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Knowledge Spillovers, Competition, and Individual Careers[†]

By Thomas Cornelissen, Christian Dustmann, and Uta Schönberg*

Exposure to better peers in the workplace can influence career trajectories through two opposing channels: *positively*, *via knowledge spillovers*, *and negatively*, *through competition for advancement. We disentangle these effects by studying untrained labor market entrants and distinguishing between coworkers in the same occupation with whom they are likely to compete versus those with whom they are unlikely to compete. We find robust evidence of persistent knowledge spillovers but also identify countervailing competition effects of comparable magnitude. Both effects are more pronounced for men than for women.* (*JEL* D83, J16, J24, M53, M54, O33)

A substantial body of economic literature attributes productivity gains in the workplace to human capital accumulation (see, for example, Becker 1994; Mincer 1974; Ben-Porath 1967; Killingsworth 1982). However, the mechanisms of skill acquisition within firms remain relatively underexplored. Beyond individual learning, productive skills may also be transmitted through coworker interactions. If so, worker education and training investments could generate externalities and multiplier effects (Glaeser, Scheinkman, and Sacerdote 2003; Becker and Murphy 2000). While the notion that skills diffuse among workers is not new—Marshall (1890); Lucas (1988); Jovanovic and Rob (1989); and Glaeser (1999), among others, have articulated this idea—the empirical evidence on knowledge spillovers remains mixed.[1]

In this paper, we revisit the question of how exposure to high-quality coworkers in one's first job affects long-term career outcomes for young labor market entrants.

* Cornelissen: Department of Economics, University of Essex, RF Berlin, and Institute for Employment Research (IAB) (email: t.cornelissen@essex.ac.uk); Dustmann: Department of Economics, University College London, and RF Berlin (email: c.dustmann@ucl.ac.uk); Schönberg: Faculty of Business and Economics, University of Hong Kong, RF Berlin, and IAB (email: schoenbe@hku.hk). Authors are listed alphabetically in this paper's byline. Chinhui Juhn was the coeditor for this article. We thank the Rockwool Foundation for financial support. Dustmann acknowledges funding by ERC Advanced Grant 833861.

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[1] Workplace-level studies generally find limited evidence of knowledge spillover effects (Cornelissen, Dustmann, and Schönberg 2017; Bentsen, Munch, and Schaur 2019). These effects tend to appear more strongly in settings where shared background characteristics are less tightly controlled (Battisti 2017; Battu, Belfield, and Sloane 2003; Martins and Jin 2010; Wirz 2008; Nix 2020). Some workplace studies focused on high-skilled occupations have documented spillovers—for example, among scientific coauthors (Azoulay, Graff Zivin, and Wang 2010), teachers (Jackson and Bruegmann 2009), and patent examiners (Frakes and Wasserman 2021). However, evidence is more mixed in academic settings, where Waldinger (2012) finds no spillover effects among university faculty. Research on knowledge spillovers in lower-skilled environments is scarce, with notable exceptions including studies on call center workers by Sandvik, Saouma, Seegert, and Stanton (2020) and De Grip and Sauermann (2012). For a broader synthesis of findings across occupational field studies, see the meta-analysis by Herbst and Mas (2015).

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We argue that, even in studies employing clean identification strategies, the existing literature on coworker peer effects often overlooks a crucial countervailing mechanism: competition. Specifically, assigning a focal worker to high-quality peers may increase the likelihood that they lose out in intrafirm competition for career opportunities. Such competition may arise over promotions (Lazear and Rosen 1981; Baker, Jensen, and Murphy 1988; DeVaro 2006; DeVaro and Kauhanen 2016), retention, bonuses, wage increases based on relative performance, or access to learning opportunities.[2] Whenever career rewards are allocated based on comparative performance and are in fixed supply, peer quality can generate negative externalities through competitive pressure. This negative spillover may attenuate or even mask positive knowledge spillover effects in empirical analyses. Consequently, studies that do not account for within-firm competition may underestimate the true extent of knowledge spillover from higher-quality peers. Our first and most important contribution is, therefore, to explicitly address the bias.[3]

To disentangle the effects of peer group quality via knowledge spillovers from those driven by competition, we exploit a unique institutional feature of the German labor market: Young individuals can either enter the labor market directly or first complete a two- to three-year firm-based apprenticeship program before obtaining a regular job. We refer to the former group as "untrained" and the latter as "trained" workers. Both types often work in the same occupations, and untrained entrants may learn from trained and untrained coworkers. However, untrained entrants are unlikely to compete with trained incumbents, as they typically follow different career trajectories within the firm—an observation we confirm in our data.[4] This distinction allows us to estimate a lower bound of knowledge spillover effects by focusing on spillovers from trained incumbents. In contrast, the quality of untrained peers may generate competition effects that could offset or even outweigh the benefits of knowledge spillovers.

Our empirical strategy draws on rich German social security data, which enables us to control for the nonrandom sorting of workers into peer groups and adjust for unobserved factors affecting the future careers of labor market entrants and the quality composition of their initial coworkers. Identification comes from comparing individuals who entered the same firm and occupation but in different years and who were therefore exposed to different peer groups upon entry. We further control flexibly for occupation-specific time trends and key observable characteristics of both focal workers and their peers.

