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? We test for whether employers statistically discriminate between workers on the basis of their schooling, by assuming we can observe a proxy for worker productivity that the employer cannot - father, brother and co-twin earnings. Using population-based Danish administrative data, we find that employers initially statistically discriminate between workers on the basis of schooling, but schooling earnings differentials fall over time as employers learn about worker productivity. We further propose a novel test for job market signaling using differences in twin pair earnings growth, and find that signaling is important at the upper end of the schooling distribution - explaining a large proportion of the college wage premium.

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 shows 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. Yet 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.

Different empirical approaches to disentangling the HC and JMS theories have been applied. 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, i.e., they *statistically discriminate* on the basis of information about workers' schooling attainment. Employers then *learn* about workers' productive types as time passes and workers gradually reveal information about themselves through job performance.[1] Our study adds to the employer learning literature by proposing a new test for the existence of JMS. Furthermore, we present new evidence on the *importance* of JMS for the private financial returns to schooling.

The early employer learning literature exploits 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.

[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)

This literature (Foster and Rosenzweig, 1993; Farber and Gibbons, 1996; Altonji and Pierret, 1997, 2001; Galindo-Rueda, 2003; Lange and Topel, 2006; Lange, 2007) evaluates how the estimated returns to schooling and ability evolve over time.

Our point of departure is the work by Altonji and Pierret (2001).[2] Using father's schooling and brother's wage as proxies for unobserved labor market productivity, they present empirical evidence supporting JMS. Applying their empirical specification and similar productivity proxies to working-age males using data from administrative records on the Danish population, we obtain results that are supportive of the existence of JMS and thus consistent with the results in Altonji and Pierret (2001). Splitting the analysis by brother type—i.e., into non-twins, dizigotic (DZ) twins, and monozygotic (MZ) twins—we further show that the wage level of a more genetically alike brother provides a better proxy for unobserved labor market productivity.

Based on this finding, we propose a new test for employer learning using a twin-difference setup: We propose a "tripple-difference" type specification contrasting the experience gradient in wage differences of MZ and DZ twins. The results from this test support the employer learning hypothesis and the existence of JMS. Further analyses show that JMS pertains to the the college graduation margin, while we find no evidence of JMS on the high school graduation margin.

Our proposed specification takes inspiration from Miller et al. (2004), who analyze labor market outcomes in a cross section of Australian twins. While our empirical approach bears some similarities to theirs, our data enables us to build on their work in several ways. First, using panel data, we can compare twin brothers at the same point in their working career even if they complete different levels of schooling. In cross-sectional data the twin comparison must be at the same point in time (same age), implying that early-career wage dynamics cannot be analyzed because one twin is still in school. Second, by using the average wage in the industry-occupation cell as a proxy for individual wages, Miller et al. (2004) analyze labor market mobility rather than employer learning. Our data contains individual wage observations. Third, Miller et al. (2004) compare the average return to schooling of MZ and DZ twins in the first 15-20 years in the labor

[2] Their work has inspired numerous later studies (Arcidiacono et al., 2010; Bauer and Haisken-DeNew, 2001; Light and McGee, 2012; Mansour, 2012)

market to the average return in the last 15-20 years in the labor market. We argue that employer learning is particular important in the early part of a worker's career and compare the wage *gradient* of MZ and DZ twins over the first 10-15 years in the labor market.

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). Using a sample of MZ twins, we show that the returns to years of schooling approach a level close to zero after 10-15 years in the labor market. Thus on this schooling margin (years of schooling), our results suggest that JMS contributes to a decreasing wage premium and that employers learn everything they need to know about individual labor market productivity of their workers within 10-15 years. However, focusing on the high school and college graduation margins, we find that the decline in returns to schooling is driven entirely by declining returns at the high school margin, where we find no evidence of JMS. These findings instead suggest complete depreciation of skills acquired in high school over the first 10-15 years in the labor market. In contrast, our results show a persistent wage premium on the college graduation margin, where we find evidence of JMS. Thus while JMS seems important for the college graduation wage premium, JMS does not determine the wage setting entirely.

The remainder of the paper is organized as follows. Section II outlines the estimation strategy proposed by Altonji and Pierret (2001) 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

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 workers's true productivity, 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.[3]

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 information 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, 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 the wage. But if the researcher has access to information about correlates of true ability, which are initially unobserved by the employer, he or she can test if employer learning and statistical discrimination are important for the 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 initially unavailable to the employer, this information cannot be priced into initial wages. We follow Altonji and Pierret and include measures of father's schooling, father's permanent income, and brother's permanent income, respectively—information we assume to be unobserved by employers at the time of labor market entry.[4]

To test empirically for employer learning and statistical discrimination, Altonji and Pierret (2001) propose the following log wage equation:

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

where t is a measure of cumulative labor market experience, w_{it} denotes log earnings of

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

[4] The inclusion of father's earnings is specific to our analysis. Altonji and Pierret (2001) use father's schooling only.

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.

Over time employers learn about actual labor market productivity of their workers rather than the proxy information included in the regression by the researcher. Therefore, a productivity proxy that is more informative about true labor market productivity should lead 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 birth spacing. DZ twins also share half their segregating genes but further share a common home environment during childhood as they are born on the same day. MZ twins share all segregating genes as well as a common home environment.

