DISCUSSION PAPER SERIES

Education Spillovers within the Workplace

Kristian Hedeager Bentsen
Jakob R. Munch
Georg Schaur

DISCUSSION PAPER SERIES

Education Spillovers within the Workplace

Kristian Hedeager Bentsen
University of Copenhagen and Rockwool Foundation Research Unit

Jakob R. Munch
University of Copenhagen and IZA

Georg Schaur
University of Tennessee

ABSTRACT

Education Spillovers within the Workplace

Education policies depend in part on the presence of externalities, but very little evidence exists to confirm the existence of such externalities. In this paper we investigate if there are spillover effects from education within peer groups at the workplace. We estimate the effect of increasing the share of higher educated workers in close peer groups on wages, using a rich data source linking workers to workplaces and specific occupations. Our empirical approach accounts for the endogenous sorting of workers into peer groups and workplaces, and, at the same time avoids the reflection problem. In our main specification we find statistically significant but economically small peer effects across all occupations. The magnitude of the effect differs across length and type of education, as well as across occupations and peer group- and workplace size.

Corresponding author:
Jakob R. Munch
Department of Economics
University of Copenhagen
Øster Farimagsgade 5
Building 26
1455 Copenhagen K
Denmark

E-mail: Jakob.Roland.Munch@econ.ku.dk

1. Introduction

Policy makers and international institutions consider education a determinant of productivity, economic growth and welfare. Consequently, governments invest into the design of education policy and individuals take advantage in expectation of improved long-term returns. Hopes for these policies are high. In addition to private returns to human capital investment, education is believed to generate various positive externalities including non-pecuniary benefits such as reduced crime, better health and improved trust (see e.g. Heckman et al. 2017). Such externalities in particular would justify current education policies in most advanced countries, but not much evidence exists to confirm the existence of these externalities.

One possible market-based education externality generates positive spillover effects at the workplace, if educated workers share their knowledge with coworkers and allow for everybody to be more productive. These complementarities known as peer effects are especially important for policy, because they justify cross-subsidization of education programs. The magnitude of these peer effects determines the extent by which education even achieved by a minority of the population affect productivity, search, hiring and wage outcomes across a potentially much larger part of the workforce. Therefore, robust empirical evidence for education related peer effects is necessary to determine the breadth of education policy effects on labor markets.

Despite their intuitive appeal and policy relevance, it is challenging to empirically evaluate these peer effects. Peer groups are not ex-ante defined in datasets resulting in measurement problems, and, the sorting of workers across workplaces leads to unobserved workplace, worker and workplace-worker interactions that are difficult to separate from peer effects. For this paper we combine ideas from the literature on education spillovers, a rich data source, and rigorous identification approaches applied in

the recent literature on productivity spillovers to provide evidence for spillover effects of education on wages at the workplace.

We match Danish employer-employee data from 1995 to 2008 connecting workers to their workplace. This rich data source allows us to track individuals' wages, level of education, education of coworkers at the workplace, and, time varying worker characteristics including age and experience. If peer effects exist, then we expect that they occur among coworkers at the same location performing a similar job. Therefore, we define a worker's peer group as all coworkers employed at the same time at the same workplace in the same occupation. Then, education related peer effects imply that a more highly educated peer group, measured by the share of workers with higher education in the peer group, affects each worker's wages. Identification requires that we account for selection of workers across jobs that result in the same statistical relationship.

We link worker's wages to the peer group's average education attainment in reduced form, where each worker's education level is predetermined. In our most careful specification we fully avoid Manski's reflection problem (Manski, 1993). Workers are exposed to varying levels of peer group education by switching firms, workplaces and occupations. Within a given peer group, turnover determines workers' exposure to peer group education. Therefore, the main threat to identification is the endogenous matching of workplaces with workers based on opportunities to earn higher wages for unobserved reasons. Our most rigorous empirical model accounts for this by absorbing occupation and workplace specific wage trends in addition to unobserved worker-workplace-occupation specific information. Therefore, in addition to unobserved ability, even if workers have specific skills that match particularly well with a certain occupation in a firm, (e.g. a firm requires and engineer with language skills in Arabic to write product manuals for the export market), we control for this selection with our fixed effects. This extensive set of fixed effects and rich data variation extend existing identification approaches in the literature on education spillovers on wages.

Our results provide evidence for positive spillover effects even in very demanding specifications. For ease of interpretation, consider a worker in a peer group with 10 coworkers half of which have a college level education. Based on our preferred specification our results imply that replacing a worker without higher level of education with another college graduate increases everybody's wages by about 0.042 percent. For comparison, an additional year of work experience for a new worker increases that worker's wages by about 0.3 percent. The 0.042 percent increase in wages from the spillover effect holds for all 10 workers on the job, whereas raising one worker's experience by one year raises total wages by approximately .03 percent.[1]

At the most rigorous level, our estimation strategy focuses on identifying variation in average education from within worker-workplace spells. Estimated spillover effects increase as we remove fixed effects one by one, confirming that occupation and firm switching as well as measurement and omitted variable concerns are important to determine the magnitudes or peer effects.

A myriad of channels can explain positive education spillover effects. Testing these channels is challenging because spillovers are not directly observable. Focusing on the workplace provides a straight forward definition of peer groups based on job related interactions and limits the number of plausible mechanisms. This allows firms, workers and policy makers to optimize on the benefits of education policy, and, informs the literature of why and where peer effects occur. We use our rich data and examine multiple definitions of peer groups to provide additional evidence.

