is the history of all past signals.⁷

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

$$A_i = \phi_{A|S} \times S_i + \varepsilon_{A_i|S_i}, \quad (2)$$

where $\varepsilon_{A_i|S_i} := A_i - \mathbb{E}[A_i|S_i]$.⁸ Under perfect competition, workers are paid their conditional expected output, given the information available to the employers. Thus, the wage in period t is equal to the expected productivity given \mathcal{E}_{it} , i.e., $W_{it} = \mathbb{E}[\chi_{it}|\mathcal{E}_{it}] = \mathbb{E}[\chi_{it}|S_i, \xi_i^t]$. Taking the expectation of (1) and using the fact that $\lambda_t(A_i + \varepsilon_{it})$ is normal with the conditional variance $v_t := \text{Var}(\lambda_t(A_i + \varepsilon_{it})|\mathcal{E}_{it})$, we get

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

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

The normality assumptions allow us to write the conditional expectation of ability $\mathbb{E}[A_i|\mathcal{E}_{it}]$ in linear form as

$$\mathbb{E}[A_i|\mathcal{E}_{it}] = \theta_t \mathbb{E}[A_i|S_i] + (1 - \theta_t) \bar{\xi}_i^t, \quad (4)$$

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

⁷Throughout the paper, we assume that employers have symmetric information about workers' abilities and past outputs. See Section 7.1 for more on this assumption.

⁸For much of what follows, S need not be Gaussian, but it simplifies the exposition of the argument. Without it, equation (2) would represent a linear projection of A_i on S_i instead of a conditional expectation.

been received, and the average of all additional signals received up to period t , i.e., $\bar{\xi}_i^t$.

The weight θ_t declines with t because with time, the accumulated observed output measures become a better predictor of individual productivity differences than S , which was the information available at $t = 0$. However, the rate at which θ_t declines depends on the parameter κ that Lange [2007] refers to as the “speed of learning.” The speed of learning governs how quickly information about productivity accumulates in the market. If the signal-to-noise ratio $\sigma_0^2/\sigma_\varepsilon^2$ is high, then κ will be close to 1, and the market learns quickly about the worker’s ability A . However, irrespective of κ , when workers have spent enough time in the market and enough signals have been accumulated, when setting wages, employers will put all weight only on the information accumulated after joining the labor market, i.e., $\lim_{t \rightarrow \infty} \theta_t = 0$.

2.1 Social and Private Returns to Education

Next, we define the *social* and the *private returns* to education. To this end, note that the coefficient $\lambda_t \times \beta_{ws}$ of S in (1) is not the causal effect of education on productivity, but only the “partial” causal effect of schooling, holding unobserved ability A fixed. The “total” causal effect of schooling on productivity also includes the effect on productivity mediated through A . We refer to this total causal effect as the *social return* to education. In contrast, the *private return* is the causal effect of schooling on wages, which captures that schooling also affects wages through employers’ expectations about a worker’s ability. Both the private and the social returns can vary with work experience.

To formalize the social and private returns, we introduce the following notation. 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 but may correlate with S . When using this notation, we also assume that the causal effect of S on Y is linear and additively separable. Then, for $Y = A$, we get

$$A_i = \delta^{A|S} S_i + \tilde{A}_i. \tag{5}$$

Substituting (5) into (1), we obtain

$$\psi_{it} = \underbrace{\lambda_t \times (\beta_{ws} + \delta^{A|S})}_{:=\delta_t^{\psi|S}} S_i + \underbrace{\lambda_t \times (\tilde{A}_i + \varepsilon_{it})}_{:=u_{it}} = \delta_t^{\psi|S} \times S_i + u_{it}. \quad (6)$$

The coefficient of schooling ($\delta_t^{\psi|S}$) in (6) is the total causal effect of schooling on productivity—the social return to education—and it captures the direct effect and the indirect effect mediated via ability. If λ_t varies with experience t , then so does the social return to education.

Consider now the private returns to education. Schooling can affect expected log-earnings at t in two different ways: (i) directly because employers use schooling to form expectations about productivity; and (ii) through learning, because schooling affects productivity, which employers learn over time by observing workers’ outputs. Substituting (2) and (4) in (3), and using $\bar{\xi}_i^t = A_i + \bar{\varepsilon}_i^t$, and the fact that $\mathbb{E}(A_i|S_i)$ is linear in S_i , we get $\ln W_{it} = \lambda_t (\beta_{ws} + \theta_t \phi_{A|S}) S_i + \lambda_t (1 - \theta_t) (A_i + \bar{\varepsilon}_i^t)$. Then, using (5) to replace A gives

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

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

$$\underbrace{\delta_t^{W|S}}_{\text{private returns}} = \underbrace{\delta_t^{\psi|S}}_{\text{social returns}} + \underbrace{\theta_t}_{\text{weight}} \times \underbrace{\lambda_t \times (\phi_{A|S} - \delta^{A|S})}_{\text{adjustment term}}. \quad (8)$$

Thus, the private returns $\delta_t^{W|S}$ differs from the social return $\delta_t^{\psi|S}$ if the marginal effect of schooling on expected A based on the information available to firms at t , $\lambda_t \times \phi_{A|S}$, differs from the causal effect of schooling on unobserved ability, $\lambda_t \times \delta^{A|S}$.

The signaling literature assumes that the “adjustment term” in (8) is non-negative, so

that education has signaling value, and hence $\delta_t^{W|S} \geq \delta_t^{\psi|S}$. For instance, in Spence [1973], the ability is constant and $\delta^{A|S} = 0$, so the adjustment term is always positive. While the adjustment term being non-negative is consistent with the theory, in a general model like ours, whether employers' expectations of worker productivity overshoot or undershoot the causal effect of schooling on ability is an empirical question. Employers eventually learn everything, i.e., $\lim_{t \rightarrow \infty} \theta_t = 0$, so, the wedge between the private and the social returns disappears with work experience, i.e., $\lim_{t \rightarrow \infty} \delta_t^{W|S} = \lim_{t \rightarrow \infty} \delta_t^{\psi|S}$.

3 Identification

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

3.1 Instrumental Variables

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

Assumption 1. (*Instrumental Variables*)

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

Under Assumption 1, the Wald estimate in period t gives

$$\text{plim } \hat{b}_t^{IV} := \frac{\mathbb{E}[\ln W_{it}|D_i = 1, t] - \mathbb{E}[\ln W_{it}|D_i = 0, t]}{\mathbb{E}[S_i|D_i = 1, t] - \mathbb{E}[S_i|D_i = 0, t]}, \quad (9)$$

which is one of the main identifying equations that we use throughout the paper.

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

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

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

In many settings, Assumption 2 is a natural assumption. The clearest examples relate to field experiments of interventions that incentivize some students to take more schooling (treatment), and not others (control). In these cases, whether a student is in the treatment or the control group is typically not known to employers. Other examples of plausibly hidden instruments from quasi-experimental settings include (i) the interaction of draft lottery number and year of birth in Angrist and Krueger [1992]; (ii) the interaction of policy intervention, family background, and season of birth in Pons and Gonzalo [2002]; (iii) parents' education and the number of siblings in Taber [2001]; and (iv) the elimination of student aid programs interacted with an indicator for a deceased father in Dynarski [2003]. Besides these, many studies also exploit interactions of birth year and location of birth with locally implemented policy reforms, e.g., Duflo [2001] and Meghir and Palme [2005], which are similar to our IV.

In contrast to the *hidden* instrument, we say that an instrument is a *transparent* instru-

ment if it satisfies Assumption 1, but is known to the employers and “priced-in” the wages. We use the superscript t in D^t to refer to a transparent IV.

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

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

Examples of instruments used in the literature that are plausibly transparent are (i) tuitions at two- and four-year state colleges in Kane and Rouse [1995]; (ii) a dummy for being a male aged 19-22 from Ontario in Lemieux and Card [2001]; (iii) local labor market conditions in Cameron and Heckman [1998]; Cameron and Taber [2004] and Carneiro 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].

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

3.2 Experience-Invariant Returns to Skill

Before considering experience-varying returns to skill, we first consider the simpler case when the productivity effect of individual skills is constant across experience, i.e., $\lambda_t = 1$ for all $t \in \mathbb{T}$ in (1). Then because S does not vary with t and $\lim_{t \rightarrow \infty} \ln W_{it} = \psi_i$, from (9) we get

$$\text{plim} \left(\lim_{t \rightarrow \infty} \hat{b}_t^{IV} \right) = \frac{\mathbb{E}[\psi_i | D_i = 1] - \mathbb{E}[\psi_i | D_i = 0]}{\mathbb{E}[S_i | D_i = 1] - \mathbb{E}[S_i | D_i = 0]} = \delta^{\psi|S}, \quad (10)$$

where $\delta^{\psi|S} := \beta_{ws} + \delta^{A|S}$ in (6), and the second equality follows from Assumption 1-(1). Thus the part of the productivity, ψ_i , not caused by schooling, S , is orthogonal to the instrument, D . So, with sufficient work experience, i.e., $t \rightarrow \infty$, the IV identifies the causal effect of schooling on productivity. Intuitively, in the long run, everything about a worker's ability is revealed to the employers, and thus, knowledge of the IV itself becomes irrelevant for wage setting. It is only through its effect on schooling, which in turn affects productivity, that the IV affects wages in the long run. In the case of intermediate work experience, i.e., $t < \infty$, what D identifies depends on whether it is hidden from, or known to, the employers.

Hidden Instrument. If we have access to a hidden IV, D_i^h , that satisfies Assumptions 1 and 2 then the numerator in (9) becomes $\mathbb{E} \left[\ln W_{it} | D_i^h, t \right] = \mathbb{E} \left[\beta_{ws} S_i + \mathbb{E} [A_i | S_i, \xi_i^t] | D_i^h, t \right]$. Here, $\ln W_{it}$ does not directly depend on D_i^h because the employers do not use D_i^h when setting the wages. Instead, D_i^h affects $\ln W_{it}$ indirectly via (S_i, ξ_i^t) that makes up the information \mathcal{E}_{it} about productivity available to the employers in period t . Then (2) allows us to express the conditional log-wages given above as

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

Subtracting the conditional log-wages at $D_i^h = 0$ from the conditional log-wages at $D_i^h = 1$ and taking the probability limit of (9) gives

$$\text{plim } \hat{b}_t^{IV^h} = \frac{\mathbb{E} \left[\ln W_{it} | D_i^h = 1, t \right] - \mathbb{E} \left[\ln W_{it} | D_i^h = 0, t \right]}{\mathbb{E} \left[S_i | D_i^h = 1 \right] - \mathbb{E} \left[S_i | D_i^h = 0 \right]} = \delta^{\psi|S} + \theta_t (\phi_{A|S} - \delta^{A|S}). \quad (11)$$

Comparing (11) with the private returns defined in (8), we can conclude that, at every work experience level t , the hidden IV identifies the private returns to education, i.e., $\text{plim } \hat{b}_t^{IV^h} = \delta_t^{W|S}$. Furthermore, from (11) we can see that as the worker gets more experience, i.e., $t \rightarrow \infty$, $\lim_{t \rightarrow \infty} \theta_t = 0$ and so $\text{plim } \hat{b}_t^{IV^h}$ converges to the social return to education, $\delta^{\psi|S}$. Thus, having access to a hidden IV and a repeated cross-section of wages across workers'

careers is sufficient to identify the productivity effect of education, i.e., the social returns to education. Using private and social returns we can identify the signaling value of education.

Besides the private and the social returns, a hidden IV also identifies the speed of learning κ . Let $b_0^{IV^h} = \delta^{\psi|S} + \phi_{A|S} - \delta^{A|S}$ denote the private return to education at $t = 0$ and $b_\infty^{IV^h} = \delta^{\psi|S}$ denote the social return to education. We can then re-write the IV estimate in (11) at experience t as a weighted average of private and social returns, as

$$\text{plim } \hat{b}_t^{IV^h} = \theta_t \times b_0^{IV^h} + (1 - \theta_t) \times b_\infty^{IV^h}. \quad (12)$$

Then, using the IV estimates for $t = 0, \dots, T$, for a large T we can estimate the RHS parameters using the non-linear least squares (NLLS). This means that we can “solve” for $\{b_0^{IV}, b_\infty^{IV}, \kappa\}$ from $\hat{b}_t^{IV^h}$ for $t = 0, \dots, T$, and once we know κ we can determine θ_t for each t .

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

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

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

where the first equality follows from (6) and the second equality from Assumption 1. Using

this expression for the conditional log-wages in (9) and taking the probability limit gives

$$\text{plim } \hat{b}_t^{IV^t} = \frac{\mathbb{E}[\ln W_{it}|D_i^t = 1, t] - \mathbb{E}[\ln W_{it}|D_i^t = 0, t]}{\mathbb{E}[S_i|D_i^t = 1] - \mathbb{E}[S_i|D_i^t = 0]} = \delta^{\psi|S}. \quad (13)$$

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

3.3 Experience-Varying Returns to Skill

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

In this case, access to only one IV, hidden or transparent, is insufficient to identify the speed of learning. If, however, we have access to both hidden and transparent IVs and if λ_t satisfies homogeneity (defined below), we can identify the speed of learning. Formally, following the same steps as in (11), with experience-varying returns to skills, D^h and D^t at t , respectively, identify the private and social returns as

$$\text{plim } \hat{b}_t^{IV^h} = \frac{\mathbb{E}[\ln W_{it}|D_i^h = 1, t] - \mathbb{E}[\ln W_{it}|D_i^h = 0, t]}{\mathbb{E}[S_i|D_i^h = 1, t] - \mathbb{E}[S_i|D_i^h = 0, t]} = \lambda_t(\delta^{\psi|S} + \theta_t(\phi_{A|S} - \delta^{A|S})); \quad (14)$$

$$\text{plim } \hat{b}_t^{IV^t} = \frac{\mathbb{E}[\ln W_{it}|D_i^t = 1, t] - \mathbb{E}[\ln W_{it}|D_i^t = 0, t]}{\mathbb{E}[S_i|D_i^t = 1, t] - \mathbb{E}[S_i|D_i^t = 0, t]} = \lambda_t \times \delta^{\psi|S} := \delta_t^{\psi|S}. \quad (15)$$

Our approach to identifying the model with experience-varying returns to skill relies on combining estimates from (14) and (15). However, in practice, two IVs are seldom available for the same sample. So, to combine estimates from different samples, we need homogeneity.

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

Assumption 4 requires that the two samples exposed to hidden and transparent IVs have the same experience-varying returns to skills. This assumption, however, does not impose any restriction on the experience-invariant components of social returns, i.e., the parameter $\delta^{\psi|S}$ in (14) and (15), so that the overall social returns can be different across the samples.

The identification then proceeds in the following steps. First, from (15) at $t = 0$ we can identify $\text{plim } \hat{b}_0^{IV^t} = \delta^{\psi|S}$. Then, substituting $\delta^{\psi|S}$ in (15) identifies $\lambda_t = \frac{\text{plim} \hat{b}_t^{IV^t}}{\text{plim} \hat{b}_0^{IV^t}}$ for $t > 0$. Once we have identified the parameters $\{\lambda_t : t > 0\}$, then under Assumption 4 we can use these parameters with D^b in (14) to identify $\{b_0^{IV^b}, b_\infty^{IV^b}, \kappa\}$ in the same way as we identified the private returns using D^b in (12). We can either estimate the parameters sequentially, by following the identification steps, or jointly-estimate the parameters from (14) and (15).

4 Data and Empirical Setting

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

4.1 Data Sources and Sample Construction

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

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

We restrict our sample to Norwegian males born between 1950 and 1980. This includes several cohorts for whom we observe earnings for most of their adult life.⁹ We restrict the sample to males because females in these cohorts had comparatively lower labor force participation, more variation in hours worked over the life-cycle, and because the military IQ test scores are available only for males born after 1950. We further exclude immigrants and Norwegian males with missing information on either schooling, childhood municipality of residence, IQ test score, or “exposure” to the compulsory schooling reform. Applying these restrictions, we retain 732,163 Norwegian males.

Our primary outcome variable is the natural logarithm of pre-tax annual labor earnings.¹⁰ Although hourly wages would have been an ideal outcome to measure, they are not available for most of our sample period. We thus restrict our sample to workers with earnings above the SGA threshold (which was U.S. \$10,650 in 2015) to minimize variation in earnings induced by variation in working hours.¹¹ We are left with 718,237 males forming a panel comprising 14,746,755 person-year observations. On average, an individual works full-time for approximately 20.5 years. We use this sample in our empirical analysis, but the sample is unbalanced across work experience. We observe earnings for 579,984 individuals at the start, i.e., experience $t = 0$, and 190,900 individuals after thirty years, i.e., experience $t = 30$.¹²

⁹In our annual income panel data from 1967 to 2014, we observe the oldest cohort (1950) between the ages 17 and 64 and the youngest cohort (1980) up to age 34.

