PEER EFFECTS IN THE WORKPLACE*

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This Version: March 2016

Abstract

Existing evidence on peer effects in a work environment stems from either laboratory experiments or real-word studies referring to a specific firm or occupation. In this paper we aim at providing more generalizable results by investigating a large local labor market, with a focus on peer effects in wages rather than productivity. Our estimation strategy—which links the average permanent productivity of workers' peers to their wages—circumvents the reflection problem and accounts for endogenous sorting of workers into peer groups and firms. On average over all occupations, and in the type of high skilled occupations investigated in studies on knowledge spillover, we find only small peer effects in wages. In the type of low skilled occupations analyzed in extant studies on social pressure, in contrast, we find larger peer effects, about half the size of those identified in similar studies on productivity. (JEL J24, J31)

Keywords: Peer effects, productivity, wage determination

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We are grateful to Josh Kinsler for comments and for sharing program code.

Uta Schönberg acknowledges funding by the European Research Council (ERC) Starter grant SPILL. Christian Dustmann acknowledges funding by an ERC Advanced Grant.

The communication and social interaction between coworkers that necessarily occur in the workplace facilitate comparison of individual versus coworker productivity. In this context, workers whose productivity falls behind that of coworkers, or falls short of a social norm, may experience personal feelings of guilt or shame. They may then act on these feelings by increasing their own efforts, a mechanism referred to in the economic literature as "peer pressure." Social interaction in the workplace may also lead to "knowledge spillover" in which coworkers learn from each other and build up skills that they otherwise would not have. Such productivity enhancing peer effects may exacerbate initial productivity differences between workers and increase long-term inequality when high quality workers cluster together in the same peer groups. Moreover, while knowledge spillover is an important source of agglomeration economies (e.g., Lucas, 1988; Marshall, 1890), social pressure further implies that workers respond not only to monetary but also to social incentives, which may alleviate the potential free-rider problem inherent whenever workers work together in a team (Kandel and Lazear, 1992).

Yet despite the economic importance of peer effects, empirical evidence on such effects in the *workplace* is as yet restricted to a handful of studies referring to very specific settings, based on either laboratory experiments or on real-world data from a single firm or occupation. For instance, Mas and Moretti's (2009) study of one large supermarket chain provides persuasive evidence that workers' productivity increases when they work alongside more productive coworkers, a finding that they attribute to increased social pressure. Likewise, a controlled laboratory experiment by Falk and Ichino (2006) reveals that students recruited to stuff letters into envelopes work faster when they share a room than when they sit alone.[1] For peer effects in

[1] Other papers focusing on social pressure include Kaur, Kremer, and Mullainathan (2010), who report productivity spillovers among data-entry workers seated next to each other in an Indian company. Similarly, Bandiera, Barankay, and Rasul (2010) find that soft-fruit pickers in one large U.K. farm are more productive if at least one of their more able friends is present on the same field but less productive if they are the most able among their friends. Peer pressure is also a likely channel in Chan et al.'s (2014) finding of peer effects in productivity among salespersons of a department store, and in the general pattern of network effects in the productivity of call-

the workplace induced by knowledge spillover, however, the evidence is mixed. Whereas Waldinger (2012) finds little evidence for knowledge spillover among scientists in the same department in a university, Azoulay, Graff Zivin, and Wang (2010) and Jackson and Bruegemann (2009) find support for learning from coworkers among medical science researchers and teachers, respectively.[2] In a comprehensive meta-analysis of peer effects in coworker productivity covering both high and low-skilled tasks, Herbst and Mas (2015) report a remarkable similarity between the cross-study average peer effect from laboratory experiments and field studies. Nonetheless, although existing studies provide compelling and clean evidence for the existence (or absence) of peer effects in specific settings, it is unclear to what extent their findings, which are all based on laboratory experiments or real-word studies referring to a specific firm or occupation, apply to the labor market in general.

In this paper, therefore, we go beyond the existing literature to investigate peer effects in the workplace for a *representative* set of workers, firms, and sectors. Our unique data set, which encompasses all workers and firms in one large local labor market over nearly two decades, allows us to compare the magnitude of peer effects across detailed sectors. It thus provides a rare opportunity to investigate whether the peer effects uncovered in the literature are confined to the specific firms or sectors studied or whether they carry over to the general labor market, thus shedding light on the external validity of the existing studies. At the same time, our comparison

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center workers found by Lindquist et al. (2015). In work that analyzes regional shirking differentials in a large Italian bank, Ichino and Maggi (2000) find that average peer absenteeism has an effect on individual absenteeism. A controlled field experiment by Babcock et al. (2011) suggests that agent awareness of their own efforts' effect on *known* peer payoffs creates incentives possibly mediated by a form of social pressure.

[2] In related work, Waldinger (2010) shows that faculty quality positively affects doctoral student outcomes, while Serafinelli (2013) provides evidence that worker mobility from high- to low-wage firms increases the productivity of low-wage firms, which is consistent with knowledge spillover. The findings by Lindquist et al. (2015) and De Grip and Sauermann (2012) suggest knowledge transfer to be a relevant source of productivity spillover when trained and untrained workers interact. Other studies (e.g., Guryan, Kroft, and Notowidigdo, 2009; Gould and Winter, 2009) analyze such knowledge spillover between team mates in sports.

of the magnitude of peer effects across sectors provides new evidence on what drives these effects, whether social pressure or knowledge spillover.

In addition, unlike the extant studies, our analysis focuses on peer effects in *wages* rather than *productivity*, thereby addressing for the first time whether or not workers are rewarded for a peer-induced productivity increase through wages. To do so, we first develop a simple theoretical framework in which peer-induced productivity effects arise because of both social pressure and knowledge spillover and translate into peer-related wage effects even when the firm extracts the entire surplus of the match. The rationale underlying this result is that, if a firm wants to ensure that workers remain with the company and exert profit-maximizing effort, it must compensate them for the extra disutility from exercising additional effort because of knowledge spillover or peer pressure.

In the subsequent empirical analysis, we then estimate the effect of the long-term or predetermined quality of a worker's current peers—measured by the average wage fixed effect of coworkers in the same occupation and workplace (which we will refer to as "firms" for brevity)—on the current wage, a formulation that directly corresponds to our theoretical model. We implement this approach using an algorithm developed by Arcidiacono, Foster, Goodpaster and Kinsler (2012), which allows simultaneous estimation of both individual and peer group fixed effects. Because we link a worker's wage to predetermined characteristics (i.e., the peers' average worker fixed effect) rather than to peer group wages or effort, we avoid Manski's (1993) reflection problem.

To deal with worker sorting (i.e., the fact that high quality workers may sort into high quality peer groups or firms), we condition on an extensive set of fixed effects similar to Card, Heining and Kline (2013). First, by including worker fixed effects in our baseline specification, we account for the potential sorting of high ability workers into high ability peer groups. In

addition, to account for the potential sorting of high ability workers into firms, occupations, or firm-occupation combinations that pay high wages, we include firm-by-occupation fixed effects. To address the possibility that firms may attract better workers and raise wages at the same time, we further include time-variant firm fixed effects (as well as time-variant occupation fixed effects). As argued in Section 2.1, this identification strategy is far tighter than most strategies used to estimate peer effects in other settings.

On average, we find only small, albeit precisely estimated, peer effects in wages. Even if peer effects are small *on average* for a representative set of occupations, they might still be substantial for specific occupations. In fact, the specific occupations and tasks analyzed in the existent studies on peer pressure (i.e., supermarket cashiers, data entry workers, envelope stuffers, fruit pickers) are occupations in which there is more opportunity for coworkers to observe each other's output, a prerequisite for peer pressure build-up. Similarly, the specific occupations and tasks analyzed in the studies on knowledge spillover (i.e., scientists, teachers) are high skilled and knowledge intensive, making learning from coworkers particularly important.

In a second analytical step, therefore, we restrict our analysis to occupations similar to those studied in that literature. Nevertheless, in line with Waldinger (2012), in occupations for which we expect knowledge spillover to be important (i.e., occupations that are particularly innovative and demand high skills), we again find only small peer effects in wages. On the other hand, in occupations where peer pressure tends to be more important (i.e., where the simple repetitive nature of the tasks makes output more easily observable to coworkers), we find larger peer effects. In these occupations, a 10% increase in peer ability increases wages by 0.6-0.9%, which is about half the size of the peer effects in coworker productivity identified by Mas and Moretti (2009) and Falk and Ichino (2006) and in the meta-analysis by Herbst and Mas (2015). Not only

are these findings remarkably robust to a battery of robustness checks, but we provide several types of additional evidence for social pressure being their primary source.

Our results are important for several reasons. First, our finding of only small peer effects in wages *on average* suggests that the larger peer effects established in specific settings in existing studies may not carry over to the labor market in general. Overall, therefore, our results suggest that peer effects do not contribute much to overall inequality in the economy.[3] Second, even though our results suggest that the findings of earlier studies cannot be extended to the entire labor market, they do indicate that they can be generalized beyond the single firm or single occupation on which they are based. That is, our findings highlight larger peer effects in low skilled occupations in which co-workers can, because of the repetitive nature of the tasks performed, easily judge each other's output—which are exactly the type of occupations most often analyzed in earlier studies on peer pressure. Our findings also add to the existing studies by showing that in such situations, peer effects lead not only to productivity spillover but also to wage spillover, as yet an unexplored topic in the literature.

The remainder of the paper is structured as follows. The next section outlines a theoretical framework that links peer effects in productivity engendered by social pressure and knowledge spillover to peer effects in wages. It also clarifies the interpretation of the peer effect identified in the empirical analysis. Sections 2 and 3 then describe our identification strategy and our data, respectively. Section 4 reports our results, and Section 5 summarizes our findings.

1. Theoretical Framework

To motivate our empirical analysis, we develop a simple principal-agent model of unobserved worker effort in which peer effects in productivity translate into peer effects in

[3] Our general result of no strong peer effects within firms is in line with a recent paper by Bloom et al. (2013), who show that workers who work from home are somewhat more productive than those who come in to work.

wages, and model two channels: knowledge spillover and social pressure. Here, we focus on the basics of the model, and delegate details to Appendix A.

1.1 Basic Model

Production Function and Knowledge Spillover

Consider a firm that employs N workers. In the theoretical analysis, we abstract from the endogenous sorting of workers into firms, which our empirical analysis takes into account. We next suppose that worker i produces individual output f_i according to the following production function:

$$f_i = y_i + \varepsilon_i = a_i + e_i(1 + \lambda^K \bar{a}_{\sim i}) + \varepsilon_i,$$

where y_i is the systematic component of worker i's productive capacity, depending on individual ability a_i, individual effort e_i and average peer ability (excluding worker i) $\bar{a}_{\sim i}$. In this production function, individual effort and peer ability are complements, meaning that workers benefit from better peers only if they themselves expend effort. In other words, the return to effort is increasing in peer ability, and the greater this increase, the more important the knowledge spillover captured by the parameter λ^K.[4] The component ε_i is a random variable reflecting output variation that is beyond the workers' control and has an expected mean of zero. Firm productivity simply equals the sum of worker outputs. Whereas a worker's ability is exogenously given and observed by all parties, effort is an endogenous choice variable. As is

[4] It should be noted that, just as in extant studies, this formulation abstracts from the dynamic implications of knowledge spillover, and is best interpreted as one of *contemporaneous* knowledge spillover through assistance and cooperation between workers on the job. The underlying rationale is that workers with better peers are more productive on the job because they receive more helpful advice from their coworkers than if they were in a low-quality peer group. Moreover, although a high quality coworker may still boost a worker's productivity even when the two are no longer working together, one would still expect current peers to be more important than past peers. In addition, this specification assumes that own effort and time is required to 'unlock' the potential of one's peers' ability. This assumption of complementarity between knowledge spillover and effort provision is one of the drivers of why knowledge spillover translates into wages in our model: workers exposed to better peers exert higher effort, for which they have to be compensated in terms of higher wages.

standard in the principal agent literature, we assume that the firm cannot separately observe either worker effort e_i or random productivity shocks ε_i.

Cost of Effort and Social Pressure

Because exerting effort is costly to the worker, we assume that in the absence of peer pressure, the cost of effort function is quadratic in effort: $C(e_i) = ke_i^2$. As in Barron and Gjerde (1997), Kandel and Lazear (1992), and Mas and Moretti (2009), we introduce peer pressure by augmenting the individual cost of effort function $C(.)$ with a social "peer pressure" function P(.) that depends on individual effort e_i and average peer output $\bar{f}_{\sim i}$ (excluding worker *i*). We propose a particularly simple functional form for the peer pressure function: $P\left(e_i, \bar{f}_{\sim i}\right) = \lambda^P (m - e_i)\bar{f}_{\sim i}$, where λ^P and m can be thought of as both the "strength" and the "pain" from peer pressure.[5] The total disutility associated with effort thus becomes

$$c_i = C(e_i) + P\left(e_i, \bar{f}_{\sim i}\right) = ke_i^2 + \lambda^P (m - e_i)\bar{f}_{\sim i} .$$

Although the exact expressions derived in this section depend on the specific functional form for the total disutility associated with effort, our general argument does not.