We find strong evidence for both knowledge spillover and competition effects. In a baseline analysis that does not distinguish peers by training status, average peer quality, measured by the average worker fixed effect from an AKM-style wage regression incorporating both firm and worker fixed effects, shows no systematic impact on entrants' future career outcomes. However, once we differentiate between

[2] Examples include Amazon's promotion policy, where only the top ranked get promoted and lower-performing workers are encouraged to leave the firm (e.g., Leonhardt 2021).

[3] Experimental designs that induce knowledge flows without altering coworker quality, thereby holding competitive dynamics constant as in Sandvik, Saouma, Seegert, and Stanton (2020), are less susceptible to bias from intrafirm competition.

[4] This interpretation aligns with findings in the literature showing larger spillover effects from coworkers at higher hierarchical levels (Espinosa and Stanton 2022; Hoffman and Tadelis 2021; Lazear, Shaw, and Stanton 2015; Englmaier et al. 2025; Chen, Hensel, and Yao 2025), where competition is likewise an unlikely confounding factor.

trained and untrained peers, a clearer picture emerges. High-quality trained coworkers generate long-lasting career benefits for untrained entrants, boosting their earnings and wages even 10 years later, an effect we attribute to knowledge spillover. In contrast, higher-quality untrained peers lead to persistent career disadvantages, which we interpret as evidence of increased competition. Overall, our findings suggest that the knowledge and competition effects roughly offset each other. We also find that a greater *share* of trained coworkers in the initial peer group improves entrants' career prospects, consistent with the idea that untrained labor market entrants learn from, but do not compete with, their trained coworkers.

The positive spillover effects from trained peers are most pronounced in occupations with more complex tasks, where learning opportunities are likely more important. A 1 standard deviation increase in the quality of trained peers raises earnings 5 years later by 3.7 percent, driven by both employment and wage gains. This effect persists over the 10-year follow-up period and remains significant even after conditioning on future peer quality. It also holds for individuals who switch employers, consistent with the acquisition of transferable knowledge and skills.

Negative spillovers from untrained peers also tend to be more pronounced in complex occupations, suggesting that the returns to promotion and retention are greater in these settings. The most substantial negative earnings effects originate from untrained peers early in their careers and thus at a similar career stage as the entrants. In contrast, we find no significant effects on earnings from untrained peers who are either further along in their careers or working in different occupational contexts where direct competition is less likely. High-quality untrained peers also increase the likelihood of focal workers switching jobs or occupations.

Our results also uncover important gender differences. While competition effects are present for both men and women, they are significantly larger for men. This is consistent with prior research highlighting men's greater career orientation or higher willingness to compete (e.g., Azmat and Petrongolo 2014; Niederle and Vesterlund 2011), as well as with findings that women may face lower returns from participating in promotion contests (e.g., Benson, Li, and Shue 2023; Blackaby, Booth, and Frank 2005). Similarly, knowledge spillover effects are also more pronounced for men. Women tend to benefit from high-quality trained peers primarily when they remain with their initial employer. In contrast, for men, such peers create opportunities beyond the initial firm, increasing the likelihood of employment in larger, higher-paying firms. This pattern aligns with evidence that job and occupational mobility play a more central role in men's wage growth than in women's (e.g., Loprest 1992; Fitzenberger and Kunze 2005; Hirsch, Schank, and Schnabel 2010).

As an alternative strategy to identify knowledge spillovers, we implement an approach that does not distinguish between workers by training status but instead conditions on both the average peer group quality and the worker's rank within the group. We argue that a worker's ordinal position can capture competition effects, while learning depends on the absolute level of peers' knowledge and skills. Thus, we identify knowledge spillovers through measures of absolute peer quality and competition through relative rank. Unlike our primary approach, this method requires additional identification assumptions and parametric restrictions, which we discuss in Section IIB.

Our estimates from the rank-based approach corroborate the findings from our primary strategy. We observe similarly positive effects of peer quality in the first job on earnings and wages five years later, once we control for ordinal rank. Additionally, relative rank positively influences future earnings and wages, consistent with the idea that a stronger relative position within the peer group confers an advantage in internal competition for career advancement.

We further apply the rank-based strategy to two additional groups: trained labor market entrants who have completed a two- to three-year apprenticeship training and entrants with a college degree. We find that exposure to high-quality peers during the apprenticeship generates particularly pronounced knowledge spillover and competition effects, possibly due to the formal and structured nature of such programs. While we also detect knowledge spillover and competition effects among college graduates in their first jobs, their magnitudes are much smaller.

These findings are consistent with insights from the education literature, which has highlighted the importance of rank effects among classmates or schoolmates (Murphy and Weinhardt 2020; Denning, Murphy, and Weinhardt 2023; Elsner, Isphording, and Zölitz 2021) and their potential to confound peer effects if not properly accounted for (Bertoni and Nisticò 2023). As in the workplace, rank effects in educational settings likely reflect competitive dynamics, whether explicit, for instance, when top-performing students are granted admission to elite universities under policies like Texas's Top 10 percent rule, or implicit, such as through increased attention or support from teachers for higher-ranked students.

