Signaling and Productivity in the Private Financial Returns to Schooling

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Abstract

Does formal schooling contribute to individual labor market productivity or does it act as a signal to employers of predetermined labor market skills? Different empirical approaches have been proposed to answer this question. Using data from administrative registries on the population of Denmark, including detailed information on sibling type, we apply two of the prominent empirical strategies in the same institutional setting and create a link between them. We further propose a novel test for the existence of job market signaling and provide new evidence on the relative importance of signaling for the private financial returns to schooling. We find that signaling explains most (if not all) of the returns to schooling. However, a human capital model with rapid skill depreciation could be part of the explanation as well.

Keywords: human capital, signaling, earnings, employer learning

JEL Classification: D8, I20, J31, J41

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I. Introduction

A large body of empirical research has shown that individual investment in formal schooling is associated with a subsequent wage premium. Although the size of the premium differs across countries, this conclusion holds across differently structured labor markets with different institutions.

Less is known about what causes the wage premium. According to human capital (HC) theory, schooling increases human capital which translates into increased individual labor market productivity. Employers value the increased productivity and pay wages accordingly. In contrast, the job market signaling (JMS) model assumes fixed predetermined individual labor market productivity. This information is private to workers, who signal their abilities to potential employers through schooling attainment.

These competing explanations have substantially different implications for society. Human capital accumulation at the individual level translates into productivity at the firm level and thus growth at the aggregate level. In contrast, if skill sets are predetermined then formal training acquired in school acts merely as an ability signal to employers, who pay a premium for the schooling investment but do not enjoy a productivity increase in exchange. Thus, in the JMS model schooling does not translate into productivity at the firm level and growth at the aggregate level. Therefore, JMS generates a wedge between private and social returns to schooling.

In the literature different empirical strategies to disentangling the HC and JMS theories have been applied. This paper creates a link between the two main estimation strategies within the most prominent strand of the literature, often referred to as "employer learning". Furthermore, this paper proposes a new test for the existence of JMS and also presents new evidence on the relative importance of JMS for the private financial returns to schooling.

The employer learning strand of the literature exploits differences in assumptions about the distribution of information in the JMS and HC models. In contrast to the HC model, the JMS model assumes that employers are initially uncertain about workers' productive types and therefore use schooling to *predict* individuals' productivity. Employers then *learn* about workers' productive types as time passes and workers gradually

reveal information about themselves through job performance.[1]

Some studies exploit presumed differences in the ease with which firms can learn about individual productivity between different industries (Wolpin, 1977; Riley, 1979) or types of job applicants (Albrecht, 1981). More recent contributions test for statistical discrimination and employer learning by assuming that the researcher can observe a pre-market ability measure that is not observed by employers. This literature (Foster and Rosenzweig, 1993; Farber and Gibbons, 1996; Altonji and Pierret, 1997, 2001; Galindo-Rueda, 2003; Lange and Topel, 2006; Lange, 2007 and others) evaluates how the estimated returns to schooling and ability evolve over time.

Rather than including a pre-market ability measure, Miller, Mulvey, and Martin (2004) compare returns to schooling differences of monozygotic (MZ) and dizygotic (DZ) twins. Under the assumption that MZ twins are more productively alike than DZ twins, employer learning implies that the the return to schooling differences for MZ twins decreases relative to DZ twins over time.

Our point of departure is the work by Altonji and Pierret (2001), who propose a test for employer learning that relies on information which is initially unavailable to employers. Their work has served as a blueprint for later studies (Arcidiacono et al., 2010; Bauer and Haisken-DeNew, 2001; Light and McGee, 2012; Mansour, 2012). Applying this test to working-age males using data from administrative records on the Danish population, we obtain results that are consistent with those of Altonji and Pierret (2001). The data further allows us to run separate regressions by sibling type (non-twin, MZ, and DZ), thereby enabling us to create a link from (Altonji and Pierret, 2001) to the twin approach to employer learning. Having established this link, we apply the test based on differences between MZ and DZ twins proposed by Miller et al. (2004) and obtain results that are consistent with their findings.

Because the data we use contain information on all Danish working-age male twins from 31 birth cohorts observed over a period of 27 years, we are able to compute between-twin differences across time while maintaining large sample sizes. This large twin panel

[1]The other main strand of the empirical literature exploits differences in out-of-equilibrium predictions following a change in the cost structure of education due to, e.g., changes in compulsory attendance laws or proximity to post-secondary education institutions (Lang and Kropp, 1986; Bedard, 2001; Chevalier et al., 2004)

data set allows us to pursue an empirical strategy that provides a more direct and precise test for the existence of JMS than those previously applied. The results from this novel test are in line with the results we obtain from existing empirical specifications, thereby supporting the employer learning hypothesis and the existence of JMS.

The real issue concerns not the mere existence of JMS or HC, but the extent to which schooling performs each of these roles (Wolpin, 1977). The speed with which employers learn ultimately limits the contribution of JMS to the private returns to schooling. A few studies aim to estimate the speed of employer learning and the relative importance of JMS (Lange and Topel, 2006; Lange, 2007). However, the estimation of (a bound on) the speed of employer learning in their setting rests heavily on functional form and distributional assumptions.

Using a sample of MZ twins, we show that the returns to years of schooling and high school completion approach a level close to zero after 10-15 years, suggesting that on these schooling margins job market signaling is important in explaining private market returns to schooling and that employers learn quickly about true individual ability. However, a human capital model with rapid skill depreciation could be part of the explanation as well. The return to college completion is fairly constant over time, but imprecisely estimated.

The remainder of the paper is organized as follows. Section II outlines the estimation strategies proposed by Altonji and Pierret (2001) and Miller et al. (2004) and proposes a new test for employer learning. Section III describes the data, explains the construction of main variables, and presents summary statistics. Section IV presents and discusses the results. Section V concludes.

II. Empirical strategy

This section outlines different empirical approaches to employer learning. Section II.A explains the specifications applied by Altonji and Pierret (2001) and by Miller et al. (2004). Section II.B proposes a more direct and precise test of the existence of job market signaling and shows how to estimate the speed of employer learning and the relative importance of job market signaling for the private financial returns to schooling.

A. Two existing tests for employer learning and the link between them

Following Altonji and Pierret (2001), we assume that employers set wages according to

$$W_{it} = \hat{P}_{it} = \hat{\alpha}_0 + \hat{\alpha}_{1t} S_i + \hat{\alpha}_{2t} A_{it}^E + \hat{\boldsymbol{\eta}} \boldsymbol{X}_{it} + f(t_i) + u_{it} \tag{1}$$

where W_{it} is the wage paid to worker i at experience level t, which equals the employer's expectation, $\hat{P}_{it}$, in period t about the true productivity of worker i, P_i. To predict worker productivity the employer relies on information about schooling, S_i, other observable characteristics, $\boldsymbol{X}_{it}$, a general experience profile, $f(t_i)$, and the employer's knowledge in period t, A_{it}^E, about worker i's true, unobserved, pre-market ability, A_i.[2]

At labor market entry, the employer knows little about true, unobserved ability and, therefore, pays wages primarily according to schooling and other observable background characteristics. Over time, as the worker reveals more about initially unobserved ability components, A_i, the employer updates his expectations about individual worker productivity. If schooling attainment is not completely informative about P then the employer will assign greater weight to A_i^E and less weight to S_i over time. Thus, employer learning implies that $\hat{\alpha}_{2t}$ increases over time while $\hat{\alpha}_{1t}$ decreases over time, assuming $\text{cov}(S, A) > 0$.

The researcher observes only the wage paid to the worker, not the employer's expectations about individual labor market productivity that drives it. But if the researcher holds information about correlates of true ability, which are initially unobserved by the employer, he can test if employer learning and statistical discrimination is—at least partially—driving wage setting.