Existing literature examines education spillovers defined at a broad level defined by a geographic unit. For example, the literature examines the hypothesis if education attainment in the local economy affects wages and productivity (Acemoglu and Angrist, 2000; Bratti and Leombruni, 2014; Glaeser, Scheinkman, and Shleifer, 1995; Rauch 1993; Glaeser and Mare, 2001; Liu 2007; Moretti 2004a; Moretti

---

[1] Assuming that wages are fairly homogenous within the peer group.

2004b; Chang et al., 2015). In his review of the literature, Henderson (2007) points out the challenge to distinguish urban knowledge spillovers from agglomeration economies. Data aggregation limits the ability of fixed effects to solve omitted variable problems. Instead, identification relies on observable variables to account for changes in the firm or workforce composition and other agglomeration related factors, or, instrumental variable techniques are employed to establish causality. Henderson (2007) discusses limits of these identification approaches. Research also confirms the importance of the peer group definition (Rosenthal and Strange, 2008; Giuri and Mariani, 2013). In addition to identification challenges, at such broad definitions of peer effects, it is difficult to say how firms can benefit from hiring more educated workers, how workers' job search decisions may be affected by peer effects, or, how education policy affects firms and their surrounding geography.

Some recent papers examine knowledge spillovers by the effects of universities on the local economy. These papers exploit the placement of universities (Liu, 2015) and the size of universities' endowments interacted with stock market shocks (Kantor and Whalley, 2014) to examine spillover effects from universities. From an interpretation point of view it is difficult to know why spillovers occur. From a policy point of view the hope is that university educated workers transfer knowledge across the entire economy, not just in the immediate vicinity of universities. Our definition of peer groups is much narrower than this, limiting spillover effects to workplace related interactions, but our economic geography is much broader. Accounting for workplace fixed effects we account for location specific heterogeneity such as the proximity to a university or other education related institution. Therefore, our regressions identify spillovers beyond these local amenities. This is important for the regional distribution of education related benefits and policy that aims to spread the benefits of education by subsidizing the hiring of workers with certain degrees in firms that have challenges attracting these kinds of workers.

Several papers examine spillover effects related to productivity (but not education) in a more narrowly defined work environment (Azoulay, 2010; Arcidiacono et al., 2017; Horton and Zeckhauser,

2016; Kato and Shu, 2009; Navon 2009). For example, Mas and Moretti (2009) provide evidence for peer effects among cashiers in supermarkets. De Grip and Sauerman (2012) find direct peer effects of training with a randomized experiment in a call center. There are many advantages of examining peer effects in narrow work environments. Productivity can be measured in comparable ways across workers, peer groups are well defined, the context facilitates the identification of mechanisms such as social pressure, and, clean identification. The disadvantage is that potential heterogeneity across work environments, workers, firms, occupations and time complicate generalization of these effects, but more general estimates of peer effects are important to understand and evaluate the effects of policy which is designed at a much broader level.

Panel data research designs at the matched worker-firm level provide a middle ground that solves several identification concerns related to unobserved agglomeration forces, unobserved worker and firm characteristics, but, pools across many occupations and allows to directly identify heterogeneity important to generalize peer effects. Two recent approaches employ matched worker-firm data with individual specific observations and exploit variation over time to understand productivity spillover effects. Battisti (2017) examines the effect of average co-worker performance on wages applying an identification approach by Arcidiacono et al. (2012). Cornelissen et al. (2017) apply a similar approach but also exploit information on worker's occupation, allowing for a narrow specification of peer groups.

Far fewer papers examine education spillover effects with matched worker-firm data and limitations in the dimensions of variation concede identification standards achieved by the literature on productivity effects. Firm level studies define peer groups at the broad firm level and do not account for individual specific unobserved effects or changes in the ability composition of the firm (Gellner et al., 2015; Martins and Jin, 2010). Instead, survey data include individual specific observations, but the lack of time variation makes it challenging to account for individual specific ability (Battu et al., 2003; Wirz, 2008).

We combine the approach of focusing on average education levels within a peer group from the earlier education spillover literature, but apply it within an identification approach as in Cornelissen et al. (2017) to account for omitted variable bias and identify effects within narrow occupation-based groups of coworkers. Contrary to firm level studies we observe workplaces, the actual location of where an employee performs her job. Therefore we are able to examine spillovers at a narrow level avoiding averaging issues across irrelevant peers that likely results in attenuation bias. Our rich sources of variation allow for fixed effects that extend the existing education literature and Cornelissen et al. (2017).

2. Data, Empirical Model and Identification

We match employer-employee data from 1995 to 2008 for the full Danish population, connecting workers to a specific workplace. Note that workplaces are defined at an even narrower level than firms, because firms may have multiple plants and spread workers across multiple locations.

Our sample keeps individuals between 18-64 years, who are in fulltime employment at the time of measurement (last week of November) in a firm in the private sector. We exclude firms in agriculture and quarrying/mining. For each worker the data report the highest completed level of education.

We define peer groups as co-workers in the same workplace with the same occupation. We mainly rely on the Danish version of the international ISCO88-classification to distinguish occupations, DISCO. We define worker i's peer group as all other workers in the same workplace who share the same 4-digit occupation at time t. A few caveats apply. Only private firms with at least 10 employees are required to report occupational codes (though some smaller firms do so as well, and we include these). For most missing observations, Statistics Denmark imputes occupations on the basis of the education or occupation that the worker's labor union represents. As the occupations need to be accurate, we only include workers with occupations reported by their employer. We only include occupations that are listed in Statistics

Denmark's documentation, and drop occupations that are related to the sectors we excluded.[2] Furthermore, as we cannot identify peer effects in groups of one, we drop workplace-time-occupation combinations that represent only one employee.