¹⁰These earnings exclude income from capital and self-employment. They also exclude unconditional cash transfers from the government, e.g., social and economic assistance, housing assistance, child allowance.

¹¹In Section 5.3, we show that our results are robust to varying the earnings threshold.

¹²In Section 5.3, we show results for alternative samples, e.g., by limiting the analysis to cohorts 1950–1965 and focusing on the early part of the workers’ careers, for which the panel is more balanced.

4.2 Measures of Schooling and IQ

Our primary variable of interest is years of schooling, corresponding to the highest level of completed education. This variable comes from Statistics Norway’s Education Register. Statistics Norway uses reports on educational attainment submitted directly by the relevant educational establishments to determine each individual’s schooling years. Thus the chance of misreporting is small. Using the years of schooling variable and the age at the start of each year, we measure of potential experience as age minus years of schooling minus school starting age.¹³

We also have information on conscripts’ IQ test scores from the Norwegian Armed Forces. In Norway, military service was compulsory for all able males in the birth cohorts we study. Before each male entered the service, the military assessed his medical and psychological suitability. Most eligible Norwegian males in our sample took this test around their 18th birthday. We use the IQ test score that is a composite unweighted mean from three speeded tests—arithmetics, word similarities, and figures.¹⁴

In Figure 1, we show the average and 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 schooling stages. This pattern could be due to substantial ability-related (psychic) costs for enrolling in high school or particular requirements for enrolling in higher education in Norway.¹⁵ This strong correlation in our data suggests that schooling predicts ability, satisfying a necessary

¹³We measure the age of individuals at the start of each year. We then follow their earnings from the first calendar year after they graduate. We do not use earnings from the year of their graduation because most individuals would be in school for some time during the said calendar year. As discussed above, we drop observations for the years when workers have either low earnings or are unemployed because we restrict our sample to workers with earnings above the SGA threshold.

¹⁴The arithmetic and word tests mirror respective tests in the Wechsler Adult Intelligence Scale, and the figures test is comparable to the Raven Progressive Matrix test, see Sundet et al. [2004], and Thrane [1977].

¹⁵Public education in Norway is meritocratic [Kirkeboen et al., 2016]. Students with higher GPAs in high school are more likely to select into fields with high demand, and they may also have higher IQ scores.

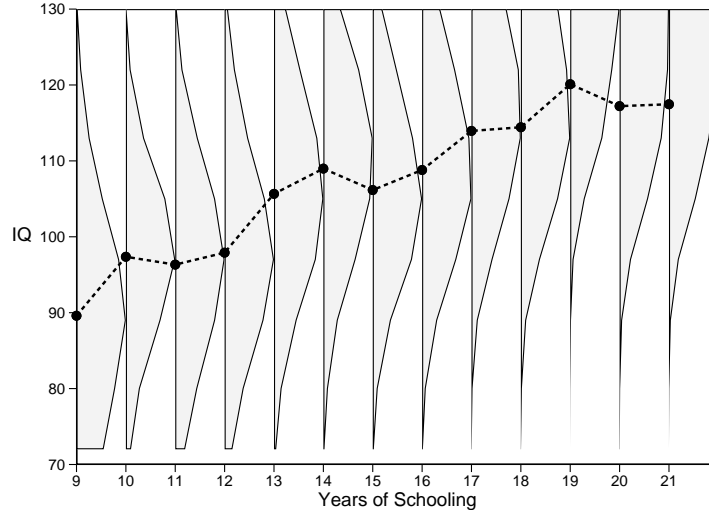


Figure 1: Conditional Probability Density of IQ Test Scores on Years of Schooling.

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.

condition for schooling to have a signaling value in our setting.

4.3 The Compulsory Schooling Reform

Between 1960 and 1975, Norway enacted a school reform that increased the minimum required schooling from 7 to 9 years. Different municipalities, which are the most decentralized local administration, implemented this reform in different years. Thus, for more than a decade, the length of compulsory schooling required of Norwegian children depended on their birth year and municipality of residence at age 14, henceforth, the childhood municipality. We use the timing differences across municipalities, induced by the reform's staggered implementation, as an IV for school years. For more on this reform, see [Black et al. \[2005\]](#).¹⁶

¹⁶This 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. \[2017\]](#). However, besides increasing the minimum required schooling, the law also standardized curricula to improve schooling quality, as noted by [Black et al. \[2005\]](#). This change in quality may raise some concern about whether an IV based on this reform satisfies the exclusion restriction (Assumption 1-(1)). Although we do not show these results for brevity, our model can incorporate this effect and provide a sharp prediction about the bias. In particular, if we replace (5) by $A_i = \delta^{A|S} S_i + \delta^{A|D} D_i + \tilde{A}_i$, where $\delta^{A|D} > 0$ is the effect of the reform on school quality (in terms of log-productivity), then it can be shown that the private and social returns estimates have the same bias, $\lambda_i(\delta^{A|D}/\varkappa)$, where $\varkappa > 0$ is the effect of the reform on years of schooling. As the difference between the

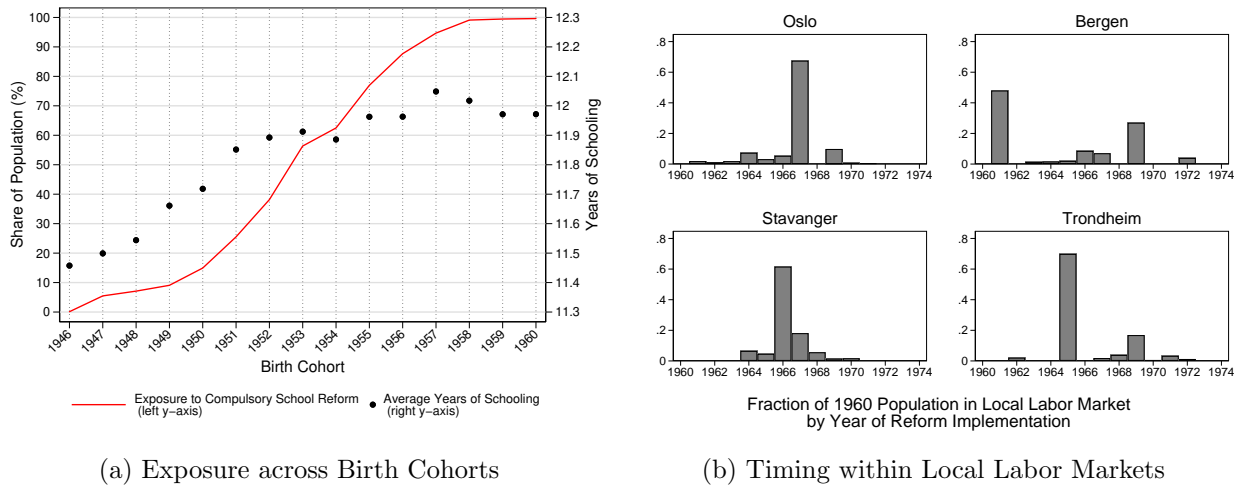


Figure 2: Compulsory School Reform Across Birth Cohorts and Local Labor Markets.

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.

For 672 out of the 732 municipalities in 1960, we can date the reform’s implementation using historical records. 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 and everybody born after 1960 were required to attend school for 9 rather than 7 years.

Figure 2-(b) shows that there is considerable variation even within the four largest local labor markets (the four most significant metropolitan areas in Norway). For instance, Oslo city’s municipality, which in 1960 accounted for two-thirds of the Oslo labor market region population, implemented the reform in 1967. In contrast, the reform’s timing varied between 1961 and 1971 across the remaining population living in one of the other 39 municipalities.¹⁷

As shown in Section 3, to separately identify the private and the social returns to educa-

private and the social return equals the signaling value, the bias term would cancel out, and the signaling value estimate would remain unchanged despite a violation of the exclusion restriction.

¹⁷We use the classification of Norway into 160 local labor markets based on geographic commuting patterns constructed by Gundersen and Juvkam [2013]. On average, each market has five municipalities.

tion in a model with experience-invariant returns to skill, the instrument should satisfy the standard IV assumption (Assumption 1) and also be a hidden instrument (Assumption 2). For an instrument to be a hidden instrument in our setting, it must mean that employers are not informed about the interaction between a worker's birth cohort and the timing of compulsory school reform in the worker's childhood municipality. On the contrary, for an instrument to be a transparent (Assumption 3), the employer must be informed about the worker's birth cohort and the timing of reform in the worker's childhood municipality.

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

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

On the other hand, employers have incentives to remain informed about significant changes to their local labor market's education system. When major cities or population centers implemented such schooling reforms, newspapers and other media are more likely

to report, and employers more likely to learn. We thus believe it is more plausible that compulsory schooling regulations pertaining to residents of the central municipalities were understood and priced into wages by employers. Based on this reasoning, we construct a plausibly hidden instrument and a plausibly transparent instrument by partitioning our analytical sample into two, based on the relative size of workers' childhood municipality in their local labor markets.

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

Similarly, to construct a hidden instrument, we include workers who grew up in non-central municipalities, defined as the complement of central municipalities in each local labor market. We assume that employers are uninformed about these workers' reform exposure status. We refer to this sample as the *hidden IV sample*, and it includes 422,749 individuals and 8,697,979 person-year observations, i.e., 59% of the full sample.

4.4 Empirical Specifications

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

$$\ln W_{it} = a_t^{IV^\tau} + b_t^{IV^\tau} S_i + e_t^{IV^\tau} X_i + u_{it}^{IV^\tau}, \quad (16)$$

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, and $\tau \in \{\mathfrak{h}, \mathfrak{t}\}$ indicates whether we use the hidden IV or the transparent IV sample. The

IV specification consists of the second-stage (16) and a first-stage equation

$$S_i = \sigma^{\mathfrak{r}} + \varkappa^{\mathfrak{r}} D_i^{\mathfrak{r}} + n^{\mathfrak{r}} X_i + v_i^{\mathfrak{r}}, \quad (17)$$

where the binary instrument $D_i^{\mathfrak{r}} \in \{0, 1\}$ is equal to 1 if i was “exposed” to compulsory schooling reform and 0 otherwise. We record individual i as exposed if the reform was implemented in i ’s childhood municipality when i was 14 years old or younger.¹⁸

We estimate (16) and (17) using 2SLS, separately for each experience year, t , and for each instrument, $\mathfrak{r} \in \{\mathfrak{h}, \mathfrak{t}\}$, which allows parameters to vary flexibly, by t and \mathfrak{r} .¹⁹ We use the childhood municipality indicators to control for unobservable determinants of earnings or schooling fixed at the municipality level. By adding these indicators, we can compare individuals who grew up in the same municipality but different years and were exposed to different compulsory schooling requirements. Thus, we exploit variation in their schooling stemming from differential exposure to the compulsory schooling law. By including birth cohort indicators, we control for aggregate changes in schooling and earnings across cohorts.

Our parameters of interest are $\{b_t^{IV^{\mathfrak{h}}}, b_t^{IV^{\mathfrak{t}}}\}$, the coefficients on schooling at experience $t = 0, 1, \dots$, resulting from the hidden IV and the transparent IV, respectively. We maintain the assumptions that (i) conditional on X , both instruments $\{D^{\mathfrak{h}}, D^{\mathfrak{t}}\}$ satisfy Assumption 1, and as discussed in Section 4.3, that in our setting (ii) $D^{\mathfrak{h}}$ satisfies the hidden IV Assumption 2 and $D^{\mathfrak{t}}$ satisfies the transparent IV Assumption 3.²⁰

As discussed in Section 3.2, under experience-invariant returns to skill and maintaining Assumption 2, $\hat{b}_t^{IV^{\mathfrak{h}}}$ converges to the experience-invariant social return to education, $\delta^{\psi|S}$, as $t \rightarrow \infty$, and moreover, $\hat{b}_t^{IV^{\mathfrak{h}}}$ provides a consistent estimate of the private return at t , $\delta_t^{W|S}$,

¹⁸At that time, seven years was the age when someone had to start school. Cohorts who were 14 years or less at the time of school reform were required to take two additional years of schooling, whereas cohorts who were older than 14 years were exempted.

¹⁹Unlike (16), the coefficient $\varkappa^{\mathfrak{r}}$ of the instrument $D^{\mathfrak{r}}$ in the first-stage equation do not change with t because $D^{\mathfrak{r}}$ and S are experience-invariant. However, with an unbalanced panel and separate estimations by experience, the first-stage estimates of $\varkappa^{\mathfrak{r}}$ can vary with t . In practice, estimates of $\varkappa^{\mathfrak{r}}$ are stable across the experience range that we consider despite differences in the sample composition by experience.

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

for any $t < \infty$. Thus, we can estimate the social returns to education as $\hat{b}_{t \rightarrow \infty}^{IV^b}$, and use the rate at which $\hat{b}_t^{IV^b}$ converges to $\hat{b}_{\infty}^{IV^b}$ to estimate the speed of learning, κ .

For the model with experience-varying returns to skill and maintaining Assumption 2 on D^b and Assumption 3 on D^t , we can recover the private returns $\delta_t^{W|S}$ for the hidden IV sample from $\hat{b}_t^{IV^b}$ and the experience-varying social returns $\delta_t^{\psi|S}$ for the transparent IV sample from $\hat{b}_t^{IV^t}$. As discussed in Section 3.3, using $\hat{b}_t^{IV^t}$, we can also recover the experience-varying skill component $\hat{\lambda}_t$ under location normalization $\lambda_0 = 1$. And, finally, maintaining Assumption 4 of homogeneity of λ_t across the two samples, we can infer the speed of employer learning, κ , and the experience-invariant part of the social returns $\delta^{\psi|S}$ for the hidden IV sample.

A challenge to identifying returns to schooling based on (16) is that individuals who grew up in different municipalities could have had different growth rates in schooling and earnings even in the absence of compulsory schooling reform. Following Bhuller 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 educational attainment and lifetime earnings estimated using data on pre-reform cohorts as additional controls. We refer to estimates based on (16)–(17) as obtained from the baseline specification and estimates that we get after controlling for municipality-specific trends.

Finally, note that by estimating (16) separately for each t , we also allow the work experience to interact with observable characteristics X_i , further weakening the functional form assumption embedded in (1). In particular, in (16) we have specified $H(t, X_i) = a_t^{IV^r} + e_t^{IV^r} X_i$, where coefficients $a_t^{IV^r}$ and $e_t^{IV^r}$ can vary flexibly by experience, and thus flexibly capture both a common experience profile and its interactions with X_i .

5 Empirical Results

This section contains our main empirical results, beginning with IV estimates by work experience for both the hidden and the transparent IV samples. These allow us to identify both

the private and the social returns to education. We use these to quantify the contribution of signaling to the returns to education, before we provide results from sensitivity analyses.

5.1 IV Estimates of the Returns to Education

In Table 1, column (1), we display the estimates of (17)–the first-stage effect of our compulsory schooling reform instrument on years of schooling–for the full sample. This estimate indicates that exposure to compulsory schooling reform increased completed schooling by 0.237 years. Columns (2) and (3) show the same first-stage estimates for the hidden IV and the transparent IV samples, respectively. All three first-stage coefficients are of similar magnitude and precisely estimated, confirming that the IV relevance condition is satisfied in all samples and that weak IV bias is not a concern for our analysis.

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

	Full Sample	Hidden IV Sample	Transparent IV Sample
	(1)	(2)	(3)
Instrument:			
<i>Exposure to Compulsory Schooling Reform</i>	0.237 ^{***} (0.025)	0.228 ^{***} (0.034)	0.240 ^{***} (0.038)
Controls:			
Municipality Fixed Effects	✓	✓	✓
Cohort Fixed Effects	✓	✓	✓
F-statistic (instrument)	87.7	45.7	55.5
Sample Mean Years of Schooling	12.36	12.36	12.27
Standard Deviation Years of Schooling	2.50	2.50	2.46
Number of Observations	14,746,755	8,697,979	6,048,776

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

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

We now turn our attention to the second-stage IV estimates of (16) for the hidden IV sample. Figure 3-(a) displays the hidden IV estimates at different work experience. Under

the assumption that this IV is indeed hidden from the employers, this provides consistent estimates of private returns to schooling at each year of work experience. The estimates suggest initially high private returns to schooling, followed by a steep decline during the first five years of work. Then, the returns gradually stabilize and approach 5.5% for those with 15 or more years of work experience.