Our paper contributes to a growing literature on within-firm competition among coworkers. In line with evidence from the Norwegian labor market, Johnsen, Ku, and Salvanes (2024) show that the departure of coworkers can advance a worker's career. Similarly, Bianchi et al. (2022) find that the delayed retirement of older workers in Italy can negatively affect the career progression of their younger colleagues.[5] We add to this literature by demonstrating that competition effects can confound positive spillovers, such as those from peer learning, and by documenting the pervasiveness of such competition effects. We show how these effects vary with the degree of substitutability between coworkers and how workers mitigate them through job and occupational mobility.

Beyond our central contribution—identifying coworker competition as a key confounding factor in estimating knowledge spillover—we also extend the empirical literature on workplace peer effects in several important ways. Prior labor market–wide studies of coworker spillovers, including our own earlier work (Cornelissen, Dustmann, and Schönberg 2017), typically examine contemporaneous effects aggregated across worker skill groups (e.g., Bentsen, Munch, and Schaur 2019; Battisti 2017; Battu, Belfield, and Sloane 2003; Martins and Jin 2010; Moretti 2004; Wirz 2008).[6] In contrast, we focus explicitly on young workers with low initial formal

[5] Related work on external labor market competition highlights similar dynamics. Lazear, Shaw, and Stanton (2018) examine the implications of competition among external job applicants for a fixed number of job slots. Borjas and Doran (2015) document adverse career effects among US academic mathematicians following the influx of Soviet mathematicians, illustrating competition from a sudden supply shock of high-quality peers. The effects of competing against superstars have also been studied in other high-skill domains, such as golf (Brown 2011) and chess (Bilen and Matros 2023), where the presence of top performers can discourage or crowd out others.

[6] Some papers that look at longer-run outcomes and past peer exposure are Jarosch, Oberfield, and Rossi-Hansberg (2021); Nix (2020); and Hong and Lattanzio (2025).

qualifications and analyze the impact of the peer quality they are exposed to in their first year in the labor market, a period when on-the-job skill acquisition is likely to be most formative.[7] We further examine long-term outcomes, allowing us to distinguish knowledge spillovers from other types of coworker peer effects, such as social pressure (Kandel and Lazear 1992; Mas and Moretti 2009; Cornelissen, Dustmann, and Schönberg 2017) or complementarities in production (e.g., teamwork).[8] In addition, we broaden the scope of the literature that has primarily focused on aggregate spillovers from average educational attainment (Acemoglu and Angrist 1999; Battu, Belfield, and Sloane 2003; Martins and Jin 2010; Wirz 2008) or college-educated peers specifically (Bentsen, Munch, and Schaur 2019; Moretti 2004; Nix 2020). Instead, we examine spillover effects from peers with firm-provided apprenticeship training, which imparts practical, occupation-specific skills that may be more easily transmitted to coworkers than the abstract knowledge acquired through university education.[9]

Finally, our alternative identification strategy, based on conditioning on a worker's ordinal rank within the peer group, complements our main approach and provides the first direct evidence that a worker's relative position among peers in their first job has significant and lasting career effects. The consistency between the results of our rank-based and training-based identification strategies suggests that conditioning on ordinal rank, under suitable identification assumptions, may offer a feasible and informative approach to estimating knowledge spillovers in institutional contexts where more detailed training-based classifications are unavailable.

I. Theoretical Framework: Coworker Competition with Knowledge Spillover

To motivate our empirical analysis, we extend a standard promotion tournament model (Lazear and Rosen 1981) by incorporating knowledge spillovers and distinguishing between coworkers who do and do not compete for promotion. Such competition naturally arises in settings where fixed job hierarchies are combined with internal hiring policies (DeVaro 2006; Lazear, Shaw, and Stanton 2018), and promotion tournaments are commonly used to incentivize effort (Lazear and Rosen 1981; Prendergast 1999). While promotion is a central example, the mechanism we emphasize here is more general: Any career opportunity within the firm that is limited in supply and allocated based on relative performance can generate similar competitive dynamics. These opportunities may include retention, bonuses, wage increases, or access to senior coworkers and task assignments that facilitate learning. We focus on "competition for promotion" throughout the model discussion for expositional clarity. Full details are provided in Supplemental Appendix A.

[7] By examining early exposure to coworkers, we also contribute to the broader literature on how initial labor market conditions shape long-term career outcomes for young workers (e.g., Arellano-Bover 2024; Arellano-Bover and Saltiel 2025; Kahn 2010; Oreopoulos, von Wachter, and Heisz 2012).

[8] Social pressure and production complementarities are mechanisms that operate only through current peers, whereas knowledge spillovers are likely to persist over time and can originate from past peers. This is because knowledge, once acquired, remains with the worker even if either the coworkers or the worker herself leave the firm.

[9] Empirical evidence on whether practical skills acquired through firm-provided training generate spillovers remains limited. One of the few studies to provide convincing evidence is De Grip and Sauermann (2012), who exploit a field experiment in a call center and show that increasing the share of trained coworkers improves the productivity of untrained workers.

A. *Setup*

Consider a setting with three workers: an untrained job starter i, an untrained incumbent j, and a trained incumbent k. There are two periods, $t = 1,2$. In period 1, knowledge spillover may occur, and there is competition for promotion between the two untrained workers, i and j. In period 2, the more productive of the two is promoted to a higher position.