In the peer pressure function, the marginal cost of exerting effort is negative (i.e., $\frac{\partial P(e_i, \bar{f}_{\sim i})}{\partial e_i} = -\lambda^P \bar{f}_{\sim i} < 0$). Thus, workers exert higher effort in the presence of peer pressure than in its absence. The peer pressure function also implies that the marginal cost of worker effort is declining in peer output (i.e., $\frac{\partial^2 P(e_i, \bar{f}_{\sim i})}{\partial e_i \partial \bar{f}_{\sim i}} = -\lambda^P < 0$). In other words, peer quality reduces the marginal cost of effort, and the stronger the peer pressure (captured by λ^P), the larger the reduction. This condition implies that it is less costly to exert an additional unit of effort when the quality of one's peers is high than when it is low. Hence, although peer pressure is often

[5] We assume that $k > \lambda^P$, which not only ensures that the Nash equilibrium is unique (requiring only $2k > \lambda^P$) but also that the firm's maximization problem has an interior solution (see Appendix A.3).

defined by the first condition $\frac{\partial P(e_i,\bar{f}_{\sim i})}{\partial e_i} < 0$ (e.g., Kandel and Lazear, 1992; Mas and Moretti, 2009), it is in fact the second condition $\frac{\partial^2 P(e_i,,\bar{f}_{\sim i})}{\partial e_i \partial \bar{f}_{\sim i}} < 0$ that generates productivity spillover.[6] We further assume that, like Barron and Gjerde (1997), workers dislike working in a high-pressure environment—which amounts to imposing a lower bound on the parameter m (capturing the "pain from peers") in the peer pressure function P(.) (see Appendix A.1 for details).

Wage Contracts and Worker Preferences

Firms choose a wage contract that provides their workers with the proper incentives to exert effort. Because the firm cannot disentangle e_i and ε_i, however, it cannot contract a worker's effort directly but must instead contract output f_i. As is typical in the related literature, we restrict the analysis to linear wage contracts.[7] Contrary to the standard principal agent model, we assume that not only firms but also workers are risk neutral, an assumption that simplifies our analysis without being a necessary condition for our general argument.

1.2 The Worker's Maximization Problem

Because of risk neutrality, workers maximize their expected wage minus the combined cost of effort. As shown in Appendix A.2, this leads to the first order condition

$$e_i = \frac{\lambda^P}{2k}\bar{e}_{\sim i} + \frac{b}{2k} + \frac{\lambda^P + b\lambda^K}{2k}\bar{a}_{\sim i} \qquad for \quad i = 1, \dots, N, \tag{1}$$

where b denotes the slope of the wage contract with respect to worker output. This first order condition not only highlights that equilibrium effort is increasing in peer ability (see last term), either because of peer pressure λ^P or knowledge spillover λ^K, but also that peer pressure ($\lambda^P >$

[6] It should further be noted that, for simplicity, we abstract from peer actions like sanctions, monitoring, or punishment, meaning that in our model, peer pressure arises solely through social comparison or "guilt" (Kandel and Lazear, 1992) rather than through sanction, punishment, or "shame." The experimental evidence from Falk and Ichino (2002) indicates that peer pressure can indeed build up from social comparison alone.

[7] Holmstrom and Milgrom (1987) show that a linear contract is optimal over a range of different environmental specifications.

0) leads to a social multiplier effect whereby the more effort exerted by peers, the more effort exerted by the worker (e_i is increasing in $\bar{e}_{\sim i}$). In sum, both peer pressure and knowledge spillover lead to spillover effects in productivity, a dynamic that incorporates the social multiplier effect arising from peer pressure.

1.3 The Firm's Optimization Problem

Firms choose the intercept and slope (or incentive) parameter of the wage contract by maximizing expected profits, $EP = \sum_i E[f_i - w_i]$, taking into account the workers' optimal effort levels e_i^*, subject to the participation constraint that workers receive a utility that is at least as high as the outside option $v(a_i)$: $EU_i \geq v(a_i)$. As is standard in the principal agent literature, we assume that the participation constraint holds with equality, implying that the firm has all the bargaining power and thus pushes each worker to the reservation utility $v(a_i)$. As we show in Appendix A.3, this implies that

$$Ew_i = v(a_i) + C(e_i^*) + P(e_i^*, \bar{y}_{\sim i}), \tag{2}$$

meaning that the firm ultimately rewards the worker for the outside option $v(a_i)$, the cost of effort $C(e_i^*)$, and the disutility from peer pressure $P(e_i^*, \bar{y}_{\sim i})$. We can then derive the firm's first order condition and an expression for the slope b^* of the optimal wage contract as detailed in Appendix A.3. In the absence of peer pressure (i.e., $\lambda^P = 0$), we obtain the standard result of an optimal incentive parameter for risk neutral workers that is equal to 1.[8]

[8] Interestingly, in the presence of peer pressure, b^* is smaller than 1.This outcome results from an externality: the failure of individual workers to internalize in their effort choices the fact that peer pressure causes their peers additional "pain" for which the firm must compensate. The firm mitigates this externality by setting $b^* < 1$. Hence, as also noted in Barron and Gjerde (1997), peer pressure constitutes a further reason for the firm to reduce incentives in addition to the well-known trade-off between risk and insurance.

1.4 The Effect of Peer Quality on Wages

Next, we show that even though the firm is able to push the worker's wage to her reservation utility, expected wages are increasing in peer ability. The rationale underlying this result is that, if a firm wants to ensure that workers remain with the company and exert profit-maximizing effort, it must compensate them for the extra disutility from exerting additional effort (see equation (2))—and effort is increasing in peer ability (see equation (1)).

The average effect of peer ability on wages, obtained by differentiating equation (2) and taking averages, is:

$$\frac{1}{N}\sum_i \frac{dEw_i}{d\bar{a}_{\sim i}} = \underbrace{\frac{1}{N}\sum_i b^* \frac{\partial f_i}{\partial e_i}\frac{de_i^*}{d\bar{a}_{\sim i}}}_{\substack{\text{Wage response to}\\ \text{own effort increase}}} + \underbrace{\frac{1}{N}\sum_i \frac{\partial P(e_i,\bar{y}_{\sim i})}{\partial \bar{y}_{\sim i}}\frac{d\bar{y}_{\sim i}}{d\bar{a}_{\sim i}}}_{\substack{\text{Wage response to disutility}\\ \text{from social pressure}}} \qquad (3)$$

where all terms are evaluated at optimal effort levels and at the optimal b. The expression in (3) corresponds to the parameter of interest that we estimate in our empirical analysis. The first term captures the wage response to the increase in workers' own effort and consists of three parts which are all positive: the slope of the wage contract, b^*, the marginal effect of effort on productivity, $\frac{\partial f_i}{\partial e_i}$, and the effect of peer ability on equilibrium effort, $\frac{de_i^*}{d\bar{a}_{\sim i}}$.[9] The second term is likewise positive and disappears when there is no peer pressure. It captures that higher peer ability is associated with higher peer output ($\frac{d\bar{y}_{\sim i}}{d\bar{a}_{\sim i}} > 0$), which causes additional "pain" from peer pressure ($\frac{\partial P(e_i,\bar{y}_{\sim i})}{\partial \bar{y}_{\sim i}} > 0$) for which the worker has to be compensated. Our model thus predicts that the average effect of peer ability on wages will be unambiguously positive.

[9] Since the terms in equation (3) are evaluated at the optimal effort choice and at the optimal b, the effect in equation (3) contains both direct and indirect (social multiplier) effects of $\bar{a}_{\sim i}$ on the wage. For example, $\frac{de_i^*}{d\bar{a}_{\sim i}}$ not only contains the direct effect of $\bar{a}_{\sim i}$ on e_i, i.e., $\frac{\lambda^P + b\lambda^K}{2k}$ in equation (1), but also additional multiplier effects stemming from the fact that own effort and peer effort reinforce each other.

2. Empirical Implementation

In our empirical analysis, we seek to estimate the average effect of peer ability on wages as derived in equation (3). While in the model above we abstract from worker sorting and assume that peer ability $\bar{a}_{\sim i}$ is observable, our empirical analysis needs to take account of non-random allocation of workers to firms and unobserved background characteristics, and we have to estimate $\bar{a}_{\sim i}$. We now describe our estimation strategy for obtaining causal estimates of peer quality on wages that correspond to expression (3). We define a worker's peer group as all workers working in the same (3-digit) occupation and in the same firm in period *t* (see Section 3.2 for a detailed discussion of the peer group definition).

2.1 Baseline Specification and Identification

First, we estimate the following baseline wage equation:[10]

$$\ln w_{iojt} = a_i + \gamma \bar{a}_{\sim i,ojt} + x'_{iojt}\beta + \omega_{ot} + \delta_{jt} + \theta_{oj} + v_{iojt} \qquad (4)$$

Here $\ln w_{iojt}$ is the individual log real wage, *i* indexes workers, *o* indexes occupations, *j* indexes workplaces or production sites (which we label "firms" for simplicity), *t* indexes time periods, and a_i is a worker fixed effect. The term $\bar{a}_{\sim i,ojt}$ corresponds to $\bar{a}_{\sim i}$ in the theoretical model, and is the average worker fixed effect in the peer group, computed by excluding individual *i*. The coefficient γ is the parameter of interest and measures (a positive monotonous transformation of) the spillover effect in wages ($\frac{1}{N}\sum_i \frac{dEw_i}{d\bar{a}_{\sim i}}$ in equation (3)) which embodies not only the direct effect of peer ability on wages, holding peer effort constant, but also the social multiplier effect arising from workers' effort reactions in response to increases in the current effort of their peers.[11]

[10] This equation extends the worker and firm fixed effects model pioneered by Abowd et al. (1999) and estimated in for instance Card et al. (2013) by making the firm fixed effect vary for each time period, by including additional high-dimensional fixed effects (occupation-by-time, firm-by-occupation), and by including the average co-worker fixed effect.

[11] It should be noted that the theoretical wage equation in (3) refers to wage levels, whereas the empirical wage equation in (4) is estimated in logs. Since the logarithm is a positive monotonic transformation, the key prediction of

Identifying this reduced-form or total effect of peers' long-term productivity on wages requires conditioning on $\bar{a}_{\sim i,ojt}$ as a measure of the peers' long-term productivity (a predetermined characteristic), but *not* conditioning on contemporaneous peer effort or productivity (or as a proxy thereof, peers' current wages).[12]

Nonetheless, identifying the causal peer effect γ is challenging because of confounding factors such as shared background characteristics. Here, we first discuss the conditions required for a causal interpretation of the peer effect γ assuming that a_i and $\bar{a}_{\sim i,ojt}$ in equation (4) are observed. Then, in Section 2.3, we outline the issues arising from the fact that a_i and $\bar{a}_{\sim i,ojt}$ are unobserved and must be estimated. While we are confident that our estimation strategy results in unbiased estimates of the effect of peer quality on wages, we will argue that any possible remaining bias is likely to be upward, so that our estimates are lower bounds of peer effects.

Peer quality may affect a worker's wage simply because high quality workers sort into high quality peer groups or high quality firms, leading to a spurious correlation between peer quality and wages. Our estimation strategy accounts for the endogenous sorting of workers into peer groups or firms by including control variables and multiple fixed effects. First, because our baseline specification in equation (4) includes worker fixed effects, it accounts for the potential sorting of high ability workers into high ability peer groups. Second, we include a vector of time-variant individual characteristics x_{iojt} that includes quadratics in age and firm tenure to control for the fact that older workers may be in better peer groups, because they have had more time to search for a better job, and that once workers are in better peer groups, they may accumulate higher job tenure because they have less incentive to leave. Thus, the worker fixed effects are net of age and job tenure, and the average peer fixed effect does not capture effects of peer age or

our model carries over to log wages: In the presence of knowledge spillover or peer pressure, both wage levels and log wages are increasing in peer ability (i.e., $\gamma > 0$).