Let A_i^R denote information available to the researcher about worker i's true abilities. A^R is assumed to be informative about the part of individual i's ability set, A, that is correlated with labor market productivity, P. Because the information available to the researcher is unavailable to the employer initially, this information cannot be priced into initial wages. We follow Altonji and Pierret and include measures of father's schooling, father's earnings, and brother's earnings, respectively—information we assume is unob-

[2] We present the outline and empirical specification of this model. For a more elaborate presentation, see Altonji and Pierret (2001).

served by employers at the time of labor market entry.[3]

The following specification provides the test for employer learning:

$$w_{it} = \beta_0 + \beta_1 S_i + \beta_2 S_i \cdot t_i + \gamma_1 A_i^R + \gamma_2 A_i^R \cdot t + \boldsymbol{\delta X}_{it} + g(t_i) + \varepsilon_{it} \quad (2)$$

where t is a measure of cumulative labor market experience, w_{it} denotes log earnings of worker i at experience level t, S_i measures years of completed schooling, A_i^R is schooling or earnings information on the father or the brother, and $\boldsymbol{X}_{it}$ denotes a vector of other background characteristics correlated with earnings. A positive estimate for γ_2 is evidence in support of employer learning; a negative estimate for β_2 is evidence that employers use schooling to statistically discriminate between workers at labor market entry. Section IV.A presents the results.

Over time, employers learn about actual individual labor market productivity rather than about the proxy information included in the regression by the researcher. Therefore, a productivity proxy that is more informative about true labor market productivity leads to a stronger correlation with earnings over time, i.e., larger $\hat{\gamma}_2$.

This prediction motivates a comparison of results based on the specification in (2) using different productivity proxies, which can be ranked according to their correlation with true unobserved ability. The test we apply uses differences in the degree of similarity among different sibling types. Non-twin full siblings share, on average, half their segregating genes and to some extent the same home environment during childhood, depending on their age difference. DZ twins also share half their segregating genes but further share a common home environment during childhood as they are the same age. MZ twins share all segregating genes as well as a common home environment. Under the assumption that both genetic and rearing components contribute to individual labor market productivity, the comparison of results from samples of different brother types provides an empirical test for this hypothesis. While $\hat{\gamma}_2$ should be at least of the same magnitude in the sample of DZ twins as in the sample of non-twin siblings, the estimation using a sample of MZ twins should produce a larger $\hat{\gamma}_2$ compared to both DZ twins and non-twin siblings.

[3]The inclusion of father's earnings is specific to our analysis. AP use father's schooling only.

As the results from samples split by sibling type support the prediction that better productivity proxies provide stronger evidence of employer learning (see section IV.A), these results provide a link from the Altonji and Pierret test of employer learning to strategies relying on twin-pair observations. The twin approach to employer learning is the focus of the remainder of this section.

Rather than relying on information about correlates of labor market productivity, which are initially unobserved by the employer, the twin approach to employer learning exploits differences in the similarity of MZ and DZ twins. We exclude A_i^R (father's schooling/earnings and brother's earnings) from the wage equation and augment the notation (add twin and family indices); then (2) becomes

$$w_{jft} = \beta_0 + \beta_1 S_{jf} + \beta_2 S_{jf} \cdot t_{jf} + \boldsymbol{\delta X}_{jft} + g(t_{jf}) + \varepsilon_{jft} \tag{3}$$

where $j = 1, 2$ denotes twin 1 and 2 in family $f = 1, ..., \frac{N}{2}$. This is more along the lines of a Mincer-type formulation of the private, financial returns to schooling where earnings is a function of schooling, S, working experience, t, and other covariates, $\boldsymbol{X}$. The omission of abilities, A, which are assumed to be positively correlated with schooling attainment, leads to a positive omitted variable bias in the returns to schooling.

We take the difference between twins in the same family in each time period and arrive at

$$\Delta_j w_{ft} = \widetilde{\beta}_0 + \beta_1 \Delta_j S_f + \beta_2 \Delta_j S_f \cdot t_f + \boldsymbol{\delta} \Delta_f \boldsymbol{X}_{ft} + g(t_f) + \Delta_j \varepsilon_{ft} \tag{4}$$

which is a twin fixed effects (FE) specification of (3).[4] The twin FE specification controls for characteristics, observed as well as unobserved, which are shared between twin brothers as these become constant (fixed) in the difference equation and drop out. Because our interest is in the dynamics of the returns to schooling, we keep the experience variable, t, in the estimation equation to distinguish between twin-pair observations at different experience levels.

In labor economics, twins-based estimates have featured prominently; see for in-

[4]To be precise, this is a twin first difference specification but since there are only two "periods" (twin 1 and 2) the first difference and fixed effects estimators are equivalent. To follow the notation in the twins literature, we refer to the specification in (4) as a twin fixed effects specification.

stance Ashenfelter and Krueger (1994); Behrman and Rosenzweig (1999); Isacsson (2004); Miller et al. (1995, 2006) and the survey in Card (1999). The twin FE estimator is usually implemented in a sample of MZ twins under the assumption that they are identical with respect to the part of earnings differences not attributable to differences in schooling. Under this assumption all relevant unobserved ability components are eliminated in the difference specification (4) and the twin FE estimator is consistent. Sandewall et al. (2014) provide suggestive evidence against this assumption of identical latent labor market skills of MZ twins. Violation of this assumption leads to biased estimates of the returns to schooling—presumably upward—and the bias in the difference specification is even potentially exacerbated compared to the cross-section estimate if most of the endogenous variation in schooling is attributable to individual rather than family characteristics (Bound and Solon, 1999). The MZ twin FE estimator may still be informative if it is smaller than the OLS estimate as it then tightens the upper bound of the returns to schooling (Bound and Solon, 1999).

The strict assumption of complete similarity of MZ twins is not needed for a test of the employer learning hypothesis. It is sufficient to assume that MZ twins are *more* alike with respect to predetermined productivity traits than DZ twins and compare the estimates from the two twin types. The between-twin differences eliminate family characteristics in both groups and eliminate the genetic endowment entirely among MZ twins but only partially among DZ twins. If individual labor market productivity has a substantial genetic component then the positive ability bias should be more persistent in the DZ twin difference as employers learn, implying that the *change* in the returns to schooling from one period to the next—net of twin fixed effects—should be smaller among MZ twins than among DZ twins. Thus, a comparison of twin differences in earnings over time provides an alternative approach to testing JMS based on the employer learning hypothesis—assuming MZ twins are more alike than DZ twins in terms of individual labor market productivity.

Miller et al. (2004) propose a test for employer learning that compares the estimates of the returns to schooling from samples of MZ and DZ twins at different points in their working career. Their test is a two-period test, comparing four estimates of the returns to schooling: ages 18-35 and 36-45 from separate estimations on samples of MZ twins

and DZ twins, respectively. That is, they estimate

$$\Delta_j w_{ft} = \widetilde{\beta}_0 + \beta_1 \Delta_j S_f + \boldsymbol{\delta} \Delta_f \boldsymbol{X}_{ft} + \Delta_j \varepsilon_{ft} \tag{5}$$

using four different samples and compare $\hat{\beta}_1$ from each of these. A relative decrease in the estimated returns from period one to period two among MZ twins compared to the change among DZ twins $\left(\text{i.e., } \hat{\beta}_1^{MZ,2} - \hat{\beta}_1^{MZ,1} < \hat{\beta}_1^{DZ,2} - \hat{\beta}_1^{DZ,1}\right)$ is evidence in support of job market signaling (see section IV.A for results).

Like the majority of analyses using samples of twins, the Miller et al. test for employer learning rely on cross-sectional data from twin surveys, thereby observing twin pairs at the same point in time and thus at the same age. The test's reliance on differences in the returns to schooling across the age distribution implies that age and birth-cohort effects are inseparable. Therefore, changes to educational or labor market institutions over time might affect the results. As individuals attend school at the same ages, changes to the educational system are particularly crucial for identification. The identifying assumption of stable institutions is quite strong and not robust to, e.g., a general increase in the quality of education over time. The next section proposes a new test for employer learning that exploits the possibility of observing twin brothers at different points in time.

B. A novel test for employer learning

The panel structure of the available twin data (see section III) enables the computation of between-twin differences using observations at different points in time, i.e., at the same work experience level rather than at the same age. Exploiting this panel structure of the data, we propose a test for employer learning based on changes in the returns to schooling differences of MZ and DZ twins by years of potential work experience over the early part of the working career—the part of the lifecycle relevant for employer learning.