Under the assumption that both genetic and rearing components matter for individual labor market productivity, the comparison of results from samples of different brother types provides an empirical test for the prediction that a better productivity proxy implies stronger evidence of employer learning in (2). 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. The results we present in Section IV (table 4) provide support for this prediction, thereby establishing a link from the specification in (2) to empirical strategies relying on observed twin differences, to which we now turn.

By augmenting the notation in (2) and taking differences between co-twins in each period, we arrive at the twin fixed effects (FE) specification

$$\Delta_j w_{ft} = \widetilde{\beta}_0 + \beta_1 \Delta_j S_f + \beta_2 \Delta_j S_f \cdot t + \boldsymbol{\delta} \Delta_f \boldsymbol{X}_{ft} + g(t) + \Delta_j \varepsilon_{ft} \tag{3}$$

where $j = 1, 2$ denotes twin 1 and 2 in family $f = 1, ..., \frac{N}{2}$. In contrast to the specification in (2) the twin FE specification in (3) does not include a measure of the co-twin's permanent income as a productivity proxy. Instead, this specification directly compares co-twin brothers' wages in each period.

While the between-twin differences eliminate *family* characteristics of all twin pairs, it eliminates the *genetic* endowment entirely among MZ twins but only partially among DZ twins. Given the difference in similarity across twin type, the employer learning hypothesis generates predictions for coefficients in (3). If individual labor market productivity has a substantial genetic component, then, as employers learn about the labor productivity of their workers (and set wages accordingly), we should observe a decrease in the schooling-experience *gradient* in wage differences among MZ twins *compared* to DZ twins, i.e., $\beta_2^{MZ} < \beta_2^{DZ}$. In other words, the employer learning hypothesis implies that the ability bias in the returns to schooling is less persistent over labor market experience in a sample of MZ twins compared to a sample of DZ twins.

To test this hypothesis empirically, we interact the specification in (3) with a twin-type indicator, MZ, taking values one for MZ twin pairs and zero for DZ twin pairs and arrive at:

$$\begin{aligned} \Delta_j w_{ft} &= \widetilde{\beta}_0 + \beta_1 \Delta_j S_f + \beta_2 \Delta_j S_f \cdot t + \beta_3 t + \beta_4 MZ_f + \beta_5 \Delta_j S_f \cdot MZ_f + \beta_6 t \cdot MZ_f \\ &+ \beta_7 \Delta_j S_f \cdot t \cdot MZ_f + \boldsymbol{\delta} \Delta_j \boldsymbol{X}_{ft} + \Delta_j \varepsilon_{ft} \end{aligned} \tag{4}$$

Because 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] The coefficient β_7 on the interaction between schooling difference, $\Delta_j S_f$, labor market experience, t, and the indicator for twin type, MZ, provides a test for this

[5] For completes, we ran a fully interacted specification of (4). Results are similar to those obtained from the specification in (4) and available upon request.

relative narrowing of MZ twin-wage differences compared to DZ twin-wage differences.[6]

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). 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 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 (4) compares the *slopes* in the earnings-difference profiles across twin type (a "tripple-difference"). Therefore, the consistency of β_7 in (4) is vulnerable to attenuation bias only if such a bias differs 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 in (4) only if the wage return from such training differs by twin type. We assume that this is not the case.

[6]We draw inspiration from Miller et al. (2004), who contrast MZ and DZ twin differences in labor market trajectories. While they compare *average* earnings differences of two ten-year *age spans* (age ranges 18-35 and 36-45), we test for differences in *slopes* from one year to the next in terms of *labor market experience* during the first part of the labor market career (0-15 years of labor market experience).

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 (wages). 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. To ensure comparability between individuals with different unemployment rates, we use annual-equivalent labor earnings, i.e., labor earnings scaled by the inverse of the 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.

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 earnings 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 and 40s.

[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. Due to this self-assessment of educational attainment and a lack of information on graduation year in the census data, we only use data on schooling from the administrative records.

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 equation (4) compares the wages of co-twins by years of experience in the labor market. If the 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 for both a general price trend and the 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 (4). 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 (4) potential experience is given by years since graduation from highest level of schooling. By convention we set $t = 0$ for the first year in the labor market..

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 of potential labor market experience.[10] The twin FE estimation (4) go up to 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 impose the criteria that the individual was at least 18 years old and employed full time 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 the two of them 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 has knowledge about 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 never registered with the same employer.

Table 1 presents summary statistics for the estimation sample, which consists of

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

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 of these proxies 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 twin brothers matching the birth cohorts of 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.[12] 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]

C. Graphical illustration

Before turning to the estimations, we present a simple graphical illustration of twin wage differences by twin type over the first years in the labor market. Figure 1 plots the average twin wage difference by years since graduation, from year 0 to 12, for MZ twins (left) and DZ twins, respectively. Wage differences are measured in log-point differences, i.e., approximate percentage-point differences, for twin brothers with different levels of schooling (solid lines) and twin brothers with the same level of schooling (dashed lines).