[12] Therefore, there is no reflection problem in estimating the peer effect γ in equation (4) (Manski, 1993).

peer job tenure. Third, time-variant occupation effects ω_{ot} are included to capture diverging time trends in occupational pay differentials. Moreover, our inclusion of time-variant firm fixed effects δ_{jt} controls for shocks that are firm specific. For example, when bad management decisions result in loss of market share and revenue, wages in that firm may increase at a slower rate than in other firms, motivating the best workers to leave. Therefore, failing to control for time-variant firm fixed effects could induce a spurious correlation between individual wages and peer ability. Finally, by controlling for firm specific occupation effects θ_{oj}, we allow for the possibility that a firm may pay specific occupations relatively well (or badly) compared to the market. For instance, firm A might be known for paying a wage premium to sales personnel but not IT personnel, while firm B is known for the opposite. As a result, firm A may attract particularly productive sales personnel, while firm B may attract particularly productive IT personnel. Hence, once again, failing to control for firm-specific occupation effects could induce a spurious association between individual wages and peer quality.

Estimation of γ in equation (4) exploits two sources of variation: changes in peer quality for workers who *switch* peer groups (after having controlled for the accompanying changes in firm and occupation specific fixed effects), and changes in peer quality for workers who *remain* with their peer group, induced by *other* workers joining or leaving the peer group. Focusing on the latter source of variation, our identification strategy can essentially be understood as a difference-in-difference estimator. To see this, denote by $\tilde{a}_{\mathrm{i}ojt}$ the peer group quality purged from effects of observables (x_{iojt}) and occupation-specific shocks common to all firms in the economy (ω_{ot}).[13] Suppose further for simplicity that all firms consist of two occupations only, denoted by o and o'. *First differencing* of equation (4) for peer group stayers now eliminates the time-constant worker and firm-occupation fixed effects a_i and θ_{oj}—and more generally any

[13] That is, $\tilde{a}_{\mathrm{i}ojt}$ is the residual from the regression of $\bar{a}_{\sim i,ojt}$ on $x_{iojt}$and ω_{ot}.

time-constant effects such as a match-specific effects m_{ioj}—but does not remove the firm-specific shock common to all occupations in the firm ($\Delta\delta_{jt}$): $\Delta lnw_{iojt} = \gamma\Delta\tilde{a}_{iojt} + \Delta\delta_{jt} + \Delta v_{iojt}$. This effect can be eliminated through differencing a second time, between occupations o and o' in the same firm that experienced different changes in peer quality:

$$\underbrace{\underbrace{\Delta\overline{lnw}^{s}_{ojt}}_{\text{first difference}} - \Delta\overline{lnw}^{s}_{o'jt}}_{\text{second difference}} = \gamma\left(\Delta\bar{\tilde{a}}^{s}_{ojt} - \Delta\bar{\tilde{a}}^{s}_{o'jt}\right) + \left(\Delta\bar{v}^{s}_{ojt} - \Delta\bar{v}^{s}_{o'jt}\right). \qquad (5)$$

This firm-level regression consistently estimates γ provided that $\mathrm{Cov}\left(\Delta\bar{\tilde{a}}^{s}_{ojt} - \Delta\bar{\tilde{a}}^{s}_{o'jt}, \Delta\bar{v}^{s}_{ojt} - \Delta\bar{v}^{s}_{o'jt}\right) = 0$. This condition says that peer group stayers in both occupations in the firm experience the same time shock, which corresponds to the standard common time trend assumption in difference-in-difference estimation.

Our identification assumptions are considerably weaker than the assumptions typically invoked in the education literature, which seek to identify exogenous spillover effects (e.g., the impact of the share of girls, Blacks, immigrants, or grade repeaters on individual performance). The most common approach in these studies—measuring grade-level peer characteristics and exploiting within-school variation over time (e.g., Gould, Lavy, and Paserman, 2009; Hanushek et al., 2003; Hoxby, 2000; Lavy and Schlosser, 2011; Lavy, Paserman, and Schlosser, 2012)—does not allow for the possibility that the average quality of students (in our case, workers) in the school (firm) changes over time, or that the school's effect on student performance (wages) may vary over time. Other research employs an alternative approach: measuring peer characteristics at the classroom level and exploiting within-school grade-year variation (e.g., Ammermueller and Pischke, 2009; Betts and Zau, 2004; McEwan, 2003; Vigdor and Nechyba, 2007). This technique, however, requires random assignment of students into classrooms within the school (equivalent to occupations within a firm), thereby ruling out within-school student tracking. Our

analysis, in contrast, can account for nonrandom selection into occupations within firms by including firm-specific occupation effects.

2.2 Within-Peer Group Estimator

One remaining concern may be the possible presence of time-variant peer group-specific wage shocks that are correlated with shocks to peer group quality, violating the common time trend assumption highlighted above. The existence of such shocks is likely to lead to an *upward* bias. For example, a firm may adopt a new technology specific to one occupation only, simultaneously raising wages and worker quality in that occupation relative to other occupations in the firm, implying that $\mathrm{Cov}\left(\Delta\bar{\tilde{a}}^{\mathrm{s}}_{ojt} - \Delta\bar{\tilde{a}}^{\mathrm{s}}_{o'jt}, \Delta\bar{v}^{\mathrm{s}}_{ojt} - \Delta\bar{v}^{\mathrm{s}}_{o'jt}\right) > 0$.

One way to deal with this problem is to condition on the full set of time-variant peer group fixed effects p_{ojt}. Note that the parameter γ remains identified because focal worker i is excluded from the average peer group quality. As a result, the average peer group quality of the same group of workers differs for each worker, and $\bar{a}_{\sim i,ojt}$ differs for each worker within a peer group at any given point in time. Using only within-peer group variation for identification yields the following estimation equation:[14]

$$\ln w_{iojt} = a_i + \gamma\bar{a}_{\sim i,ojt} + x^T_{iojt}\beta + p_{ojt} + \varepsilon_{iojt} \tag{6}$$

This within-peer group estimator, however, although it effectively deals with unobserved time-variant peer group characteristics, employs only limited and specific variation in $\bar{a}_{\sim i,ojt}$. That is, as shown in Appendix B, the spillover effect in equation (6) is identified only if peer groups vary in size. The advantage of being able to control for time-variant shocks to the peer group is thus countered by the disadvantage that only one particular type of variation is used to identify the

[14] Because the fixed effects δ_{jt}, ω_{ot} and θ_{oj} do not vary within peer groups at any given point in time, they are absorbed by p_{ojt}.

effect. The within-peer group estimator in equation (6), therefore, serves as a robustness check only, rather than as our main specification.

2.3 Estimation

Whereas our discussion so far assumes that the individual and average worker fixed effects a_i and $\bar{a}_{\sim i,ojt}$ are observed, they are in fact unobserved and must be estimated. The multiplication of $\bar{a}_{\sim i,ojt}$ and γ, both parameters to be estimated, turns equations (4) and (6) into a nonlinear least squares problem. Because the fixed effects are high dimensional (i.e., we have approximately 600,000 firm years, 200,000 occupation-firm combinations, and 2,100,000 workers), using standard nonlinear least squares routines to solve the problem is infeasible. Rather, we adopt the alternative estimation procedure suggested by Arcidiacono, Foster, Goodpaster and Kinsler (2012), which is detailed in Appendix C. An appealing characteristic of this estimation procedure is that the nonlinear least squares estimator for γ is consistent as the sample size grows in panels with a *limited number of time periods*, even though the individual worker fixed effects a_i are generally inconsistent in this situation. Hence, the well-known incidental parameters problem, which often renders fixed effects estimators in non-linear models inconsistent, does not apply to this model.[15] This, however, requires further assumptions in addition to those we discussed above for the case when a_i and $\bar{a}_{\sim i,ojt}$ are observed (see Theorem 1 in Arcidiacono, Foster, Goodpaster and Kinsler, 2012). Most importantly, the error terms between any two observations (v_{iojt} in our equation (4) baseline specification and ε_{iojt} in our equation (6) within-peer group estimator) must be uncorrelated. In our baseline specification, this

[15] In contrast, a simple two-step estimator in which the fixed effects are pre-estimated in a first step and then used as regressor in a second step does not allow to estimate γ in equations (4) and (6) consistently. This is because in panels with a fixed T, the estimates of the fixed effects in the first step are not consistent. In Mas and Moretti (2009), who use such a two-step estimator, this is less likely to be a problem because their study includes workers who are observed for many time periods.

assumption rules out any wage shocks common to the peer group, even those uncorrelated with peer group quality.

This additional assumption is necessary for consistent estimation when a_i and $\bar{a}_{\sim i,ojt}$ are unobserved because peer group-specific wage shocks affect not only peer group member wages but also estimated fixed effects in panels with a short *T*. Any such impact could lead to a spurious correlation between individual wages and the *estimated* worker fixed effects in the peer group even when the peer group-specific wage shocks are uncorrelated with the *true* worker fixed effects in the peer group. Results from a Monte Carlo study discussed in Appendix F.1 show that time-varying random peer group level shocks are likely to lead to an *upward* bias in panels with short *T*. However, under realistic assumptions the bias is not large enough to spuriously generate the level of peer effects that we find in low skilled repetitive occupations. Moreover, results from the Monte Carlo study confirm that even if time-varying peer group shocks were present, the within-peer group estimator of equation (6) deals directly with the bias problem—as it conditions on peer-group level wage shocks. We find similar magnitudes of peer effects from the estimation of the baseline model (4) and that of the peer-group fixed effects model (6), corroborating that wage shocks correlated at the peer group level—whether or not they are correlated with shocks to peer ability—do not affect our estimation.

3. Data

Our data set comes from over three decades of German social security records that cover every man and woman in the system observed on June 30 of each year. It therefore includes virtually the whole employed population except for civil servants, the self-employed, and military personnel. This data set is particularly suited for the analytical purpose because it includes identifiers for single production sites or workplaces (referred to as "firms" for simplicity), as well as detailed occupational codes that distinguish 331 occupations. Such detail

allows us to define peer groups of coworkers in the same firm who are likely to interact. We can also observe all workers in each firm, which allows precise calculation of the average peer group characteristics and ensures that our findings are representative of both the firm and the workers. Finally, the longitudinal nature of the data allows us to follow workers, their coworkers, and their firms over time, as required by our identification strategy, which relies on the estimation of firm and worker fixed effects.

3.1 Sample Selection

Focusing on the years 1989–2005, we select all workers aged between 16 and 65 in one large metropolitan labor market, the city of Munich and its surrounding districts.[16] Because most workers who change jobs remain in their local labor market, concentrating on one large metropolitan labor market rather than a random sample of workers ensures that our sample captures most worker mobility between firms, which is important for our identification strategy of estimating firm and worker fixed effects. Since only the fixed effects within a group of firms connected by worker mobility are identified relative to each other, we restrict our sample to the biggest connected mobility group (which makes up 99.5% of the initial sample; see Section 4.5 for more details).[17] Because the wages of part-time workers and apprentices cannot be meaningfully compared to those of regular full-time workers, we base our estimations on full-time workers not in apprenticeship. Additionally, to ensure that every worker is matched with at least one peer, we drop peer groups (firm-occupation-year combinations) with only one worker.

[16] We focus on the large metropolitan labor market rather than Germany as a whole in order to reduce the computational burden, which is far higher than in conventional linear worker and firm fixed effects models (as in e.g. Card et al. (2013)), due to the inclusion of average peer quality in addition to firm-by-time and firm-by occupation-effects. Drawing a random sample of firms as one may do in other applications is not an alternative in our case, as our estimators depend on the share of job switches that occurs between firms within the sample. Robustness checks we provide below and comparisons between the Munich area and Germany as a whole discussed in Appendix F.2 suggest that results for the whole of Germany would not be very different.

[17] Two firms are considered directly connected if worker mobility is observed between them in any sample period. A "connected mobility group" is the group of firms that are either directly or indirectly (via other firms) connected in this way.

3.2 Definition of the Peer Group

We define the worker's peer group as all workers employed in the same firm and the same 3-digit occupation, the smallest occupation level available in the social security data. These include detailed occupational definitions such as bricklayers, florists, plumbers, pharmacists, and high school teachers. Defining the peer group at the 3-digit (as opposed to the 1- or 2-digit) occupation level not only ensures that workers in the same peer group are likely to interact with each other, a prerequisite for knowledge spillover, but also that workers in the same peer group perform similar tasks and are thus likely to judge each other's output, a prerequisite for peer pressure build-up. Occupations at the 2-digit level, in contrast, often lump together rather different occupations. For example, the 3-digit occupation of a cashier is part of the same 2-digit occupation as accountants and computer and data processing consultants. Defining peer groups at the 3-digit level also increases the variation in peer quality within firms (exploited by our baseline specification) as well as within peer groups (exploited by the within-peer group specification, which vanishes as peer groups become large). In Appendix D, we show that defining the peer group as too large or too small is likely to lead to attenuation bias of the true peer effect. However, the robustness and placebo tests in Tables 6 and 8 suggest that our peer group definition at the 3-digit occupation level is plausible, and that the downward bias due to wrong peer group definition is small.