To test for difference in the slopes of the returns to schooling differences between MZ and DZ twins, we interact the specification in (4) with a twin-type indicator, MZ, taking values one for MZ twin pairs and zero for DZ twin pairs:

$$\Delta_j w_{ft} = \widetilde{\beta}_0 + \beta_1 \Delta_j S_f + \beta_2 \Delta_j S_f \cdot t_f + \beta_3 t_f + \beta_4 MZ_f + \beta_5 \Delta_j S_f \cdot MZ_f + \beta_6 t_f \cdot MZ_f + \beta_7 \Delta_j S_f \cdot t_f \cdot MZ_f + \boldsymbol{\delta} \Delta_j \boldsymbol{X}_{ft} + \Delta_j \varepsilon_{ft} \tag{6}$$

The parameter β_7 captures the difference in the *slopes* of the returns to schooling between MZ and DZ twins. If $\hat{\beta}_7 < 0$ the change in the returns to schooling from one period to the next among MZ twins is smaller than the change in the returns to schooling among DZ twins; thus, $\hat{\beta}_7$ provides a direct test for job market signaling. As we expect the twin differences in controls, $\Delta_j \boldsymbol{X}_{ft}$, to affect earnings differences similarly among MZ and DZ twins, we do not interact these variables with the twin-type indicator.[5] Figure 1 provides a stylized graphical representation of the empirical specification in (6).

[Figure 1 about here]

A concern often raised in relation to twin fixed-effects estimation is the issue of measurement error. The between-twins difference specification is more prone to measurement error in the schooling variable than the cross-section specification (Bound and Solon, 1999; Griliches, 1979; Neumark, 1999). Classical measurement errors attenuate the estimate, thereby working in the opposite direction of the (presumed) positive bias from endogeneity. The concern is whether the FE estimates are reduced due to attenuation or selection bias or due to elimination of endowment differences, which is the purpose of the difference specification. The comparison of MZ to DZ twins in a difference specification alleviates the measurement error problem caused by differencing. Also, the MZ and DZ twins are equally sensitive to measurement errors (given third-party reports on schooling) and endogeneity.

Because MZ twins are more similar than DZ twins, they make more similar schooling choices, implying less variation in the between-twins schooling difference among MZ twins. If schooling is measured with error then the smaller amount of variation in the schooling difference of MZ twins causes a numerically larger attenuation bias in the

[5]For completes, we ran a fully interacted specification of (6). Results are similar to those obtained from the specification in (6) and available upon request.

estimated coefficient among MZ twins. This could potentially invalidate a test for differences between MZ and DZ twins. However, rather than comparing the returns to schooling across twin type, the empirical specification in (6) compares the *slopes* in the earnings-difference profiles across twin type. Therefore, the consistency of β_7 is vulnerable to attenuation bias only if such a bias differ across twin type *over time*. Also, because the data we use consists of high quality third-party reports drawn from administrative registries (Jensen and Rasmussen, 2011), the measurement error issue reduces substantially compared to the existing twins-based papers that normally use self-reports on educational attainment.

Another issue regarding estimation of returns to schooling is on-the-job training, i.e., job-specific skill accumulation that increases expected productivity, and thus wages, and happens after entry to the labor market. On-the-job training could bias the estimate of the returns to schooling either if employers invest in their workers differentially across the schooling distribution or if benefits from on-the-job training differ across schooling levels. On-the-job training invalidates the test for different slopes in the returns to schooling (6) only if the wage return from such training differs by twin type. We assume that this is not the case. Section IV.B presents the results from this new test of the existence of employer learning

Although the specification in (6) provides a test for the existence of employer learning and a signaling value of formal schooling, this test is neither informative about the learning profile of the employers nor about the relative importance of JMS for the private financial returns to schooling. However, because MZ twins are intuitively close to providing counterfactual outcomes at the individual level, estimates from the sample of MZ twins are potentially informative about the speed of employer learning and the relative importance of signaling.

We run a specification similar to (6), allowing for a non-linear time trend in the returns to schooling.[6] Using only the sample of MZ twins, the specification becomes

$$\Delta_j w_{ft} = b_0 + b_1 \Delta_j S_f + b_2 t_f + b_3 t_f^2 + b_4 \Delta_j S_f \cdot t_f + b_5 \Delta_j S_f \cdot t_f^2 + \boldsymbol{\delta} \Delta_f \boldsymbol{X}_{ft} + \Delta_j \varepsilon_{ft} \qquad (7)$$

[6] We model work experience by a second-order polynomial. Higher order does not change the results.

We evaluate the returns to schooling at each point in time, i.e. $\frac{\partial \Delta w}{\partial \Delta S}$ for each t. A decrease in the partial effect of schooling differences, $\frac{\partial^2 \Delta w}{\partial \Delta S \partial t} < 0$, is consistent with employer learning and provides an estimate of the speed with which employers learn about initially unobserved ability. Section IV.B presents the results.

If MZ twins were in fact identical in all aspects then $\frac{\partial^2 \Delta w}{\partial \Delta S \partial t}$ would provide an estimate of the speed with which employers learn about initially unobserved ability. Although MZ twins are intuitively close to providing counterfactual outcomes, we do not assume that they are identical. Instead, we maintain the assumption behind both the JMS and the HC model that the more productive brother selects more schooling. Thus, if MZ twins complete different levels of schooling, the twin who completes *more* schooling is assumed to be *at least as* productive in the labor market as the co-twin with less schooling. If MZ twins with different schooling are equally productive they will serve as each others counterfactual outcome, whereas if the twin with more schooling is more productive than his co-twin, the estimation bias will work against the hypothesis of twin-earnings convergence over time. In that case, the change in the returns to schooling differences over time provides a lower bound for the rate of convergence; i.e. the convergence happens at least as fast as implied by the results.

Like the comparison of MZ to DZ twins in (6), the study of MZ twins in (7) evaluates the wage premium from schooling differences over time. Therefore, these results are not particularly vulnerable to measurement error in the schooling variable unless a potential measurement error attenuates the estimates of the wage premium differentially over time. Although any difference in a potential attenuation bias over time would imply inconsistent results, only a decreasing (increasing in absolute value) attenuation bias over time would invalidate this design using MZ twins.

The *level* of the estimates, i.e. $\frac{\partial \Delta w}{\partial \Delta S}$ at each t, is of interest as well because it provides an estimate of the remaining wage premium at a given time; i.e. how much employers pay for an extra year of schooling at a given level of work experience. The level estimates are more vulnerable to attenuation bias from measurement error in the schooling variable and, therefore, these results must be interpreted with caution. However, as the data used to estimate (7) consists of high-quality administrative third-party reports on schooling, measurement error is less of a concern compared to previously published twin

estimates using survey data.

III. Data

We use data from Danish administrative records, which are linked at the individual level. The data hold information on the entire Danish population, recorded at an annual frequency covering 1980-2006. The main variables come from education ministry records (schooling) and tax authority records (labor earnings). Other administrative registries provide information on background characteristics and family composition. In addition, the Danish Twin Register (DTR) provides information on Danish twins.[7] In particular, DTR contains information on twin type (MZ/DZ), which is the key feature of the data that enables us to carry out the analysis.

A. Key variables

The registers contain separate records on gross earnings for all jobs held by an individual and distinguish between full-time and part-time jobs. We sum earnings for all full-time jobs held during the year, and to ensure comparability between individuals with different unemployment rates, we use annual-equivalent labor earnings, i.e., labor earnings scaled by same-year unemployment duration.[8]

Schooling information is reported directly by the educational institutions.[9] We define exit from the educational system as graduation from an education prior to or in 2004 with no re-entry into the educational system by the end of 2006. Unless stated otherwise, we define schooling by *length* (years) of the highest completed level of schooling.

The estimation of (2) includes (constant) measures of the father's and the brother's labor earnings as proxies for unobserved earnings potential. To avoid picking up transitory shocks in father and brother earnings, we calculate the average earnings over a ten-year period. Also, if the employer learning hypothesis is true then father and brother earn-

[7]The reader is referred to http://www.sdu.dk/en/Om_SDU/Institutter_centre/Ist_sundhedstjenesteforsk/Centre/DTR for an introduction to DTR.

[8]Earnings are price adjusted by the consumer price index.

[9]Before 1970 information on educational attainment was collected via censuses and based on self-assessment. We base the analysis only on data from the administrative records, partly due to the self-assessment of educational attainment in the census data and partly due to lack of information on graduation year in those data.

ings observed in the early part of their careers would likely reflect an employer learning process and thus be poor proxies for true earnings potential. Therefore, we calculate the average earnings of brothers in their 30s and of fathers in their 30s or 40s.