A few workers we observe start out with no university education but later on complete their degrees. This is a very small subsample of the population. A standard way to account for these workers is to specify an indicator that accounts for workers' own education. For this subsample the concern is that these workers respond to positive wage shocks of holding a university degree. If their own education increases, then the peer group education level for this workplace increases for everybody else. As a result, both the worker's own education level and the peer group are subject to endogeneity concern. Because this only affects 1 percent of the sample, we choose to drop observations that obtain the relevant level of education that is relevant (for the specific analysis performed) within a workplace spell.

Table 1 reports summary statistics for this sample. Our overall sample contains 1,603,373 workers, 74,660 workplaces and 1,088,386 individual peer groups. The average worker is present in the sample for 5.23 years, works at 1.76 different workplaces and has worked in 1.99 different peer groups throughout the sample period. In each year in the sample period, the average workplace consists of 2.62 peer groups. These peer groups involve about 8 workers on average, while median peer group size is 3.

[TABLE 1 HERE]

Our main independent variable of interest is the education intensity of the peer group. Let indicator, E_{iojt}, equal 1 if individual i in occupation o employed in workplace j in year t has completed education at MA-level or above and zero otherwise. We construct education intensity as average peer

---

[2] For a full list of occupations and those that are excluded, see Appendix A (not yet produced for this version).

group education, $\bar{E}_{ojt-i}$, defined as the simple mean over workers' own education E_{iojt} within the peer group *o-j* in year *t*, but excluding worker i. Therefore $0 \leq \bar{E}_{ojt-i} \leq 1$ measures the share of workers within a peer group with MA education not including worker *i*'s level of education. To examine what levels of education spills over and to inform policy, we also experiment with alternative definitions of, E_{iojt}, based on lower levels of education in robustness tests.

We also refer to $\bar{E}_{ojt-i}$ as the Average Peer Group Education (APGE), and Table 1 shows that APGE has a standard deviation of 0.17. The change in this peer group education intensity from one year to the next has a standard deviation of 0.07. Not surprisingly, the standard deviation of this change is around 3 times higher (0.14 versus 0.05) for workers that change peer groups (Movers) than for workers who remain in their original peer group (Stayers). Nevertheless, almost all workers experience at least some change in their peer groups throughout the sample period. The share of workers in peer groups with worker turnover is 90 percent.   The average share of workers replaced by turnover is computed as the number of workers who join or leave a peer group, times 0.5 to avoid double counting, and, divided by the peer group size. This amounts to 20 percent, which indicates that every fifth worker in a peer group is replaced every year. 43 percent of all workers in the sample experience at some point a change in the peer group education intensity reflecting that many occupations rarely employ workers with education at MA-level or above.

We furthermore find that 28 percent of workers in the dataset are employed in a peer group where at least one co-worker has MA-level education (i.e. $\bar{E}_{ojt-i} > 0$), and 40 percent of workers do at any time in the years we observe them work in such a peer group. Amongst workers we observe in two consecutive years, 32 percent experience a change in $\bar{E}_{ojt-i}$ from *t-1* to *t*.

Our main identification strategy will account for unobserved variables with fixed effects. However, individual specific characteristics that change over time cannot be conditioned out with such an

approach. Therefore, we augment our dataset with worker's age and work experience to proxy for all worker characteristics that change over time and predict wages.

Our dependent variable of interest is the log hourly wage, $\ln(w_{iojt})$. Wages are calculated as total annual labor income adjusted for pension contributions divided by annual hours worked. To minimize the effect of extreme observations in the wage distribution, we censor these at the 1st and 99th percentile of the full distribution of wages for all years 1995-2008 after they are deflated by the yearly CPI.

Let α_i, γ_{ot}, μ_j, be worker, occupation-year and workplace fixed effects. Putting all the pieces from the data together our most basic empirical model of interest relates log wages to age, experience, peer measures and fixed effects.

$$\ln(w_{iojt}) = \alpha_i + \beta_1 \bar{E}_{ojt-i} + \beta_2 Age_{iojt} + \beta_3 Age_{iojt}^2 + \beta_4 Experience_{iojt} + \beta_5 Experience_{iojt}^2 + \gamma_{ot} + \mu_j + u_{iojt} \ (1)$$

Under well-known identification assumptions related to the regression disturbance u_{iojt} pooled OLS, including indicator variables to account for fixed effects, consistently identifies the coefficients of interest.

The main parameter of interest, β_1, is the reduced-form spillover effect of co-workers' average level of education. We expect $\beta_1 > 0$. The idea that interaction at the workplace increases productivity has been around for a long time (Marhsall, 1890). Lucas (1988) discusses the creative process and learning from each other as a potential mechanism for spillover effects. Several authors discuss the ability to learn and how early education may determine the ability to learn later on (Niehaus, 2012; Heckman and Carneiro, 2003). Combined, this implies that the creative process with the ability to participate and absorb this process affects everybody's wages. Testing for specific mechanisms is difficult due to the obvious issue that these mechanisms are not directly observable. However, in its most general form, empirical models that do examine spillover effects may include endogenous spillovers determined by peer-group achievement measured based on the dependent variable, or, in our case wages (Manski, 1993).