3.3 Isolating Occupations with High Levels of Peer Pressure and Knowledge Spillover

One important precondition for the build-up of peer pressure is that workers can mutually observe and judge each other's output, an evaluation facilitated when tasks are relatively simple and standardized but more difficult when job duties are diverse and complex. To identify occupations characterized by more standardized tasks, for which we expect peer pressure to be important, we rely on a further data source, the 1991/92 wave of the Qualification and Career

Survey (see Gathmann and Schönberg, 2010, for a detailed description). In addition to detailed questions on task usage, respondents are asked how frequently they perform repetitive tasks and tasks that are predefined in detail. From the answers, we generate a combined score on which to rank occupations. We then choose the set of occupations with the highest incidence of repetitive and predefined tasks, which encompasses 5% of the workers in our sample (see column (1) of Appendix Table F.3 for a full list of the occupations in this group). This group of most repetitive occupations includes agricultural workers, the subject of the Bandiera et al. (2010) study, and cashiers, the focus of the Mas and Moretti (2010) study. The remaining occupations are mostly low skilled manual occupations, such as unskilled laborers, packagers, or metal workers.

For robustness, we also estimate peer effects for the exact same occupations as in the extant studies using real-world data—that is, cashiers (Mas and Moretti, 2009), agricultural helpers (Bandiera et al., 2010), and data entry workers (Kaur et al., 2010)—as well as for a handpicked set of low skilled occupations in which, after initial induction, on-the-job learning is limited. This subgroup, which includes waiters, cashiers, agricultural helpers, vehicle cleaners, and packagers among others, makes up 14% of the total sample (see column (2) of Appendix Table F.3 for a full list). Unlike the 5% most repetitive occupations, this group excludes specialized skill craft occupations in which learning may be important, such as ceramic workers or pattern makers.

To isolate occupations in which we expect high knowledge spillover, we select the 10% most skilled occupations in terms of workers' educational attainment (average share of university graduates), which includes not only the scientists, academics, and teachers used in previous studies, but also professionals such as architects and physicians. As a robustness check, we also construct a combined index based on two additional items in the Qualification and Career Survey: whether individuals need to learn new tasks and think anew, and whether they need to

experiment and try out new ideas. From this index, we derive the 10% of occupations with the highest scores, which again includes scientists and academics but also musicians and IT specialists. We further handpick a group of occupations that appear very knowledge intensive, including doctors, lawyers, scientists, teachers, and academics (see columns (3) to (5) of Appendix Table F.3 for a full list of occupations in these three groups).

It should be noted that when focusing on occupational subgroups, we still estimate the model on the full sample and allow the peer effect to differ for both the respective subgroups and the remaining occupations. Doing so ensures that we use all information available for firms and workers, which makes the estimated firm-year and worker fixed effects—and hence the measure for average peer quality—more reliable.

3.4 Wage Censoring

As is common in social security data, wages in our database are right censored at the social security contribution ceiling. Such censoring, although it affects only 0.7% of the wage observations in the 5% most repetitive occupations, is high in occupations with high expected knowledge spillover. We therefore impute top-coded wages using a procedure similar to that employed by Dustmann et al. (2009) and Card et al. (2013) (see Appendix E for details). Whether or not we impute wages, however, our results remain similar even in the high skilled occupations with high censoring. This finding is not surprising given that censoring generally causes the distributions of both worker fixed effects and average peer quality to be compressed in the same way as the dependent variable, meaning that censoring need not lead to a large bias in the estimated peer effect.[18]

[18] In a linear least squares regression with normally distributed regressors, censoring of the dependent variable from above leads to an attenuation of the regression coefficients by a factor equal to the proportion of uncensored observations (Greene, 1981). Hence, censoring the top 15% of observations of the dependent variable attenuates the coefficients by a factor of .85 (analogous to the effect of multiplying the dependent variable by .85, which would also attenuate the coefficients by the same factor). In a model of the form $\ln w_{it} = x_{it}'\beta + a_i + \gamma\bar{a}_{\mathrm{it}} + r_{it}$ (a stylized

3.5 Descriptive Statistics

In Table 1, we compare the 5% most repetitive occupations, in which we expect particularly high peer pressure, and the 10% most skilled occupations, in which we expect high knowledge spillover, against all occupations in our sample. Clearly, the 5% most repetitive occupations are low skilled occupations: nearly half (47%) the workers have no post-secondary education (compared to 17% in the full sample and 4% in the skilled occupations sample) and virtually no worker has graduated from a college or university (compared to 18% in the full sample and 80% in the skilled occupations sample). As expected, the learning content in the 5% most repetitive occupations is low, while it is high in the 10% most skilled occupations, as implied by responses to whether individuals need to learn new tasks or to experiment with new ideas. The median peer group size of 3 or 4 workers per peer group is similar in all three samples. Not surprisingly, peer group size is heavily skewed, with the mean peer group size exceeding the median peer group size by a factor of about 3-4 in the three samples.

To identify peer effects in wages, individual wages must be flexible enough to react to peer quality induced changes in productivity. There is nothing to suggest that wages in Germany are particularly rigid or cannot be differentiated across workers in the same firm and occupation. In the period we study, there was no nationally binding minimum wage, and although coverage rates of collective bargaining are relatively high, collective agreements are quite flexible.[19] In

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version of our baseline specification (4)), we would therefore expect the parameters that enter the model linearly, β and a_i (and hence also $\bar{a}_{\mathrm{it}}$), to be attenuated. However, because the variances of $\ln w_{it}$ and $\bar{a}_{\mathrm{it}}$ are both attenuated through censoring in the same way, we would expect the peer effects parameter γ to be unaffected (analogous to multiplying both the dependent variable and the 'regressor' $\bar{a}_{\mathrm{it}}$ by .85, which would leave the coefficient γ unaffected).

[19] In 2004 wages of 68 per cent of West German employees were covered by collective agreements (Addison et al. 2007). However, these agreements allow for substantial real wage flexibility. First, nothing prevents firms from paying wages above the level stipulated by collective agreements, or to pay extra bonuses based on performance. The fraction of West German firms paying wages above the level stipulated in the agreement is above 40 per cent (Jung and Schnabel 2011). Cutting back such bonuses allows for wage flexibility without violating the collective agreement. Second, wages are usually tied to job titles, not to occupations. Hence within occupations in the same firm, there can be different ranks of job titles into which workers can be promoted based on their productivity.

Figure 1 and the bottom half of Table 1 we show that the wages of workers with the same observable characteristics in the same peer group are far from uniform: the overall standard deviations of log wages are 0.47 in the full, 0.33 in the repetitive, and 0.37 in the skilled occupations sample, respectively. Equally important, the within-peer group standard deviation of the log wage residuals (obtained from a regression of log wages on quadratics in age and firm tenure and aggregate time trends) is about half the overall standard deviation in the full sample (0.24 vs. 0.47), about two thirds in the 5% most repetitive occupations sample (0.20 vs. 0.33), and about three quarters in the 10% most skilled occupations sample (0.27 vs. 0.37). These figures suggest considerable wage variation among coworkers in the same occupation at the same firm at the same point in time. The last row in Table 1 further reveals that real wages are downwardly flexible: about 9% of peer-group stayers in the full sample, 3% in the skilled occupations sample, and 13% in the repetitive occupations sample experience a real wage cut from one year to another of at least 5%. Also, as the figures in the bottom half of Table 1 illustrate, average real wage growth over our sample period was positive and in the order of 2% per year, implying that decreases in productivity can be accommodated by raising wages more slowly rather than actually cutting nominal or real wages. Overall, therefore, there is considerable flexibility in individual wages and possibly higher wage flexibility for workers in high-skilled occupations than in repetitive occupations. Thus, larger peer effects in wages in the repetitive sector than in the high skilled sector—one of our key findings—are unlikely due to a weaker transmission of productivity into wages in the high skilled than in the repetitive sector.

Third, collectively bargained wage floors are agreed in nominal terms, which allows for real-wage cuts by freezing nominal wages. Based on data from the Socio-Economic Panel (SOEP), we illustrate in Appendix Table F.4 that 90% of workers in both high-skilled and repetitive occupations receive wage supplements on top of their base wage, and that for workers in high-skilled occupations these supplements contain more variable types of compensation (profit shares and bonuses) and constitute a higher share in total compensation; see Appendix F.4 for details.

We provide additional information on the structure of our sample in Table 2. Our overall sample consists of 2,115,544 workers, 89,581 firms, and 1,387,216 peer groups; workers are observed on average for 6.1 time periods and there are 2.3 peer groups on average per firm and year. Separately identifying worker, firm-occupation and firm-time fixed effects requires worker mobility between occupations and firms. In our sample, workers have on average worked for 1.6 firms and in 1.4 different occupations. This amount of mobility is sufficient to identify firm-year and firm-occupation fixed effects for nearly the entire sample: the biggest connected groups for firm-time effects and firm-occupation fixed effects contain 99.4% and 98.3% of the original observations, respectively, compared to 99.5% for the more standard firm fixed effects.[20]

In our baseline specification based on equation (4), the standard deviation of the estimated worker fixed effects for the full sample (a_i in equation (4)) is 0.32 or 70% of the overall standard deviation of log wages. The average worker fixed effects in the peer group (excluding the focal worker $\bar{a}_{\sim i,ojt}$ in equation (4)) has a standard deviation of 0.24, which is about 50% of the overall standard deviation of the log wage.

As explained in Section 2.1, our baseline specification identifies the causal effect of peers on wages by exploiting two main sources of variation in peer quality: changes to the peer group make-up as workers join and leave the group, and moves to new peer groups by the focal worker. In Figure 2, we plot the kernel density estimates of the change in a worker's average peer quality from one year to the next separately for those who remain in the peer group (stayers) and those who leave (movers). Not surprisingly, the standard deviation of the change in average peer quality is more than three times as high for peer group movers than for peer group stayers (0.20 vs. 0.06; see also Table 2). Yet even for workers who remain in their peer group, there is

[20] Note that all firm stayers are "movers" between firm-time units so that it is not surprising that the connected group is nearly as large for firm-by-time fixed effects as for the firm fixed effects. Further note that unlike standard firm-fixed effects, the firm-occupation fixed effects are identified not only though worker mobility across firms, but also through worker mobility between occupations within firms.

considerable variation in average peer quality from one year to the next, corresponding to roughly 20% of the overall variation in average peer quality. As expected, for peer group stayers, the kernel density has a mass point at zero, corresponding to stayers in peer groups that no worker joins or leaves. Nonetheless, peer groups without turnover are rare. In our sample, 90% of observations are in peer groups with at least some worker turnover. Hence, most workers experience some change in the average peer quality, even without switching peer groups. At 20%, the average peer group turnover in our sample, computed as 0.5 times the number of workers who join or leave divided by peer group size, is quite large and implies that nearly 20% of workers in the peer group are replaced every year.

At the bottom of the table, we report correlation coefficients between the various fixed effects in equation (4). In our sample, the individual worker fixed effect and the average fixed effect of the peer group are with a correlation coefficient of 0.64 strongly positively correlated. In line with Card, Heining and Kline (2013), we also find a positive correlation between the worker and the firm-time and firm-occupation fixed effects of 0.14 and 0.16, respectively. These correlations illustrate the endogenous sorting of high-ability workers into high ability and high wage peer groups and underscore the need to account for sorting in our estimates.

4. Results

4.1 Descriptive evidence on peer effects in wages

In Table 3 we present some first illustrative evidence on peer effects in wages by regressing the wage change of peer group stayers on the average quality of joiners to the own peer group and to other peer groups in the firm. The quality of joiners is measured by their average lagged wage multiplied by the share of joiners relative to the overall peer group size. We do not include any additional control variables, but it should be noted that first-differencing for stayers nets out

the own individual effect and the firm-by occupation effect. The results show that the average lagged wage of joiners to the own peer group has a positive and statistically significant effect on incumbents' own wage growth, whereas the quality of joiners to other occupations in the same firm has no statistically significant effect. Moreover, the effect of joiners to the own peer group is larger for workers in the 5% most repetitive and the 10% most skilled occupations compared to all occupations, and relative to joiners into other occupations it is largest in the 5% most repetitive occupations.