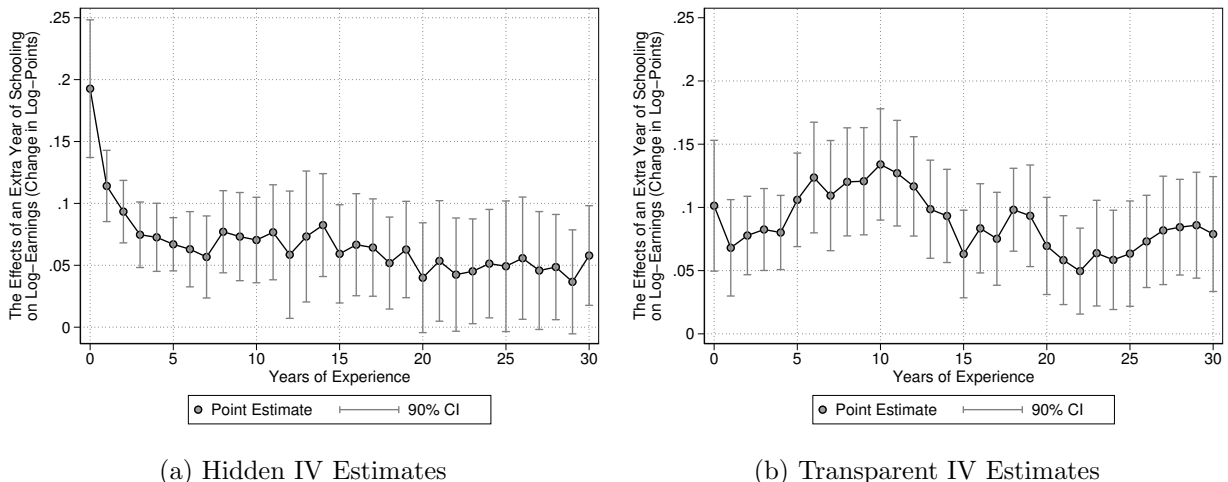


Figure 3: Hidden and Transparent IV Estimates of the Returns to Schooling.

Note: The estimation sample consists of Norwegian males born 1950-1980 observed in earnings data over years 1967-2014 with years of potential experience between 0 and 30 years and annual earnings above 1 SGA threshold ($N=14,746,755$). The hidden IV sample (plot (a)) further drops individuals who grew up in the municipality with the largest population size in each of the 160 labor market regions in Norway ($N=8,697,979$), while the transparent IV sample (plot (b)) retains only individuals who grew up in the municipality with the largest population size in each labor market ($N=6,048,776$). Plots (a) and (b) display IV estimates from separate estimations of (16) for each year of experience using the hidden and the transparent IV samples. All estimations include fixed effects for birth cohort and childhood municipality. Standard errors are clustered at the local labor market region. Vertical bars denote the 90% confidence intervals.

Next, we turn to the transparent IV estimates of (16). These estimates are displayed in Figure 3-(b) and they correspond to the social returns to schooling across experience. In contrast to the hidden IV estimates in Figure 3-(a), the transparent IV estimates do not decline with experience and are generally flatter across work experience. These estimates are indeed consistent with constant social returns and, in turn, we fail to reject the standard model of employer learning with experience-invariant returns to skill.²¹

²¹Formally, we use $\hat{\lambda}_t = (\hat{b}_t^{IV^1} / \hat{b}_0^{IV^1})$, and test the hypothesis that for every $t \in \mathbb{T}_{>0}$ the ratio $\hat{\lambda}_t = 1$. The F-statistic from this joint test is 0.9, which means that we cannot statistically reject the hypothesis of constant social returns. The $\hat{\lambda}_t$ estimates and the associated 90% confidence intervals are presented in the Online Appendix, Figure A.1. By comparison, the F-statistic for the joint test of constant hidden IV estimates is 3.3 and we can easily reject the hypothesis that the private returns are constant over experience.

The transparent IV estimates in Figure 3-(b) imply that the average social return (across experience) is 8.8%. This social return is higher than the long-term value that the hidden IV estimates shown in Figure 3-(a) converge towards, indicating that the experience-invariant part of the social returns is not homogenous across the transparent IV and the hidden IV samples. While we do not explore the sources of such heterogeneity in social returns, our identification approach does allow differences in the experience-invariant part of the social returns across workers from central and non-central locations to exist.²²

5.2 The Speed of Learning and the Signaling Value of Education

We now use the hidden IV estimates and the transparent IV estimates, shown in Figure 3, to determine the private and social returns to schooling, the speed of employer learning, and the signaling value of education.

We first present estimates that allow for experience-varying returns to skill, which are constructed by combining estimates from the hidden and the transparent IVs, as discussed in Section 3.3. We report the NLLS estimates in Table 2 columns (1)-(2), based on sequential (i.e., estimate (15) before (14)) and joint estimations (i.e., estimate (15) and (14) jointly), respectively. Consistent with Figure 3-(a), in column (1), we estimate an initial private return to education ($b_0^{IV^h}$) of 19.8%, a limit return ($b_\infty^{IV^h}$) of 5.5% and a speed of learning (κ) estimate of 0.505, which implies rapid learning on the part of employers. In particular, this estimate of κ implies that after the first five years of employment, employers put 16.4% weight on the initial signal they received from workers. As we can see from panel B, column (1), after 15 years, this weight declines further to 6.1%. Comparing across columns (1)-(2), we find that our estimates are robust to whether we sequentially or jointly estimate (14)-(15).

In addition to estimates of our key model parameters ($b_0^{IV^h}, b_\infty^{IV^h}, \kappa$), we also report the

²²Notably, Assumption 4 requires that the *experience-varying* component of returns to skill is identical across samples. However, this assumption does not restrict the *experience-invariant* component of social returns. Such heterogeneity in social returns could, for instance, exist due to differences in inputs factor in the production of human capital during childhood across workers who grew up in central and non-central locations (e.g., child-care access, parental resources, etc., see discussions in Cunha and Heckman [2007]).

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

Model Specifications:	Experience-Varying Skill Returns		Experience-Invariant Skill Returns
	<i>Sequential Estimation</i> (1)	<i>Joint Estimation</i> (2)	(3)
A. Parameters of Interest:			
Speed of Learning κ	0.505 ^{***} (0.107)	0.550 ^{***} (0.126)	0.532 ^{***} (0.058)
Initial Return $b_0^{IV^h}$	0.198 ^{***} (0.015)	0.195 ^{***} (0.012)	0.192 ^{***} (0.010)
Limit Return $b_\infty^{IV^h}$	0.055 ^{***} (0.006)	0.055 ^{***} (0.008)	0.050 ^{***} (0.003)
Average Return b^{IV^h}	0.064 ^{***} (0.003)	0.066 ^{***} (0.002)	0.067 ^{***} (0.007)
Average Return b^{IV^t}	0.088 ^{***} (0.024)	0.089 ^{***} (0.019)	—
B. Weight on Initial Signal:			
θ_t at $t = 5$	16.4%	14.1%	15.0%
θ_t at $t = 10$	8.9%	7.6%	8.1%
θ_t at $t = 15$	6.1%	5.2%	5.5%
C. Internal Rate of Return:			
Private Return	0.079	0.076	0.072
Social Return	0.055	0.055	0.050
Signaling Value	30.4%	28.0%	30.6%

Note: The estimation sample consists of Norwegian males born 1950-1980 observed in earnings data over years 1967-2014 with years of potential experience between 0 and 30 years and annual earnings above 1 SGA threshold (N=14,746,755), partitioned in a hidden IV sample (N=8,697,979) and a transparent IV sample (N=6,048,776) as discussed in Section 4.3. Panel A provides estimates of key model parameters for different model specifications. Columns (1)-(2) provide parameter estimates for the specification with experience-varying returns to skill, which combine the hidden and the transparent IV estimates plotted in Figure 3(a)-(b). In the sequential estimation approach (column (1)), we first estimate the $\hat{\lambda}_t$ profile based on the transparent IV estimates using (15) under location normalization $\lambda_0 = 1$, and then insert $\hat{\lambda}_t$ in (14) and solve for other model parameters using the non-linear least squares estimation. In the joint estimation approach (column (3)), we jointly solve for all model parameters and λ_t using (14)-(15) by non-linear least squares estimation. Column (3) provides parameter estimates for the specification with experience-invariant returns to skill, which are based solely on the hidden IV estimates plotted in Figure 3(a) and constructed using non-linear least squares estimation of (11). Panel B provides the implied weight θ_t on initial signal corresponding to each estimate of the speed of employer learning κ . Panel C provides the implied internal rates of private and social returns, and the signaling contribution to private returns, for each model specification.

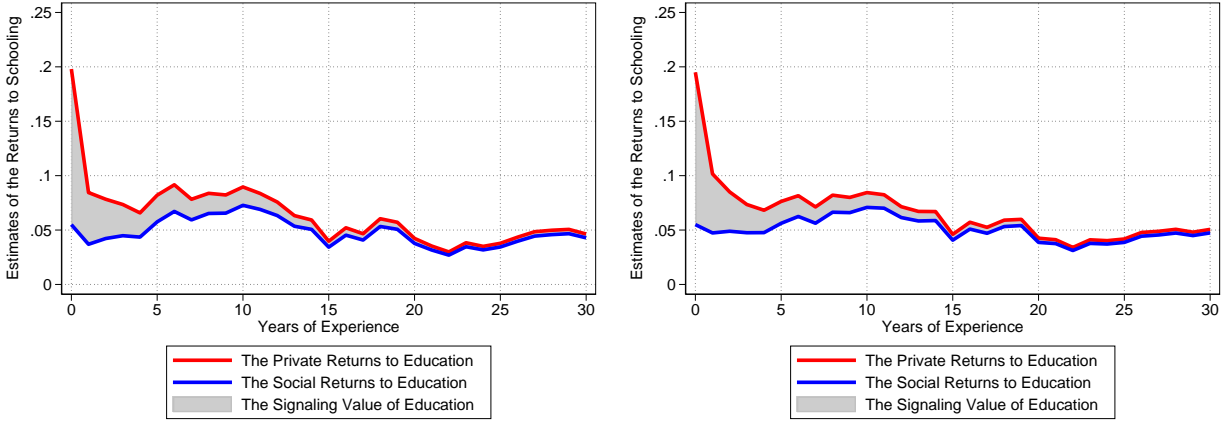
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

average return (b^{IV^t}) implied by the transparent IV estimates, and similarly, the average return (b^{IV^h}) implied by the hidden IV estimates across years of experience 0 to 30. The average return for the transparent IV sample is estimated to be 8.8%, as compared to an average return of 6.4% and a limit return of 5.5% for the hidden IV sample.

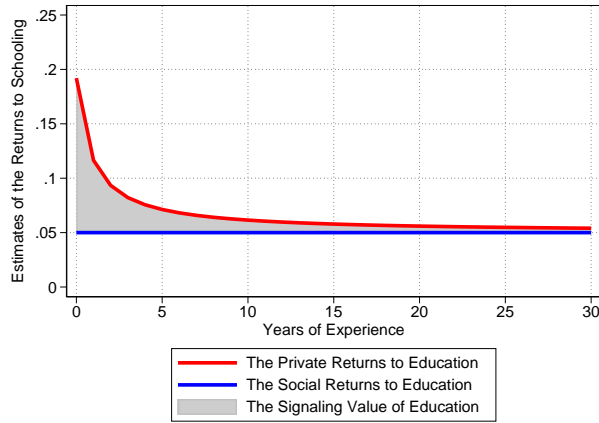
Next, we present estimates under the assumption of experience-invariant returns to skill following the results in Section 3.2. In Table 2, panel A, column (3), we display the estimates of initial private returns ($b_0^{IV^b}$), limit returns ($b_\infty^{IV^b}$) and the speed of learning (κ) obtained solely from the hidden IV estimates shown in Figure 3-(a) based on NLLS estimation. In this case, we estimate an initial return of 19.2%, a limit return of 5% and a speed of learning estimate of 0.532, which again implies rapid learning on the part of employers. Comparing across columns (1)-(3), we find that imposing experience-invariant returns to skill does not materially change our conclusions regarding the speed of learning and the evolution of returns. This finding is consistent with the estimates in Figure 3-(b), based on which we could not statistically reject the null hypothesis of constant social returns across experience.

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

Figure 4 highlights that a sizable fraction of the private returns that accrue during the early stages of workers' careers are attributable to signaling. To provide a summary measure of the contribution of signaling for the private returns to education over the workers' careers, we calculate the *internal rate of return* (IRR) for an additional year of schooling implied by our estimates. Following Heckman et al. [2006a], we define IRR as the discount rate that equates the present discounted value of earnings over the career for different choices of



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



(c) Experience-Invariant Returns to Skill

Figure 4: The Private Returns, the Social Returns and the Signaling Value of Education.
Note: The private and social returns plotted in the red and the blue lines, respectively, are constructed using the estimates in Table 2, columns (1)–(3). The difference between the private and social returns provides the signaling value of education.

schooling.²³ The IRR captures that schooling benefits that accrue early in workers’ careers are more valuable. Even with fast employer learning, one could therefore expect a sizable signaling value as the signaling returns occur primarily early in workers’ careers.

Using the experience-specific returns estimates in Figure 4-(a), we estimate the private IRR to be 7.9%, see Table 2 panel C, column (1). Thus the private IRR is 2.4 percentage

²³Formally, the private IRR r solves $\sum_{t=0}^T \frac{\hat{W}(S+1,t)}{(1+r)^{t+1}} - \sum_{t=0}^T \frac{\hat{W}(S,t)}{(1+r)^t} = 0$, where, for simplicity, we set $\hat{W}(S+1,t) = \hat{W}(S,t)(1 + b_t^{IV^h}) = \bar{W}(t)(1 + b_t^{IV^h})$, $\bar{W}(t)$ denotes average earnings for male workers at experience t , and we use a fixed length of work career $T = 40$. In this calculation, we rely solely on the opportunity cost of schooling to compute the IRR, and abstract from tuition and psychic costs of schooling and any earnings while in school. For $t > 31$, we assume that $\theta_t = 0$ and $\lambda_t = 1$.

points greater than the social returns at 5.5%. Based on these estimates, we attribute 70% of the private return to education raising workers' productivity and 30% to signaling. In columns (2)-(3), we perform the same calculation based on the estimates of the returns in Figure 4, (b)-(c), and again find a signaling component of a similar magnitude.

5.3 Robustness and Additional Results

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

Differential Trends. As discussed in Section 4.4, differential trends across individuals who grew up in different municipalities, even in the absence of the compulsory schooling reform, pose a challenge to identifying returns to schooling based on (16). Following Bhuller et al. [2017], we verify that our estimates do not change when we include extrapolated linear and quadratic municipality-specific trends. The results are presented in the Online Appendix Tables A.1 and A.2. We can see that both the first and the second-stage IV estimates of key model parameters are robust to the inclusion of differential trends.²⁴ Similarly, from Figure A.2, we can see that the experience-specific IV estimates are also robust to differential trends.

Sample Selection. Next, we consider the sensitivity of our results to using alternative samples. As described in Section 4.1, our estimation sample consists of male workers born 1950–1980, and we retain yearly observations for each of the years when a worker has annual earnings above the SGA threshold. By construction, this sample is unbalanced because our earnings data are limited to calendar years 1967–2014 (data availability), and because some individuals could have been unemployed or worked part-time in some years (partial employment). Due to data availability, the unbalancedness is more severe for younger cohorts

²⁴Indeed, based on the estimates in Table A.2 that allow for differential trends we cannot reject that the experience-invariant part of the social returns is equal across the transparent IV and the hidden IV samples.

as earnings towards the latter part of these workers’ careers are not measured by 2014.

To address concerns related to having an unbalanced sample in our baseline, we proceed in two ways. In the first case, we retain our baseline sample of male workers born 1950–1980 but use yearly observations only up to 10 years of work experience in our estimation. In the second case, we restrict our sample to male workers born 1950–1965 and use observations up to 30 years of work experience, as in our primary analysis. In both of these alternative estimations, the sample is substantially more balanced over time.²⁵

The results from these estimations are presented in the Online Appendix Table A.3, columns (2)-(3) and in Figure A.3-(b). These results show that our returns estimates are robust to using more balanced samples with fewer observations. Furthermore, in Table A.3, column (4), and Figure A.3-(c), we report estimation results when we extend our sample to male workers born 1940-1980.²⁶ Our findings are also robust to using this extended sample.