As the data span many years, the earnings of some brothers and fathers are observed in the early 1980s while for others up to 20 years later, implying that structural changes in labor market institutions could potentially induce noise in these wage measures that proxy for ability. We therefore construct a measure of the relative position in the wage distribution. By year, we split the income distribution of the entire population of Danish male, working-age, private-sector employees into percentiles (i.e. 100 bins) and assign a value between 1 and 100 for father and brother earnings. As this measure captures the father's and the brother's relative position in the aggregate male wage distribution each year, it is independent of aggregate economic fluctuations and wage dispersion over time. We standardize this relative earnings variable to ease interpretation of the corresponding coefficients.

The twin FE estimation (6) is a regression of between-twin earnings differences at each year of labor market experience. If two twin brothers graduate in different years, their earnings differences at any level of work experience are calculated from earnings in different years. To account not only for a general price trend but also for earnings variation due to changes in structural labor market conditions, we use earnings net of year fixed effects as the outcome variable in these regressions.

The employer learning model imposes the assumption that labor market experience, t, is observable to the employer as well as the researcher. Because *actual* accumulated working experience potentially reveals information to employers, which the researcher cannot observe, we follow the tradition within the employer-learning literature and use a measure of *potential* rather than actual working experience. Due to data limitations we include different measures of potential experience in the estimations of (2) and (6), respectively. In (2) potential experience is given by age minus the education ministry standard completion time for highest completed level of schooling (in years) minus six (the default school starting age in Denmark). In (6) potential experience is given by years since graduation from highest level of schooling with $t = 0$ being the year following graduation.

B. Estimation sample and summary statistics

Because the test for employer learning is relevant for the early part of an individual's working career, we link administrative records for twin (non-twin) brothers born between 1950 (1960) and 1980, thereby ensuring that no one enters the sample later than age 30 and everyone turns at least 26 during the sample period (to avoid censoring issues in the upper part of the educational distribution). We include observations within the first 16 years after graduation from highest level of completed schooling.[10] The twin FE estimation (6) includes 13 years after graduation due to sparse data on twin differences for longer periods.

We restrict the sample to male wage earners to avoid dealing with female labor market participation decisions, which are substantially influenced by family formation, especially during the early part of the working career. As wage negotiations are more centralized in the public sector than in private sector, public sector wages are less likely to reflect individual skill sets (Dahl et al., 2013); thus, we consider only private sector earnings. We further select on earnings from full-time occupation earned by individuals who were at least 18 years old and employed for at least six months in the year of observation. We deal with outliers by excluding the top and bottom 0.5 percent of the earnings distribution each year. To preclude odd schooling and labor market trajectories, we exclude individuals who were younger than 14 or older than 35 at labor market entry.

An individual in the sample is matched to his brother if they were born no more than ten years apart. If an individual is initially matched to more than one brother, priority is given to the first-born brother. Twins are always matched to each other.

The test for employer learning rests on the assumption that the researcher observes individual productivity traits that are unobserved by employers at labor market entry. If brothers are employed by the same firm—even years apart—the employer could have picked up signals about individual productivity traits from the performance of the brother. Because the employer's possibility of observing a brother invalidates the assumption that this information is initially unavailable, we include only brothers who are not registered with the same firm throughout the sample period.

[10]This restriction alleviates potential issues of non-linearity in the slope of the earnings profile for longer periods (Altonji and Pierret, 2001).

Table 1 presents summary statistics for the estimation sample, which consists of 158,813 non-twin brothers and 3,889 twin brothers.[11] The first thing to note is that non-twin brothers and twin brothers are quite similar with respect to background characteristics. On average, both groups were born in 1967/1968, completed 13 years of schooling, and entered the labor market in 1990/1991 at age 23. The other thing to note is that the means of the proxies for individual labor market ability, i.e., the brother's earning and the father's schooling or earnings are the same in the two groups; all tests for equal means are accepted. The brother's relative position in the earnings distribution is on average 52 (out of 100), while the father's relative position is a little higher, around 58; this is expected because we also include earnings for fathers in their 40s.

[Table 1 about here]

The twin FE estimation of returns to schooling relies on variation in schooling differences within twin pairs, i.e., twin brothers completing different levels of schooling. One concern might be that twin brothers, especially MZ twin brothers, are so similar in all respects that they will in general choose the same level of schooling. Table 2 shows the difference in length of completed schooling within twin pairs. As expected, the table shows that MZ twins are more likely to complete the same level of schooling relative to DZ twins. For 43 (25) percent of MZ (DZ) twin pairs in the sample both twins complete the same length of schooling (in months), whereas for 28 (34) percent of MZ (DZ) twin pairs the length of schooling differs by 1 to 12 months. For the remaining 30 (41) percent of MZ (DZ) twin pairs, the difference in length of highest completed schooling is more than one year.

[Table 2 about here]

Table 3 shows a matrix of schooling levels for twins, separately for MZ and DZ twins. For twin pairs on the diagonal, the completed level of schooling of the two brothers is the same, while off-diagonal elements contain twin pairs with a difference in schooling levels. The majority of the sample lies on the diagonal and around half the sample is

[11]To make the samples of non-twin and twin brothers comparable, we compute summary statistics for the subsample of the twin brothers matching the birth cohorts of the non-twin brothers, i.e., $1960 - 1980$.

found in the middle of the matrix where both brothers in a twin pair complete high school and then enter the labor market. However, the off-diagonal elements for both MZ and DZ twins make up a substantial part of the sample as well. Although twins tend to make fairly similar schooling choices, tables 2 and 3 show that there is variation in the between-twins schooling difference.

[Table 3 about here]

IV. Results

The structure of this section follows the chronology of the presentation of the empirical strategy (section II). Section IV.A takes the existing tests for employer learning, proposed by Altonji and Pierret (2) and Miller et al. (5), respectively, to the Danish administrative register data and shows that these results are consistent with previous findings. Section IV.B presents and discusses the results from the new test for employer learning (6). As these results confirm the existence of job market signaling, the section further provides estimates of the speed of employer learning and the relative importance of JMS from estimation of (7) using the sample of MZ twins.

A. Applying existing tests for employer learning to the Danish administrative data

Table 4 provides ten sets of results obtained by OLS regression of (2). The ten sets represent five distinct groups of estimates, each group represented by two sets: a and b. For any a-b pair of estimates, the sample and variables are identical and the only difference between a and b estimates is that in every set of a-estimates, the coefficient on the interaction between the ability proxy and time, γ_2, is set to zero. The five *groups* of estimates differ by the proxy, A^R, for innate ability and/or by estimation sample. Columns (1a) and (1b) include the father's schooling as the hard-to-observe correlate of productivity, these results are comparable to Altonji and Pierret (2001, table 2, columns 5-6). Columns (2a) and (2b) instead include the labor earnings of the father as the proxy for labor market productivity. The remaining six columns ((3a)-(5b)) all include the brother's earnings—rather than the father's—as a proxy for ability; they differ with respect to the estimation sample: The results in (3a) and (3b) are obtained from the sample of non-twin brothers

and comparable to Altonji and Pierret (2001, table 2 columns 1-2)[12], columns (4a) and (4b) use the sample of DZ twins and columns (5a) and (5b) use the sample of MZ twins. Standard errors are clustered by siblings.

[Table 4 about here]

The first thing to note from the results in Table 4 is that the coefficient on the interaction between schooling and experience is negative across all specifications, suggesting that employers use their available information on schooling attainment of workers to determine wages initially (statistical discrimination) but put less weight on schooling information over time as they learn more about their workers. The next thing to note is that in all specifications with $\gamma_2 = 0$ (a-columns) the correlation between earnings and the unobserved (to employers) correlate of true productivity is positive and statistically significant at the one-percent level; this holds for father's schooling, father's earnings as well as brother's earnings. These positive estimates of coefficients on the productivity proxies are informative about the average correlation with earnings over the early part of the working career.

With regard to employer learning, the interest lies in the wage return to the productivity proxies over time. The b-columns show this by allowing the coefficient on the productivity proxy to vary over time. The results show the same pattern across different productivity proxies and different samples: The entire positive correlation between the productivity proxy and earnings is captured by the interaction term; i.e., the correlation rises over time as experience accumulates and employers learn more about true ability.[13] Thus, the results in Table 4 from Danish administrative registries provide empirical evidence in line with the findings of Altonji and Pierret (2001) and support the employer-learning hypothesis in a Danish context.

[12]Altonji and Pierret do not distinguish between different types of brothers. However, the share of twin brothers is small and, therefore, the results in (3a)-(3b) are almost identical to those obtained from pooling the samples in (3a)-(5b).