4.2 Baseline results

We report estimates for the impact of average peer quality, measured as the average worker fixed effect of co-workers in the peer group, on wages for the full sample in Table 4. Each column of the table introduces additional control variables to account for shared background characteristics. In column (1), we control for the worker's own fixed effect (a_i in equation (4)), for quadratics in age and firm tenure (captured by x_{iojt} in equation (4)), and for time-variant occupation fixed effects (ω_{ot} in equation (4) which proxy for outside options), in addition to firm fixed effects. The coefficient of .148 implies that a 10% increase in peer quality increases wages by 1.48%.[21] While this specification accounts for the possibility that workers employed in high-wage firms work with better peers, it does not allow for firms which overpay specific occupations relative to the market to attract better workers into these occupations. To deal with this type of worker sorting, we control in column (2) for firm-occupation fixed effects ((θ_{oj}in equation (4)) instead of simple firm fixed effects. This produces a much smaller estimate: a 10% increase in peer quality now increases the individual wage by only 0.66%.

[21] This specification and the associated estimates are roughly in line with those reported by Lengerman (2002) and Battisti (2012), who also analyze the effects of coworker quality on wages.

Including firm-occupation fixed effects is similar to estimating equation (4) in first differences for peer group stayers (but additionally exploits variation in peer quality for peer group switchers). As discussed in Section 3.1, this specification does not yet filter out time-varying shocks at firm level. Such shocks turn out to be important: When adding time-variant firm fixed effects (δ_{jt} in equation (4)) in column (3), we find that a 10% increase in peer quality raises individual wages by merely 0.1%. Translated into standard deviations, this outcome implies that a one standard deviation increase in peer ability increases wages by 0.3 percentage points or 0.6 percent of a standard deviation. This effect is about 10–15 times smaller than the effects previously identified for *productivity* among supermarket cashiers in a single firm (Mas and Moretti 2009) and students carrying out a simple task in an experiment (Falk and Ichino 2006) – which incidentally are very close to the average effect reported by Herbst and Mas (2015) from a larger range of studies mostly covering specific field or lab settings. Hence, we do not confirm similarly large spillover effects in *wages* for a *representative* set of occupations and firms.

4.3 Effects for Occupational Subgroups

Peer Pressure

Even if peer effects in wages are small *on average* for a representative set of occupations, they might still be substantial for specific occupations. Hence, in Panel A of Table 5, we report the results for the 5% of occupations with the most repetitive and predefined tasks (see Appendix Table F.3 for a full list), in which we expect particularly high peer pressure. These occupations also more closely resemble those used in earlier studies on peer pressure. All specifications in the table refer to the baseline specification given by equation (4) and condition on occupation-year, firm-year, and firm-occupation fixed effects, meaning that they correspond to specification (3) in the previous table.

For these repetitive occupations, we find a substantially larger effect of peer quality on wages than in the full sample: a 10% increase in peer quality raises wages by 0.64% (see column (1)) compared to 0.1% in the full sample (see column (4) of Table 4). This outcome implies that a one standard deviation increase in peer quality increases the wage by 0.84%, about half the size of the peer effects in coworker productivity identified by Mas and Moretti (2009) and Falk and Ichino (2006) and in the meta-analysis by Herbst and Mas (2015).

Column (2) of Panel A lists the peer effects for the three occupations used in earlier studies (agricultural helpers, cashiers and data entry workers), whose magnitudes are remarkably similar to that for the 5% most repetitive occupations shown in column (1). Finally, column (3) reports the results for the handpicked group of occupations in which we expect easily observable output and, following initial induction, limited on-the-job learning. The estimated effect for this occupational group is slightly smaller than that for the 5% most repetitive occupations sample but still about five times as large as the effect for the full occupational sample.

Knowledge Spillover

In Panel B of Table 5, we restrict the analysis to particularly high skilled and innovative occupations with a high scope for learning, in which we expect knowledge spillover to be important. Yet regardless of how we define high skilled occupations (columns (1) to (3)), peer effects in these groups are small and resemble those in the full sample. Overall, therefore, we identify sizeable peer effects in wages only in occupations characterized by standardized tasks and low learning content, which are exactly the occupations in which we expect peer pressure to matter and which closely resemble the specific occupations investigated in the extant studies on peer pressure.

By looking at the 5% most repetitive and the 10% most skilled occupations we have distinguished between the two extreme ends of the two indexes of repetitiveness and skill from which the definition of these groups was derived. In Figure 3 we show results from a more complete analysis that lets the peer effect coefficient vary by bins of these two indexes. They show a symmetric pattern, with highest peer effects in the most repetitive / least skilled categories, smallest peer effects in the middle categories, and again slightly higher but still small effects in the least repetitive / most skilled categories.[22] The U-shape of the estimated peer effects in these indexes provides support for our hypothesis that peer pressure and knowledge spillover are two possible mechanism for peer effects, where the former operates predominantly in the most repetitive (and least skilled) occupations, while the latter is most pronounced in the least repetitive and most skilled occupations.

4.4 Robustness Checks

As Table 6 shows, the above conclusion remains robust to a number of alternative specifications for repetitive and for high skilled occupations (first and second column). As a point of reference, the row (i) replicates the results from the baseline specification of column (1), Table 5.

Row (ii) refers to our most important robustness check and reports estimates using the within-peer group estimator (see equation (6)). As we point out above, this estimator is robust to unobserved time-variant peer group characteristics or spurious correlation between individual wages and the *estimated* worker fixed effects of peers, due to peer-group level shocks. In both repetitive occupations (first column) and high skilled occupations (second column), the estimated peer effects based on the within-peer group specification are very close to the effect derived in the respective baseline specification. This similarity in estimates corroborates that time-variant

[22] The skill and repetitiveness indexes are strongly correlated with a correlation coefficient of -.76

peer group-specific wage shocks are not important, and provides reassurance that we are picking up a true peer effect rather than a spurious correlation.

As illustrated in Figure 2, our baseline specification exploits two sources of variation in peer quality: changes to the peer group make-up as workers join and leave the group, and moves to new peer groups by the focal worker. Unlike the latter, the former allows for the presence of time-constant *match*-specific effects m_{ioj} that are correlated with peer ability (as first differencing eliminates these for peer group stayers, but not for peer group movers). Rows (iii) and (iv) show that both sources of variation lead to very similar peer effects, indicating that our baseline peer estimates are not biased because of match-specific shocks.[23]

In row (v), we report results when the censored wage observations are not imputed. In row (vi), we relax the assumption that observable characteristics have the same effects in repetitive occupations and high skilled occupations. In row (vii), we extend our estimation sample to include not only the metropolitan area of Munich, but also additional surrounding rural areas. In row (viii), we add to the regression the average observed characteristics (firm tenure, age, and schooling) of peers. In rows (ix) and (x), we display peer effect estimates separately for small (≤ 10) and large (>100) peer groups. Remarkably, for both samples (repetitive and high skilled occupations), all these different specifications yield similar estimates as the baseline estimates reported in Table 5.[24]

In rows (xi) to (xiii), we display peer effect estimates for alternative peer group definitions: two wider definitions where we define the peer group at the 2-digit and 1-digit rather than the 3-digit occupational level, and one narrower definition where we split peer groups at the 3-digit occupational level further up into two age groups (above and below median age). Interestingly,

[23] This is in line with Card, Heining and Kline (2013) and Card, Cardoso and Kline (2014) who show that once firm fixed effects are accounted for, match-specific effects are not an important determinant of worker mobility.

[24] This is also true across a full set of firm size and peer group size categories, for which we report estimates in Appendix F.5.

our baseline peer group definition at the 3-digit occupational level (row (i)) yields the highest estimated peer effect compared to alternative wider and narrower definitions, which is in line with the hypothesis that the alternative definitions do not define the peer group correctly and thus lead to an attenuation bias. Differences are small when moving from the 3-digit level to the wider 2-digit or 1-digit level (rows (xii) and (xii)), but the peer effect drops sharply when defining the peer group at the narrower 3-digit-by-age level (row (xiii)). Overall, these findings suggest that individuals do interact with all colleagues in the same 3-digit occupation regardless of their age, and that our baseline peer group definition at the 3-digit occupational level is the most plausible.

4.5 Timing of Effects

Figure 4 provides a first visual impression of the timing of the wage response to a change in peer quality in the 5% most repetitive occupations where peer effects are largest. Panels A and B show the evolutions of peer quality and residualized wages (purged of the observables and fixed effects included in equation (4)) of peer group stayers experiencing an exceptionally large rise or fall in peer quality (of at least 0.055), while Panel C depicts the corresponding evolutions for peer group movers experiencing an increase in peer quality (of at least 0.10). The figures illustrate that for both peer group stayers and movers, the increase (or decrease) in peer quality is accompanied by an immediate increase (or decrease) in wages in the same year, with little evidence for dynamic effects. [25]

[25] It should be noted that any visual illustration of the relationship between two continuously varying variables (peer quality and wages) in an event study graph will necessarily select the underlying sample and reduce the sample size. For instance, workers in the most repetitive occupations who have been with the same firm for at least five periods, and have experienced a rise in peer quality of at least .055 in period zero (the "event"), are more likely to be in small peer groups, because the average of peer quality is more variable in small groups and thus large rises are more common. It should therefore not be surprising if the graphical examples slightly deviate from the overall estimates that use the entire sample.

We analyze the timing of peer effects more systematically in Table 7, by including lags and leads of peer quality (computed from the estimated worker fixed effects from the baseline model). In column (1) of Table 7, we first augment our baseline model by adding the quality of a worker's peers in two future periods ($t+1$ and $t+2$). The inclusion of future peer quality represents a placebo test, as workers cannot feel peer pressure or learn from colleagues whom they have not yet met. Reassuringly, we find that the effect of future peers is essentially zero in both repetitive (Panel A) and high skilled occupations (Panel B), whereas the effect of current peers remains of the same magnitude as in our baseline specification.

In column (2) of Table 7, we add the average worker fixed effects for the peer group lagged by one and two periods into our baseline regression. The effects of lagged peer quality are informative about the mechanisms for peer effects: If peer effects are generated by peer pressure, then past peers should be irrelevant conditional on current peers in that workers should feel peer pressure only from these latter. If, on the other hand, peer effects result from learning, both past and current peers should matter, since the skills learnt from a coworker should be valuable even after the worker or coworker has left the peer group. We find that in the repetitive sector, the average quality of lagged peers has almost no effect on current wages, suggesting that knowledge spillover is not the primary channel of the peer effects in that sector. [26] The general pattern of results that only contemporary peer quality matters does not change when including lags and leads jointly in column (3) of Table 7.

[26] Relative to the contemporaneous effect, the lagged effects are slightly more important in skilled occupations (columns (2) and (3) of Panel B Table 7), but overall effects continue to be very small.

4.6 Geographically and Economically Close Workers Outside of the Immediate Peer Group

In Table 8, we further assess whether the quality of workers outside of the immediate peer group affects wages. While providing a further test of whether our peer group definition is appropriate, the results also shed light on the potential channel of peer effects. In the case of peer pressure, the relevant peers are contemporaneous coworkers in the immediate peer group within the firm who frequently interact and carry out comparable tasks, as peer pressure can only build up if workers work alongside each other and can observe and compare each other's output. If, in contrast, peer effects result from knowledge spillover, a much wider group of peers is potentially relevant, since knowledge spillover is not restricted to occur within the firm only. In fact, knowledge spillover is often assumed to operate through interactions of workers who do not necessarily work in the same firm but are geographically or economically close (see, e.g., Lucas, 1988; Moretti, 2004).

In Panel A Table 8, we estimate the effects of the quality of workers in other occupations within the same firm on wages. In rows (i) and (ii) of Panel A, we randomly choose a 3-digit occupation (other than the worker's own occupation) in the same firm, and distinguish whether the randomly assigned occupation is economically close or far, as measured by observed worker flows between occupations in the overall sample.[27] The results show that in both the repetitive and skilled sector, the quality of co-workers in other occupations within the same firm has virtually no impact on wages, no matter whether the occupation is close or far. We corroborate these findings in rows (iii) and (iv) of Panel A, where we display the impact on wages of the quality of workers in the economically closest and farthest occupation (relative to the focal worker's own occupation) in the same firm, again measuring economic closeness by observed

[27] We define a pair of occupations as "close" if the proportion of workers switching between these occupations is above median, and "far" if it is below median.

worker flows between occupations in the overall sample. Overall, these findings provide strong evidence for the validity of defining the peer group as workers from the same 3-digit occupation in the same firm, and speak against knowledge spillover or peer pressure across occupations within the same firm.