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

IQ as a Hidden Correlate. Next, we briefly describe our findings when we use IQ as a hidden correlate of ability. As mentioned in Section 4.2, we use an IQ test score from the

²⁵In our baseline hidden IV sample of male workers born 1950–1980, we include 422,749 individuals and 8,697,979 person-year observations, among whom we observe earnings for 344,547 in the initial year of work experience, 355,602 in the 10th year, and 116,385 in the 30th year. In the sample of males born 1950–1965, we include 213,206 individuals and 5,326,336 person-year observations, among whom we observe earnings for 173,465 in the initial year of work experience, 179,027 in the 10th year, and 116,385 in the 30th year.

²⁶The reason we restricted our baseline sample to birth cohorts born 1950–1980 was that the military IQ test scores are not available for those born before 1950. In Table A.3, column (4), we drop this requirement.

Norwegian Armed Forces available for conscripts born 1950–1980. See the Online Appendix B for details on the identification strategy.²⁷ We present the estimates from the hidden correlate approach in the Online Appendix Table B.1, panel A. Similar to the IV estimates in Table 2, these estimates also suggest relatively fast employer learning.

The estimated return to one standard deviation increase in the IQ test score goes from near zero in the first few years in the labor market to about 7% after around 15 years of experience. Additionally, we find that the OLS estimates of the coefficients on years of schooling decline rapidly from about 10% to about 3% within the first 20 years. These patterns are similar to those found by Altonji and Pierret [2001], and Lange [2007] for the NLSY using the hidden correlate approach. Furthermore, similar to Arcidiacono et al. [2010], our estimates suggest that employer learning is concentrated among the less educated.

Causal Effect of Education on IQ. In other related studies [e.g., Brinch and Galloway, 2012; Carlsson et al., 2015], researchers have provided causal estimates of education on measures of workers’ ability. In a similar spirit, we regress the standardized IQ test score as a function of years of schooling, using the compulsory schooling reform as an instrument for schooling and present the results in Online Appendix Table A.4. These IV estimates show a strong effect of schooling on IQ: an additional year of schooling at age 18 *causes* approximately 1/4 of a standard deviation increase in IQ.²⁸ Thus, schooling increases worker ability, which we expect to be positively correlated with workers’ productive skills. A back-of-the-envelope calculation suggests that the schooling-induced increase in IQ results in 3.2% higher earnings, which is almost 60% of the 5.5% social return estimate presented in Table

²⁷The validity of this approach requires that employers do not (fully) observe IQ test scores in the hiring process, which is a reasonable assumption in our context. Notably, the Norwegian Armed Forces do not provide any certification that the conscripts can use to inform the employers about their IQ credibly. That said, our model can also be extended to allow for the possibility that the employers observe other correlations (as captured by Q in the Online Appendix Section E) of applicants’ ability during the hiring process.

²⁸The IQ test score is standardized to have mean zero and a standard deviation equal to one. Using the same compulsory schooling reform for Norway, Brinch and Galloway [2012] documented that an additional year of schooling increased IQ scores measured at age 19 by 3.7 points. Their IQ test score measure is scaled to have a mean of 100 and a standard deviation of 15. In terms of magnitude, their estimate also corresponds to 1/4 of a standard deviation increase in IQ. Carlsson et al. [2015] also document similar results for Sweden.

2, column (1).²⁹ While we are cautious in interpreting these estimates as “direct” evidence of a productivity return to education, they suggest that part of social returns to education in (6) could be attributed to the causal effect of education on workers’ abilities.

6 Extensions

6.1 Partially-Transparent Instrument

So far, we assumed that researchers could determine whether each individual worker was treated by a hidden or a transparent IV. We now consider an extension that accommodates the situation when researchers are unable to determine the type of IV each worker was exposed to. This is relevant, as in many settings, the distinction between hidden and transparent IVs can be blurry. To operationalize this idea, assume that an IV was transparent for a fraction ρ of workers, and hidden for the remaining fraction $(1 - \rho)$. Researchers do not know this fraction ρ , nor do they know the type of IV each worker was exposed to. In this situation, we say that we have access to a “partially-transparent” instrument. We summarize the main identification results in this setting and leave details to the Online Appendix C.

In this setting, the conditional expectation of log wages becomes a weighted average of the conditional expectation of log wages given hidden and transparent IVs, respectively. When the returns to skill are constant, a partially-transparent IV identifies a lower bound on the private returns to education. Nevertheless, as with the hidden IV, after long enough work experience, this lower bound converges to the social returns. We thus identify the speed of learning and can provide a lower bound for the signaling value. When the returns to skill vary with work experience, we can identify lower bounds on the private returns and the signaling value if we have access to a transparent *and* a partially-transparent IV. To

²⁹For this calculation, we use the estimated 1/4 of a standard deviation increase in IQ from an additional year of schooling in the Online Appendix Table A.4, and an OLS regression of log-wages on IQ, controlling flexibly for experience, which shows that a standard deviation increase in IQ is associated with 12.8% higher earnings. For the purpose of this exercise, we interpret each coefficient estimates as causal, and conclude that a schooling-induced increase in IQ leads to 3.2% ($\approx 12.8\% \times 1/4$) higher earnings.

point-identify all parameters, it suffices to have access to a hidden IV in addition to the partially transparent IV if we also strengthen Assumption 4 and instead assume that the social returns are the same across the two IV samples.

6.2 Heterogeneous Returns

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

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

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

4), then having access to the two IVs is again sufficient to identify the speed of learning.

7 Discussion

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

7.1 Asymmetric Employer Learning

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

7.2 Market Power

We also assume that employers perfectly compete for workers, so that workers are paid their expected productivity based on the information available to employers. While convenient, this assumption is at odds with new research suggesting that employers have market power, see Manning [2021] for a review. However, such monopsony power in itself does not imply

a systematic bias in our estimates of the social and private returns to education. When employers have market power, they will mark-down wages so that for the marginal worker, expected productivity exceeds the wage. Estimates of the returns to education are, however, based on relative wages across schooling levels. For instance, if the mark-downs are proportional to the expected productivity then they “cancel-out” when we estimate log-linear earnings equations.

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

7.3 Information and Assignment

In our framework, the social returns to education are equal to the direct increase in workers’ skills. However, information revealed during schooling might also affect the process of allocating workers to jobs in which they have a comparative advantage. If education improves workers’ assignment to employers, this represents an additional channel through which education raises productivity by improving allocative efficiency. A key question then is to whom do these social returns accrue? In a perfectly competitive market, potential employers bid for employees’ services based on workers’ expected contribution. The highest bidder will employ the workers in their highest productivity job and pay the expected marginal product. These

social returns would thus accrue to the worker. Our estimates of the returns to education obtained using either a hidden or a transparent IV would then pick up these returns.

7.4 General Equilibrium Effects

Many instruments rely on large interventions that change educational attainment in the population. These interventions are likely to engender general equilibrium effects, because among other reasons, changes in educational attainment in the population imply movements along production isoquants. Besides, signaling model predicts that the equilibrium wage function changes if interventions change the population wide joint distribution of education and ability. Moreover, because the wage function affects schooling decisions, the signaling aspect of education generates additional feedback from the wage to education decisions.³⁰

While we abstract from such equilibrium effects, they may affect our analysis. First, our analysis applies best to instruments that affect small population groups (relative to the economy) and are thus unlikely to generate equilibrium effects. Thus, the ideal setting for an empirical application along the lines of our approach would be an experiment that treats only a small subset of the population and sidesteps the concern about equilibrium effects.³¹

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

³⁰See [Lang and Kropp \[1986\]](#) and [Bedard \[2001\]](#) for evidence on such feedback effects.

³¹In practice, estimating effects of education interacted with experience using IVs requires large datasets. A complementary approach is to use cross-country variation in educational attainment to identify social return to education, including any general equilibrium effects. For this approach to work, however, we need strong exogeneity assumptions. We refer the interested reader to [Lange and Topel \[2006\]](#).

8 Conclusion

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

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

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

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Online Appendix

A Additional Results

Table A.1: First-Stage Estimates on Years of Schooling – Controlling for Differential Trends.

	A. Full Sample		B. Hidden IV Sample		C. Transparent IV Sample	
	Baseline (1)	Trends (2)	Baseline (3)	Trends (4)	Baseline (5)	Trends (6)
Instrument:						
<i>Exposure to Reform</i>	0.237 ^{***} (0.025)	0.209 ^{***} (0.034)	0.228 ^{***} (0.034)	0.209 ^{***} (0.040)	0.240 ^{***} (0.032)	0.199 ^{***} (0.038)
Controls:						
Municipality Fixed Effects	✓	✓	✓	✓	✓	✓
Cohort Fixed Effects	✓	✓	✓	✓	✓	✓
Municipality-Specific Trends		✓		✓		✓
Number of Observations	14,746,755		8,697,979		6,048,776	

Note: The full sample (panel A) 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 hidden IV sample (panel B) further drops individuals who grew up in the municipality with the largest population size in each of the 160 labor market regions in Norway (N=8,697,979), while the transparent IV sample (panel C) retains only individuals who grew up in the municipality with the largest population size in each labor market (N=6,048,776). All estimations include fixed effects for birth cohort and childhood municipality. The trends specifications in columns (2), (4) and (6) control 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.

* p < 0.10, ** < 0.05, *** p < 0.01.

Table A.2: IV Estimates of the Speed of Employer Learning, Initial and Limit Returns to Education, and the Signaling Value Contribution – Controlling for Differential Trends.

Model Specifications:	A. Experience-Varying Returns to Skill				B. Experience-Invariant Returns to Skill	
	<i>Sequential Estimation</i>		<i>Joint Estimation</i>			
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Trends	Baseline	Trends	Baseline	Trends
Parameters of Interest:						
Speed of Learning κ	0.505 ^{***} (0.107)	0.506 ^{***} (0.152)	0.550 ^{***} (0.126)	0.592 ^{***} (0.205)	0.532 ^{***} (0.058)	0.565 ^{***} (0.055)
Initial Return $b_0^{IV^h}$	0.198 ^{***} (0.015)	0.209 ^{***} (0.023)	0.195 ^{***} (0.012)	0.205 ^{***} (0.017)	0.192 ^{***} (0.010)	0.204 ^{***} (0.010)
Limit Return $b_\infty^{IV^h}$	0.055 ^{***} (0.006)	0.049 ^{***} (0.010)	0.055 ^{***} (0.008)	0.054 ^{***} (0.015)	0.050 ^{***} (0.003)	0.045 ^{***} (0.003)
Average Return b^{IV^h}	0.064 ^{***} (0.003)	0.061 ^{***} (0.003)	0.066 ^{***} (0.002)	0.060 ^{***} (0.004)	0.067 ^{***} (0.007)	0.062 ^{***} (0.007)
Average Return b^{IV^t}	0.088 ^{***} (0.024)	0.058 ^{**} (0.029)	0.089 ^{***} (0.019)	0.060 ^{***} (0.005)	–	–
Internal Rate of Return:						
Private Return	0.079	0.075	0.076	0.075	0.072	0.067
Social Return	0.055	0.049	0.055	0.054	0.050	0.045
Signaling Value	30.4%	34.6%	27.6%	28.0%	30.6%	32.8%
Controls:						
Municipality Fixed Effects	✓	✓	✓	✓	✓	✓
Cohort Fixed Effects	✓	✓	✓	✓	✓	✓
Differential Trends		✓		✓		✓

Note: The estimation sample consists of Norwegian males born 1950-1980 observed in earnings data over years 1967-2014 with years of potential experience between 0 and 30 years and annual earnings above 1 SGA threshold (N=14,746,755), partitioned in a hidden IV sample (N=8,697,979) and a transparent IV sample (N=6,048,776) as discussed in Section 4.3. Parameter estimates for the model specification with experience-varying returns to skill (panel A) are based on a combination of the hidden and the transparent IV estimates plotted in Figure 3(a)-(b). In the sequential estimation approach (columns (1)-(2)), we first estimate the $\hat{\lambda}_t$ profile based on the transparent IV estimates using (15) under location normalization $\lambda_0 = 1$, and then insert $\hat{\lambda}_t$ in (14) and solve for the model parameters using the non-linear least squares estimation. In the joint estimation approach (columns (3)-(4)), we jointly solve for the model parameters and λ_t using (14)-(15) by non-linear least squares estimation. Parameters estimates for the model specification with experience-invariant returns to skill (panel B) are based on the hidden IV estimates plotted in Figure 3(a) and constructed using non-linear least squares estimation of (11). Columns (2), (4) and (6) rely on IV estimates that control 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.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Alternative IV Estimates of the Speed of Learning, Initial and Limit Return.

	A. Baseline	B. Sensitivity to Unbalanced Panel of Worker Careers			C. Sensitivity to Alternative Earnings Thresholds		
		Years of Experience $t \leq 10$	Birth Cohorts 1950–65	Birth Cohorts 1940–80	Earnings > .5 SGA	Earnings > .75 SGA	Earnings > 1.5 SGA
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Parameters:							
Speed of Learning κ	0.532 ^{***} (0.058)	0.605 ^{***} (0.065)	0.753 ^{***} (0.079)	0.485 ^{***} (0.049)	0.594 ^{***} (0.070)	0.498 ^{***} (0.062)	0.656 ^{***} (0.041)
Initial Return $b_0^{IV^h}$	0.192 ^{***} (0.010)	0.193 ^{***} (0.008)	0.159 ^{***} (0.010)	0.161 ^{***} (0.007)	0.209 ^{***} (0.015)	0.231 ^{***} (0.011)	0.189 ^{***} (0.007)
Limit Return $b_\infty^{IV^h}$	0.050 ^{***} (0.003)	0.056 ^{***} (0.005)	0.041 ^{***} (0.002)	0.042 ^{***} (0.002)	0.054 ^{***} (0.004)	0.049 ^{***} (0.004)	0.050 ^{***} (0.002)
Controls:							
Municipality FEs	✓	✓	✓	✓	✓	✓	✓
Cohort FEs	✓	✓	✓	✓	✓	✓	✓

Note: The baseline hidden IV sample in column (1) 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, excluding 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). In column (2), we restrict estimations to years of potential experience between 0 and 10 years for the same sample as in column (1). In column (3), we restrict the sample in column (1) to males born 1950-1965 (N=5,326,336), while in column (4), we instead expand this sample to males born 1950-1980 and also include observations with missing IQ data (N=13,172,738). In column (5), we expand the sample in column (1) to include individuals with annual earnings above 50% of the SGA threshold (N=8,918,180), while in column (6), we similarly include individuals with annual earnings above 75% of the SGA threshold (N=8,803,482). In column (7), we instead restrict the sample in column (1) to include individuals with annual earnings above 150% of the SGA threshold (N=8,486,015).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: IV Estimates of Years of Schooling on Standardized IQ Test Scores.

	A. Full Sample		B. Hidden IV Sample		C. Transparent IV Sample	
	Baseline (1)	Trends (2)	Baseline (3)	Trends (4)	Baseline (5)	Trends (6)
Reduced Form:						
<i>Exposure to Reform</i>	0.041 ^{***} (0.009)	0.036 ^{***} (0.013)	0.047 ^{***} (0.014)	0.037 ^{***} (0.016)	0.037 ^{***} (0.010)	0.035 ^{***} (0.011)
IV Estimates:						
<i>Years of Schooling at Age 18</i>	0.265 ^{***} (0.055)	0.235 ^{***} (0.074)	0.318 ^{***} (0.075)	0.258 ^{**} (0.099)	0.229 ^{***} (0.060)	0.219 ^{**} (0.068)
Municipality Fixed Effects	✓	✓	✓	✓	✓	✓
Cohort Fixed Effects	✓	✓	✓	✓	✓	✓
Municipality-Specific Trends		✓		✓		✓

Note: The full sample (panel A) 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 hidden IV sample (panel B) further drops individuals who grew up in the municipality with the largest population size in each of the 160 labor market regions in Norway (N=8,697,979), while the transparent IV sample (panel C) retains only individuals who grew up in the municipality with the largest population size in each labor market (N=6,048,776). All estimations include fixed effects for birth cohort and childhood municipality. The trends specifications in columns (2), (4) and (6) 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.

* p < 0.10, ** < 0.05, *** p < 0.01.