[13]In some specifications in Table 4 the initial correlation between the included productivity proxy and earnings becomes negtive and statistically significant, which may seem spurious. However, the researcher is unable to observe all the information employers use initially to predict workers' labor market productivity and, therefore, some coefficients are affected by such omitted variables. This does not change the conclusions drawn here regarding employer learning. For a detailed presentation of the model. see Altonji and Pierret (2001).

The specifications in columns (3a)-(5b) of Table 4 all include brother's wage as the productivity proxy but differ by type of brother. The first set of estimates is based on a sample of non-twin brothers, the second set on DZ twin brothers, and the third set on MZ twin brothers. The estimates of coefficients related to brother's earnings are similar in magnitude among non-twin brothers and DZ twin brothers. However, as expected (see section II), the results show a stronger correlation between own earnings and brother's earnings for MZ twins than for non-twins and DZ twins—the estimates for MZ twins are more than twice as large and the difference is statistically significant. These results provide a link between the Altonji and Pierret (2001) approach and the twin approach to employer learning and job market signaling.[14]

Having established the link, we turn to the test of employer learning that relies on twin differences. Table 5 applies the specification in (5), i.e., the test proposed by Miller et al. (2004), to the population of Danish male twins. The returns to schooling differences of MZ twins in the middle part of their career (ages 36-45) is smaller than the returns to schooling differences among younger MZ twins. For DZ twins, the returns increase slightly from the early to the later part of their working career. These results tell a story that is consistent with the hypothesis of a decrease in the returns to schooling from the early to the late part of the career among MZ twins relative to DZ twins—although the estimated coefficients are not statistically different from each other.

[Table 5 about here]

The results in Table 5 are not directly comparable to the results by Miller et al. who use a cross section of Australian twins. As previously mentioned, using cross-sectional data for this analysis, they cannot distinguish age from birth-cohort effects. In contrast, because we use data covering many years, the results in Table 5 are not vulnerable to such cohort effects. Also, Miller et al. do not observe information about individual earnings. Instead, they impute individual earnings by occupation means, implying that their

[14] As noted by Light and McGee (2012), due to multicollinearity a comparison of estimates obtained using different productivity proxies is not straightforward. The specifications in columns (3a)-(5b) use the same proxy for ability, brother's earnings, but compare results across different samples. For completeness, we apply Light and McGee's proposed method (originally formulated by Farber and Gibbons, 1996) and run the same regressions using only the variation in brother's earnings that is orthogonal to all other variables included in the regression (see appendix A). The conclusions drawn from Table 4 do not change.

results rely entirely on inter-occupational earnings variation and reflects only the part of earnings dynamics (conditional on educational attainment) driven by occupational choice.

B. Taking the new test to the data

Before turning to the estimation of (6), we provide a graphical presentation of the patterns that emerge from the data. For MZ (left) and DZ twin pairs, respectively, Figure 2 plots the mean earnings difference for twins with different schooling levels (top line) and twins with the same schooling level (bottom line) by years after graduation from highest completed level of schooling.[15] The figure serves as a graphical presentation of the difference in the change in returns to schooling over time between twin types. It shows that among both types of twins additional schooling is associated with a substantial initial wage premium of approximately 15 to 20 percent given by the difference between the top and bottom line.[16] While the wage premium for DZ twins seems fairly constant over time, it decreases with work experience for MZ twins. This is a first indication of a difference in slopes in the private financial returns to schooling between MZ and DZ twins.

[Figure 2 about here]

A concern is that the pattern of wage convergence is to some extent driven by mean reversion at the aggregate level, i.e. the distribution of wages narrows with experience. Figure 3 therefore plots the interquartile wage range by years since graduation for different years in the sample period using the entire population of Danish male wage earners, selected in the same way as the estimation sample (see section III). The graphs clearly show a positive relationship for all years considered, which suggests that the pattern in

[15]Twin brothers with the same *level* of schooling may have completed different *lengths* of schooling, e.g. if two college degrees in different subjects are of different lengths. For the sake of illustration in this figure, we order twin brothers by months of completed schooling, which is why the earnings difference among twins with same level of schooling (bottom line) is positive.

[16]The difference in schooling levels is given by a dummy variable taking value 1 if one twin brother completed a higher level of schooling than his co-twin, also if he completed two levels more. Therefore, the numbers in the graph cannot be interpreted as the wage premium associated with one extra level of schooling but must be interpreted more generally and with caution.

Figure 2 is not driven by convergence at the aggregate level. In fact, convergence of twin wages happens despite increasing wage dispersion at the aggregate level.

[Figure 3 about here]

As explained in section II, equation (6) provides a formal test for the difference in slopes between MZ and DZ twins. Table 6 presents four sets of estimates obtained from OLS regressions of (6). In column 1 schooling attainment is given by years of completed schooling, whereas in column 2 the schooling variable takes values 0, 1, and 2 for compulsory, high school, and college, respectively. In column 3 and 4 schooling is given by a binary variable for completing at least high school and college, respectively.

[Table 6 about here]

The interaction between the schooling difference, years since graduation, and the indicator for MZ twin pair ($\Delta S \cdot experience \cdot MZ$) provides a direct test for difference in the slopes of the returns to schooling between MZ and DZ twins. The results show that the returns to an extra year of schooling among MZ twins declines 0.3 percentage points per year relative to that of DZ twins (column 1); the estimate is statistically significant at the five-percent level. The return to an extra *level* of schooling declines faster among MZ twins as well, around 0.8 percentage points per year (significant at the ten-percent level); this difference is attributable to the difference at the college margin of around 1.5 percentage points per year (significant at the five-percent level).

The finding that the returns to schooling differences of MZ twins decrease over the early career is consistent with a story of employer learning, where employers learn about true abilities as they repeatedly observe individual job performance and adjust wages accordingly. This finding supports the job market signaling model, while being inconsistent with an explanation of the returns to schooling based entirely on human capital accumulation.

While the estimates from the specification in (6) provide empirical evidence for the existence of JMS, they are not informative about the relative importance of signaling. Figure 4 depicts the marginal effect of schooling differences at each level of working

experience during the early working career among MZ twin brothers.[17] The four graphs show the marginal effect of schooling at different schooling margins, i.e. an extra year of schooling, higher level of schooling, and a decomposition of the "level results" into high school and college margins. The sample of MZ twins used here is the same as the one used in the previous estimations but we further include observations between 14 and 16 years after graduation to capture the non-linearity in those later years.[18]

[Figure 4 about here]

The estimates in figure 4 show convergence of earnings between MZ twins on all schooling margins, although the returns to a college degree is imprecisely estimated. The returns to an extra year of schooling decreases monotonously over the years after graduation and stabilizes after approximately 12 years. Using a discrete schooling variable—0, 1 and 2 for compulsory schooling, high school, and college, respectively—the pattern is similar, although convergence seems to happen somewhat more slowly. The decomposition of this estimate shows faster convergence on the lower schooling margin and slower convergence on the upper schooling margin although the latter is imprecisely estimated.

The slope of the marginal effects in Figure 4 imply that employers learn all about initially unobserved individual productivity traits within the first 10 to 15 years of working experience. As explained in Section II.B, the estimated speed with which employers learn serves as a lower bound for the speed of employer learning; i.e., convergence happens at least as fast as these results imply.

Taken at face value, the results in Figure 4 show that the average financial return to one extra year of schooling is eliminated after the first part of the working career, the same is observed for high school completion, while it is difficult to conclude much from the college return. These marginal effects imply that the entire schooling earnings premium is paid to workers during the first 10 to 15 years of their career. After that, schooling attainment differences are uncorrelated with earnings, which instead are com-

[17] In Figure 4 experience is modeled as a second-order polynomial. Using higher order polynomials does not change the results.

[18] When we extend the period even longer, the number of twin-pair observations falls and the estimates become less precise.

pletely determined by other personal characteristics. This finding is consistent with a JMS model, where individuals do not accumulate human capital of value to employers but use schooling only to signal ability to employers. If signaling was the sole driver of this finding, schooling would serve only as a sorting mechanism and the cost to employers amounts to the accumulated schooling wage premium they must pay while learning about their workers, i.e. the area beneath the curves in Figure 4.

As previously noted (section II.B), this result is vulnerable to measurement error and should therefore be interpreted with caution. However, we argue that the use of administrative third-party reports on schooling greatly alleviates any potential measurement error issue, and it is thus unlikely that the results are subject to substantial attenuation bias.