[Figure 1 about here]

While simplified and based on the unconditional distribution of twin wage differences, the graphs in Figure 1 illustrate the patterns that emerge from our data. In both samples (MZ and DZ) the initial wage difference associated with at least one extra level of schooling (solid lines) is around 30 percent at labor market entry. As experience accumulates, the wage premium declines in both samples, but the decline happens at a faster rate among MZ twins than among DZ twins. These graphs provide preliminary descriptive evidence in support of the test for different schooling-experience gradients in wage differences between MZ and DZ twins. Section IV presents the results based on the specification in (4).

The dashed lines in Figure 1 shows how wages differ between twins with the same level of schooling (compulsory, high school, and college), but possibly different length of schooling, e.g., two vocational educations of different length. Thus these graph can be viewed as a simple graphical illustration of the dynamics on the internal margin of

[12] Our definition of high school includes vocational training.

schooling levels. The graphs show an average between-twin wage difference of 5 to 10 percent at labor market entry for both MZ and DZ twins. The initial wage difference declines with years since graduation at an equal rate for MZ and DZ twins, suggesting that the wage convergence on the internal margin is driven by factors other than employer learning and statistical discrimination.

A concern is that the pattern of wage convergence in Figure 1 could be driven by mean reversion at the aggregate level, i.e. the distribution of wages narrows with experience. If such mean reversion happens at different rates in different industries, this might affect MZ and DZ twin wage differences differentially, if MZ twins are more likely to work in the same or similar industries.

To assess whether the wage convergence is caused by mean reversion, we plot the corresponding mean wage differences for the population at large. For each male wage earner in the population, who meet the same selection criteria as individuals in our estimation sample (see section III.B), we calculate the average difference between that person's wage and the wage of everyone else in the population sample, who were born on the same day. Figure 2 presents these population-wide mean differences by experience level. For individuals born on the same day, the initial mean wage difference associated with a difference in schooling levels (solid line) is almost 30 percent, equivalent to the difference observed in the twin samples (figure 1). But in contrast to the twin wage differences, the schooling-experience wage gradient in Figure 2 is positive, suggesting that wages are not mean reverting.

[Figure 2 about here]

IV. Results

The first part of this section presents the results from individual-level log-earnings regressions as specified in (2). The purpose of presenting these results is two-fold. First, we show that the conclusions drawn by Altonji and Pierret (2001) hold in a Danish institutional setting using Danish administrative records. Second, we test the prediction that the association between own earnings and the labor market performance of a brother is stronger the more alike the two brothers are. The second part of this section takes the test for employer learning based on between-twin wage differences in (4) to the data, followed by an investigation of the speed with which employers learn about initially unobserved labor productivity of their workers.

Table 4 presents OLS regression results from different versions of the log wage equation proposed in (2). The ten columns represent five sets of estimates with each to specifications: one specification that includes a productivity proxy ("a" columns) and one specification that further includes the interaction between the productivity proxy and labor market experience ("b" columns). The five sets of estimates differ by productivity proxy: father's education, father's earnings or brother's earnings, and by estimation sample: all brothers, non-twin brothers , DZ twin brothers or MZ twin brothers.

[Table 4 about here]

Columns (1a) and (1b) include the father's schooling as the hard-to-observe correlate of productivity. The first thing to note from the results in these columns is the negative estimate of the coefficient on the interaction between schooling and experience. This negative schooling-experience gradient in wages implies that employers statistically discriminate among workers based on the workers' schooling attainment. The next thing to note is the strong positive association between wages and father's schooling in column (1a). The positive association between father's schooling and wages over and above schooling suggests that the father's schooling attainment serves well as a proxy for unobserved components of individual labor productivity. Yet once we include the interaction between father's schooling and own experience (column 1b), the estimates show a positive correlation with father's schooling over time.[13]

The strong positive association with father's schooling over time is consistent with the employer learning hypothesis. The estimates from the specifications in columns 1a and 1b are comparable to the results in Altonji and Pierret (2001, table 2, columns 5-6). While obtained under a different institutional setting using different data sources, our results are qualitatively similar to their findings. If we instead include the father's labor

[13]In fact the estimate of the main effect of father's schooling becomes negative in our sample. This negaive estimate of the association with father's schooling at labor market entry may arise from early-career differences in the slopes of earnings profiles across fathers' schooling levels.

market performance (earnings) as the proxy for unobserved labor productivity (columns 2a and 2b), the results do not change.

The other specifications in Table 4 (columns 3a-5b) all include the brother's earnings as a proxy for individual labor productivity but differ by estimation sample. Estimates in columns 3a and 3b come from the sample of non-twin brothers, estimates in columns 4a and 4b come from the sample of DZ twin brothers, and estimates in columns 5a and 5b come from the sample of MZ twin brothers. The purpose of the sample split is to compare estimates of the association with the brother's labor market performance for different types of brothers.