The trained worker k does not participate in this promotion tournament, as she follows a distinct career path—an assumption we relax in Supplemental Appendix Section A.2. This reflects the idea that tournaments between workers with systematically different skill levels are inefficient and fail to provide optimal incentive structures (Lazear and Rosen 1981; Brown 2011; Bilen and Matros 2023; Xiao 2020).

Individual output q_w for worker w (at the end of period 1) has three components:

$$q_w = s_w + \omega_w + \epsilon_w, \quad w \in \{i,j,k\},$$

where s_w denotes the skill level of worker w, ω_w their effort, and ϵ_w an idiosyncratic productivity shock. The skill component s_w is composed of the worker's initial ability μ_w and a knowledge spillover component derived from the ability μ of more senior or more highly trained incumbent coworkers:

$$s_i = \mu_i + \lambda_j \mu_j + \lambda_k \mu_k,$$

$$s_j = \mu_j + \lambda_k \mu_k,$$

$$s_k = \mu_k.$$

Here, $\lambda_w > 0$ (for $w \in \{j,k\}$) denotes the knowledge spillover coefficient. This formulation reflects the simplifying assumption that untrained workers i, j do not generate knowledge spillovers for trained coworkers k. Knowledge spillovers are modeled as exogenous, arising through informal channels such as day-to-day social interactions and observational learning in the workplace.

B. *Workers' Maximization Problem*

In period 1, untrained workers exert effort ω to increase their probability of winning the promotion in period 2. Untrained workers have no incentive to exert effort in period 2; trained workers do not exert any effort in either period 1 or 2. Untrained workers choose their optimal effort levels in period 1 to maximize their expected wages in period 2. The expected period 2 wages of the untrained job starter and the untrained incumbent are given by

$$E[W_i] = f(s_i) + aP(q_i > q_j), \text{ and} \tag{1}$$

$$E[W_j] = f(s_j) + a\big(1 - P(q_i > q_j)\big).$$

Here, $f(s_w)$ is a positive monotone function, with $f' > 0$, that forms part of the wage contract.[10] The probability that the job starter i is promoted rather than the incumbent j is given by $P(q_i > q_j)$, and a denotes the wage spread associated with the promotion (the promotion prize). Thus, the period 2 wage consists of two components: the skill-based wage $f(s_w)$ and the expected return from promotion.

The probability that the job starter wins the promotion tournament is

$$P(q_i > q_j) = G\big(\mu_i - (1 - \lambda_j)\mu_j + \omega_i - \omega_j\big),$$

where $G(\cdot)$ is the cumulative distribution function (CDF) of $\epsilon_j - \epsilon_i$, with probability density function (PDF) $g(.)$.

The job starter chooses effort ω_i to maximize expected wages in period 2:

$$\max_{\omega_i} f(s_i) + G\big(\mu_i - (1 - \lambda_j)\mu_j + \omega_i - \omega_j\big)a - C(\omega_i),$$

yielding the first-order condition (F.O.C.):

$$g\big(\mu_i - (1 - \lambda_j)\mu_j + \omega_i - \omega_j\big)a - C'(\omega_i) = 0. \tag{2}$$

Assuming the second-order condition is satisfied, this condition implicitly defines the optimal effort level ω_i^*. The optimal effort level of the untrained incumbent ω_j^* is derived analogously; see Supplemental Appendix A.1.

The untrained job starter's expected wage at equilibrium effort level is (see Supplemental Appendix A.1 for details):

$$E[W_i] = f(\mu_i + \lambda_j\mu_j + \lambda_k\mu_k) + G\big(\mu_i - (1 - \lambda_j)\mu_j\big)a. \tag{3}$$

C. *Comparative Statics and Empirical Implications*

Untrained Incumbent Worker Quality.—Totally differentiating equation (3) with respect to the quality of untrained incumbent workers and rearranging terms yields

$$\frac{dE[W_i]}{d\mu_j} = \underbrace{\lambda_j f'\big(\mu_i + \lambda_j\mu_j + \lambda_k\mu_k\big)}_{\text{knowledge spillover effect}} - \underbrace{(1 - \lambda_j)g\big(\mu_i - (1 - \lambda_j)\mu_j\big)a}_{\text{competition effect}}. \tag{4}$$

The knowledge spillover effect and the competition effect operate in opposite directions. The positive knowledge spillover effect increases with the knowledge spillover parameter λ_j and the marginal return to skills $f'(.)$. The negative competition effect depends on the promotion prize a. If the competition effect dominates, the term $\frac{dE[W_i]}{d\mu_j}$ will be negative, thus providing a lower bound on the competition effect (i.e., the true competition effect is more negative than the estimate suggests).

[10] Since outside firms can observe s_w, for example, through skills and knowledge demonstrated in job interviews, skills form part of the workers' outside options. As a result, the current employer must compensate workers for their higher skills.