Identification of endogenous peer effects requires valid instruments that we don't have available. Instead we estimate the reduced form model (1) and let β_1 capture all education related peer effects similar as in Andersen (2016). In addition to taking care of sorting into workplaces based on own ability, the individual fixed effect in α_i also captures all private returns of own education.

If matching of workers with firms is random conditional on the fixed effects, several sources of variation identify peer effects. Workers benefit from peer effects by moving to different workplaces, changing occupations, or, hiring more educated workers to join their work environment. Sequentially introducing more rigorous fixed effects accounts for endogenous selection, but also eliminates these sources of spillover effects. We first extend our model with workplace-occupation fixed effects. This accounts for the concern that more productive work environments may be able to attract a more educated work force. Extending the model with workplace-year effects accounts for the concern that workplaces with greater growth potential pay higher wages and attract more educated workers. Augmenting the model with worker-workplace specific effects accounts for the concern that specific worker-workplace interactions determine selection. At this level of variation, peer effects and all other effects are identified off variation within worker-workplace combinations. This eliminates the possibility that workers switch across workplaces to take advantage of peer effects, but workers still may switch occupations within a given workplace to increase wages.

Workers switch jobs for many reasons. These include wage opportunities due to productivity differences, personal reasons, geographic preferences etc. In addition, workers may have preferences for a friendly work environment, social interaction, and, coworkers that think alike due to their social and education background. As in Cornelissen et al. (2017) our empirical models account for all of these with various combinations of fixed effects. At the most rigorous level, the model even accounts for unobserved interactions based on unobserved workplace and worker characteristics.

For interpretation of magnitudes it is important that we keep in mind that we measure peer groups as a share between zero and one. Therefore, because the dependent variable is in logs, $\hat{\beta}_1$ is approximately equal to the percentage change in wages when $\bar{E}_{ojt-i}$ increases by one percentage point. It is difficult to predict how estimates based on our identification approach compare to the existing literature, because it solves multiple competing biases. For example, omitting unobserved individual ability results in positive bias if high ability people make higher wages and like to sort with more educated people. Moretti (2004c) provides a framework that shows that heterogeneity across firms, workers and regions leads to a potential bias of OLS estimates that is difficult to sign. Specifying peer groups at a narrower level may leads to greater estimates because we would expect that averaging over potentially irrelevant peers in broader firm-level definitions attenuates spillover estimates. On the other hand, at a broader definition average education levels may be correlated with broader agglomeration forces, leading to positive omitted variable bias.

Our standard definition of peer effects follows the literature, but it implicitly imposes non-linearity. Compare a worker in peer group with 10 coworkers with a worker in a peer group of 100 coworkers where in both cases 50% of the workers have higher level university education. Switching one worker's education in the small peer group raises average education from 0.5 to 0.6, or, by 10 percentage points. In the large peer group, the same experiment raises average education only by 1 percentage point. Multiplied into the same coefficient, β_1, our standard definition of peer effects implies smaller wage changes in large peer-groups. We will examine this in our robustness checks.

3. Results

Table 2 presents baseline estimation results. $\bar{E}_{ojt-i}$ reports the main coefficient of interest, the average peer group effect of education attainment on workers' wages. All standard errors are clustered at the firm-year level.[3]

As expected, across all columns an increase in education attainment by the peer group raises wages. However, across the columns as we introduce more stringent fixed effects the size drops by almost an order of magnitude. Column (A) controls for the possibility that workers in relatively well-paying workplaces could be employed in peer groups with more productive workers. It does not capture that some workplaces might compensate certain occupations better than average, which potentially could attract better employees to these occupations. Therefore, moving from column A to column B we account for occupation-workplace unobserved productivity differences. Moving to column C we augment the specification with workplace-year fixed effects, to take care of shocks that affects workplace performance and at the same time attracts a more educated workforce. Such shocks could be new technology or increasing demand from export markets.

For each worker a change in $\bar{E}_{ojt-i}$ can happen for several reasons. First, the workplace can hire more educated workers in the worker's peer group or educated workers may leave the workplace. Similarly, low educated workers may enter and leave the peer group. Second, a worker can change workplaces and join a new peer group. In model (C) both types of variation identify spillover effects. To shed light on which effect is more important, column (D) extends the model with worker-workplace fixed effects to isolate identification to the first type of variation, i.e. only variation in education intensity within the peer group at the workplace determines the spillover and identifies the coefficient. We still find positive peer effects, but the magnitude of the coefficient is almost half of what we found in model (C).

---

[3] We also experimented with clustering at the firm level and conclusions remained the same.

This indicated that both sources of spillovers are important. As we are not specifically interested in one type of variation over another, we focus on model (C) in the following results.

Changes in the education intensity of peer groups may, as described above, happen due to a variety of reasons. Another type of change may happen if workers change their education level from one year to another. Only very few workers obtain a MA while in employment, but to investigate if it is important for our results, we define $\bar{E}_{ojt-i}$ only based on workers that enter or leave the peer groups. For workers who change their education status during a peer group spell, we keep constant their education, using the initial level they had upon entry into the peer group.[4] This model thereby avoids the so-called reflection problem (Manski, 1993), as APGE in the peer group is fully pre-determined. We find that results in model (D) are very similar to model (C), and see this as the main result of interest in Table 2. In the following analyses, we base APGE on the specification applied in model (E).