As for the productivity proxies based on the father's outcomes, we see that the positive correlation with the brother's earnings only arises as experience accumulates. This is true in all three samples of different brother types. The results in columns 3a and 3b are comparable to—and in accordance with—the results presented by Altonji and Pierret (2001, table 2 columns 1-2).[14]

Comparing the estimates from the three samples, we expect to see the association with the brother's earnings being strongest in the sample of MZ twins, while being at least as strong in the sample of DZ twins as in the sample of non-twin brothers (see section II). The estimate of the coefficient on the interaction between brother's earnings and experience in the sample of MZ twins is 0.09, while the corresponding estimate from the samples of non-twin brothers and DZ twin brothers is 0.03—the difference is statistically significant.[15] These results confirm our prediction and motivate further analysis based on contrasts between MZ and DZ between-twin wage differences, to which we now turn.

[14] 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).

[15] 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 (results available upon request). The conclusions drawn from Table 4 do not change.

A. Twin schooling-experience wage gradients

Table 5 presents three sets of estimates obtained from OLS regressions of (4). In column 1 schooling attainment is defined as years of completed schooling. In column 2 the schooling variable is binary, taking value one for (at least) high school graduation and zero otherwise. In column 3 the schooling variable is also binary, taking value one for college graduation and zero otherwise.

[Table 5 about here]

Across the three specifications in Table 5 the results show that schooling is initially associated with a positive wage premium among DZ twins ($\hat{\beta}_1 > 0$). As the employer learning hypothesis implies that the wage differences should evolve differently between twin type *over time* (as employers learn), we expect to see no difference between the initial wage premium of DZ and MZ twins. The estimates of β_5, which are not statistically different from zero, confirm this expectation.

To test the employer learning hypothesis, we compare the schooling-experience gradient in wage differences of the two twin types. In the sample of DZ twins the initial premium to an extra year of schooling (column 1) declines by 0.5 percentage points per year of labor market experience ($\hat{\beta}_4 = -0.005$). In the sample of MZ twins the wage premium declines a faster rate, the difference is around 0.3 percentage points per year of labor market experience ($\hat{\beta}_7 = -0.003$ and statistically significant at the five-percent level). These results support the employer learning hypothesis, given the definition of schooling as years of completed schooling.

Column 2 focuses on the high school graduation margin. Among DZ twins the schooling-experience gradient in wages declines on the high school margin, at a rate of three percentage points per year, which is not statistically different from the rate of decline among MZ twins. Thus our results do not support the employer learning hypothesis at the high school margin.

The results in column 3 focus on the college margin. The estimates show a positive schooling-experience gradient in wage differences among DZ twins of 1.5 percentage points per year, i.e., the wage gap increases. Relative to this increasing wage gap among

DZ twins, the wage difference among MZ twins declines by -1.5 percentage points per year of labor market experience (statistically significant at the five-percent level). These results support the employer learning hypothesis on the college margin. Thus the empirical support for the existence of JMS seem to be driven by early-career wage dynamics at the upper part of the schooling distribution.

While the results in Table 5 provide empirical evidence in support of employer learning, one might be concerned that differences in wages are driven by differences in occupational choice. Figure 3 plots by years since graduation the share of twin-pair observation with both twins working in the same industry—this is the case for around one in five twin-pair observations. As expected, given their more similar schooling choices, MZ twins are more likely to work in the same industry compared to DZ twins. Over time the share of same-industry twin-pair observations decline slowly among both MZ and DZ twins. However, the important thing to note is that the decline happens at the same rate for MZ and DZ twins. We therefore argue that the relative wage convergence among MZ twins compared to DZ twins is not driven by industry-of-occupation choices.

B. Importance of JMS for private financial returns to schooling

The results in Table 5 are informative about the *existence* of employer learning; yet they say little about the importance of JMS for the total private financial returns to schooling. As MZ twins come intuitively close to providing counterfactual outcomes at the individual level, estimates from the sample of MZ twins may contribute new insights on this issue.

Figure 4 plots the marginal effect of schooling at each year of experience for MZ twins at different schooling margins. The results come from the estimation of (3), allowing for non-linearity in experience.[16] The sample of MZ twins used here is the same as for the previous estimations with the addition of observations between 14 and 16 years after graduation to capture the non-linearity in those later years.

[Figure 4 about here]

[16]The estimation includes a second-order polynomial in experience. The results are almost identical for higher-order polynomials.

In the sample of MZ twins the initial wage difference associated with an extra year of schooling is around ten percent. As experience accumulates, the wage difference declines until it is statistically no different from zero after around 13 years. As this narrowing of the wage gap among MZ twins happens at a faster rate than among DZ twins (table 5, column 1), some of the initial wage premium to an extra year of schooling seems to be attributable to JMS. Whatever the signaling value is at labor market entry, the signal has no value after around 13 years. While we cannot distinguish between job market signaling and skill depreciation, the area beneath the curve provides an upper bound for the contribution of signaling to private financial returns to an extra year of schooling.

The returns to graduating from high school in the sample of MZ twins is initially around 50 percent. This wage gap narrows with experience and is eliminated after a little less than a decade of labor market experience. As we did not find empirical support for the existence of JMS at the high school margin (table 5, column 2), the results suggest that at the high school margin wage convergence is driven entirely by skill depreciation, which happens within ten years from high school graduation.