Trained Incumbent Worker Quality.—By contrast, the effect of an increase in the trained incumbent's quality μ_k on the job starter's wage does not include a competition component since the trained worker is not part of the promotion tournament:

$$\frac{dE[W_i]}{d\mu_k} = \lambda_k f'(\mu_i + \lambda_j \mu_j + \lambda_j \mu_k). \tag{5}$$

This derivative is unambiguously positive, reflecting a pure knowledge spillover effect. If we relax the assumption that the trained worker is not a competitor in the promotion tournament, the impact of the incumbent's quality on the entrant's wage can be interpreted as a lower bound on the knowledge spillover effect; that is, the true spillover effect may be even larger. Our baseline identification strategy exploits this distinction by comparing the impact of trained and untrained incumbent quality on the wages of untrained job starters, thereby disentangling the roles of knowledge spillover and competition.[11]

As an alternative identification approach, we infer the knowledge spillover effect from untrained incumbent coworkers by conditioning on the worker's relative rank in the promotion tournament. Define $rank_i = 1$ if $q_i > q_j$ and $rank_i = 0$ otherwise. Then expected wages conditional on rank are $E[W_i | rank_i] = f(s_i) + rank_i a$. Differentiating with respect to μ_j yields

$$\frac{dE[W_i | rank_i]}{d\mu_j} = \lambda_j f'(\mu_i + \lambda_j \mu_j + \lambda_k \mu_k),$$

which isolates the pure knowledge spillover effect from the untrained incumbent's quality, net of competition. We discuss this approach in more detail in Section IIB.

II. Empirical Approach

A. *Distinguishing Peers by Training Status*

Our primary identification strategy for estimating knowledge spillover effects leverages a key institutional feature of the German labor market. Within the same firms and occupations, some workers are graduates of the German apprenticeship training system ("trained"), while others enter directly without such training ("untrained").[12] Because completing an apprenticeship typically takes two to three years, trained workers in our data have already spent time in the labor market. Despite this, trained and untrained workers frequently work side by side in the same occupations and workplaces, providing opportunities for interaction and thus facilitating knowledge spillovers. At the same time, because trained and untrained workers, as

[11] Even when accounting for competition, the model predicts a clear ranking in the magnitude of effects: $\frac{dE[W_i]}{d\mu_k} > \frac{dE[W_i]}{d\mu_j}$, provided that the untrained job starter competes less with the trained incumbent than the untrained incumbent and learns more from the trained incumbent (i.e., $\lambda_k > \lambda_j$). This ordering would hold in the absence of competition effects, assuming that $\lambda_k > \lambda_j$. However, in the absence of competition, $\frac{dE[W_i]}{d\mu_j}$ could not turn negative.

[12] See Supplemental Appendix B for further details on the apprenticeship system.

we show in Section IIIB, are on different career tracks, direct competition between the two groups is likely to be minimal. This institutional setup allows us to identify knowledge spillovers from trained workers to untrained job starters with limited confounding from competition effects.

Our empirical model relates the outcomes of an untrained worker i, who enters occupation o in firm j in year t, to the quality of trained and untrained coworkers in their first job:

$$y_{i,t+\tau} = \mu + \gamma^u u_{jot} + \gamma^s s_{jot} + x'_{ijot}\beta + \omega_{ot} + \delta_{jo} + v_{i,t+\tau}. \tag{6}$$

Here, $y_{i,t+\tau}$ is a labor market outcome measured τ years after entry, and $v_{i,t+\tau}$ is an idiosyncratic error term. The variables u_{jot} and s_{jot} capture the peer quality of untrained and trained coworkers, respectively, in the same firm and occupation ("peer group"), excluding focal labor market entrants. These are averages of individual wage fixed effects from an AKM-style wage regression, normalized to mean zero and unit variance (see Section IIIB). The parameter γ^u reflects a combination of knowledge spillovers and competition from untrained coworkers, as in equation (4). Assuming no competition between trained and untrained workers, γ^s captures the knowledge spillover effect in equation (5).

To interpret γ^u and γ^S as causal, we need to account for the endogenous sorting of workers into peer groups and for shared background characteristics. To address worker sorting based on their own quality, we include the focal worker's age at labor market entry and the initial wage during the first year on the job in the control variable vector x'_{ijot}.[13]

To adjust for shared background characteristics of focal workers and their peers, we build on Cornelissen, Dustmann, and Schönberg (2017) and include initial-firm-by-initial-occupation fixed effects (δ_{jo}) and initial-occupation-by-year-of-entry fixed effects (ω_{ot}). By conditioning on occupation-by-entry-year fixed effects ω_{ot}, we allow the selection of workers into occupations to change over time. By controlling for firm-occupation fixed effects δ_{jo}, we absorb time-constant unobserved peer group characteristics that might influence worker sorting and exploit variation in peer quality across cohorts within the same firm and occupation, driven by turnover in coworkers. Such turnover could reflect either coworker entry and exit due to job creation and destruction (expanding or shrinking peer groups) or churn (worker reallocation across stable peer groups). These flows may result from factors such as firms' responses to market conditions, workers' job search decisions, or retirements. The key identification assumption is that untrained entrants do not systematically sort into peer groups based on year-to-year changes in peer quality—an assumption we view as plausible given that such workers are unlikely to observe or respond to coworker turnover.