Although the results depicted in Figure 4 are consistent with an all JMS explanation of the returns to schooling, we cannot rule out that a HC model with rapid skill depreciation partially explains the results. However, the findings are not easily reconciled with a standard HC model with no, or only moderate, skill depreciation. Furthermore, they are not consistent with an on-the-job training explanation unless employers invest *more* in workers with less education, enough for these less educated workers to catch up on the training they did not receive while in school. As the provision of on-the-job training is costly to employers both directly (fees) and indirectly (worker being absent), we find it unlikely that such investments drive the results.

V. Conclusion

The human capital and job market signaling models provide competing explanations for the returns to schooling; but they have very different implications for society. Previous empirical studies suggest different ways to distinguish between the two models, the most prominent being the test for employer learning that requires information about latent labor market skills initially unobserved by the employer. We apply this framework to administrative records on all Danish working-age male wage earners in the private sector and provide estimates in line with the existing literature. We then run separate estimations for different sibling types to investigate heterogeneity in the earnings response

from ability proxies of different quality; thereby creating the link from the dominant empirical strategy within the existing literature to twins-based estimation of employer learning.

The large sample of Danish male twins, combined with detailed information on schooling and earnings, enables us to perform a more direct and precise test for employer learning compared to existing empirical evidence based on twin data. We study how earnings differences vary with work experience over the early part of the working career for MZ and DZ twins. The results show a decline in the returns to years of schooling differences for MZ twins relative to DZ twins of 0.3 percent per year in the labor market. This finding is consistent with some degree of job market signaling while not easily reconciled with an explanation of private financial returns to schooling based on human capital accumulation alone. When we define schooling as high school and college completion, respectively, the results are qualitatively similar, although imprecisely estimated on the college margin.

We use the sample of MZ twins to assess the relative importance of JMS for the private financial returns to schooling and the speed with which employers learn about initially unobserved ability components. For years of schooling and high school completion the return to schooling differences among MZ twins decreases during the first 10 to 15 years of work experience and seems to stabilize at a level close to zero. The rate of convergence of MZ twin earnings provides a lower bound for the speed with which earnings adjust if twins with less schooling do not receive (or benefit) more from on-the-job training than their better educated co-twin. In that case, the rate of convergence could be either positively or negatively biased.

Taken at face value, the results obtained from the sample of MZ twins show that almost the entire private financial returns to an extra year of schooling or high school completion are earned during the first 10 to 15 years in the labor market. This suggests that JMS explains a substantial part of the returns to schooling and that employers quickly learn about true abilities. However, a human capital model with rapid skill depreciation could be part of the explanation as well. But the results are inconsistent with a standard HC model that assumes no, or only little, skill depreciation. In contrast to the results for years of schooling and high school completion, the estimated return to

college completion is persistent and fairly constant but it is imprecisely estimated.

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Tables

Table 1: Summary statistics

	Non-twin	Twin	Diff.[a]
Background characteristics			
Year of birth	1967.8	1968.2	-0.4**
	(4.7)	(4.9)	(0.080)
Schooling (years)	12.9	12.9	0.0
	(2.2)	(2.3)	(0.037)
First year in the labor market	1990.6	1990.8	-0.3**
	(6.7)	(6.7)	(0.109)
Age at entry into labor market	22.8	22.6	0.1
	(4.1)	(4.1)	(0.067)
Ability proxies			
Brother's earnings (age 30-39)[b]	51.8	51.8	-0.1
	(23.7)	(24.3)	(0.394)
Father's schooling (years)	11.0	11.0	0.0
	(3.4)	(3.4)	(0.055)
Father's earnings (age 30-49)[b]	57.9	57.3	0.6
	(25.2)	(24.6)	(0.534)
Individuals	158813	3889	

Standard deviations in parentheses

[a]Standard errors in parentheses

* $<$ .05 ** $<$ .01

[b]Percentile position in the earnings distribution. Mean over the age range.

Note: The sample of twin brothers is restricted to birth cohorts $1960 - 1980$ to make it comparable to the sample of non-twin brothers.

Table 2: Twin differences in years of completed schooling

Years diff.	MZ twins		DZ twins	
0	3293	(43%)	2770	(25%)
(0,1]	2137	(28%)	3773	(34%)
(1,2]	919	(12%)	1464	(13%)
(2,3]	507	(7%)	825	(7%)
(3,4]	488	(6%)	1188	(11%)
(4,5]	309	(4%)	776	(7%)
(5,6]	54	(1%)	179	(2%)
(6,7]	3	(0%)	65	(1%)
(7,8]	0	(0%)	21	(0%)

Column shares (percent) in parentheses.

Table 3: Distribution of schooling attainment of twin brothers in the estimation sample.

	MZ twin pairs		
	Compulsory	High school	College
Compulsory	524 (7%)		
High school	888 (12%)	3922 (51%)	
College	64 (1%)	1067 (14%)	1245 (16%)
	DZ twin pairs		
	Compulsory	High school	College
Compulsory	799 (7%)		
High school	1976 (18%)	4942 (45%)	
College	218 (2%)	1940 (18%)	1186 (11%)

In parentheses: share (in percent) of cell to total number of observation within twin type.

Table 4: The effects of schooling, father's education, father's earnings, and brother's earnings on wages. Dependent variable: Employment-corrected wage earnings; Experience measure: Potential experience. Year 0-15 after graduation. OLS estimates.

	All brothers				Non-twin brothers		DZ twins		MZ twins	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)	(5a)	(5b)
Education (years)	0.0952**	0.0997**	0.0981**	0.102**	0.0938**	0.0971**	0.0744**	0.0771**	0.0849**	0.0951**
	(0.000588)	(0.000596)	(0.000764)	(0.000774)	(0.000593)	(0.000597)	(0.00379)	(0.00382)	(0.00512)	(0.00519)
Education*Experience/10	-0.0289**	-0.0335**	-0.0333**	-0.0372**	-0.0288**	-0.0320**	-0.0146**	-0.0171**	-0.0253**	-0.0346**
	(0.000540)	(0.000557)	(0.000717)	(0.000732)	(0.000542)	(0.000547)	(0.00292)	(0.00290)	(0.00408)	(0.00416)
Father's education (10 years)	0.0440**	-0.0694**								
	(0.00184)	(0.00306)								
Father's education*Experience/10		0.112**								
		(0.00313)								
Father's earnings[a]			0.0313**	-0.00637**						
			(0.000792)	(0.00134)						
Father's earnings*Experience/10				0.0377**						
				(0.00140)						
Brother's earnings[b]					0.0301**	-0.00386**	0.0466**	0.0114	0.101**	0.00788
					(0.000679)	(0.00103)	(0.00557)	(0.00783)	(0.00647)	(0.00990)
Brother's earnings*Experience/10						0.0336**		0.0318**		0.0850**
						(0.00112)		(0.00751)		(0.00958)
Spouse	0.0585**	0.0579**	0.0583**	0.0576**	0.0578**	0.0574**	0.0465**	0.0465**	0.0361**	0.0349**
	(0.00104)	(0.00104)	(0.00132)	(0.00131)	(0.00104)	(0.00104)	(0.00724)	(0.00723)	(0.00827)	(0.00828)
# Kids	-0.00543**	-0.00504**	-0.00485**	-0.00457**	-0.00571**	-0.00581**				
	(0.000636)	(0.000635)	(0.000808)	(0.000807)	(0.000637)	(0.000636)				
Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Industry, first job	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Experience (3^{rd} order polynomial)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	1410269	1410269	846441	846441	1376539	1376539	38766	38766	24617	24617
R^2	0.373	0.375	0.370	0.372	0.378	0.380	0.537	0.538	0.562	0.568

Standard errors in parentheses

Standard errors clustered by sibling group

* p<0.05, ** p<0.01

[a]The average relative position in the wage distribution between ages 30 and 49. The relative position of individual earnings each year is calculatd as the value corresponding to the percentile in the earnings distribution of the entire population of Danish working-age males, who are employed in the private sector and employed for at least 50% of the year. Thus, everyone is given a value between 1 and 100. This relative-earnings variable is then standardized to ease interpretation.

[b]The average relative position in the wage distribution between ages 30 and 39 calculated in the same way as father's earnings.