We draw several conclusions. First, even with a rigorous set of fixed effects, the results provide evidence for positive education spillover effects. Second, accounting for unobserved worker, workplace, occupation characteristics, and, interactions of these unobserved omitted variables is important to identify the magnitude of the spillover effects.

[TABLE 2 HERE]

Across the columns the coefficient changes a lot and it is worth understanding where these differences are coming from. In particular, the identifying sample changes significantly across the specifications. The reason is that we drop singleton observations to limit the sample to actual identifying observations based on the more rigorous sets of fixed effects. Therefore, we estimate models (A) to (C)

---

[4] As explained in Section 2 we drop from the sample workers who obtain a MA degree during the sample window (see also note to Table 2 for details). This restriction is only imposed for the selection of workers in the sample, and it does not clean the peer group variable, $\bar{E}_{ojt-i}$, in the same way.

on the most restrictive sample in column (D). The coefficients are similar as what we report in Table 2. We conclude that is the introduction of the fixed effects that is important for identification of magnitudes and not sampling. Results are available upon request.

It is difficult to compare results to the literature because of the vast differences in data and research designs. For the sake of illustration, Moretti (2004b) focuses on local labor market spillovers, employs instrumental variables and estimates that a 1 percentage point increase in college educated workers raises wages between 0.6 and 1.2 percent. Our estimates are smaller than his. Our most parsimonious model estimates an effect of 0.016 percent while our preferred model implies a spillover effect of 0.0042 percent. Imagine a worker in a peer group with 10 co-workers and switch one coworker to hold an advanced degree. At average annual wages of $ 48,352[5] this implies an additional $ 20. At the high end, the estimates suggest an additional $ 77. There are two potential explanations for these differences in magnitudes. First, we capture only spillovers at the workplace whereas Moretti captures spillover effects at a broader level. Second, we account for a rigorous set of unobserved heterogeneity using fixed effects.

Finally, age and experience have a positive effect on wages as we would expect in our parsimonious specifications. These effects become less clear in the more rigorous models. A potential xreason is that once we account for many fixed effects, age and experience are identified from correlated variation. Focusing on column (E), a worker with one year of work experience improves her wages by about 0.29 percent from an additional year of work experience. The difference to the peer effect is that while returns to experience are private, the social returns to college educated workers are spread across all co-workers.

---

[5] 2008 average annual wage in Denmark in $US, 2016 constant prices. Source: https://stats.oecd.org/Index.aspx?DataSetCode=AV_AN_WAGE.

In Table 3 we examine heterogeneity in peer effects for workers based on education level and different definitions of peer groups based on educational level and education specializations. We focus on model (E) from Table 2 and account for worker, occupation-year, occupation-workplace, and workplace-year heterogeneity. This considers the tradeoffs in employing identifying variation while being rigorous with unobserved heterogeneity.

[TABLE 3 HERE]

Column (1) of Table 3 examines the effect of our baseline definition of peer groups but excluding all observations of workers with higher levels of education, MA or above. The spillover effect is similar to the estimates in Table 2. This implies that spillover effects are experienced by a large part of the sample characterized by lower levels of education. This is relevant from a policy point of view, because it implies that workers with lower levels of education, and therefore a large part of the working population, benefit from cross subsidization of higher-level education. We also estimate this model on the sample of workers with higher level education. Coefficient magnitudes are similar, but not statistically different from zero.

For Table 3 column (2) we estimate over all workers, but change the peer group definition to measure the average number of workers with BA education or above. Similarly, in column (3) we define the peer group as the average number of workers with vocational training or above. Peer effects for both definitions are lower than for the baseline definition applied in Table 2. This suggests that it is especially higher level education that creates spillover effects. In column (4) we generate the peer group variable by three different education fields, STEM ("Science, Technology, Engineering and Math"), Humanities, and Social Sciences, grouped according to Kvalitetsudvalget (2015). We include all three in the specification. Results show that only Social Science education generates peer effects. STEM education also has a large coefficient but is statistically insignificant. The data do not identify significant peer effects for the humanities. The conclusion is that humanities do not deliver positive spillover effects in the private sector,

but we must be cautious with interpretation. The share of workers with higher level education in the humanities is small compared to STEM and Social Sciences, 0.005 versus 0.03 and 0.02. Therefore, it may be that workers with degrees in the humanities select to work in the public sector and potentially do generate spillover effects that we do not observe.

In Table 4 we differentiate each workers peer group by skill level. We distinguish a worker's peer group according to eight skill levels based on the skill level of the occupation according to the DISCO-classification.[6] Skill level 6 is excluded because we do not observe workers in this group. There are several interesting takeaways. Top-middle skill levels have the greatest spillover effects. However, taking into account standard errors, magnitudes are comparable to the pooled estimates in Table 2. Similar as Cornelissen et al. (2017) we also experiment with separating workers based on the routine intensity of their job. We could not identify meaningful systematic differences based on this distinction. The same holds for distinctions based on industry codes.