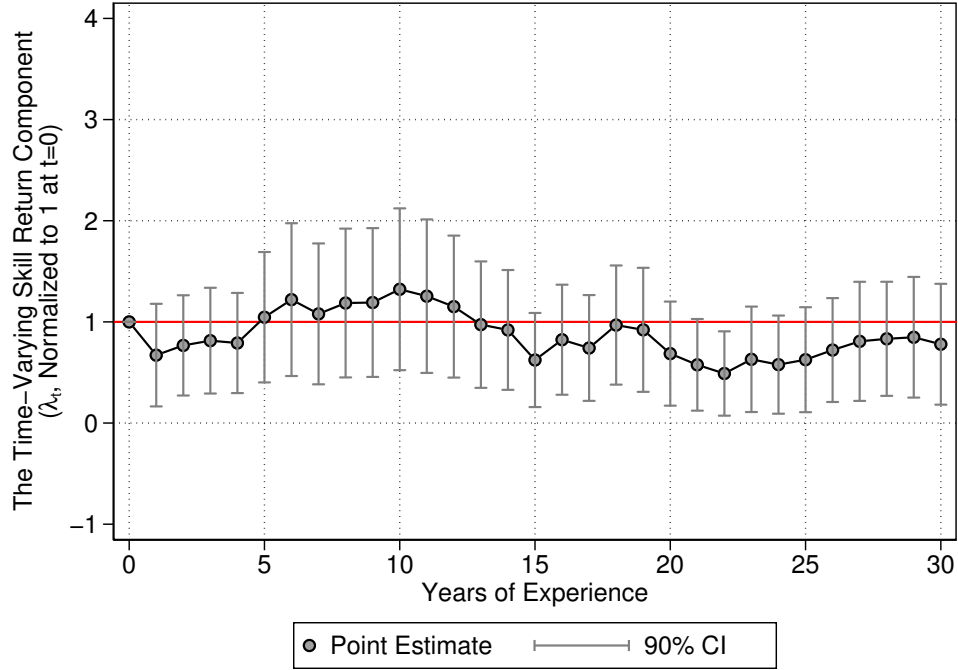
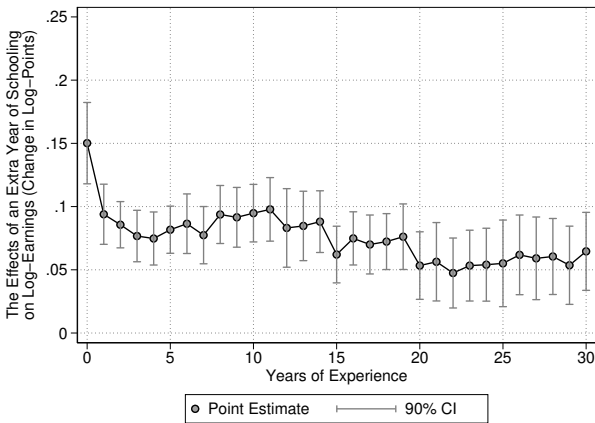
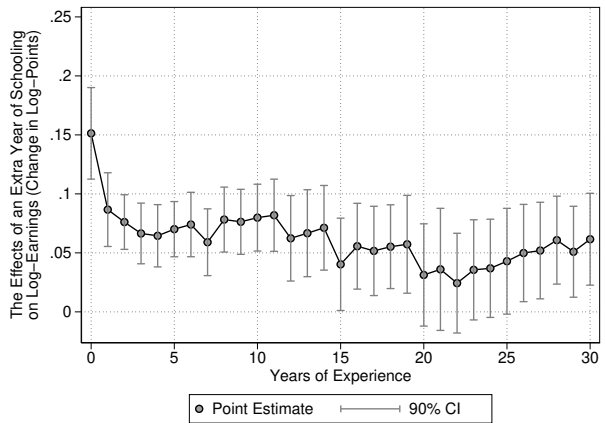


Figure A.1: The Experience-Varying Component of Returns to Skill – λ_t .

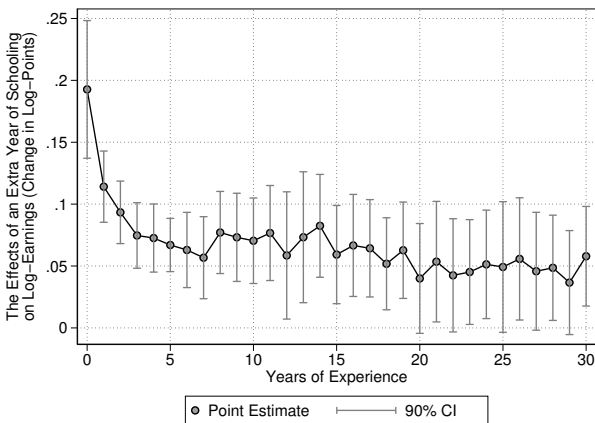
Note: The $\hat{\lambda}_t$ estimates are constructed using the IV estimates displayed in Figure 3-(b) for the transparent IV sample. We use the formula $\hat{\lambda}_t = (\hat{b}_t^{IV^t} / \hat{b}_0^{IV^t})$ for $t > 0$ and $\lambda_0 = 1$ (location normalization), and employ the delta-method to construct standard errors. The 90% confidence intervals corresponding to each point estimate are displayed as vertical bars. The joint test for the hypothesis that for all $t \in \mathbb{T}_{>0}$ the ratio $\hat{\lambda}_t = 1$ provides an F-statistic of 0.9, which means that we cannot statistically reject the hypothesis of constant social returns. The transparent IV 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 who who grew up in the municipality with the largest population size in each labor market (N=6,048,776).



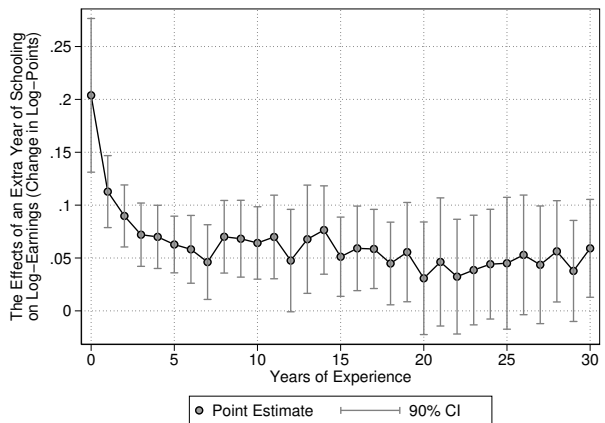
(a) Full Sample



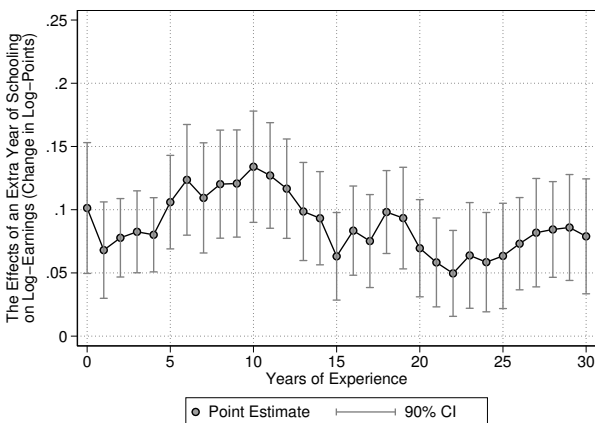
(b) Full Sample w/ Differential Trends



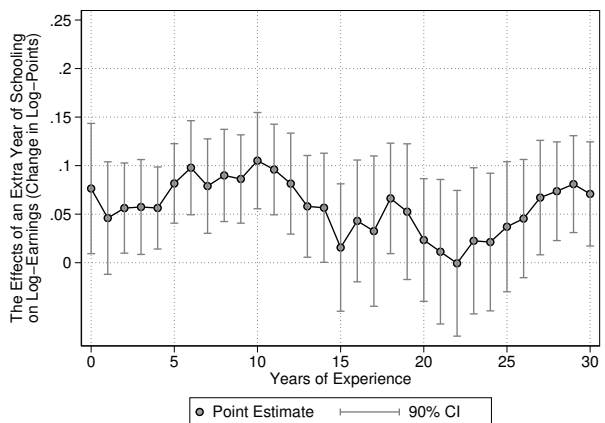
(c) Hidden IV Sample



(d) Hidden IV Sample w/ Differential Trends



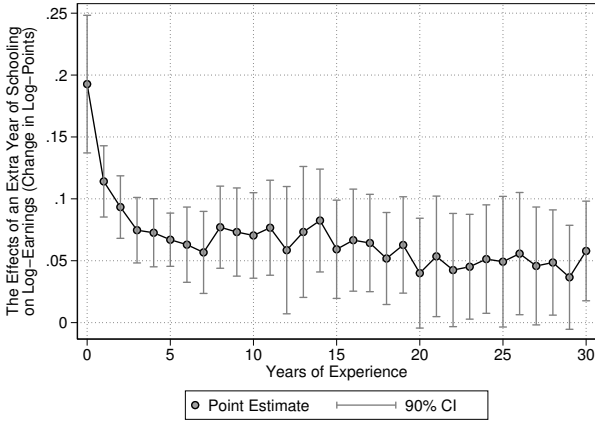
(e) Transparent IV Sample



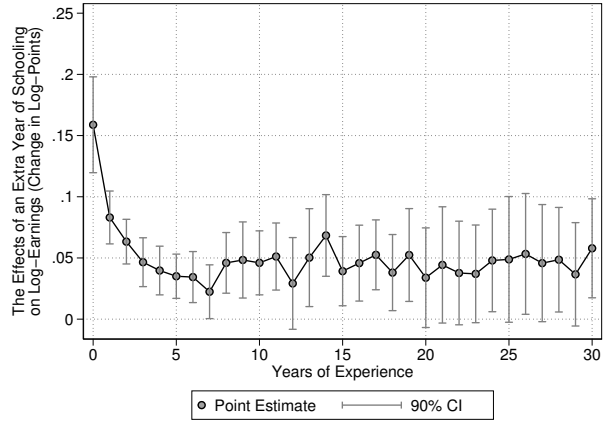
(f) Transparent IV Sample w/ Differential Trends

Figure A.2: IV Estimates of the Returns to Schooling – Controlling for Differential Trends.

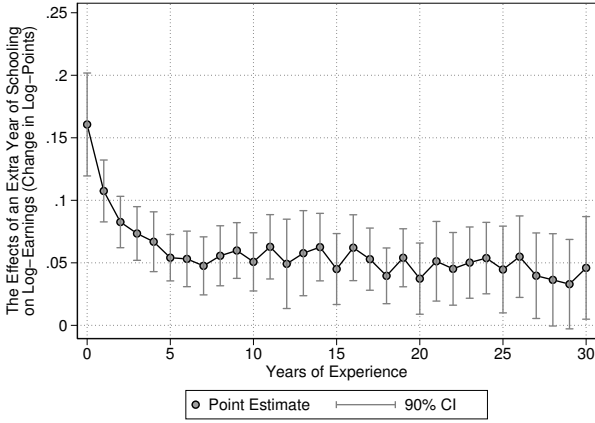
Note: See notes below Figure 3 for details on each estimation. Plots (b), (d) and (f) control for linear and quadratic municipality-specific trends estimated using data on all pre-reform cohorts born 1930 or later and extrapolated to post-reform cohorts.



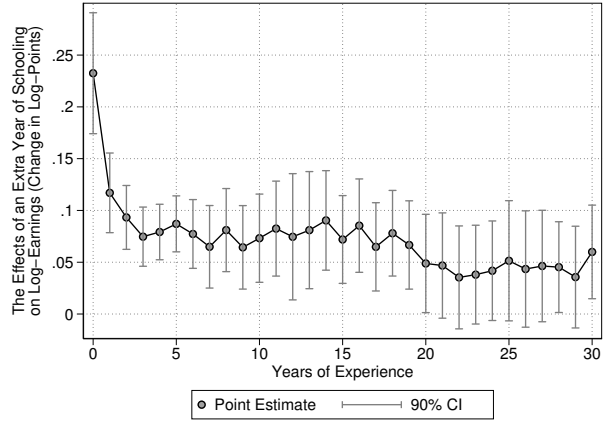
(a) Baseline Hidden IV



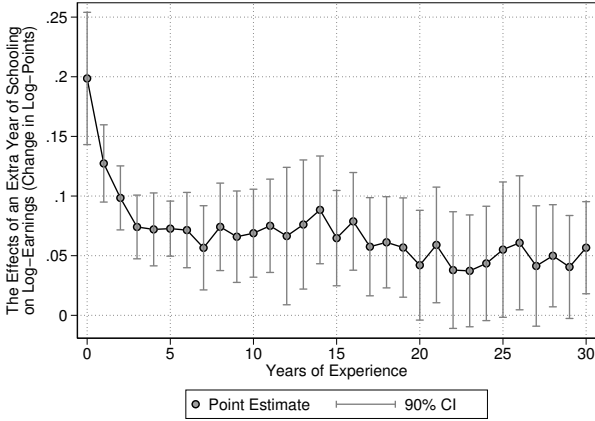
(b) Birth Cohorts 1950-65



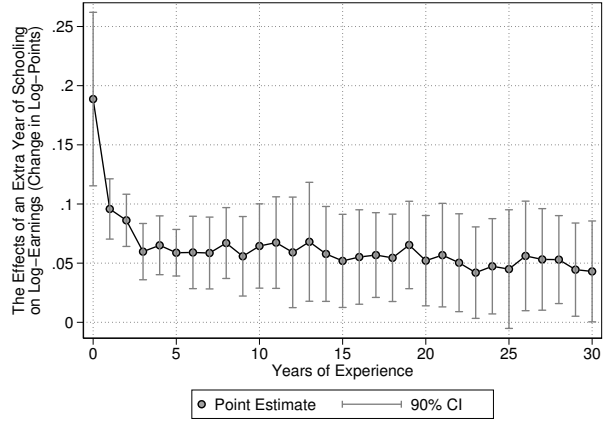
(c) Birth Cohorts 1940-80



(d) Annual Earnings > .5 SGA



(e) Annual Earnings > .75 SGA



(f) Annual Earnings > 1.5 SGA

Figure A.3: Alternative Hidden IV Estimates of the Returns to Schooling.

Note: See notes below Table A.3 for details on each estimation.

B Implementation of the Hidden Correlate Approach

We discuss here identification and evidence using a hidden correlate of ability, as in [Altonji and Pierret \[2001\]](#) and [Lange \[2007\]](#). For this purpose we use an IQ test score from the Norwegian Armed Forces available for Norwegian male conscripts born 1950–1980.

B.1 Identification

We consider identifying the standard employer learning model with experience-invariant returns to skill, i.e., $\lambda_t = 1$ for all $t \in \mathbb{T}$, using a hidden correlate of ability. This approach relies entirely on examining the changes in the regression coefficients on observed and hidden correlates with experience in order to estimate the speed of learning. The identification results below do not extend to experience-varying returns to skill, as in [Section 3.3](#).

Bias in the OLS. We begin by considering the case where no additional information beyond schooling and earnings at each experience is available. The researcher considers the regression of log-earnings on years of schooling for any given level of experience. Using [\(7\)](#), we can derive the probability limit of the OLS estimate of the coefficient for schooling, evaluated at experience t , to be

$$\text{plim } \hat{b}_t^{OLS} = \underbrace{\delta_t^{W|S}}_{\text{private returns}} + \underbrace{(1 - \theta_t) \frac{\text{cov}(\tilde{A}, S)}{\text{var}(S)}}_{\text{omitted variable bias}}. \quad (\text{B.1})$$

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{A}) correlate with, but are not *caused* by, schooling.¹ This omitted ability bias is the main reason why researchers rely on instrumental variable approach to identify the returns to education. The magnitude of the

¹At $t = 0$, however, $\text{plim } \hat{b}_t^{OLS} = \delta_t^{W|S}$ since $\theta_0 = 1$. Note that this result doesn't hold if, unlike the researcher, the employers also observe an ability correlate Q_i (see [Equation \(E.1\)](#)), as the bias term will then also depend on the employer-observed ability component correlated with schooling. The OLS probability limit is $\text{plim } \hat{b}_t^{OLS} = \delta_t^{W|S} + (\beta_{wq} + \theta_t \phi_{A|Q}) \frac{\text{cov}(\tilde{Q}, S)}{\text{var}(S)} + (1 - \theta_t) \frac{\text{cov}(\tilde{A}, S)}{\text{var}(S)}$, so the bias is present even at $t = 0$.

bias depends on the speed of learning, κ , which determines the weight θ_t employers put on the initial signal at time t .

Now, let us consider what happens to this bias as workers accumulate work experience, i.e., as $t \rightarrow \infty$. Taking the limit in (B.1) and using $\lim_{t \rightarrow \infty} \delta_t^{W|S} = \delta^{\psi|S}$ from (8) we get

$$\text{plim} \left(\lim_{t \rightarrow \infty} \hat{b}_t^{OLS} \right) = \underbrace{\delta^{\psi|S}}_{\text{social returns}} + \underbrace{\frac{\text{cov}(\tilde{A}, S)}{\text{var}(S)}}_{\text{remaining bias}}. \quad (\text{B.2})$$

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.