[13] The vector x'_{ijot} further includes the average age of the peer group, the share of trained workers in the peer group, and own and average peer gender. We further control for the share of college graduates in the peer group and their average quality but do not focus on these variables, as the exposure of untrained labor market entrants to college graduates is small (1–2 percent). If a particular education group is absent in the peer group, we impute their average peer quality by the sample mean and include a dummy variable to indicate these cases. In practice, the inclusion of these additional controls has little impact on our estimates.

We probe this specification by implementing a range of robustness checks. For instance, we use past coworkers who left before the focal worker joined as placebo peers, we allow firm-occupation fixed effects to vary across three-year intervals, or we include firm-class-by-year-of-entry fixed effects to account for broader time-varying firm heterogeneity.[14]

B. *Alternative Approach*: *Controlling for Rank*

In an alternative strategy, we identify knowledge spillover effects from measures of *absolute* peer quality and competition effects from measures of a worker's *ordinal rank* within the peer group, while controlling for workers' own quality. This strategy is motivated by the following insights. First, workers compete for career opportunities with their immediate peer group rather than the broader population. Second, success in such competitions depends on relative performance, specifically, a worker's rank, rather than the magnitude by which they outperform others. Thus, the competition effect is primarily driven by a worker's ordinal position within the group, while knowledge spillovers depend on peers' absolute skill and knowledge levels. We estimate the following alternative regression:

$$y_{i,t+\tau} = \mu + \gamma p_{jot} + \theta r_{jot} + x'_{ijot}\beta + \tilde{\omega}_{ot} + \tilde{\delta}_{jo} + \vartheta_{i,t+\tau}. \tag{7}$$

Here, p_{jot} denotes the average wage fixed effect ("peer quality") of all coworkers in the same occupation (excluding focal labor market entrants), irrespective of their training status, and γ captures knowledge spillover. In turn, r_{jot} represents the ordinal rank of the focal worker among her peers, with θ capturing competition effects. As in equation (6), we include initial-occupation-by-year-of-entry and initial-firm-by-initial-occupation fixed effects ($\tilde{\omega}_{ot}$ and $\tilde{\delta}_{jo}$) and control for the same observables. Rank is based on wage residuals during the worker's first year on the job, net of experience to better isolate effort and ability.[15,16]

Average peer quality, conditional on rank, identifies the full knowledge spillover effect only if rank perfectly captures within-firm competition, an assumption difficult to satisfy in practice. The econometrician typically lacks information on the exact performance metrics used for pay, promotion, and retention decisions, as well as on the timing and scope of competitive interactions. As a result, rank is an imperfect proxy for within-firm competition, and the estimated effect of peer quality, conditional on rank, may not accurately capture knowledge spillovers.

[14] While including firm-by-year-of-entry fixed effects is feasible in principle, it severely limits identifying variation, as it requires firms to hire untrained labor market entrants into at least two occupations in the same year—a condition met by only a small number of firms in our sample.

[15] An alternative would be to base rank on worker fixed effects from a wage regression. However, using wages offers several advantages. First, wages capture time-varying productivity relevant for promotion, whereas fixed effects reflect average, long-term productivity—more suited to measuring spillovers. Second, wages reflect information observable to the employer at the time, while fixed effects may include future or unobservable components. Third, fixed effects are estimated over the worker's career and may themselves be affected by knowledge spillovers, making them unsuitable for constructing an exogenous rank measure.

[16] Consider two workers earning the same wage. One is more experienced but underperforms relative to their seniority, while the other is less experienced but performs strongly for their level. Firms are more likely to promote the latter, as higher performance conditional on experience signals greater potential.

Even with perfect performance data, separating the effects of rank, peer quality, and own ability presents econometric challenges. Identifying these effects relies on functional form assumptions about how they influence outcomes. The specification in equation (7) assumes that average peer quality, rank, and own peer quality affect outcomes linearly. Under this (or any other) parameterization, ambiguity may remain as to whether the rank effect captures nonlinearities in peer quality or vice versa.[17] For these reasons, we treat this strategy primarily as a robustness check and continue to rely on our main approach, which distinguishes between trained and untrained coworkers.

III. Data

A. *Data Description and Sample Selection*

We use German social security records provided by the Institute of Employment Research (IAB), which cover the universe of employees in regular employment (excluding civil servants, the self-employed, and military personnel) observed every year on June 30 (BeH 2010).[18] These data are well suited to our analysis, as they contain workplace-level identifiers ("firms" for simplicity) and detailed occupational codes that distinguish over 300 occupations. This granularity allows us to define peer groups by occupation within firms, settings where coworkers are likely to interact. The dataset also includes the whole workforce within each firm, enabling precise measurement of peer group characteristics and ensuring representativeness for both firms and workers. Crucially, it distinguishes between workers who have and have not completed apprenticeship training before entering the labor market—our "trained" and "untrained" groups. The longitudinal structure further allows us to track workers, their coworkers, and firms over time.

Our baseline sample includes all untrained labor market entrants aged 15–25—defined as those holding neither a vocational nor university degree—who began their first full-time job in West Germany between 1984 and 1998. We follow these workers for up to 10 years to assess whether the quality of their initial coworkers influences long-term outcomes. Table 1, panel A summarizes the sample: 774,701 untrained job starters across 206,861 firms and 329 occupations.