The results presented in Table 5 (column 3) support the existence of JMS, thereby implying that (some of) the returns to college graduation reflect a signaling value. Yet the wage premium to college graduation seems persistent over the period we observe, i.e., the first 15 years after graduation (figure 4). Thus while JMS seems important for the college graduation wage premium, JMS does not determine the wage setting entirely. Furthermore, even if skills acquired in college depreciate with labor market experience, employers are willing to keep paying a premium for workers' college education for as long as 15 years after graduation.

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 working-age male wage earners in the private sector in Denmark 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.

The large sample of 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 evidence in support 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 focus on high school and college completion, respectively, we find evidence of JMS only at the college margin.

We use the sample of MZ twins to assess the relative importance of JMS for the private financial returns to schooling. The returns to years of schooling approach a level close to zero after 10-15 years in the labor market. This declining wage premium is driven entirely by the early-career wage dynamics at the high school graduation margin, where we do no find empirical support for the existence of JMS. Our results therefore suggest complete skill depreciation at the high school margin within 10-15 years of graduation. In contrast, the college graduation wage premium is persistent over the first 15 years after graduation (the period for which we have data), implying that while JMS seems important for the college graduation wage premium, JMS does not determine the wage setting entirely—employers keep paying a wage premium for their workers' college education for as long as 15 years after graduation.

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Tables

Table 1
Summary statistics.

	Non-twin	Twin	Difference
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)	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)	57.9	57.3	0.6
	(25.2)	(24.6)	(0.534)
Individuals	158813	3889	

NOTE.—The table shows summary statistics (means and standard deviations) for our samples of non-twin and twin brothers together with tests for equal means (differences and standard errors). While our estimation sample includes twin birth cohorts 1950-1980, we present summary statistics for twin birth cohorts 1960-1980—which corresponds to the non-twin birth cohorts in our estimation sample—to allow for a comparison of variable means across brother type.

Our sample includes men who has at least one brother; their birth spacing cannot exceed ten years. By individual and year we include observations if the man was a full-time wage earner (at least 18 years old) in the private sector, who was employed at least six months in the year of observation, and if the year of observation is between 0 and 15 years after graduation from highest completed level of schooling. We deal with outliers by excluding the top and bottom 0.5 percent of the wage distribution and individuals who graduated from their highest level of schooling before age 14 or after age 35.

We do an employment correction of all earnings measures by scaling observed earnings by the inverse of the employment share. The brother's and the father's earnings is the mean percentile rank in the population wage distribution of working-age private-sector male wage earners, who were employed full time for at least six months in the year of observation.

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%)

NOTE.—Separate for MZ and DZ twin pairs, the table shows the distribution of between-twin differences in the education ministry standard completion time for highest completed level of schooling. Twin pair-year observations. Column shares (percent) in parentheses.

Table 3
Twin differences by level of completed schooling.

	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%)

NOTE.—Separate for MZ and DZ twin pairs, the table shows the distribution of between-twin differences in schooling levels. Our definition of high school includes vocational education. Twin pair-year observations. Share (percent) of twin-type observations in parentheses.

Table 4
Individual-level wage regressions.

	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	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
# Kids	✓	✓	✓	✓	✓	✓				
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

NOTE.—The columns contain estimated coefficients from ten different OLS regressions of log earnings on schooling, experience, schooling interacted with experience, a proxy for individual labor productivity (assumed unobserved by the employer), industry code for first job, and information about household composition. All specifications include year fixed effects. Within each a-b column pair specifications differ by the inclusion of the interaction between the productivity proxy and experience. The five a-b sets of estimates differ either by the variable included as proxy for individual labor productivity or by estimation sample. Columns 1a-2b use the full sample of all brothers, including the father's schooling and the father's earnings, respectively, as aproxy for labor productivity. Columns 3a-5b all include the brother's earnings as a proxy for individual labor productivity but differ by estimation sample: Estimates in columns 3a-3b come from the sample of non-twin brothers, estimates in columns 4a-4b (5a-5b) come from the sample DZ (MZ) twin brothers.
Experience is given by age minus the education ministry standard completion time for highest completed level of schooling (in years) minus six. The brother's (father's) earnings is the age 30-39 (30-49) mean percentile rank in the population wage distribution of working-age private-sector male wage earners, who were employed full time for at least six months in the year of observation. We standardize this measure to ease interpretation of the results.
Standard errors clustered by brother pair in parentheses. * p<0.05, ** p<0.01.

Table 5
Twin fixed effects estimates.