In Panel B of Table 8, we augment our baseline model by adding the quality of workers in other *firms* that are economically close (in terms of worker flows) to the focal worker's peer group. First, we include the average peer fixed effect of workers who in year t are in peer groups (firm-occupation combinations) in other firms that at any point during the observation period have exchanged workers with the focal worker's peer group. Second, we identify the peer groups from which new joiners to the focal peer group came (i.e., the peer groups in which the joiners were in t-1), and we add the average worker fixed effect of the workers who were in these peer groups in t-1 but who did not join the focal peer group (i.e., the joiners' past peers). Effects from these economically close workers in other firms are virtually zero, providing little evidence for knowledge spillover across peer groups in different firms linked by worker mobility.

In Panel C of Table 8 we augment our baseline model by adding the average fixed effect of all workers residing in the same municipality and working in the same occupation (but not necessarily in the same firm) as the focal worker. We find that peer quality in the municipality has no effect on wages, whereas the effect of peer quality in the firm remains unchanged.

In sum, the results in Table 8 provide little evidence of knowledge spillover from workers outside the immediate peer group. Rather, they suggest that peer effects (in the repetitive sector) are confined to the same (3-digit) occupation and firm, as one would expect if peer pressure is the main driving force behind peer effects.

4.7 Heterogeneous Peer Effects

We now provide additional evidence that peer effects in the 5% most repetitive occupations—where we have found the strongest effects—are driven primarily by peer pressure, by investigating heterogeneity in peer effects in that sector.

Age and job tenure

In the low skilled, repetitive occupations we consider, we expect that almost all the on-the-job learning takes place when workers are young or have only just joined the peer group. In Panel A of Table 9, we therefore allow the peer effect to differ for older (age >35 years) and younger workers (age <=35 years) and for workers who have been with the peer group for more or less than two years. Although we do find that peer effects are larger for younger workers, which is in line with knowledge spillover, we also find positive peer effects for older workers. Moreover, peer effects vary little with tenure in the peer group. Both these findings are difficult to reconcile with peer effects arising from knowledge spillover alone. It should also be noted that although the smaller peer effect for older workers is consistent with knowledge spillover, it is also in line with younger workers responding more strongly to peer pressure or suffering more from the "pain" of peer pressure than older or more experienced workers.

Symmetry of Effects

In Panel B of Table 9 we investigate whether improvements in the average peer group quality have similar effects as deteriorations. To this end, using the peer group stayers, we regress the change in log wages on the change in peer group quality (using the pre-estimated worker fixed effects from our baseline specification) and allow this effect to vary according to whether peer group quality improves or deteriorates (see Mas and Moretti, 2009, for a similar specification). Our results show relatively symmetric effects for both improvements and

deteriorations. This once again points against knowledge spillover as they primary driver behind peer effects in the repetitive sector, as it is unlikely that workers immediately "unlearn" skills when peers get worse, and thus we would expect negative changes of peer quality to have a smaller effect (in absolute terms) on wages than positive changes if knowledge spillover were the main channel.

Low versus High Ability Workers

In Panel C of Table 9, we explore whether the peer effects in wages differ for low versus high ability workers in the peer group (i.e., workers below and above the median in the firm-occupation cell). Like Mas and Moretti (2009), we find that peer effects are almost twice as large for low as for high ability workers. One possible explanation is that low ability workers increase their effort more than high ability workers in response to an increase in peer quality (i.e., the peer effect in *productivity* is higher for low than for high ability workers). If this latter does indeed explain peer effect differences between low and high ability workers, then, as Mas and Moretti (2009) emphasize, firms may want to increase peer group diversity—and maximize productivity—by grouping low ability with high ability workers.

Our model, however, also suggests an alternative interpretation; namely, that low ability workers suffer more from the pain of peer pressure than high ability workers, leading to higher peer effects in *wages* for low than high ability workers, even when peer effects in *productivity* are the same.[28] If such "pain" is the reason for the larger peer effects among the low ability

[28] In our model, low and high ability workers increase their effort by the same amount in response to an increase in peer ability (see equation (A.2)), meaning that the peer effect in productivity is the same for both groups. Note, however, that the rate at which higher peer ability translates into "pain" from peer pressure, $\frac{\partial P(e_i, \bar{y}_{\sim i})}{\partial \bar{y}_{\sim i}} = \lambda^P(m - e_i^*)$ varies inversely with a worker's own optimal effort e_i^*, which in turn varies positively with individual ability (see equation (A.4)), implying that the pain from peer pressure for a given increase in peer ability is higher for low ability than for high ability workers.

workers, then firms may prefer homogenous peer groups over diverse peer groups because they will save wage costs without lowering productivity.

Bottom versus Top Peers

Whereas all our previous specifications estimate the effect of *average* peer quality on wages, in Panel D of Table 9, we estimate the effect of the quality of the *top* and *bottom* workers in the peer group on wages. To do so, we split the peer group into three groups: the top 10%, the middle 80%, and the bottom 10% of peers based on the estimated worker fixed effects from our baseline regression.[29] We then regress individual wages on the average worker fixed effect for the three groups, controlling for the same covariates and fixed effects as in our baseline specification and restricting the sample to workers in the middle group. We find that the effect of the average peer quality in the middle group on wages is similar to our baseline effect, while the average productivities of peers in the bottom or top groups have no significant effect on wages. Hence, our baseline peer effects are neither driven entirely by very bad workers nor driven entirely by very good workers. This observation rules out a simple chain production model in which team productivity is determined by the productivity of the "weakest link in the chain"; that is, the least productive worker. It also suggests that the peer effects in the 5% most repetitive occupations are not driven solely by the most productive workers in the peer group, even though these latter may increase overall peer group productivity by motivating and guiding their coworkers.[30]

[29] Although these shares are quite exact in large peer groups, in small peer groups, the top and bottom do not exactly equal 10%. For example, in a peer group with four workers, one worker falls at the top, one at the bottom, and two in the middle.

[30] In an interesting study in a technology-based services company, Lazear et al. (2012) find that the quality of bosses has significant effects on the productivity of their subordinates. While it might be tempting to interpret the quality of the top 10% of peers in our study as a proxy for boss quality, we prefer not to interpret our findings as informative on boss effects: first, we cannot ascertain whether more able peers are indeed more likely to become team leaders or supervisors, and second, bosses need not necessarily belong to the same occupation as their subordinates and thus need not be in the same peer group as defined by our data.

5. Conclusions

Although peer effects in the classroom have been extensively studied in the literature (see Sacerdote, 2011, for an overview), empirical evidence on peer effects in the workplace is as yet restricted to a handful of studies based on either laboratory experiments or real-world data from a single firm or occupation. Our study sheds light on the external validity of these extant studies by carrying out the first investigation to date into peer effects in a general workplace setting. Unlike the previous research, our study focuses on peer effects in *wages* rather than in *productivity*.

On average, we find only small, albeit precisely estimated, peer effects in wages, suggesting that the larger peer effects found in existing studies may not carry over to the labor market in general. Yet our results also reveal larger peer effects in low skilled occupations where coworkers can easily observe each other's output, which are exactly the occupations most often analyzed in the previous studies on peer pressure. In these types of occupations, therefore, the findings of previous studies extend beyond the specific firms or tasks which they explore. Our results also indicate that in this segment of the labor market, productivity spillovers translate into wage spillovers—a dynamic as yet totally unexplored in the literature—and suggest that indeed peer pressure, and not knowledge spillover, is the main source of the peer effect.

Overall, we conclude that peer effects in the workplace, despite being important in some specific settings, do not importantly affect the wage setting of firms, nor do they contribute significantly to overall inequality in the labor market.

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Table 1: Skill content, peer group size and wage flexibility for different groups of occupations

	all occupations	5% most repetitive occupations	10% most skilled occupations
Skill Content			
Share without postsecondary education	0.17	0.47	0.04
Share with university degree	0.18	0.01	0.80
To what extent does the following occur in your daily work? (0=never, …, 4=all the time)			
need to learn new tasks and think anew	2.25	1.36	2.98
need to experiment and try out new ideas	1.80	0.96	2.56
Peer Group Size			
median	3	4	3
mean	9.3	12.3	13.1
Wage Flexibility			
Average wage growth	0.022	0.016	0.023
Average wage growth - stayers	0.018	0.013	0.021
Average wage growth - movers	0.053	0.044	0.054
St. dev. of log real wage	0.455	0.322	0.223
St. dev. of log real wage (imputed)	0.472	0.326	0.371
St. dev. of log real wage residual [a)]	0.377	0.308	0.365
Within-peer group st. dev. of log real wage residual [a)]	0.243	0.200	0.269
Probability of >5% real wage cut (peer group stayers)	0.088	0.130	0.034

Note: The table compares all occupations (N=12,832,842 worker-year observations) with the 5% most repetitive occupations (N=681,391) and the 10% most skilled occupations (N=1,309,070). See Appendix Table F.3 for a full list of occupations, and section 4.3 of the text for the definition of "repetitive" and "skilled" occupations.
a) Residual from a log-wage regression, after controlling for aggregate time effects, education, and quadratics in firm tenure and age.

Data Source: Social Security Data, One Large Local Labor Market, 1989-2005, combined with information from Qualification and Career Survey 1991/1992.

Table 2: Structure of Sample

Panel structure		
(i)	No. of workers	2,115,544
(ii)	No. of firms	89,581
(iii)	Number of peer groups (occupations within firm-years)	1,387,216
(iv)	Average number of time periods per worker	6.07
(v)	Number of peer groups per firm-year	2.30
(vi)	Average number of employers per worker	1.60
(vii)	Average number of occupations per worker	1.40
(viii)	Share of mobility group with identified firm fixed effects	0.995
(ix)	Share of mobility group with identified firm-time fixed effects	0.994
(xi)	Share of mobility group with identified firm-occupation fixed effects	0.983
Variation in wages, peer quality and worker turnover		
(xii)	St. dev. worker fixed effect	0.32
(xiii)	St. dev. average peer fixed effect	0.24
(xiv)	St. dev. change of average peer fixed effect from t-1 to t	0.09
(xv)	St. dev. change of average peer fixed effect from t-1 to t - Movers	0.20
(xvi)	St. dev. change of average peer fixed effect from t-1 to t - Stayers	0.06
(xvii)	Share of worker-year observations in peer groups with turnover	0.90
(xviii)	Average share of workers replaced by turnover	0.20
(ixx)	Correlation worker fixed effect / average peer fixed effect	0.64
(xx)	Correlation worker fixed effect / firm-time effect	0.14
(xxi)	Correlation worker fixed effect / firm-occupation effect	0.16

Note: The table shows descriptive statistics describing the panel structure of the data set, as well as the variation in in wages, peer quality and worker turnover which we exploit in subsequent estimations. N=12,832,842.

Data Source: Social Security Data, One Large Local Labor Market, 1989-2005.

Table 3: Descriptive evidence on the effect of peer quality on wages

	(1) all occupations	(2) 5% most repetitive occupations	(3) 10% most skilled occupations
Average lagged wage of peer group joiners * Share of joiners	0.003	0.006	0.006
	(0.001)	(0.002)	(0.002)
Average lagged wage of joiners to other peer groups of the firm * Share of joiners	0.001	-0.002	0.003
	(0.001)	(0.003)	(0.004)
Constant	0.024	0.015	0.031
	(0.001)	(0.001)	(0.002)
Observations	4,341,750	259,927	592,903

Note: The table reports estimates from a regression of the wage change of peer group stayers on the quality of joiners to the own peer group and joiners to other peer groups in the firm. The quality of joiners is measured by their average lagged wage multiplied by the share of joiners relative to the overall peer group size. First-differencing for peer group stayers nets out the own individual fixed effect and the firm-by-occupation fixed effect, but not the firm-by-year fixed effect.

Data Source: Social Security Data, One Large Local Labor Market, 1989-2005.

Table 4: Peer Effects in the Full Sample

	(1) observables, occupation-year and firm fixed effects	(2) plus firm-occupation fixed effects	(3) plus firm-occupation and firm-year fixed effects
Average peer fixed effect	0.148	0.066	0.011
	(0.002)	(0.002)	(0.001)
Worker Fixed Effects	Yes	Yes	Yes
Occupation X Year Effects	Yes	Yes	Yes
Firm Effects	Yes	-	-
Occupation X Firm Effects	-	Yes	Yes
Firm X Year Effects	-	-	Yes

Note: The table shows the effect of average peer quality on the individual log wage in the overall sample. Peer quality is measured as the average fixed worker effect of the co-workers in the same 3-digit occupation at the same firm in the same point of time. In column (1), we only control for worker fixed effects, firm fixed effects, occupation-by-year fixed effects, and quadratics in age and firm tenure. We then successively add firm-occupation fixed effects (column (2)), and firm-by-year fixed effects (column (3)). Specification (3) corresponds to the baseline specification described in equation (4) in the text. Coefficients can approximately be interpreted as elasticities, and the coefficient of 0.011 in the baseline specification in column (3) implies that a 10% increase in average peer quality increases wages by 0.1%. Bootstrapped standard errors with clustering at firm level in parentheses. N=12,832,842.