We also construct a subsample of untrained entrants who start in occupations with more complex tasks, where knowledge spillovers are likely to be more relevant. This is based on the 1991–1992 wave of the Qualification and Career Survey (BIBB/IAB 1995; see also Gathmann and Schönberg 2010), which includes detailed data on task frequency. We classify occupations as "complex" if they fall within the 25 percent of workers reporting the lowest incidence of predefined tasks. This yields a subsample of 172,242 focal workers in 33,023 firms across 63 occupations. Our main results

[17] Rank is defined as the percentile of the peer quality distribution corresponding to the focal worker's own quality. Given the distribution and a fixed level of own quality, rank is fully determined, leaving no independent variation. As a result, conditional on all features of the peer quality distribution, own ability fully determines rank, making it impossible to nonparametrically identify the separate effects of own ability, rank, and peer quality.

[18] These administrative social security records are processed and kept by IAB, according to the German Social Code III. There are certain legal restrictions due to the protection of data privacy. The data contain sensitive information and therefore are subject to the confidentiality regulations of the German Social Code (Book I, Section 35, Paragraph 1). Further information on data access is contained in the replication package to this article.

TABLE 1—DESCRIPTIVE STATISTICS

	Overall sample (1)	Complex occupations (2)
Panel A. Sample characteristics		
Number of unskilled focal workers	774,701	172,242
Number of firms	206,861	33,023
Number of occupations	329	63
Number of firm-occupation cells	275,862	37,455
Panel B. Untrained job starters		
Average age at initial job	21	20
Female	0.41	0.44
Employed in $t + 5$	0.43	0.50
Daily earnings in euros in $t + 5$ (if not employed $= 0$)	25	30
Same firm in $t + 5$ (conditional on employment)	0.49	0.55
Same occupation in $t + 5$ (conditional on employment)	0.55	0.58
Panel C. Career progression of firm stayers		
Share moved $\geq$ fifth percentile of firm's wage distribution by $t + 5$	0.28	0.29
Move to higher-paying occupations	0.06	0.06
Exceeded coworkers' annual wage growth by 15 percentage points at least once by $t + 5$	0.22	0.20
Panel D. Peer group characteristics		
Standard deviation of peer quality, trained peers	0.19	0.16
Standard deviation of peer quality, untrained peers	0.20	0.14
Manufacturing	0.40	0.80
Construction	0.09	0.02
Services	0.44	0.15
Other	0.08	0.03
Median peer group size	10	23
AKM log wage establishment fixed effect in $t + 5$	1.12	1.17
Coworker share of untrained	0.35	0.44
Coworker share of trained	0.63	0.55
Coworker share of university graduates	0.02	0.01
Share with ratio of experienced trained to untrained workers < 1	0.46	0.58
Coworker share of females	0.37	0.39

Notes: The table reports descriptive statistics for the overall sample of all initial occupations of untrained labor market entrants (column 1) and for a subsample of initial occupations with more complex tasks (column 2).

Source: German social security records from the *Beschäftigtenhistorik* (BEH)

are robust to alternative cutoffs for defining task complexity (see Supplemental Appendix Table D.7).

As is common in administrative data, wages are top-coded at the social security contribution ceiling. Since we focus on untrained entrants, only about 1 percent of wage observations are affected. We address this censoring using the imputation procedure described in Cornelissen, Dustmann, and Schönberg (2017), using the upper earnings limits provided by the Research Data Centre of the German Federal Employment Agency (n.d.).

B. *Peer Group Definition and Measures of Peer Group Quality*

We define peer groups as all workers employed in the same firm and three-digit occupation—the most granular occupational classification available in the social security data. Since we focus on untrained job starters, the sample is concentrated in predominantly low-skilled occupations such as warehouse workers,

gardeners, salespeople, assembly workers, construction workers, and office assistants (see Supplemental Appendix Table D.1 for the 20 most common occupations). Consequently, the share of college graduates in peer groups is negligible (2 percent). Instead, coworkers are almost exclusively either untrained peers with no formal qualification (35 percent) or trained peers with an apprenticeship degree (63 percent)—see Table 1, panel D. These two groups correspond to the untrained and trained incumbents in our theoretical model.

To construct measures of worker quality, we use a sample of all full-time, regularly employed workers in West Germany from 1984 to 1998—comprising 260 million observations on 33 million workers across 2.6 million firms. We estimate an AKM-style wage regression with worker (a_i) and firm (θ_j) fixed effects:[19]

$$\ln(w)_{it} = a_i + \theta_j + \vartheta_t + \varepsilon_{it}. \tag{8}$$

We then retain the estimated worker fixed effects, $\hat{a}_i$, and use them to compute the average peer quality of untrained, trained, and all peers, u_{jot}, s_{jot} and p_{jot} in equations (6) and (7). These peer quality measures are standardized to have a mean of 0 and a standard deviation of 1. As a result, estimated coefficients represent the effect of a 1 standard deviation increase in peer quality. In our sample, the standard deviation of peer quality is approximately 0.2 for both untrained and trained workers, so the estimated effects can also be interpreted as reflecting a 20 percent increase in peer quality.

Since peer quality is based on preestimated worker fixed effects, the spillover parameters γ^u and γ^s in equation (6) could potentially be biased due to sampling error or a reflection problem. In Section IVA, we present several pieces of evidence suggesting that any such bias is likely small.