	Dependent variable: Δ Log wage		
Schooling variable:	Years	High school+ $= 1$	College $= 1$
(β_1) Δ Schooling	0.0841***	0.360***	0.190***
	(0.00484)	(0.0277)	(0.0235)
(β_3) Potential work experience (years)	-0.00119	-0.00161	-0.00236*
	(0.00142)	(0.00145)	(0.00142)
(β_2) Δ Schooling $\cdot$ experience	-0.00487***	-0.0289***	0.0145***
	(0.000682)	(0.00357)	(0.00355)
(β_4) MZ twin pair $= 1$	0.0120	0.0123	0.00496
	(0.0136)	(0.0145)	(0.0146)
(β_5) Δ Schooling $\cdot$ MZ	0.00993	0.0234	-0.00467
	(0.00861)	(0.0498)	(0.0397)
(β_6) Experience $\cdot$ MZ	-0.00164	-0.00137	-0.000489
	(0.00205)	(0.00209)	(0.00208)
(β_7) Δ Schooling $\cdot$ experience $\cdot$ MZ	-0.00264**	-0.00754	-0.0149**
	(0.00118)	(0.00632)	(0.00629)
Δ Spouse	✓	✓	✓
Observations	18771	18771	18771

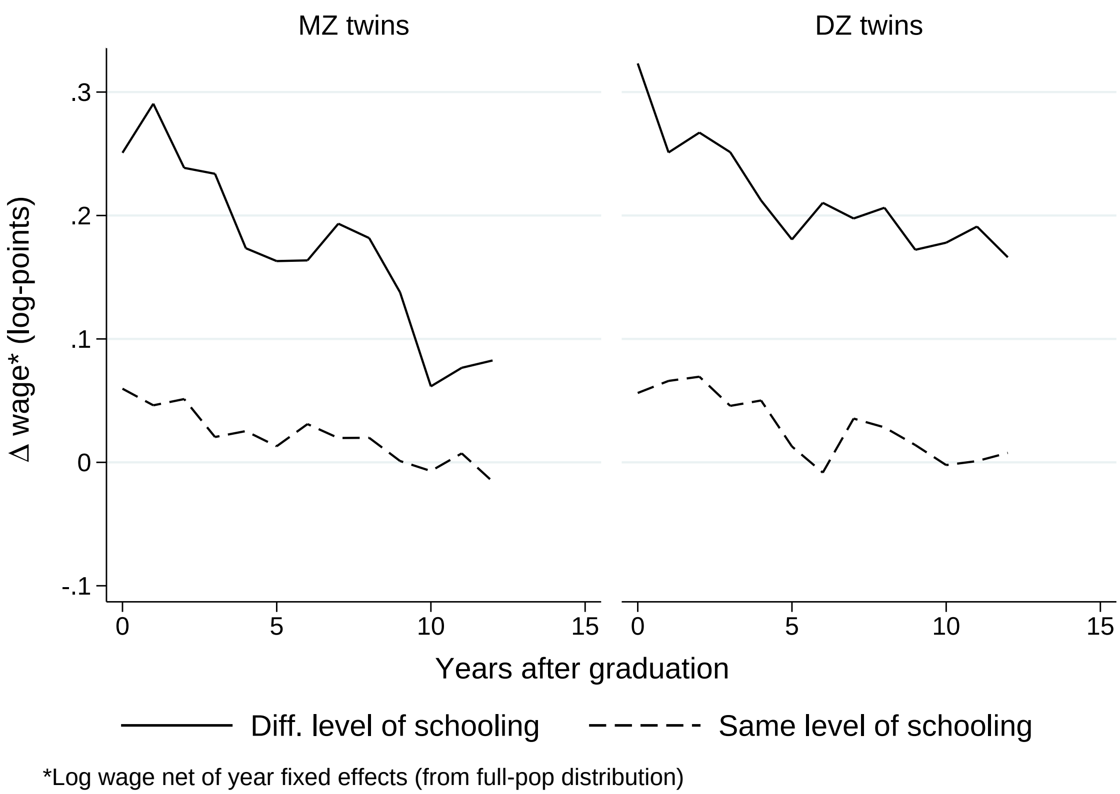
NOTE.—The columns contain estimated coefficients from three OLS regression of between-twin log wage differences on differences in schooling, interacted with potential work experience (years since graduation) and twin type. Wages are net of year fixed effects obtained from the population wage distribution of working-age private-sector male wage earners, who were employed full time for at least six months in the year of observation.

The three specifications differ by definition of the schooling variable. In the first column schooling is given by years of schooling. In the second column schooling is a binary variable taking values one for (at least) graduating high school and zero otherwise. In the third column schooling is a binary variable taking values one for college graduation and zero otherwise.

Standard errors clustered by twin pairs in parentheses. * $p<0.1$,** $p<0.05$, *** $p<0.01$