Data Source: Social Security Data, One Large Local Labor Market, 1989-2005.

Table 5: Peer Effects in Sub-Samples of Occupations

	(1)	(2)	(3)
Panel A: Peer Effects for Sub-Samples of Low Skilled Occupations			
	5% most repetitive occupations	As in case studies	Low learning content
Average peer fixed effect	0.064	0.067	0.052
	(0.0070)	(0.0116)	(0.0031)
Panel B: Peer Effects for Sub-Samples of High Skilled Occupations			
	10% most skilled occupations	10% most innovative occupations	High learning content
Average peer fixed effect	0.013	0.007	0.017
	(0.0039)	(0.0044)	(0.0028)

Note: The table replicates the baseline peer effects estimates of column (3) in Table 4 for different occupational groups—see Appendix Table F.3 for a full list of occupations in each of the sub-samples used in this table, and section 4.3 in the text for a description of the way in which the different sub-samples were constructed. In panel A, column (1), we show the effect for the 5% most repetitive occupations. In panel A, column (2), we show the effect for agricultural helpers, cashiers and data entry workers, which have been used in related case-studies on peer effects in the workplace. In panel A, column (3), we report the effect for occupations characterized by standardized tasks (as the 5% most repetitive occupations) and limited learning content (i.e., cashiers, warehouse workers, drivers, removal workers, cleaners, agricultural helpers, and waiters). In panel B, column (1) we present results for the 10% most skilled occupations, as measured by the share of workers with a college degree in that occupation. In panel B, column (2) we present results for the 10% most innovative occupations, defined by occupation averages of workers' responses to an index of how frequently they need to experiment with new ideas. In panel B, column (3) we present results for occupations with complex tasks and a high learning content (such as doctors, lawyers, scientists, teachers, and academics). Bootstrapped standard errors with clustering at firm level in parentheses. N=12,832,842.

Data Source: Social Security Data, One Large Local Labor Market, 1989-2005.

Table 6: Robustness Checks

		5% Most Repetitive Occupations	10% Most Skilled Occupations
(i)	Baseline	0.064	0.013
		(0.0070)	(0.0039)
(ii)	Within peer group estimator	0.061	0.016
		(0.006)	(0.004)
(iii)	Separate effect for stayers	0.061	0.014
		(0.006)	(0.003)
(iv)	Separate effect for movers	0.073	0.006
		(0.006)	(0.004)
(v)	Wage not imputed	0.086	0.017
		(0.007)	(0.007)
(vi)	Varying coeff. on observables	0.082	0.007
		(0.008)	(0.004)
(vii)	Expansion of sample to larger region with	0.082	0.014
	additional rural areas	(0.008)	(0.002)
(viii)	Include peer observables	0.071	0.010
		(0.006)	(0.005)
By peer group size			
(ix)	Peer groups size <=10	0.068	0.014
		(0.002)	(0.001)
(x)	Peer groups size >100	0.081	0.014
		(0.004)	(0.003)
Alternative peer group definitions			
(xi)	Peer group defined at 2-digit occupational level	0.058	0.011
		(0.006)	(0.004)
(xii)	Peer group defined at 1-digit occupational level	0.052	0.006
		(0.007)	(0.001)
(xiii)	Peer group defined by age groups within 3-digit	0.032	0.006
	occupations	(0.007)	(0.004)

Note: The table reports a number of robustness checks for the effect of average peer quality on log wages. The first column refers to the group of the 5% most repetitive occupations, as in column (1), Panel A of Table 5. The second column refers to the group of the 10% most skilled occupations, as in column (1), Panel B, of Table 5. For comparison, we replicate the results from the baseline specification of column (1), Table 5 in row (i). In row (ii), we present the within peer group estimate, see equation (6) in the text. The within-estimator is based on pre-estimated worker fixed effects from the baseline model in equation (4) in the text. The remaining rows refer to our baseline specification given by equation (4) in the text. In rows (iii) and (iv) we show separate peer effects for stayers and movers. Workers are defined as stayers in periods when they are in the same firm and occupation in period t as in period t-1, and as movers when they switch the firm or occupation between periods t-1 and t. In row (v), we do not impute censored wage observations. In row (vi), we allow the coefficients on the observable characteristics (quadratics in age and firm tenure) to vary between the 5% most repetitive (or 10% most skilled) occupations and the remaining occupations. In row (vii) we extend our estimation sample to include not only the metropolitan area of Munich, but also additional surrounding rural areas. In row (vii), we augment the baseline model by adding peer averages of observed characteristics (firm tenure, age, and schooling). In rows (ix) and (x), we report the peer effect coefficient for small and large peer groups respectively. In rows (xi) to (xiii), we report results for three alternative peer group definitions: two wider peer group definitions at 2-digit-occupation-firm level and 1-digit-occupation-firm level, and one narrower peer group definition at the 3-digit-occupation-age-firm level. Bootstrapped standard errors with clustering at firm level in parentheses. N=12,832,842.

Data Source: Social Security Data, One Large Local Labor Market, 1989-2005.

Table 7: Timing of Effects

	(1)	(2)	(3)
Panel A: 5% Most Repetitive Occupations			
Average peer fixed effect	0.066	0.046	0.036
	(0.005)	(0.005)	(0.007)
Average peer fixed effect, t+1	0.006		0.003
	(0.005)		(0.006)
Average peer fixed effect, t+2	0.001		0.004
	(0.005)		(0.006)
Average peer fixed effect, t-1		0.0006	0.008
		(0.004)	(0.005)
Average peer fixed effect, t-2		-0.007	-0.001
		(0.004)	(0.005)
Observations:	392,937	392,937	250,911
Panel B: 10% Most Skilled Occupations			
Average peer fixed effect	0.017	0.020	0.016
	(0.003)	(0.004)	(0.005)
Average peer fixed effect, t+1	-0.002		0.007
	(0.004)		(0.006)
Average peer fixed effect, t+2	-0.006		-0.017
	(0.003)		(0.007)
Average peer fixed effect, t-1		-0.002	0.004
		(0.004)	(0.005)
Average peer fixed effect, t-2		0.008	0.012
		(0.005)	(0.006)
Observations:	815,052	815,052	522,338

Note: The table investigates the dynamic effects of average peer quality on log wages, based on pre-estimated fixed effects from the baseline specification. Panel A shows results for the group of the 5% most repetitive occupations, as in column (1), Panel A of Table 5. Panel B reports results for the group of the 10% most skilled occupations, as in column (1), Panel B, of Table 5. In column (1) we add the peer quality of the focal worker's future peers from the periods t+1 and t+2 to our baseline specification as a placebo test. In column (2) we add the average fixed effects of the lagged peer group to equation (4). In column (3) we present a more complete specification including both leads and lags. Bootstrapped standard errors with clustering at firm level in parentheses.

Data Source: Social Security Data, One Large Local Labor Market, 1989-2005.

Table 8: Other peer groups inside and outside the firm

		(1) 5% Most Repetitive Occupations	(2) 10% Most Skilled Occupations
Panel A: Economically close and far peer groups within the same firm			
(i)	Other occupation in the same firm with above-median closeness ("close")	-0.0006	-0.0005
		(0.0009)	(0.0011)
(ii)	Other occupation in the same firm with below-median closeness ("far")	-0.0009	0.0007
		(0.0005)	(0.0012)
(iii)	'Closest' other occupation in the same firm	-0.0162	0.0097
		(0.0050)	(0.0037)
(iv)	'Farthest' other occupation in the same firm	-0.020	-0.018
		(0.004)	(0.003)
Panel B: Economically close peer groups in other firms			
(i)	Average worker fixed effect of own peer group	0.0841	0.0134
		(0.0034)	(0.0031)
(ii)	Average worker fixed effect in economically close peer groups in other firms	-0.0012	-0.0004
		(0.0015)	(0.0010)
(iii)	Average worker fixed effect of joiners' past peers	-0.0008	0.0002
		(0.0007)	(0.0006)
Panel C: Geographically close peers outside the firm			
(i)	Average worker fixed effect of own peer group	0.0758	0.0202
		(0.0074)	(0.0038)
(iii)	Average peer fixed effect in municipality	0.0029	-0.0035
		(0.0043)	(0.0033)

Note: The table reports effects on log wages of the peer quality in peer groups that are economically or geographically close (or far) to the focal worker's own peer group. Column (1) shows the results for the group of the 5% most repetitive occupations, as in column (1), Panel A of Table 5. Column (2) reports results for the group of the 10% most skilled occupations, as in column (1), Panel B, of Table 5. In rows (i) and (ii) of Panel A, we report results when the peer group consists of workers from a randomly chosen 3-digit occupation (other than the worker's own occupation) in the same firm; distinguishing whether the randomly assigned peer group is from an economically "close" or an economically "far" occupation, where economic closeness is measured by worker flows between occupations in the overall sample. A pair of occupations is defined as "close" if the proportion of workers switching between these occupations is above median, and "far" if it is below median. This specification drops firm-year observations with only one occupation. In rows (iii) and (iv) of Panel A, we show the effect of peer quality in the economically closest and farthest 3-digit occupation (other than the worker's own occupation) in the same firm, where closeness is again measured by worker flows between occupations in the overall sample. This specification is based on firm-year observations with at least three occupations per firm. In Panel B, we report results when adding peer quality of workers in peer groups in other firms that are economically close to the focal worker's peer group to our baseline specification, where we again measure economic closeness by worker flows in the overall sample. In row (ii), we report the coefficient on the average worker fixed effect of workers who in year t are in peer groups (firm-occupation combinations) in other firms that at any point during the observation period have exchanged workers with the focal worker's peer group. In row (iii), we report the coefficient on the peer quality of the past peers of recent joiners to the focal worker's peer group. For this we first identify the peer groups from which new joiners to the focal peer group came from (i.e., the peer groups in which the joiners were in t-1) and compute the average worker fixed effect of the workers who were in these peer groups in t-1 but who did not join the focal peer group t. In Panel C, we augment the baseline model by adding the average peer quality of workers living in the same municipality who are employed in the same occupation (but not necessarily in the same firm) as the focal worker. This equation is run on a sample for the period 1999-2010 because the indicator of the municipality of residence is only available from 1999. Bootstrapped standard errors with clustering at firm level in parentheses.

Data Source: Social Security Data, One Large Local Labor Market, 1989-2005.

Table 9: Heterogeneous peer effects (5% most repetitive occupations)

Panel A: Heterogenous Effects by age and peer group tenure

	age <= 35 years	age > 35 years
Average peer fixed effect	0.081	0.053
	(0.005)	(0.005)

	Peer group tenure<2	Peer group tenure>=2
Average peer fixed effect	0.058	0.066
	(0.007)	(0.006)

Panel B: Symmetry of Peer Effects (First Differences, Peer Group Stayers, Pre-Estimated Effects)

	Negative Change	Positive Change
Change in average peer fixed effect	0.055	0.048
	(0.010)	(0.008)

Panel C: Heterogeous Effects by Relative Position within the Peer Group

	focal worker below median	focal worker above median
Average peer fixed effect	0.066	0.032
	(0.006)	(0.006)