Table 5: Cross-sectional and between-twins difference estimates of log earnings[a]

	Individuals		Twin fixed effects			
			DZ twins		MZ twins	
	18-35	36-45	18-35	36-45	18-35	36-45
Schooling (years)	0.0315***	0.0414***	0.0229***	0.0244***	0.00995***	0.00697
	(0.00163)	(0.00235)	(0.00289)	(0.00397)	(0.00350)	(0.00528)
Age	0.0716***	0.0384				
	(0.00707)	(0.0249)				
Age^2	-0.000969***	-0.000469				
	(0.000122)	(0.000312)				
Spouse	0.0586***	0.103***	0.0511***	0.0978***	0.0164	0.0352*
	(0.00614)	(0.0103)	(0.00980)	(0.0165)	(0.0109)	(0.0182)
Constant	-1.645***	-1.243**	-0.00317	0.0163	0.0000289	0.0156
	(0.100)	(0.498)	(0.00688)	(0.0114)	(0.00670)	(0.0133)
Observations	40786	21856	12306	7017	8087	3911
R^2	0.132	0.103	0.029	0.046	0.004	0.005

Standard errors in parentheses

Standard errors clustered at twin-pair level

* p<0.1, ** p<0.05, *** p<0.01

[a]Net of year fixed effects obtained from the entire population of working-age, private-sector, male wage earners, who were employed in a full time job for at least six months during the year of observation.

Table 6: Twin FE estimation of returns to schooling. OLS regressions of between-twins earnings differences from a sample of MZ and DZ twin brothers.

	Dependent variable: Δ Log wage[a]			
Schooling variable:	Years	Levels $(0, 1, 2)$	High school $(0/1)$	College $(0/1)$
Δ Schooling (years)	0.0841***	0.254***	0.360***	0.190***
	(0.00484)	(0.0175)	(0.0277)	(0.0235)
Potential work experience (years)	-0.00119	-0.00146	-0.00161	-0.00236*
	(0.00142)	(0.00143)	(0.00145)	(0.00142)
Δ Schooling $\cdot$ experience	-0.00487***	-0.00845***	-0.0289***	0.0145***
	(0.000682)	(0.00254)	(0.00357)	(0.00355)
MZ twin pair $(0/1)$	0.0120	0.00984	0.0123	0.00496
	(0.0136)	(0.0140)	(0.0145)	(0.0146)
Δ Schooling $\cdot$ MZ	0.00993	0.00549	0.0234	-0.00467
	(0.00861)	(0.0308)	(0.0498)	(0.0397)
Experience $\cdot$ MZ	-0.00164	-0.00135	-0.00137	-0.000489
	(0.00205)	(0.00208)	(0.00209)	(0.00208)
Δ Schooling $\cdot$ experience $\cdot$ MZ	-0.00264**	-0.00802*	-0.00754	-0.0149**
	(0.00118)	(0.00442)	(0.00632)	(0.00629)
Δ Spouse	0.0318***	0.0240***	0.0519***	0.0357***
	(0.00849)	(0.00842)	(0.00910)	(0.00818)
Constant	0.0146	0.0150	0.0176*	0.0174*
	(0.00964)	(0.00988)	(0.0103)	(0.0103)
Observations	18771	18771	18771	18771

Standard errors in parentheses

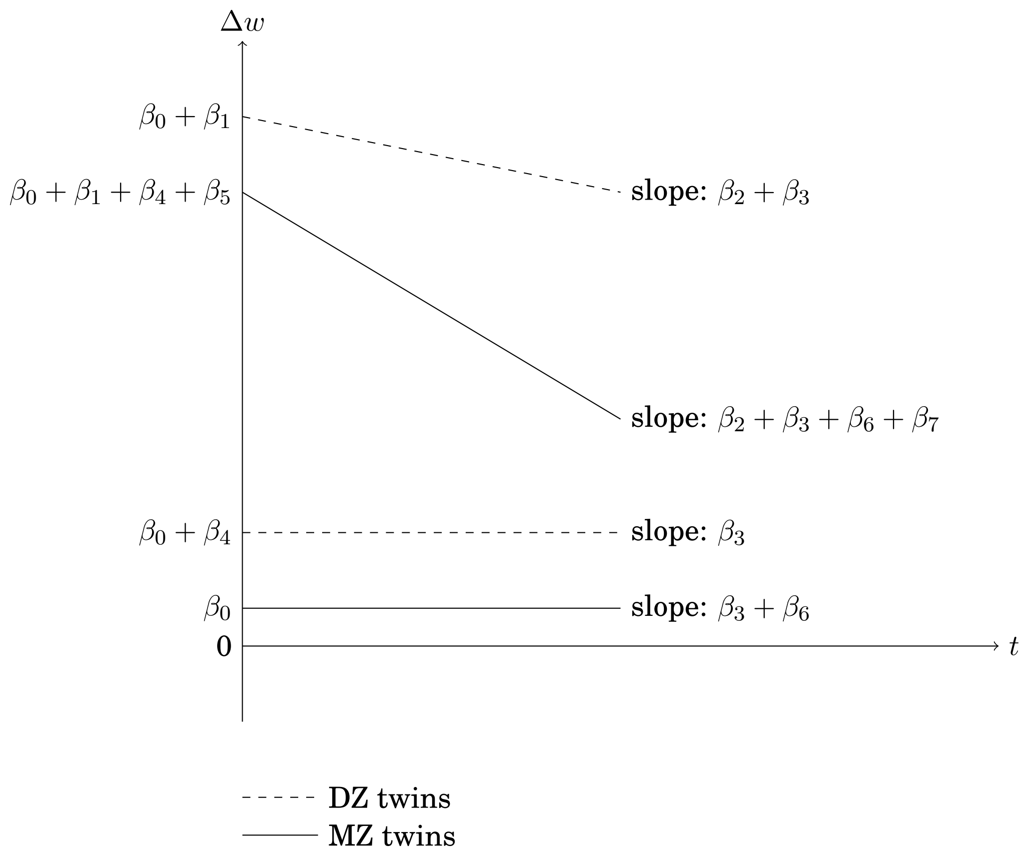
Standard errors clustered at twin-pair level

* p<0.1, ** p<0.05, *** p<0.01

[a]Net of year fixed effects obtained from the entire population of working-age, private-sector, male wage earners, who were employed in a full time job for at least six months during the year of observation.

Figures

Figure 1: Stylized illustration of main empirical specification (without controls)



Specification

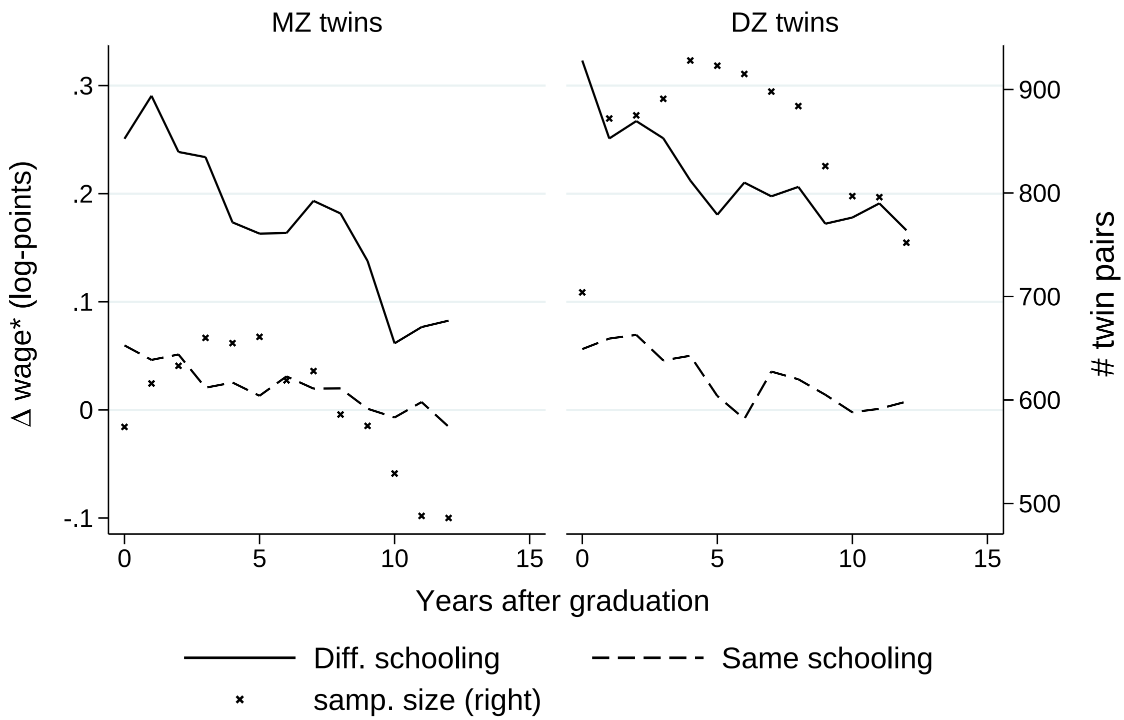
$$\begin{aligned}\Delta_j w_{ft} &= \widetilde{\beta}_0 + \beta_1 \Delta_j S_f + \beta_2 \Delta_j S_f \cdot t_f + \beta_3 t_f + \beta_4 MZ_f + \beta_5 \Delta_j S_f \cdot MZ_f + \beta_6 t_f \cdot MZ_f \\ &+ \beta_7 \Delta_j S_f \cdot t_f \cdot MZ_f + \boldsymbol{\delta} \Delta_j \boldsymbol{X}_{ft} + \varepsilon_{ft}\end{aligned}$$