Exploiting a Hidden Correlate of Ability. We briefly review the approach based on hidden correlates developed by [Altonji and Pierret \[2001\]](#) and [Lange \[2007\]](#).²

Suppose that we have access to a correlate of ability, denoted by Z , and suppose Z is unobserved by employers. We refer to Z as a “hidden” correlate of ability. Furthermore, suppose that $A_i = \beta_{Az}Z_i + \eta_i$ and $\eta_i \perp Z_i$ so that η_i represents the productivity component observed by neither researchers nor employers. Substituting A_i in (1) and $\lambda_t = 1$ for all $t \in \mathbb{T}$ gives

$$\psi_{it} = \ln(\chi_{it}) = (\beta_{ws}S_i + \beta_{Az}Z_i + \eta_i) + H(t) + \varepsilon_{it}.$$

Then following the steps from Section B in [[Lange, 2007](#)], we can show that

$$\mathbb{E}[\ln W_{it}|S, Z, t] = (\theta_t \mathbb{E}[\ln W_{i0}|S, Z] + (1 - \theta_t) \mathbb{E}[\ln W_{i\infty}|S, Z]), \quad (\text{B.3})$$

where W_{i0} is the wage received in period $t = 0$ and $W_{i\infty}$ is the wage received at $t \rightarrow \infty$, when enough information has been revealed so that worker productivity is known in the market.

To estimate the speed of learning we first project log-earnings on (S, Z) for each ex-

²Our parameterization differs from that in [Altonji and Pierret \[2001\]](#) and [Lange \[2007\]](#). Lange does not directly parameterize the causal effect of schooling ($\delta_t^{\psi|S}$). Rather, he estimates the speed of learning from the changes in the covariance structure across wages and another ability correlate across experience.

perience t . At each t , the regressions coefficients are a mixture of those at $t = 0$ and the hypothetical regression coefficients for the limit $t = \infty$. The weights in this mixture are provided by θ_t . The functional form of θ_t is known and θ_t depends only on the speed of learning κ and t . This implies that we can identify κ from the rate at which the regression coefficients converge from their $t = 0$ to their $t = \infty$ values.

The projection coefficients obtained from estimating (B.3) across different experience levels, however, do not identify the causal effect of S or Z on productivity because (S, Z) might correlate with the omitted variable (η). Thus, while we can identify κ if we have a hidden correlate of ability, we cannot identify the private or the social returns to education without additional information.³

B.2 Empirical Specification

As discussed above, we can estimate the speed of learning κ using the OLS estimates relying on the IQ test score as a hidden correlate. This approach requires projecting log-earnings on schooling S , IQ score, Z , and other control variables, X , at different work experience level t :

$$\ln W_{it} = a_t^{OLS} + b_t^{OLS} S_i + c_t^{OLS} Z_i + e_t^{OLS} X_i + u_{it}^{OLS}. \quad (\text{B.4})$$

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 κ . See Lange [2007] for further details.

Nonlinear Returns and Reweighting. When the true relationship between log-earnings and schooling is non-linear, comparisons of OLS and IV estimates that we get from (16) and (B.4) are complicated because the OLS and the IV estimates weigh different marginal returns

³Lange [2007] uses optimality conditions of schooling as this additional information. He bounds the gap between the private and social returns to schooling, which is the signaling value of education, at 25% of the life-time private return to schooling.

to schooling differently.⁴ It is, however, possible to re-weight margin-specific OLS estimates and construct IV-weighted OLS estimates that are comparable to the IV estimates.

Consider the following non-linear relationship between log-earnings and schooling:

$$\begin{aligned} \ln W_{it} &= a_t^{OLS} + \sum_{s=8}^{21} \gamma_{1,s,t}^{OLS} \times \mathbb{1}(S_i \geq s) + \sum_{s=7}^{21} \gamma_{2,s,t}^{OLS} \times \mathbb{1}(S_i = s) \times Z_{it} + e_t X_i + u_{it}^{OLS}; \\ \pi_s^{IV} &= \frac{\text{cov}(\mathbb{1}(S_i \geq s), D_i)}{\text{cov}(S_i, D_i)}; \\ b_t^{WOLS} &= \sum_{s=8}^{21} \gamma_{1,s,t}^{OLS} \times \pi_s^{IV}; \quad c_t^{WOLS} = \sum_{s=8}^{21} \gamma_{2,s,t}^{OLS} \times \pi_s^{IV}, \end{aligned} \tag{B.5}$$

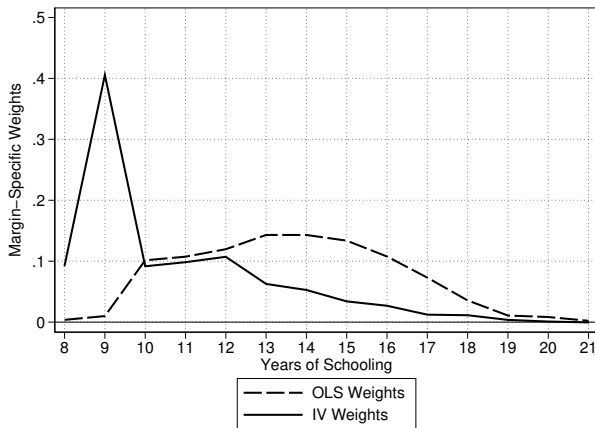
where $\mathbb{1}(S_i \geq s)$ is an indicator for having at least s years of schooling and $\mathbb{1}(S_i = s)$ is an indicator for having exactly s years of schooling. The non-linear relationship between log-earnings and schooling is captured by parameters $\gamma_{1,s,t}^{OLS}$, i.e., the marginal return to schooling year s compared to $s-1$. Similarly, the returns to IQ can also depend on schooling, $\gamma_{2,s,t}^{OLS}$.

Following Løken et al. [2012], we now construct the margin-specific IV weights π_s^{IV} that mimic the variation exploited by the IV estimator. As before, $D_i \in \{0, 1\}$ is a binary instrument equal to 1 if individual i was exposed to the compulsory schooling reform 0 otherwise. Intuitively, since this reform increased schooling attainment mainly at the lower end of schooling distribution, the IV estimates also emphasize the marginal effects of schooling for these individuals most affected by the instrument. The IV weights are shown in Figure B.1-(a). 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 schooling's marginal effects in the lower end of the schooling distribution. This result is consistent with the reform instrument triggering schooling attainment changes mainly at the lower end of the schooling distribution.⁵

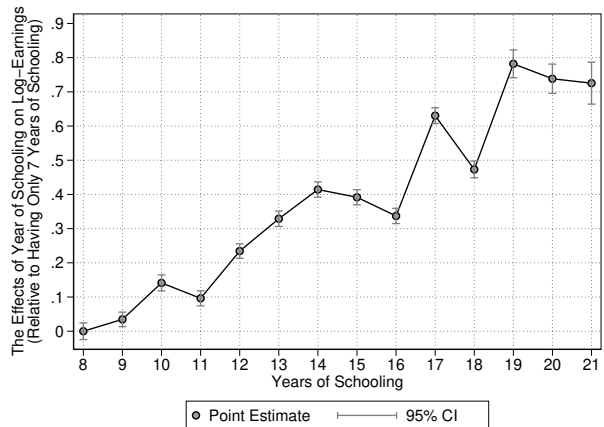
To construct comparable OLS estimates of returns to schooling and IQ that pertain to

⁴See, e.g., discussions in Angrist and Imbens [1995]; Angrist and Krueger [1999]; Heckman et al. [2006b]; Løken et al. [2012]; Lochner and Moretti [2015] and Mogstad and Wiswall [2016].

⁵One can easily extend this approach to allow the IV weights π_s^{IV} to depend on predetermined covariates by estimating the covariances between the IV and schooling dummies separately for different subsamples.



(a) Margin-Specific OLS and IV Weights



(b) Non-Linear Returns to Schooling

Figure B.1: Non-Linear Returns to Schooling and Margin-Specific OLS and IV Weights.

Note: Panel (a) plots the margin-specific OLS and IV weights at each year of schooling. Panel (b) 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 the 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 schooling.

the same part of the schooling distribution as the IV, we now use the estimated IV weights π_s^{IV} together with OLS estimates of margin-specific returns to schooling $\gamma_{1,s,t}^{OLS}$ and schooling-specific estimates of returns to IQ $\gamma_{2,s,t}^{OLS}$. Let b_t^{WOLS} denote the IV-weighted OLS returns to education. The parameter b_t^{WOLS} is essentially a weighted sum of margin-specific OLS estimates $\gamma_{1,s,t}^{OLS}$ from a non-linear relationship between schooling and log-earnings, constructed using IV weights π_s^{IV} . Similarly, let c_t^{WOLS} denote the IV-weighted OLS return of IQ. Analog to b_t^{WOLS} , the parameter c_t^{WOLS} is a weighted sum of schooling-specific OLS estimates of returns to IQ $\gamma_{2,s,t}^{OLS}$, which are again weighted up using IV weights π_s^{IV} .

Using the specification in (B.5), we can thus construct IV-weighted OLS estimates of returns to education and returns to IQ. Two components determine the differences between the IV-weighted returns to education and the standard linear OLS estimates. One is the extent of non-linearity in the margin-specific OLS estimates $\gamma_{1,s,t}^{OLS}$, and the other is the differences between the IV weights π_s^{IV} and the corresponding OLS regression weights. In Figure B.1-(b), we display the (stacked average of) estimates $\gamma_{1,s,t}^{OLS}$ at experiences $t = (10, \dots, 20)$ for each additional year of schooling relative to 7 years of compulsory schooling, which illustrates

the non-linear relationship between log-earnings and schooling in our data.

This reweighting approach allows us to construct OLS estimates of returns to education that can be compared to the IV estimates of returns to education, in the sense that the two estimators weigh the potentially different returns at different schooling margins similarly. The reweighting approach, however, does not solve concerns about ability bias in the OLS estimates, and even IV-weighted OLS estimates do not capture private or social returns. See [Lochner and Moretti \[2015\]](#) for tests for endogeneity in such settings.

B.3 Evidence Using a Hidden Correlate of Ability

We start with the estimates from a standard Mincer returns to education. That is, we examine the coefficient on years of schooling in a specification that controls for a flexible experience profile, but does not allow for the effect of schooling on log-earnings to vary with experience. This allows us to compare the social returns in [Table 2](#) with the observed average differences in log-earnings associated with schooling in the population. This comparison is of interest because the Mincer coefficient is a commonly used indicator of return to education. We find a Mincer coefficient of 6.9%, exceeding the social returns by 1.4 percentage points. We also estimate [\(B.4\)](#) including IQ (i.e., Z_i) as an additional regressor, again with b^{OLS} and c^{OLS} as constant coefficients. In that case, we get a Mincer estimate of 5.6% and an IQ estimate of 5.2%, where the former indicates sizable ability bias in standard Mincer returns estimates, and the latter shows large wage effects of a standard deviation increase in IQ.

Next, we present results from OLS estimates that use IQ as a hidden (from the employers) correlate of ability. We begin by presenting IV-weighted OLS estimates of [\(B.4\)](#), 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 B.2-\(a\) and B.2-\(b\)](#) 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

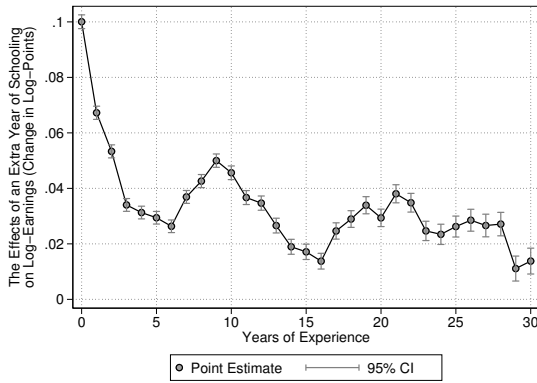
then slowly until stabilizing after 15 years.

Comparing Figure B.2-(a) and Figure 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 returns to schooling and IQ over the workers' careers are similar to those found in the NLSY using the AFQT score.

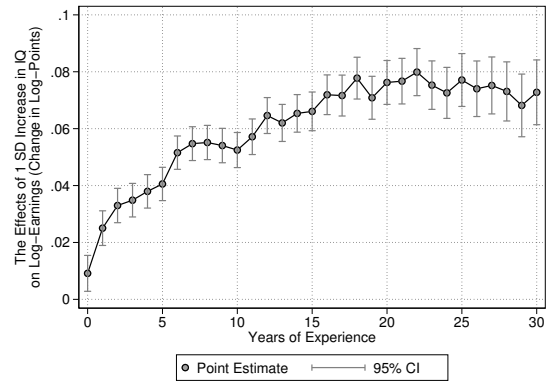
Using also the NLSY data, Arcidiacono et al. [2010] find that the returns to the AFQT score increase with experience only among those with a high school degree or less. They do not vary with experience for those with a college degree. These findings led Arcidiacono et al. [2010] to conclude that a college degree has a direct role in revealing ability, maybe because transcripts, the field of study, reference letters, and internships and other work during college are informative about students' ability.

When we perform the same analysis as Arcidiacono et al. [2010], i.e., split our estimation sample into two groups—one with at most a high school degree and the other with a college degree—we find similar results in our data. Figures B.2-(c) and B.2-(d) display the OLS estimates of returns to schooling and IQ, respectively, for the first sample and Figures B.2-(e) and B.2-(f) show the corresponding estimates for the second sample. We find that returns to IQ increase with experience, but only for those with high school degrees or less. 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 in the Norwegian labor market.

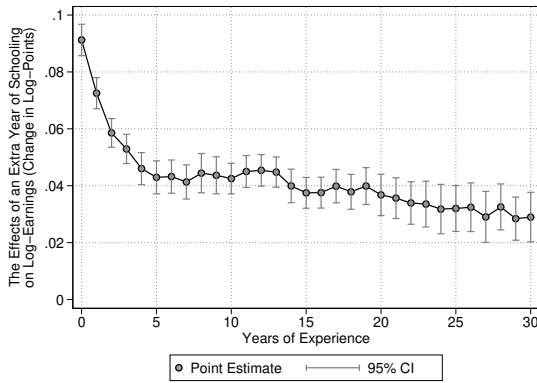
Next, we compare the IV-weighted OLS estimates to the OLS estimates we obtained from separate estimations for the two groups of workers differentiated by their educational attainment. Comparing the estimates of returns to schooling in Figures B.2-(a) and B.2-(c) and returns to IQ in Figures B.2-(b) and B.2-(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 pattern is reassuring because the IV-weighted OLS estimates must put substantially more weight on marginal returns in the lower end of the schooling distribution. In contrast,



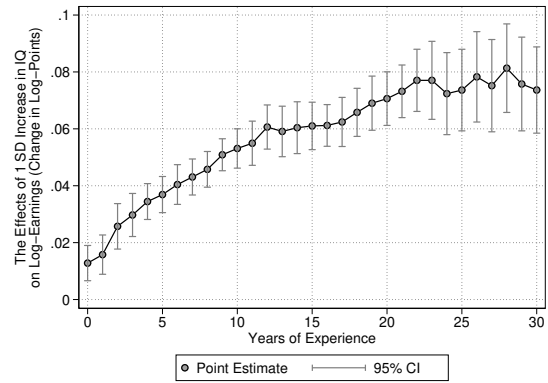
(a) Returns to Schooling: IV-Weighted OLS



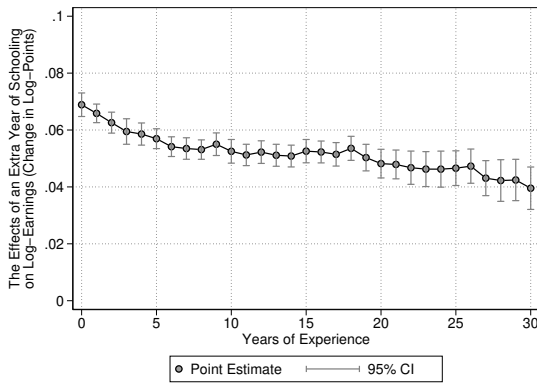
(b) Returns to IQ: IV-Weighted OLS



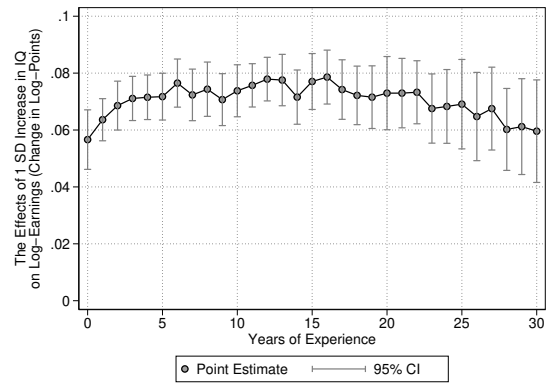
(c) Returns to Schooling: High School or Below



(d) Returns to IQ: High School or Below



(e) Returns to Schooling: College or University



(f) Returns to IQ: College or University

Figure B.2: 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).