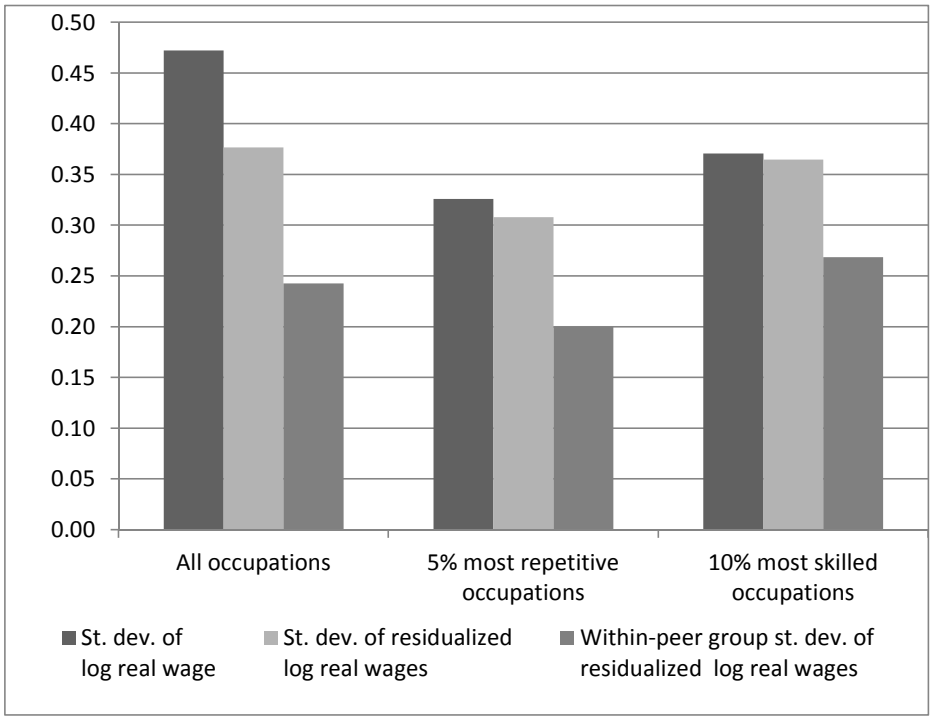
Panel D: Distinguishing between Top vs. Bottom Peers (pre-estimated)

	top vs. bottom peers
Average fixed effect of middle 80% peers	0.072
	(0.0045)
Average fixed effect of top 10% of peers	0.004
	(0.0018)
Average fixed effect of bottom 10% of peers	0.007
	(0.0017)

Note: The table investigates possible heterogeneous effects of peer quality in the 5% most repetitive sector. In Panel A we allow the effect of average peer quality on log wages to differ between workers below and above age 35, and between workers who have been in the peer group more and less than 2 years. In Panel B we investigate whether improvements and deteriorations in average peer quality have similarly sized effects. To do this, we adopt an approach similar to Mas and Moretti (2009) and regress, for peer group stayers, the change in log wages on the change in peer group quality (using the pre-estimated worker fixed effects from our baseline specification), and allow this effect to vary according to whether peer group quality improved or deteriorated. In Panel C we let the peer effect vary by whether the focal worker is above or below the peer-group mean of ability. In Panel D, we split the worker's peers up into the middle 80%, top 10% and bottom 10% according to their ability ranking. This specification is again based on pre-estimated worker fixed effects, and is run on the sample for the middle 80% of workers only.

Data Source: German Social Security Data, One Large Local Labor Market, 1989-2005.

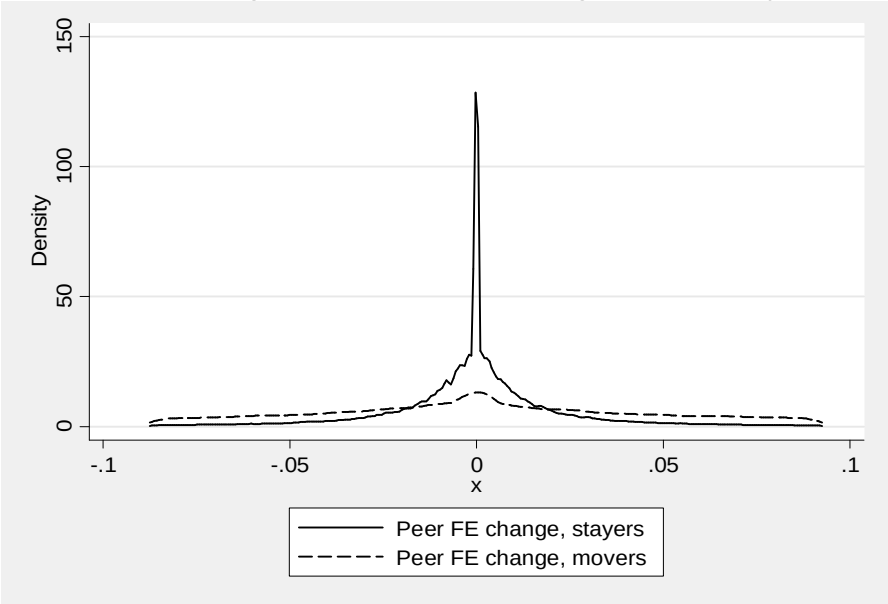
Figure 1: Variablity of Wages Across and Within Peer Groups.



Note: The figure compares all occupations (N=12,832,842), the 5% most repetitive occupations (N=681,391), and the 10% most skilled occupations (N=1,309,070) in terms of the variability of wages. Residualized wages are computed from a log-wage regression controlling for aggregate time effects, education, and quadratics in firm tenure and age.

Data Source: German Social Security Data, One Large Local Labor Market, 1989-2005.

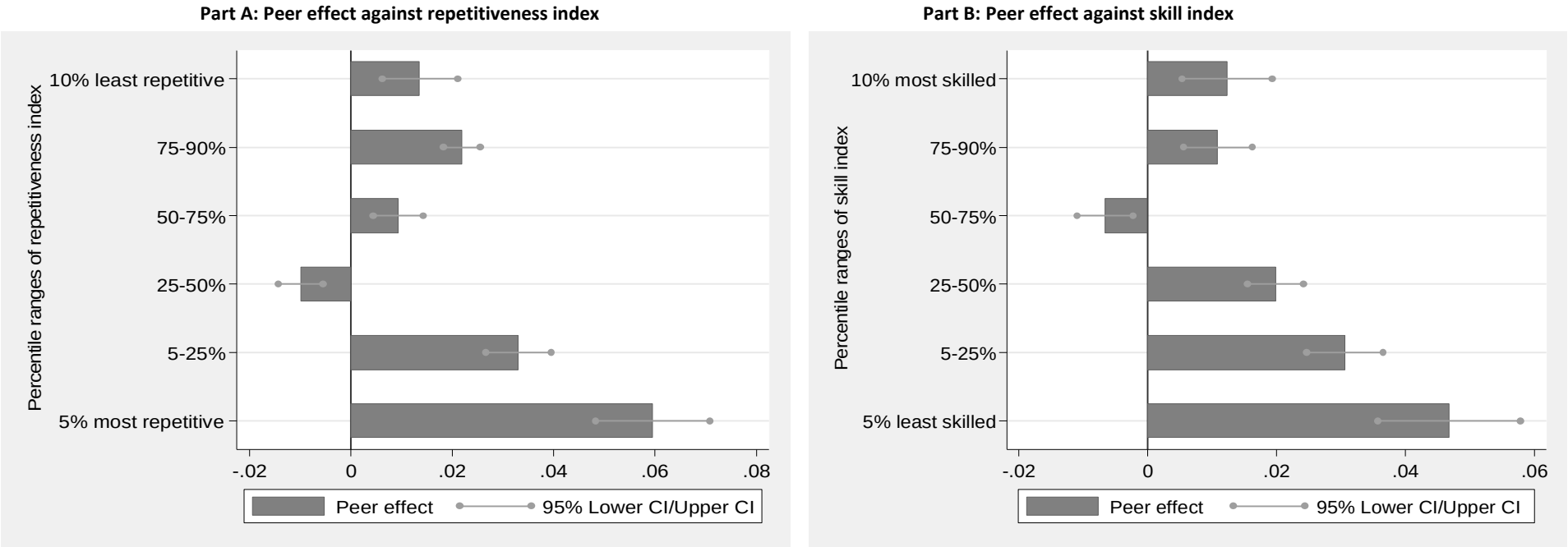
Figure 2: Variation of the Change in Peer Quality



Note: The figure plots a kernel density estimate of the change in the average peer fixed effect (FE) separately for peer group stayers and peer group movers. Peer group quality varies more strongly for movers. For stayers, there is a mass point at zero, corresponding to stayers in peer groups that had no turnover. The figure is trimmed at the 5% and 95% percentile of the distribution.

Data Source: German Social Security Data, One Large Local Labor Market, 1989-2005.

Figure 3: Additional heterogeneity of the peer efffect across bins of the repetitive and skilled index

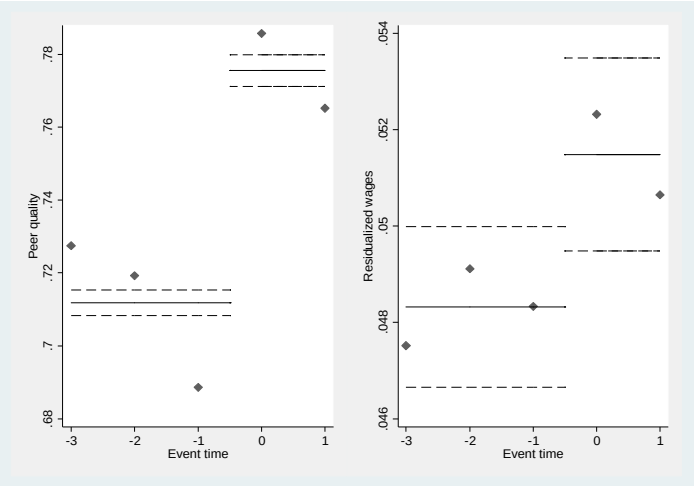


Note: The graphs plot the peer effect across bins of the repetitiveness and the skill index. The bottom bar in Part A of the figure corresponds to the 5% most repetitive occupations used in previous tables (as for example in column (1), Panel A of Table 5), and the top bar in Part B of the figure corresponds to the 10% most skilled occupations used in previous tables (as for example in column (1), Panel B of Table 5).

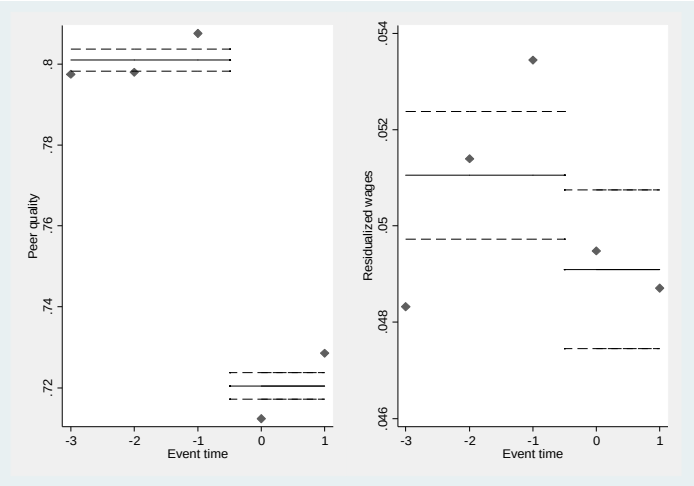
Data Source: German Social Security Data, One Large Local Labor Market, 1989-2005.

Figure 4: Wage variation induced by changes in peer quality (5% Most Repetitive Occupations)

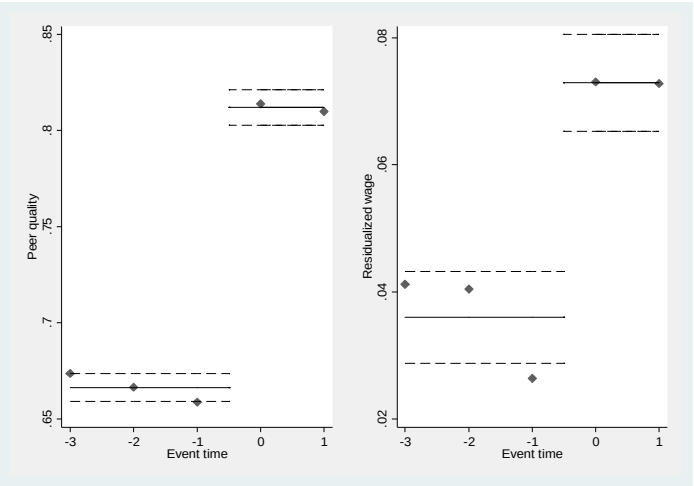
Part A: Rise in peer quality - Stayers



Part B: Fall in peer quality - Stayers



Part C: Rise in peer quality - Movers



Note: The figures show, for the 5% most repetitive occupations, the evolution of peer quality with an exceptionally large rise and fall in peer quality (greater than 0.055 from period -1 to period 0) on the left hand side, and the corresponding evolution of residualized wages for peer group stayers (in Parts A and B) in these peer groups on the right hand side. Average peer quality and residualized wages are shown three periods before and two periods after the large change in peer quality. Part C shows the evolution of peer quality and residualized wages for individuals who have moved peer group in period 0 and experienced an accompanying rise in peer quality of greater than .10, but have stayed in the same peer group in the pre and post periods. Residualised wages have been obtained by a regression of the wage level on fixed effects and observables and are purged of the observables and fixed effects included in baseline equation (4) in the text (except for peer effects, which are not netted out). Sample sizes: 3432 individuals (Part A), 326 individuals (Part B), 4989 individuals (Part C).

Data Source: German Social Security Data, One Large Local Labor Market, 5% most repetitive occupations, 1989-2005.