Average returns to schooling: $$\frac{\partial \Delta w}{\partial \Delta S} = \beta_1 + \beta_2 t + \beta_5 MZ + \beta_7 t \cdot MZ$$

Slope in the returns to schooling: $$\frac{\partial^2 \Delta w}{\partial \Delta S \, \partial t} = \beta_2 + \beta_7 MZ$$

Diff. in slopes between MZ and DZ: $$\frac{\partial^3 \Delta w}{\partial \Delta S \, \partial t \, \partial MZ} = \beta_7$$

Figure 2: Twin mean wage differences over years after graduation from highest completed level of schooling. By schooling differences and twin type.



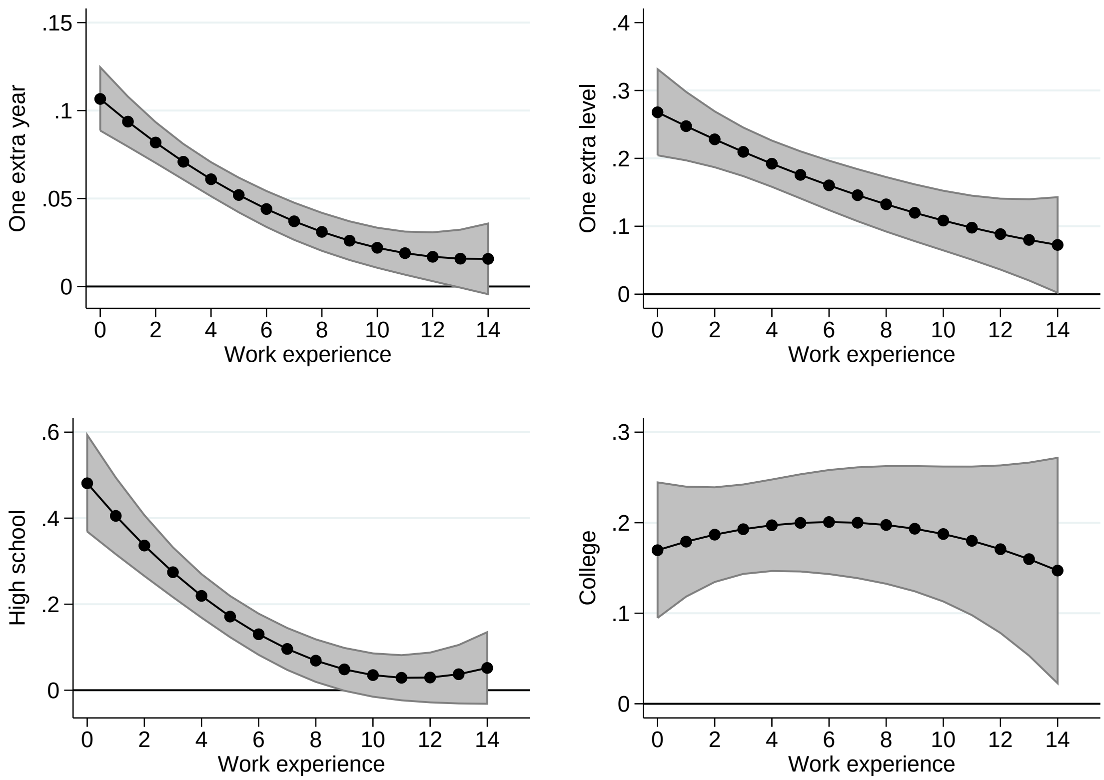
Total number of twin-pair observations in graph: 8K (MZ) 11K (DZ)
*Log wage net of year fixed effects (from full-pop distribution)

Figure 3: Interquartile wage range for all Danish male, private-sector, full-time employees[a]

Note: The 1985 graph only extends until 13 years after graduation because the administrative educational register with third-party reports on graduation date starts in 1971.

[a]Obtained from the entire population of working-age, private-sector, male wage earners, who were employed in a full time job for at least six months during the year of observation.

Figure 4: OLS estimation of between-twin earnings differences on between-twin schoooling differences (twin FE) on the sample of MZ twin pairs. Estimates obtained using four different definitions of schooling attainment.



Note: 95 percent confidence bands in gray. Standard errors clustered by twin pair.

Appendix

A. Orthogonality results

Table 7: The effects of schooling, father's education, father's earnings, and brother's earnings on wages. Uses only the part of the variation in father and brother variables that is independent of schooling and all other regressors. Dependent variable: Employment-corrected wage earnings; Experience measure: Potential experience. Years 0-15 after graduation. OLS estimates.

	All brothers				Non-twin brothers		DZ twins		MZ twins	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)	(5a)	(5b)
Education (years)	0.0963**	0.0962**	0.101**	0.101**	0.0965**	0.0964**	0.0754**	0.0751**	0.0863**	0.0882**
	(0.000585)	(0.000584)	(0.000760)	(0.000759)	(0.000589)	(0.000588)	(0.00379)	(0.00379)	(0.00515)	(0.00515)
Education*Experience/10	-0.0287**	-0.0286**	-0.0329**	-0.0328**	-0.0288**	-0.0287**	-0.0147**	-0.0144**	-0.0249**	-0.0267**
	(0.000540)	(0.000539)	(0.000717)	(0.000715)	(0.000542)	(0.000538)	(0.00293)	(0.00291)	(0.00410)	(0.00403)
Father's education (10 years)	0.0147**	-0.0128**								
	(0.000580)	(0.000981)								
Father's education*Experience/10		0.0272**								
		(0.00101)								
Father's earnings[a]			0.0306**	0.00296*						
			(0.000768)	(0.00131)						
Father's earnings*Experience/10				0.0277**						
				(0.00138)						
Brother's earnings[b]					0.0287**	0.00276**	0.0398**	0.0202**	0.0862**	0.00814
					(0.000670)	(0.00102)	(0.00487)	(0.00714)	(0.00570)	(0.00991)
Brother's earnings*Experience/10						0.0257**		0.0179*		0.0721**
						(0.00111)		(0.00710)		(0.0102)
Spouse	0.0585**	0.0582**	0.0583**	0.0580**	0.0584**	0.0582**	0.0482**	0.0481**	0.0404**	0.0397**
	(0.00104)	(0.00104)	(0.00132)	(0.00131)	(0.00105)	(0.00104)	(0.00725)	(0.00724)	(0.00832)	(0.00831)
# Kids	-0.00572**	-0.00563**	-0.00531**	-0.00527**	-0.00568**	-0.00571**				
	(0.000635)	(0.000635)	(0.000808)	(0.000808)	(0.000638)	(0.000637)				
Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Industry, first job	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Experience (3^{rd} order polynomial)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	1410269	1410269	846441	846441	1376539	1376539	38766	38766	24617	24617
R^2	0.373	0.374	0.370	0.371	0.378	0.379	0.537	0.537	0.561	0.565

Standard errors in parentheses

Standard errors clustered by sibling group

* p<0.05, ** p<0.01

[a]The average relative position in the wage distribution between ages 30 and 49. The relative position of individual earnings each year is calculatd as the value corresponding to the percentile in the earnings distribution of the entire population of Danish working-age males, who are employed in the private sector and employed for at least 50% of the year. Thus, everyone is given a value between 1 and 100. This relative-earnings variable is then standardized to ease interpretation.

[b]The average relative position in the wage distribution between ages 30 and 39 calculated in the same way as father's earnings.