[TABLE 4 HERE]

Table 5 examines nonlinearity in peer group effects and workplace size. This is important for interpretation of peer effects and provides robustness with respect to peer group definitions. We generate three indicators for peer group size. I(PGSize<=10) equals one if the peer group contains less than 10 people and zero otherwise. Similarly we generate an indicator for intermediate peer group sizes I(PGSize11-100) and large peer groups I(PGSize>100). We then interact these indicators with our baseline definition of peer groups and include it in models (C) in Table 2 instead of APGE. This is akin to a spline regression that allow APGE to have different effects across peer-group size categories. Column (1) Table 5 shows that peer effects are similar for peer groups between 2 and 100 employees. But the effect is considerably larger for peer groups with more than 100 employees. Similar patterns emerge when we

_____

[6] This corresponds to the first digit in the DISCO occupational codes.

estimate a more rigorous model as in column (2), where we repeat the specification from column (1), but also include indicators to test for structural wage differences across peer group sizes. These indicators are not significant, likely because all the other fixed effects already combine to absorb many of such differences. As a consequence, adding these indicators only has small effects on the coefficient estimates, but the coefficient for the middle peer group size indicator interaction turns significant. To illustrate the effect of this non-linearity found in column (2), suppose you are in a peer group with 50 co-workers where 50 percent of them have higher education. Switching one of your coworkers to hold an advanced degree raises APGE by 2 percentage points. Based on column (2) this raises wages by 0.01 percent (0.005 X 0.02 X 100). On the other hand, suppose you are working in a group of 500. Then the effect of switching a peer's education equals about 0.003 percent (0.0158 X 0.002 X 100). These results suggest that small and large firms can take advantage of peer effects. While it seems intuitive that peer effects are smaller in larger groups, there is a tradeoff. Because the smaller effect in larger groups affects more workers, hiring an additional highly educated worker may have a greater effect on total productivity.

[TABLE 5 HERE]

For completeness we also experimented with non-linear specifications such as including squared peer group variables. However, squared APGE did not result in significant coefficient estimates. A challenge with such specifications is the often high collinearity between the linear and squared variable of interest.

Table 5 also examines non-linearity in workplace size in column (3), which similar as in columns (1) and (2) employs spline regressions. Column (3) is still akin to models C and E in Table 2 and accounts for worker, occupation-year, and occupation-workplace heterogeneity and workplace specific time effects. Column (3) suggests that spillover effects are greater in larger workplaces with more than 750 employees. A one percentage point increase in APGE raises average wages by 0.0125 percent while it

raises average wages at firms with less than 50 employees by only 0.0051 percent. This is possible, because a one percent increase in APGE at large workplaces implies many more highly educated workers than at small workplaces. On the other hand it is counterintuitive, as we would expect spillover effects to be greater in small firms where worker likely have to work more tightly together and collaborate.

4. Conclusions

In this paper we examine education spillover effects for Danish workers in the private sector. We take advantage of highly detailed matched worker-firm data to extend standard identification approaches to account for endogenous selection of workers with firms based on unobserved variables. Even in our most rigorous specifications we find evidence that workers benefit from coworkers with higher levels of education joining the team. From a policy point of view this implies that cross subsidizing education increases productivity for all workers. We focus on the private sector and find that especially higher levels of education, and, graduates in the sciences and social sciences deliver positive wage spillover effects.

As we introduce more demanding and rigorous empirical models, spillover effects decrease and are small compared to the existing literature. A possible reason is that accounting for high dimensional unobserved variables focuses identification on a narrow micro channel that transmits peer effects with limited levels of data variation. To reach policy conclusions magnitudes of policy effects are important. This suggests future research that evaluates the effect of education policy potentially taking advantage of quasi-random experiments.

This paper has its focus on narrow education spillovers from workers within occupations in workplaces. There may of course exist spillover effects from education across occupations within a workplace. This could especially be the case in smaller workplaces where interaction between different

occupations is very likely to happen. In the presence of such spillovers, our results under estimate the full spillover effect from educated workers.

In future versions of this paper we will estimate effects from a Danish education policy action, where small firms can qualify to receive large subsidies for wage payments when hiring highly educated workers.

References

Acemoglu Daron and Joshua Angrist, 2000, How Large are Human-Capital Externalities? Evidence from Compulsory Schooling Laws, NBER Macroeconomics Annual, 15: 9-59.

Andersen, Mikael B., 2016, Long-Run Peer Effects: Some Danish Evidence, Working Paper

Arcidiacono, Peter, Foster, Gigi, Goodpaster, Natalie, Kinsler, Josh, 2012. Estimating spillovers using panel data, with an application to the classroom. Quant. Econ. 3, 421–470.

Arcidiacono Peter, Josh Kinsler and Joseph Price, Productivity Spillovers in Team Production: Evidence from Professional Basketball, Journal of Labor Economics, 35(1) (January 2017), 191-225.

Azoulay Pierre, Jposhua S. Graff Zivin and Jialan Wang, 2010, Superstar Extinction, The Quarterly Journal of Economics, 125(2): 549-589.

Battisti Michele, 2017, High wager workers and high wage peers, Labour Economics, 46(2017), 47-63.

Bratti Massimiliano, Roberto Leombruni, 2014, Local human capital externalities and wages at the firm level: Evidence from Italian manufacturing, Economics of Education Review, Volume 41, 2014, Pages 161-175, ISSN 0272-7757, http://dx.doi.org/10.1016/j.econedurev.2014.05.002.

Backes Gellner Uschi, Christian Rupietta and Simone N. Tuor, 2015,Educational Spillovers at the Firm Level: Who Benefits from Whom?, Working Paper Nr. 65.

Ching-Fu Chang, Ping Wang, Jin-Tan Liu, Knowledge spillovers, human capital and productivity, Journal of Macroeconomics, Volume 47, 2016, Pages 214-232.