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

B.2 (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 (B.3) we know that $\{b_t^{WOLS}, c_t^{WOLS}\}_{t=0}^T$ satisfy

$$b_t^{WOLS} = \theta_t b_0^{WOLS} + (1 - \theta_t) b_\infty^{WOLS} \quad \text{and} \quad c_t^{WOLS} = \theta_t c_0^{WOLS} + (1 - \theta_t) c_\infty^{WOLS}, \quad (\text{B.6})$$

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 B.2 (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.

Table B.1 displays estimates of the speed of employer learning, κ , 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 NLLS estimation for the three sets of OLS estimates of returns to schooling and IQ displayed in Figure B.2. Estimates in each panel in Table B.1 correspond to one of the three sets of estimates in Figure B.2. As noted by Lange [2007], κ is over-identified when a hidden correlate of ability is available so that we can construct two different estimates of κ 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 κ should be identical. Alternatively, we can restrict the estimate of κ using the returns to schooling to be the same as the κ using the returns to IQ. We implement both methods and present the estimation results in Table B.1. 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 B.2, 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 B.1. 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

Table B.1: OLS Estimates of the Speed of Employer Learning, Initial and Limit Return.

	Two Values of κ		One Value of κ	
	(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 κ	0.476 ^{***} (0.069)	0.126 ^{***} (0.034)	0.244 ^{***} (0.044)	
Initial Return (b_0^{WOLS}, c_0^{WOLS})	0.100 ^{***} (0.007)	0.011 ^{**} (0.005)	0.084 ^{***} (0.006)	0.001 (0.006)
Limit Return ($b_\infty^{WOLS}, c_\infty^{WOLS}$)	0.023 ^{***} (0.002)	0.093 ^{***} (0.006)	0.016 ^{***} (0.003)	0.081 ^{***} (0.003)
B. Compulsory/High School Sample – Standard OLS				
Speed of Learning κ	0.333 ^{***} (0.034)	0.065 ^{***} (0.010)	0.104 ^{***} (0.015)	
Initial Return (b_0^{OLS}, c_0^{OLS})	0.091 ^{***} (0.003)	0.012 ^{***} (0.002)	0.074 ^{***} (0.003)	0.006 ^{**} (0.003)
Limit Return ($b_\infty^{OLS}, c_\infty^{OLS}$)	0.030 ^{***} (0.001)	0.111 ^{***} (0.006)	0.017 ^{***} (0.003)	0.097 ^{***} (0.004)
C. College/University Sample – Standard OLS				
Speed of Learning κ	0.061 (0.056)	0.788 (0.603)	0.115 (0.080)	
Initial Return (b_0^{OLS}, c_0^{OLS})	0.067 ^{***} (0.003)	0.056 ^{***} (0.004)	0.069 ^{***} (0.003)	0.068 ^{***} (0.002)
Limit Return ($b_\infty^{OLS}, c_\infty^{OLS}$)	0.033 ^{***} (0.008)	0.071 ^{***} (0.001)	0.039 ^{***} (0.004)	0.072 ^{***} (0.002)
Municipality Fixed Effects	✓	✓	✓	✓
Cohort Fixed Effects	✓	✓	✓	✓

Note: The estimation sample consists of Norwegian males born 1950-1980 observed in earnings data over the years 1967-2014 with years of potential experience between 0 and 30 years and annual earnings above 1 SGA threshold (N=14,746,755). We obtain the estimates of the speed of learning, initial values of returns to schooling and IQ, and limit values of returns to schooling from non-linear least squares estimations on the experience-specific returns to schooling and IQ presented in Figure B.2.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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, indicate relatively fast learning by employers. And, compared to the corresponding IV estimates we presented in Table 2, column (1), the IV-weighted OLS estimates of the speed of learning in Table B.1, panel A, column (1), are similar.

There are, however, two striking differences among the estimates in Table B.1. First, comparing estimates of the speed of learning κ across columns (1)-(2), we can reject the assumption of a common learning process for schooling and IQ over worker experience. 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 does not hold 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 B.1, unlike the results shown in panels A-B.

In summary, 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 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%.⁶

C Identification with Partially-Transparent Instrument

In this section, we extend the identification with partially-transparent instrument variables. Let $D^p \in \{0, 1\}$ denote the partially-transparent IV such that a $\rho \in [0, 1]$ fraction of workers be “exposed” to a transparent IV, and $(1 - \rho)$ fraction to a hidden IV. Here, the fraction ρ is unobserved to the researchers.

⁶We also find patterns very similar to Arcidiacono et al. [2010] who report that the evidence of employer learning 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 6-7% for all years of experience.

Experience-Invariant Returns to Skill. D^p satisfies Assumption 1, and with experience-invariant returns to skill, the conditional mean of log wages at t given D^p is

$$\begin{aligned}\mathbb{E} [\ln W_{it}|D_i^p, t] &= \rho \times \mathbb{E} [\ln W_{it}|D_i^t, t] + (1 - \rho) \times \mathbb{E} [\ln W_{it}|D_i^h, t] \\ &= \rho \times \delta^{\psi|S} \times \mathbb{E} [S_i|D_i^t] + (1 - \rho) \times (\delta^{\psi|S} + \theta_t (\phi_{A|S} - \delta^{A|S})) \mathbb{E} [S_i|D_i^h],\end{aligned}\quad (\text{C.1})$$

which is the weighted average of the conditional log wage under transparent and hidden IV. For notational simplicity, and without loss of generality, we assume that the first-stage effect of D^p on schooling does not depend on whether it is hidden or transparent, i.e., $\mathbb{E}[S_i|D_i^h = 1] - \mathbb{E}[S_i|D_i^h = 0] = \mathbb{E}[S_i|D_i^t = 1] - \mathbb{E}[S_i|D_i^t = 0]$. Then from (9), with $\lambda_t = 1$

$$\begin{aligned}\text{plim } \hat{b}_t^{IV^p} &= \frac{\mathbb{E} [\ln W_{it}|D_i^p = 1, t] - \mathbb{E} [\ln W_{it}|D_i^p = 0, t]}{\mathbb{E} [S_i|D_i^p = 1, t] - \mathbb{E} [S_i|D_i^p = 0, t]} \\ &= \delta^{\psi|S} \rho + (\delta^{\psi|S} + \theta_t (\phi_{A|S} - \delta^{A|S})) (1 - \rho) \\ &= \delta^{\psi|S} + \theta_t (\phi_{A|S} - \delta^{A|S}) (1 - \rho),\end{aligned}\quad (\text{C.2})$$

where the second equality follows from (C.1). Thus, a partially-transparent IV identifies a *lower bound* on the private return to education. To identify the social return we have to rely on the information at $t = \infty$, because $\lim_{t \rightarrow \infty} \theta_t = 0$, and from (C.2) we get $\text{plim} \left(\lim_{t \rightarrow \infty} \hat{b}_t^{IV^p} \right) = \delta^{\psi|S}$. Then, subtracting $\text{plim} \left(\lim_{t \rightarrow \infty} \hat{b}_t^{IV^p} \right)$ from (C.2) evaluated at two finite experience levels, $t \neq t'$, and taking their ratios identify $\theta_t/\theta_{t'}$, identifies the speed of learning parameter κ and with it the lower bound of the signaling value.

Experience-Varying Returns to Skill. When returns to skill vary with experience, access to a partially-transparent IV is insufficient to bound the returns to signaling because both λ_t and θ_t vary with t . However, if we have access to a transparent IV and a partially-transparent IV, then under Assumptions 1-4, we can identify the lower bound of the returns to signaling. Then we can use (15) to identify $\{\lambda_t : t \in \mathbb{T}\}$, and the rest of the identification strategy follows the same steps as with the experience-invariant returns to skill.

Furthermore, if we have a hidden IV and a partially-transparent IV, we can point-identify private returns to education and the returns to signaling. As we show next, for this identification result, we rely on the homogeneity of social returns across the two IV samples.

We begin with observation that as with (C.2), D^p identifies a mixture of social and private returns, i.e., from (9) we get $\text{plim } \hat{b}_t^{IV^p} = \lambda_t \times (\delta^{\psi|S} + \theta_t (\phi_{A|S} - \delta^{A|S}) \times (1 - \rho))$. Simplifying further and using $b_0^{IV^h} = \delta^{\psi|S} + \phi_{A|S} - \delta^{A|S}$ and $b_\infty^{IV^h} = \delta^{\psi|S}$ gives

$$\begin{aligned} \text{plim } \hat{b}_t^{IV^p} &= \lambda_t \times \left(\theta_t \times b_0^{IV^h} + (1 - \theta_t) \times b_\infty^{IV^h} \right) - \lambda_t \times \theta_t \times (\rho \times (\phi_{A|S} - \delta^{A|S})) \\ &= \text{plim } \hat{b}_t^{IV^h} - \lambda_t \times \theta_t \times \rho \times (b_0^{IV^h} - b_0^{IV^t}), \end{aligned} \quad (\text{C.3})$$

where the last equality follows from (12). Next, we make the following assumption.

Assumption 4'. (*Homogeneity of Social Returns*) *Let the social returns $\lambda_t \times \delta^{\psi|S}$ be homogenous across the hidden IV sample and partially-transparent IV sample at each $t \in \mathbb{T}$.*

Although Assumption 4' is stronger than Assumption 4 it has a testable implication. In particular, it implies that the hidden IV estimate is always larger than the partially-transparent IV estimate at every $t > 0$.⁷ Suppose D^h and D^p satisfy Assumptions 1-3 and 4'. Evaluating (C.3) at $t = 0$ and using $\theta_0 = 1$ and $\lambda_0 = 1$ give

$$\text{plim } \hat{b}_0^{IV^h} - \text{plim } \hat{b}_0^{IV^p} = \rho \times (b_0^{IV^h} - b_0^{IV^t}), \quad (\text{C.4})$$

which identifies ρ up to $(b_0^{IV^h} - b_0^{IV^t})$. Substituting (C.4) in (C.3) gives $\lambda_t \times \theta_t = \frac{\text{plim } \hat{b}_t^{IV^h} - \text{plim } \hat{b}_t^{IV^p}}{\text{plim } \hat{b}_0^{IV^h} - \text{plim } \hat{b}_0^{IV^p}}$, and substituting this in $\text{plim } \hat{b}_t^{IV^h}$ for $1 \leq t \leq \infty$, and simplifying gives

$$\text{plim } \hat{b}_t^{IV^h} \times \left(\frac{\text{plim } \hat{b}_t^{IV^h} - \text{plim } \hat{b}_t^{IV^p}}{\text{plim } \hat{b}_0^{IV^h} - \text{plim } \hat{b}_0^{IV^p}} \right)^{-1} = b_0^{IV^h} + b_\infty^{IV^h} \times \left(\frac{1 - \theta_t}{\theta_t} \right). \quad (\text{C.5})$$

Assumption 4' implies that $\text{plim } \hat{b}_t^{IV^h} > \text{plim } \hat{b}_t^{IV^p}$ and $\text{plim } \hat{b}_0^{IV^h} \geq \text{plim } \hat{b}_0^{IV^p}$ for $t > 0$.

⁷For instance, this assumption is rejected in our sample. As we can see from the estimates in Figure 3, for some intermediate t , the social returns estimated from transparent IV sample (Figure 3-(b)) is larger than the private returns estimated from hidden IV sample (Figure 3-(a)).

Thus, with a sufficiently large panel, we can use the NLLS method to estimate the right-hand side parameters of (C.5) and from that the speed of learning parameter κ .

D Identification with Heterogeneous Returns

In this section, we extend the employer learning model to allow heterogeneous returns to education and determine conditions under which we can use IV to identify key model parameters. We use the binary potential outcomes framework of Neyman-Rubin. Let schooling takes two values, $S_i \in \{0, 1\}$, where $S_i = 1$ (respectively, 0) denotes a higher (respectively, lower) level of schooling. Worker i is characterized by a vector of potential outcomes $\{\psi_{0,i}, \psi_{1,i}\}$, where $\psi_{S,i}$ is the experience-invariant component of i 's productivity, which subsumes A_i in (1).

As in (1), let $\varepsilon_{i,t} \stackrel{i.i.d}{\sim} \mathcal{N}(0, \sigma_\varepsilon^2)$ are mean-zero “noise” in the production process that are independent of the model primitives. Then the realized productivity $\psi_{i,t}$ at time t is:

$$\psi_{i,t} = S_i \times [\psi_{1,i} + \varepsilon_{1,i,t}] + (1 - S_i) \times [\psi_{0,i} + \varepsilon_{0,i,t}] + H(t). \quad (\text{D.1})$$

For notational ease, we suppress $H(t)$ in the following.⁸ Worker i knows his potential outcomes $\{\psi_{0,i}, \psi_{1,i}\}$, but employers only observe (S_i, ψ_i^t) , where $\psi_i^t := \{\psi_{i,\tau}\}_{\tau < t}$ are observed only for $S_i = S$. Note that observing $\psi_{i,t}$, conditional on S_i is informationally equivalent to observing $\xi_{S_i,i,t} = \psi_{S_i,i} + \varepsilon_{S_i,i,t}$. We can thus denote the employers' information set by $\mathcal{E}_{i,t}^{S_i} = (S_i, \xi_{S_i,i}^t)$, where $\xi_{S_i,i}^t = \{\xi_{S_i,i,\tau}\}_{\tau < t}$. Wages are set equal to the expected productivity, conditional on information $\mathcal{E}_{i,t}^{S_i}$. Let $W_{S_i,i,t} := \mathbb{E}[\psi_{1,i} | \mathcal{E}_{i,t}^{S_i}]$ denotes potential wage outcomes for different S_i . Then we can write the wage equation as

$$W_{i,t} = \mathbb{E}[\psi_{i,t} | \mathcal{E}_{i,t}^{S_i}] = S_i \times W_{1,i,t} + (1 - S_i) \times W_{0,i,t}, \quad (\text{D.2})$$

where the second equality follows from (D.1) and the independence and zero-mean properties

⁸Note 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. We continue to maintain the assumption that S and $H(t)$ are additively separable.

of $\varepsilon_{S_i,i,t}$.

Then, the social returns and the private returns to schooling for i are, respectively

$$\delta_i^{\psi|S_i} := \psi_{1,i} - \psi_{0,i}; \quad (\text{D.3})$$

$$\delta_{i,t}^{W|S_i} := W_{1,i,t} - W_{0,i,t} = \mathbb{E} [\psi_{1,i} | \mathcal{E}_{i,t}^1] - \mathbb{E} [\psi_{0,i} | \mathcal{E}_{i,t}^0]. \quad (\text{D.4})$$

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 (D.3) and (D.4), respectively, while measures such as *social returns for the treated* and *private returns for the treated* are averages across the corresponding populations.