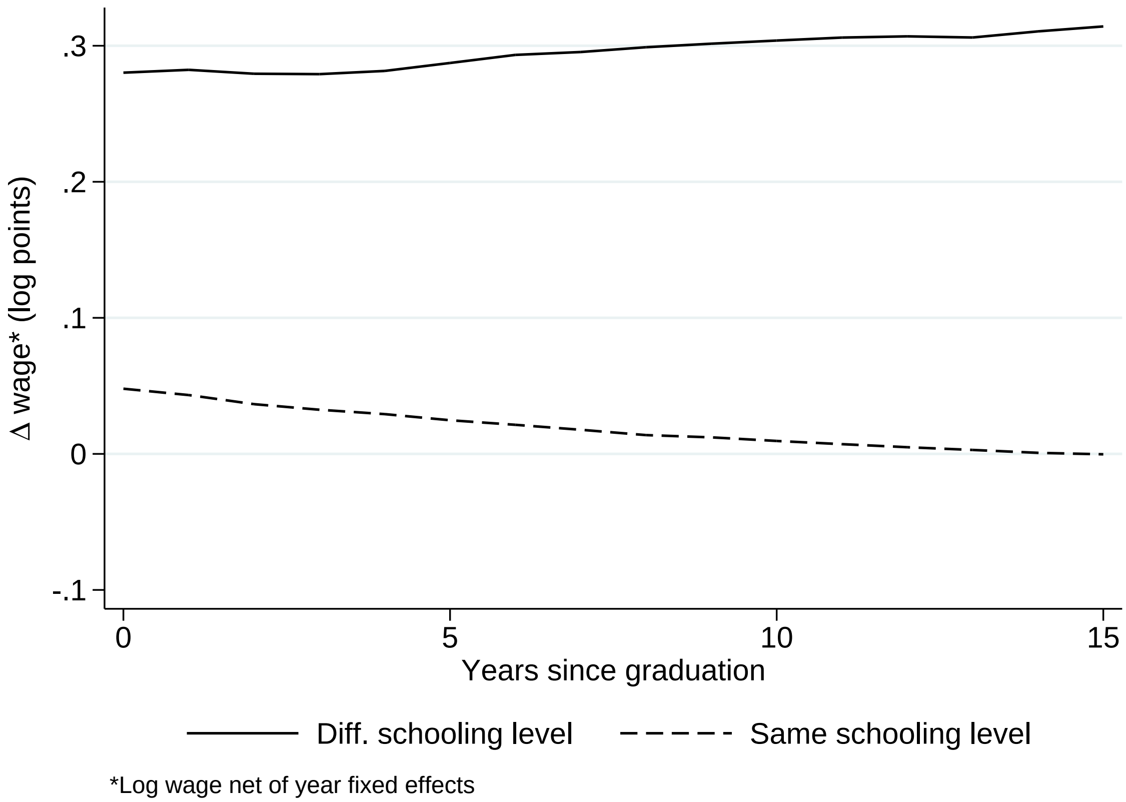
Figures

Figure 1.—Twin mean wage differences over years since graduation. By schooling differences and twin type.



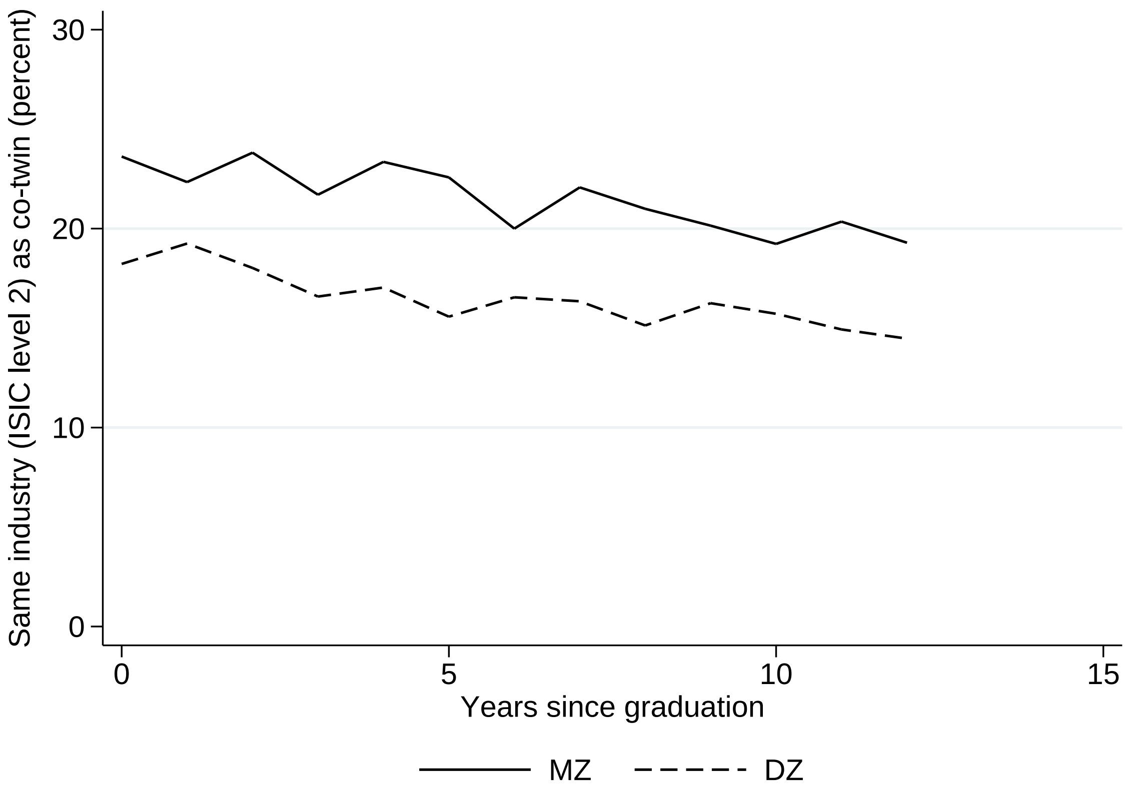
NOTE.—The figure shows average between-twin log wage differences for the MZ twins (left) and DZ male twins in our sample by years since graduation from highest completed level of schooling. Wages are net of year fixed effects obtained from the population wage distribution of working-age private-sector male wage earners, who were employed full time for at least six months in the year of observation. The solid lines represent between-twin wage differences associated with a between-twin difference in the level of schooling (compulsory, high school, college), while dashed lines represent between-twin wage differences associated with no between-twin difference in schooling level. Twin pairs with no school-level difference are sorted by the education ministry standard completion time for highest completed level of schooling (measured in months), implying that $\Delta S^{months} \geq 0$. In these graphs we do not distinguish between one or two levels of difference in schooling.