Cornelissen, Thomas, Christian Dustmann, and Uta Schönberg. 2017. "Peer Effects in the Workplace." American Economic Review, 107(2): 425-56.

Glaeser, E., Mare, D., 2001. Cities and skills. Journal of Labor Economics 19, 316–342.

Glaeser, E., Scheinkman, J., Schleifer, A., 1995. Economic growth in a cross-section of cities. Journal of Monetary Economics 36, 117–134.

Giuri Paola and Myriam Mariani, 2013, When Distance Disappears: Inventors, Education, And The Locus of Knowledge Spillovers, The Review of Economics and Statistics, 95(2): 449-463.

Heckman James and Pedro Carneiro, 2003, Human Capital Policy, NBER Working Paper 9495.

Heckman, J. J., J. E. Humphries og G. Veramendi 2017, The Non-Market Benefits of Education and Ability, NBER Working Paper No. 23896.

Harminder Battu, Clive R. Belfield and Peter J. Sloane, 2003, Human Capital Spillovers within the workplace: evidence for Great Britain, Oxford Bulletin of Economics and Statistics, 65(5):575-594

Horton John J. and Richard J. Zeckhauser, 2016, Evidence from a Series of Field Experiments, NBER Nr. 22386.

Kantor Shawn and Whalley Alexander, 2014, Knowledge Spillovers from Research Universities: Evidence from Endowment Value Shocks, The Review of Economics and Statistics, 96(1): 171-188.

Kato Takao and Pian Shu, 2009, Peer effects, Social Networks and Intergroup Competition in the Workplace, Working Paper 09-12

Kvalitetsudvalget, 2015, Nye Veje og Høje Mål, Udvalg for Kvalitet og Relevens i de Videregående Uddannelser, Danish Ministry of Higher Education and Science.

Lindquist Matthew J., Jan Sauermann and Yves Zenou, 2015, Network Effects on Worker Productivity, Working Paper.

Liu Zhiqiang, 2007,The external returns to education: Evidence from Chinese cities, Journal of Urban Economics, Volume 61, Issue 3, Pages 542-564

Liu Shimeng, 2015, Spillovers from universities: Evidence form the land-grant program, Journal of Urban Economics, 87:25-41.

Lucas, Richard E., 1988, On the mechanisms of economic development, Journal of Monetary Economics, 22, 3-42.

Marshall, A., 1890, Principles of Economics, MacMillan.

Manski, Charles F. 1993. "Identification of Endogenous Social Effects: The Reflection Problem." The Review of Economic Studies, 60(3): 531–542.

Mas Alexandra and Moretti Enrico, 2009, Peers at Work, American Economic Review, 99(1):112-145.

Martins Pedro S. and Jim Y. Jin, 2010,Firm-level social returns to education, Journal of Population Economics, 23(2): 539-558

Moretti, E., 2004a. Workers' education, spillovers, and productivity: Evidence from plant-level production functions. American Economic Review 94.

Moretti, E., 2004b. Estimating the return to higher education: Evidence from cross-section and longitudinal data. Journal of Econometrics 121.

Moretti, E., 2004c, Human Capital Externalities in Cities, Handbook of Regional and Urban Economics, edition 1, volume 4, chapter 51, 2243-2291.

Navon Guy, 2009, Human Capital Spillovers in the Workplace: Labor Diversity and Productivity, MPRA Paper No. 17741.

Niehaus, Paul, 2012, Education and Spillover Effects, Working Paper.

Rauch, J., 1993. Productivity gains and geographic concentration of human capital: Evidence from the cities. Journal of Urban Economics 34, 380–400.

Wirz Aniela Maria, 2008, Private Returns to education versus education spill-over effects or what coworkers account for, Empirical Economics, 34:315-342.

Tables

Table 1: Summary Statistics

Panel structure	
(i) Number of workers	1,603,373
(ii) Number of workplaces	74,660
(iii) Number of peer groups	1,088,386
(iv) Average number of time periods per worker	5.23
(v) Average number of workplaces per worker	1.76
(vi) Average number of peer groups per worker	1.99
(vii) Average number of peer groups per workplace-year	2.62
(viii) Average peer group size	8.01
(ix) Standard deviation peer group size	21.77
(x) Average peer group education intensity (%)	5.30
Variation in peer group education and worker turnover	
(xi) Standard deviation peer group education intensity	0.17
(xii) Standard deviation change of peer group education intensity from t - 1 to t	0.07
(xiii) Standard deviation change of peer group education intensity from t - 1 to t – Movers	0.14
(xiv) Standard deviation change of peer group education intensity from t - 1 to t – Stayers	0.05
(xv) Share of worker-year observations in peer groups with turnover	0.90
(xvi) Average share of workers replaced by turnovers	0.20

Note: The table presents descriptive statistics for the sample that is used as estimation sample in the baseline regressions. N = 8,227,426.