Identification Using Instrumental Variables

To understand what a binary IV identifies, we proceed analogously to [Imbens and Angrist \[1994\]](#). Let $S_i(D_i)$ denote potential schooling, which is a function of $D_i \in \{0, 1\}$, 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 $\mathbb{A} \equiv \{i | S_i(1) = 1, \text{ and } S_i(0) = 1\}$ 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

$$\text{plim } \hat{b}_t^{IV} := \frac{\mathbb{E}[W_{i,t} | D_i = 1] - \mathbb{E}[W_{i,t} | D_i = 0]}{\mathbb{E}[S_i | D_i = 1] - \mathbb{E}[S_i | D_i = 0]}. \quad (\text{D.5})$$

As D satisfies the monotonicity condition, $\Pr(\mathbb{D}) = 0$, (D.5)'s denominator simplifies to

$$\begin{aligned} \mathbb{E}[S_i | D_i = 1] - \mathbb{E}[S_i | D_i = 0] &= (\mathbb{E}[S_i | D_i = 1, \mathbb{A}] - \mathbb{E}[S_i | D_i = 0, \mathbb{A}]) \times \Pr(\mathbb{A}) \\ &+ (\mathbb{E}[S_i | D_i = 1, \mathbb{N}] - \mathbb{E}[S_i | D_i = 0, \mathbb{N}]) \times \Pr(\mathbb{N}) \\ &+ (\mathbb{E}[S_i | D_i = 1, \mathbb{C}] - \mathbb{E}[S_i | D_i = 0, \mathbb{C}]) \times \Pr(\mathbb{C}) \\ &+ (\mathbb{E}[S_i | D_i = 1, \mathbb{D}] - \mathbb{E}[S_i | D_i = 0, \mathbb{D}]) \times \Pr(\mathbb{D}) = \Pr(\mathbb{C}). \end{aligned} \quad (\text{D.6})$$

As before, $D_i^h \in \{0, 1\}$ denotes a hidden IV and $D_i^t \in \{0, 1\}$ a transparent IV. 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^h , (D.5)'s numerator can be expressed as

$$\begin{aligned} \mathbb{E}[W_{i,t}|D_i^h = 1] - \mathbb{E}[W_{i,t}|D_i^h = 0] &= \left(\mathbb{E}[W_{i,t}|D_i^h = 1, \mathbb{C}] - \mathbb{E}[W_{i,t}|D_i^h = 0, \mathbb{C}] \right) \times \Pr(\mathbb{C}) \\ &= \left(\mathbb{E}[W_{1,i,t}|\mathbb{C}] - \mathbb{E}[W_{0,i,t}|\mathbb{C}] \right) \times \Pr(\mathbb{C}) = \mathbb{E}[\delta_{i,t}^{W|S}|\mathbb{C}] \times \Pr(\mathbb{C}). \end{aligned} \quad (\text{D.7})$$

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 (D.2) and using the properties of a hidden IV, and the last from the definition of private returns in (D.4). Using (D.6) and (D.7) in (D.5) with a hidden IV, gives $\text{plim } \hat{b}_i^{IV^h} = \mathbb{E}[\delta_{i,t}^{W|S}|\mathbb{C}]$. Thus, a binary hidden IV identifies the *average private returns* to education among compliers.

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

$$\begin{aligned} \mathbb{E}[W_{i,t}|D_i^t = 1] - \mathbb{E}[W_{i,t}|D_i^t = 0] &= \mathbb{E}[\psi_{i,t}|D_i^t = 1] - \mathbb{E}[\psi_{i,t}|D_i^t = 0] \\ &= \mathbb{E}[\psi_{1,i} - \psi_{0,i}|\mathbb{C}] \times \Pr(\mathbb{C}) = \mathbb{E}[\delta_i^{\psi|S}|\mathbb{C}] \times \Pr(\mathbb{C}). \end{aligned} \quad (\text{D.8})$$

Using (D.6) and (D.8) in (D.5) gives $\text{plim } \hat{b}_i^{IV^t} = \mathbb{E}[\delta_i^{\psi|S}|\mathbb{C}]$. Thus, a binary transparent IV identifies the *average social returns* to education among compliers. Therefore, with heterogeneous returns, a transparent IV identifies the average social returns and a hidden IV identifies the average private returns to education for the compliers.

Speed of Learning. Next, we consider identifying the speed of learning by determining how quickly the market learns workers' ability. The speed of learning will depend on schooling and the selection between schooling and unobserved ability. Let the unobserved components

of productivity, conditional on schooling S , follow a Normal distribution, i.e., $\psi_{S,i}|S \sim \mathcal{N}(\mu_{\psi,S}, \sigma_{\psi}^2)$. Using the potential wage outcomes defined in (D.2), and the Kalman property as in (4), we can express wages as

$$W_{S_i,i,t} = \mathbb{E}[\psi_{S_i,i}|\mathcal{E}_{i,t}^{S_i}] = \theta_t \times \mu_{\psi,S} + (1 - \theta_t) \times \bar{\xi}_{S_i,i}^t, \quad (\text{D.9})$$

where, $\theta_t = \frac{1-\kappa}{1+(t-1)\kappa}$, $\kappa = \frac{\sigma_{\psi}^2}{\sigma_{\psi}^2 + \sigma_{\varepsilon}^2}$ is the speed of learning, and $\bar{\xi}_{S_i,i}^t = \frac{1}{t} \sum_{\tau < t} \xi_{S_i,i,\tau}$ is the average of signals up to period t . Conditional expectation of (D.9) for $(S_i, \psi_{S_i,i})$ gives

$$\begin{aligned} \mathbb{E}[W_{S_i,i,t}|S_i, \psi_{S_i,i}] &= \theta_t \times \mu_{\psi,S} + (1 - \theta_t) \times \mathbb{E}[\bar{\xi}_{S_i,i}^t|S_i, \tilde{\psi}_{S_i,i}] = \theta_t \times \mu_{\psi,S} + (1 - \theta_t) \times \psi_{S_i,i} \\ &= \mu_{\psi,S} + (1 - \theta_t) \times (\psi_{S_i,i} - \mu_{\psi,S}). \end{aligned} \quad (\text{D.10})$$

Thus, the wage of a worker with schooling S_i at with t years of work experience is the sum of the average productivity $\mu_{\psi,S}$ and the deviation of i 's productivity from its mean $(\psi_{S_i,i} - \mu_{\psi,S})$ augmented by employers' learning $(1 - \theta_t)$. So, at the start, i.e., $t = 0$, i 's wage is the average productivity because $\theta_0 = 1$, and as information about i 's ability is accumulated in the market, i 's wage becomes more responsive to i 's true productivity as $\lim_{t \rightarrow \infty} \theta_t = 0$. Using (D.4), (D.7) and (D.10) in (D.5) the hidden IV estimate becomes

$$\begin{aligned} \text{plim } \hat{b}_t^{IV^b} &= \mathbb{E}[(\mu_{\psi,1} - \mu_{\psi,0}) + (1 - \theta_t)(\psi_{1,i} - \mu_{\psi,1}) - (1 - \theta_t)(\psi_{0,i} - \mu_{\psi,0}) | \mathbb{C}] \\ &= \underbrace{\mathbb{E}[(\mu_{\psi,1} - \mu_{\psi,0}) | \mathbb{C}]}_{:=\Upsilon} + (1 - \theta_t) \left(\underbrace{\mathbb{E}[(\psi_{1,i} - \mu_{\psi,1}) | \mathbb{C}]}_{:=\Upsilon_1} - \underbrace{\mathbb{E}[(\psi_{0,i} - \mu_{\psi,0}) | \mathbb{C}]}_{:=\Upsilon_0} \right) \\ &:= \Upsilon + (1 - \theta_t)(\Upsilon_1 - \Upsilon_0). \end{aligned} \quad (\text{D.11})$$

Furthermore, because private returns are greater than social returns, it follows from (D.3) and (D.4) that $\Upsilon_1 < \Upsilon_0$, and T is sufficiently large. Then we can use (D.11) to identify θ_t and the speed of learning parameter κ . For instance, at $t = 0$ we can get $\text{plim } \hat{b}_0^{IV^b} = \Upsilon$ and at $t \rightarrow \infty$ we get $\text{plim } \hat{b}_{\infty}^{IV^b} = \Upsilon + (\Upsilon_1 - \Upsilon_0)$. So, for $0 < t < \infty$ we identify

$$\theta_t = \frac{\text{plim } \hat{b}_t^{IV^b} - \text{plim } \hat{b}_0^{IV^b}}{\text{plim } \hat{b}_{\infty}^{IV^b} - \text{plim } \hat{b}_0^{IV^b}}.$$

Experience-Varying Returns to Skills. We end this section by briefly considering the identification with experience-varying returns to skill. First note that with experience-varying returns to skill, realized productivity (D.1) becomes

$$\psi_{i,t} = \lambda_t \times (S_i \times [\psi_{1,i} + \varepsilon_{1,i,t}] + (1 - S_i) \times [\psi_{0,i} + \varepsilon_{0,i,t}]) + H(t). \quad (\text{D.12})$$

Once we re-define social and private returns to include the effect of λ_t , following the same identification strategy as above, it follows that the hidden IV identifies the average private returns and transparent IV identifies the average social returns, among the compliers, at each work experience $t \in \mathbb{T}$. Suppose, as before, we have access to a hidden IV and a transparent IV and suppose $\{\lambda_t : t \in \mathbb{T}\}$ satisfy Assumption 4.

The hidden IV and the transparent IV estimates the (average) private and (average) social returns for different complier groups. Under Assumption 4, however, we can combine the two sets of estimates to identify the model with experience-varying returns for hidden IV compliers, even though the transparent IV estimates relate to a different set of compliers.

In particular, following the same step as in (D.11) we get

$$\text{plim } \hat{b}_t^{IV^b} = \lambda_t \Upsilon + \lambda_t (1 - \theta_t) (\Upsilon_1 - \Upsilon_0); \quad (\text{D.13})$$

$$\text{plim } \hat{b}_t^{IV^t} = \lambda_t \Upsilon + \lambda_t (\Upsilon_1 - \Upsilon_0). \quad (\text{D.14})$$

Evaluating (D.13) at $t = 0$, and using $\lambda_0 = 1$, identifies $\text{plim } \hat{b}_0^{IV^b} = \Upsilon$. Substituting it in (D.14) at $t = 0$ identifies $\text{plim } \hat{b}_0^{IV^t} - \text{plim } \hat{b}_0^{IV^b} = (\Upsilon_1 - \Upsilon_0)$. Then from the transparent IV (D.14) we can identify $\{\lambda_t : t > 0\}$. Then using these variables in (D.13) we identify κ .

E Identification with Employer-Observed Correlate

In this section, we extend our primary model to allow employers to observe a correlate of ability Q that the researcher does not observe. Throughout this section, we maintain all

other assumptions from our model. Worker i 's log-productivity for $t \in \mathbb{T}$ is given by

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

where Q is a correlate of ability observed by employers and is possibly correlated with A . An example of a Q could be knowledge of a foreign language, which is typically mentioned in job applicants' résumés, and can be verified by the employers.

To model employer learning in addition to $\varepsilon_{it} \stackrel{i.i.d.}{\sim} \mathcal{N}(0, \sigma_\varepsilon^2)$ let $(S_i, Q_i, A_i) \stackrel{i.i.d.}{\sim} \mathcal{N}(\boldsymbol{\mu}, \Sigma)$, across workers and across time. The joint normality assumption allows us to express A as a linear function of (S, Q)

$$A_i = \phi_{A|S} S_i + \phi_{A|Q} Q_i + \varepsilon_{A_i|S_i, Q_i}, \quad (\text{E.2})$$

where $\varepsilon_{A_i|S_i, Q_i} := A_i - \mathbb{E}[A_i|S_i, Q_i]$. Under perfect competition, log wages is

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

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

The normality assumptions also allow us to use the Kalman filter to write the conditional expectation of ability $\mathbb{E}[A_i|\mathcal{E}_{it}]$ in linear form as

$$\mathbb{E}[A_i|\mathcal{E}_{it}] = \theta_t \mathbb{E}[A_i|S_i, Q_i] + (1 - \theta_t) \bar{\xi}_i^t, \quad (\text{E.4})$$

where $\bar{\xi}_i^t = \frac{1}{t} \sum_{\tau < t} \xi_{i\tau}$ is the average of signals up to period t and $\theta_t = \frac{1-\kappa}{1+(t-1)\kappa} \in [0, 1]$ is the weight on the initial signal (S_i, Q_i) with $\kappa = \frac{\sigma_0^2}{\sigma_0^2 + \sigma_\varepsilon^2} \in [0, 1]$. Next, we define the social and private returns to education. Recall the notation that for Y , $\delta^{Y|S}$ denotes the causal effect of S on Y and \tilde{Y} denote the part of Y that is not caused by schooling S but may correlate

with S . Using these notations and assumptions for $Y = Q$ and $Y = A$, we get, respectively,

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

Then, substituting (E.5) into (E.1), we obtain

$$\psi_{it} = \lambda_t \times (\beta_{ws} + \beta_{wq} \delta^{Q|S} + \delta^{A|S}) S_i + \lambda_t \times (\beta_{wq} \tilde{Q}_i + \tilde{A}_i + \varepsilon_{it}) := \delta_t^{\psi|S} \times S_i + u_{it}. \quad (\text{E.6})$$

The coefficient ($\delta_t^{\psi|S}$) in (E.6) is the total causal effect of schooling on productivity –the social return to education– and it captures the direct and indirect effect on other ability components, i.e., (Q, A). Thus (E.6) shows that an extra year of S increases Q by $\delta^{Q|S}$ and A by $\delta^{A|S}$ units, and in turn, they increase the productivity by $\lambda_t \beta_{wq}$ and λ_t , respectively.

Consider now the private returns to education. Substituting (E.2) and (E.4) in (E.3), and using $\bar{\xi}_i^t := \frac{1}{t} \sum_{\tau < t} (A_i + \varepsilon_{i\tau}) = A_i + \bar{\varepsilon}_i^t$, and the fact that $\mathbb{E}(A_i | S_i, Q_i)$ is linear and separable in S_i and Q_i , we can write the log-earnings as $\ln W_{it} = \lambda_t \times (\beta_{ws} + \theta_t \phi_{A|S}) S_i + \lambda_t \times (\beta_{wq} + \theta_t \phi_{A|Q}) Q_i + \lambda_t \times (1 - \theta_t) (A_i + \bar{\varepsilon}_i^t)$. Then, using (E.5) to replace Q and A gives

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

The coefficient of schooling ($\delta_t^{W|S}$) in (E.7) is the private return to education. Comparing this coefficient with the coefficient of schooling $\delta_t^{\psi|S}$ in (6), gives:

$$\delta_t^{W|S} = \delta_t^{\psi|S} + \theta_t \times \lambda_t \times (\phi_{A|S} + \phi_{A|Q} \delta^{Q|S} - \delta^{A|S}). \quad (\text{E.8})$$

Once we have augmented the definition of the social returns and the adjustment term to capture the effect of Q in (E.8), the rest of the identification results applies verbatim. In particular, when the returns to skill is experience-invariant and the hidden IV, D^h , satisfies

the assumption $\ln W_{it} \perp D_i^h | (S_i, Q_i, \xi_i^t)$ then it identifies the private returns to education, i.e.,

$$\text{plim } \hat{b}_t^{IV^h} = \frac{\mathbb{E} [\ln W_{it} | D_i^h = 1, t] - \mathbb{E} [\ln W_{it} | D_i^h = 0, t]}{\mathbb{E} [S_i | D_i^h = 1] - \mathbb{E} [S_i | D_i^h = 0]} = \delta^{\psi|S} + \theta_t (\phi_{A|S} + \phi_{A|Q} \delta^{Q|S} - \delta^{A|S}). \quad (\text{E.9})$$

Comparing (E.9) with the private returns defined in (E.8), we can conclude that, for every work experience level t , the hidden IV identifies the private returns to education, i.e., $\text{plim } \hat{b}_t^{IV^h} = \delta_t^{W|S}$. Hidden IV also identifies the speed of learning. Likewise, the Wald estimator for a transparent IV, D^t , identifies the social returns to education at all t , i.e.,

$$\text{plim } \hat{b}_t^{IV^t} = \frac{\mathbb{E} [\ln W_{it} | D_i^t = 1, t] - \mathbb{E} [\ln W_{it} | D_i^t = 0, t]}{\mathbb{E} [S_i | D_i^t = 1] - \mathbb{E} [S_i | D_i^t = 0]} = \delta^{\psi|S}. \quad (\text{E.10})$$

Next, we consider the case when the returns to skill vary with experience. As before, a hidden IV identifies the private returns to education, and a transparent IV identifies the experience-varying social returns to education. Formally, following the same steps as in (E.9), D^h and D^t at t , respectively, identify the private and social returns as

$$\text{plim } \hat{b}_t^{IV^h} = \frac{\mathbb{E} [\ln W_{it} | D_i^h = 1, t] - \mathbb{E} [\ln W_{it} | D_i^h = 0, t]}{\mathbb{E} [S_i | D_i^h = 1, t] - \mathbb{E} [S_i | D_i^h = 0, t]} = \lambda_t (\delta^{\psi|S} + \theta_t (\phi_{A|S} + \phi_{A|Q} \delta^{Q|S} - \delta^{A|S})); \quad (\text{E.11})$$

$$\text{plim } \hat{b}_t^{IV^t} = \frac{\mathbb{E} [\ln W_{it} | D_i^t = 1, t] - \mathbb{E} [\ln W_{it} | D_i^t = 0, t]}{\mathbb{E} [S_i | D_i^t = 1, t] - \mathbb{E} [S_i | D_i^t = 0, t]} = \lambda_t \times \delta^{\psi|S} := \delta_t^{\psi|S}. \quad (\text{E.12})$$

Note that with access to both IVs that satisfy Assumption 4, we can identify $\{\lambda_t : t \in \mathbb{T}\}$.