Figure 2.—Average between-person log wage differences by years since graduation for males born on the same day.



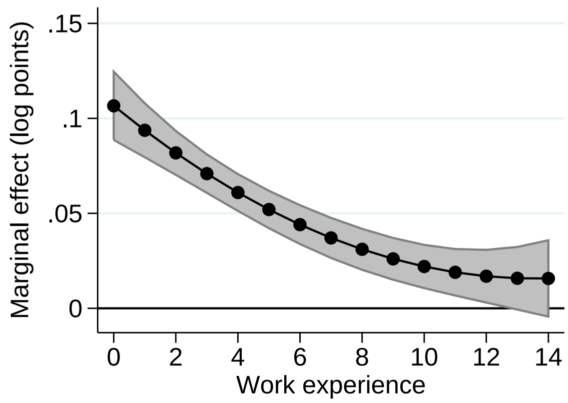
Note.—From the population of Danish males we include those who meet the same selection criteria as the brothers in our estimation sample, i.e., full-time wage earners (at least 18 years old) in the private sector, who were observed between 0 and 15 years after graduation and employed at least six months in the year of observation, and if the year of observation is between 0 and 15 years after graduation from highest completed level of schooling. We exclude the top and bottom 0.5 percent of the wage distribution and individuals who graduated from their highest level of schooling before age 14 or after age 35. For each individual in this sample we calculate the average wage difference to all other men born the same day and then take the average over individuals to produce these graphs. Wages are net of year fixed effects obtained from the population wage distribution of working-age private-sector male wage earners, who were employed full time for at least six months in the year of observation. The solid line show between-person log wage differences for between-person differences in the level of schooling (compulsory, high school, college), while the dashed line represent no school-level difference. Pairs of men with no school-level difference are sorted by the education ministry standard completion time for highest completed level of schooling (measured in months), implying that $\Delta S^{months} \geq 0$. In these graphs we do not distinguish between one or two levels of difference in schooling.

Figure 3.—Similarity in industry of occupation by years since graduation and twin type.

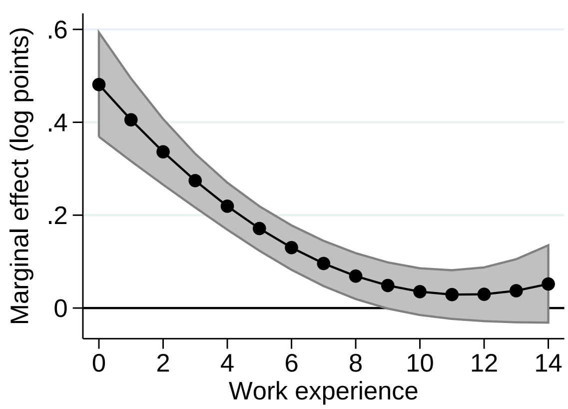


NOTE.—By years since graduation and twin type, the figure depicts the share (percent) of twin pair-year observations with both twins working in the same industry. The industry grouping has 27 categories and corresponds to the level 2 International Standard Industrial Classification (ISIC).

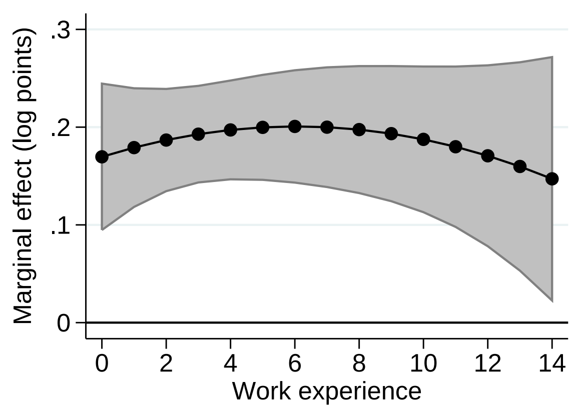
Figure 4.—Marginal effects from twin fixed effects log wage regressions using the sample of MZ twins, by years since graduation and definition of schooling.



(a) Wage returns to years of schooling



(b) Wage returns to high school+



(c) Wage returns to college

NOTE.—Using the sample of MZ twins, the figure shows marginal effects from OLS regressions of between-twin log wage differences on between-twin differences in schooling and marital status, by years since graduation. Wages are net of year fixed effects obtained from the population wage distribution of working-age private-sector male wage earners, who were employed full time for at least six months in the year of observation. Each graph show marginal effects for different definitions of the schooling variable. Gray-scaled areas indicate 95-percent confidence bands, calculated from standard errors clustered by twin pair.