Table 2: Peer Group Effects on Log Wages

	(A)	(B)	(C)	(D)	(E)
$\overline{E}_{ojt-i}$	0.0161***	0.0098***	0.0049***	0.0028**	0.0042***
	(0.0011)	(0.0013)	(0.0012)	(0.0013)	(0.0012)
Age	0.0447***	0.0417***	0.0416***	0.0358***	0.0416***
	(0.0005)	(0.0005)	(0.0005)	(0.0006)	(0.0005)
Age^2	-0.0006***	-0.0005***	-0.0005***	-0.0004***	-0.0005***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Experience	0.0312***	0.0295***	0.0291***	0.0034***	0.0291***
	(0.0003)	(0.0003)	(0.0003)	(0.0007)	(0.0003)
$Experience^2$	-0.0004***	-0.0004***	-0.0004***	-0.0003***	-0.0004***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Worker FE	√	√	√	—	√
Occupation-Year FE	√	√	√	√	√
Workplace FE	√	—	—	—	—
Occupation-Workplace FE	—	√	√	√	√
Workplace-Year FE	—	—	√	√	√
Worker-Workplace FE	—	—	—	√	—
R^2	0.835	0.847	0.855	0.901	0.855
N	8,034,749	8,010,477	7,995,383	7,112,673	7,995,383

Notes: Dependent variable is *log* (hourly wage). Average peer group education for individual *i* is calculated as the share of peer group members with education at minimum a MA-level (excluding worker *i*). The sample holds workers in fulltime employment in all private sector firms except those in agriculture or mining, years 1995-2008. Observations with wages above or below the top and bottom 1 %- percentiles in the full distribution are dropped. Occupations that are not found in Statistics Denmark's documentation and occupations in public administration and agriculture are not included. In columns (A)-(C) and (E), workers who obtain a MA-level education while in the sample are dropped, and in column (D) workers who obtain MA-education within a workplace spell are dropped. The difference between models (C) and (E) is that in the calculation of APGE in model (E), we set the education level of each co-worker to the education level first observed when this co-worker enters a peer group. N varies across models as more singleton-observations are dropped when more fixed effects are included. Robust standard errors clustered at workplace-year level are reported in parenthesis. *: p<0.1, **: p<0.05, ***: p<0.01

Source: Own calculations based on administrative data from Statistics Denmark.

Table 3 - Subgroups and Alternative Education Variables

Sample or APGE definition	(1)	(2)	(3)	(4)
Workers without MA	0.0045***			
	(0.0015)			
APGE: BA or above		0.0027**		
		(0.0012)		
APGE: Vocational or above			0.0014**	
			(0.0006)	
STEM				0.0024
				(0.0018)
Humanities				0.0005
				(0.0030)
Social Sciences				0.0068***
				(0.0019)
R^2	0.843	0.855	0.877	0.855
N	7,552,379	8,003,532	7,368,424	8,007,183

Notes: Dependent variable is *log* (hourly wage). All models are specified as in column (E) of table 2, and include i, ot, oj, and jt fixed effects. In column (1), the sample only includes workers with less than MA-level education. Columns (2)-(3) are estimated on the full sample, but use Bachelor's or Vocational education, respectively, to calculate APGE. In column (4) we again use MA-level education, and include three dummies for APGE in the model based on field of study. Controls for age and experience (both squared) are also included, but not reported. Robust standard errors clustered at workplace-year level are reported in parenthesis. *: $p<0.1$, **: $p<0.05$, ***: $p<0.01$

Source: Own calculations based on administrative data from Statistics Denmark.

Table 4 - Peer Groups by Skill Levels

APGE x I(Skill level 1)	0,0031
	(0.0033)
APGE x I(Skill level 2)	0,0019
	(0.0021)
APGE x I(Skill level 3)	0.0061***
	(0.0020)
APGE x I(Skill level 4)	0.0082*
	(0.0042)
APGE x I(Skill level 5)	0.0135
	(0.0155)
APGE x I(Skill level 7)	-0.0023
	(0.0140)
APGE x I(Skill level 8)	0.012
	(0.0153)
APGE x I(Skill level 9)	-0.0091
	(0.0104)
R^2	0.855
N	7,995,383

Notes: Dependent variable is *log* (hourly wage). Results are based on the same sample as column (E) of table 2, and include i, ot, oj, and jt fixed effects. Skill levels are based on 1-digit DISCO-classifications, descending in skill levels from 1 to 9. Controls for age and experience (both squared) are also included, but not reported. Robust standard errors clustered at workplace-year level are reported in parenthesis. *: $p<0.1$, **: $p<0.05$, ***: $p<0.01$.

Source: Own calculations based on administrative data from Statistics Denmark.

Table 5 - Nonlinearity in Peer Group and Workplace Size

	(1)	(2)	(3)
APGE x I(PGsize<=10)	0.0041***	0.0039***	
	(0.0013)	(0.0013)	
APGE x I(PGsize, 11-100)	0.0038	0.0048**	
	(0.0023)	(0.0024)	
APGE x I(PGsize > 100)	0.0167***	0.0158***	
	(0.0042)	(0.0047)	
I(PGsize<=10)		-0.0002	
		(0.0012)	
I(PGsize, 11-100)		-0.0009	
		(0.0011)	
APGE x I(Workplace size<=50)			0.0051**
			(0.0021)
APGE x I(Workplace size, 51-750)			0.0024*
			(0.0014)
APGE x I(Workplace size > 750)			0.0125***
			(0.0036)
R^2	0.855	0.855	0.855
N	7,995,383	7,995,383	7,995,383

Notes: Dependent variable is *log* (hourly wage). Results are based on the same sample as column (E) of table 2, and include i, ot, oj, and jt fixed effects. Controls for age and experience (both squared) are also included, but not reported. Robust standard errors clustered at workplace-year level are reported in parenthesis. *: $p<0.1$, **: $p<0.05$, ***: $p<0.01$

Source: Own calculations based on administrative data from Statistics Denmark.