Table 1 presents descriptive statistics for our estimation sample. As shown in panel B, focal workers are on average 21 years old, and just over 40 percent are female. Roughly 40–50 percent remain employed 5 years after labor market entry, and among those, about half have switched firms during that period.[20] Conditional on being employed, about half of the focal workers have switched firms within the first five years.

Panel C reports information on career progression during the first five years for workers who remain with their initial employer. Although we do not observe promotions directly, we proxy career advancement using movements within the firm's wage distribution and changes in occupational status. In complex occupations, 29 percent of untrained labor market entrants have reached at least the fifth decile of the firm's wage distribution of trained and untrained workers after 5 years, and 6 percent have transitioned into a higher-paying occupation within the firm. Additionally, 20 percent have experienced at least one instance of unusually high wage growth, exceeding

[19] We purge $\ln(w)$ of age effects using a cross-sectional OLS regression and use the residual as the dependent variable in the AKM model. Since our main specification includes average peer age as a control, removing age effects from the fixed effects allows for a cleaner interpretation. Specifically, the coefficient on average peer age captures spillovers from older, more experienced coworkers, while the coefficient on the average peer wage fixed effect (net of age) captures spillovers from peer quality, adjusted for age.

[20] The relatively low employment rate may reflect several factors. First, our focus on low-skilled workers, who tend to have weaker labor market attachment, makes unemployment spells more likely. Second, the sample consists of young workers who may transition in and out of employment due to job shopping, search unemployment, or life events such as childbearing (for women) or military service (for men).

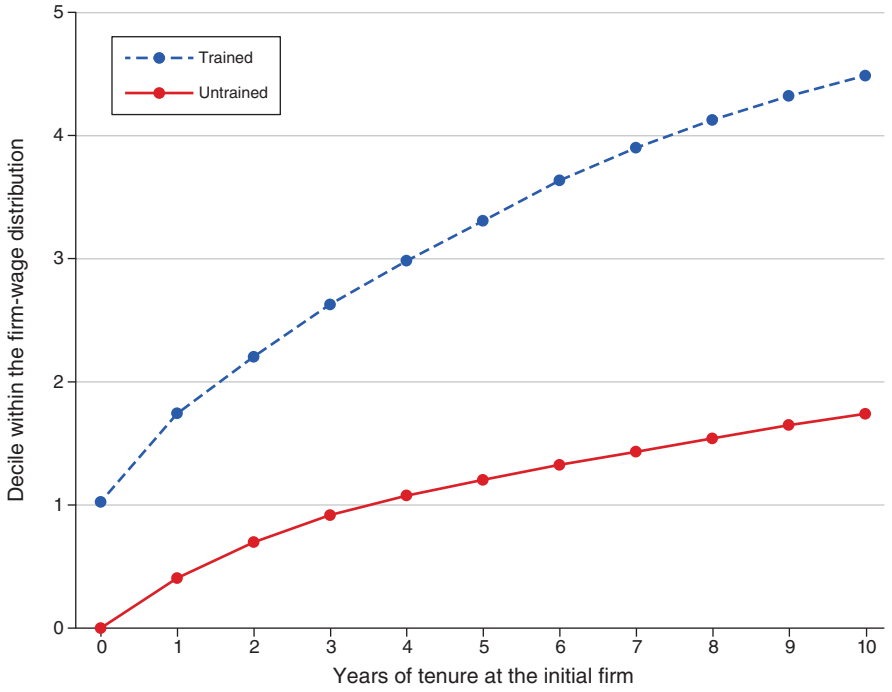


FIGURE 1. CAREER PROGRESSION OF UNTRAINED AND TRAINED WORKERS IN THE INITIAL FIRM (STAYERS)

Notes: The figure plots the evolution of workers' positions within the deciles of the firm-specific wage distribution, relative to the starting decile of untrained workers. For all firms in our sample, we partition the within-firm wage distribution (excluding college graduates) each year into deciles and compute each worker's decile position. The starting difference (at zero years of tenure) between trained and untrained workers is derived from a regression (reported in Supplemental Appendix Table D.6) of the decile position at labor market entry on an indicator variable for whether a worker is trained versus untrained, controlling for firm-by-occupation-by-year effects. Using the sample of stayers at their initial firm, the slopes of the tenure profiles are then derived from a regression of the decile position on interactions of the trained dummy with dummy variables indicating the years of tenure from 1 to 10, conditional on worker fixed effects.

Source: German social security records from the *Beschäftigtenhistorik* (BEH)

that of their coworkers by at least 15 percentage points—a proxy for promotion proposed by Bronson and Skogman Thoursie (2021).

Panel D of Table 1 shows that the initial firms of untrained entrants are roughly evenly split between manufacturing (40 percent) and services (44 percent). In the sample of complex occupations, the share of manufacturing rises to 80 percent. The median peer group size is 10 in the full sample and 23 in complex occupations. Peer groups in complex occupations are in firms that pay higher wages on average, with AKM firm fixed effects about 5 percent higher, likely reflecting their concentration in manufacturing.

Our identification strategy rests on the assumption that untrained job starters are more likely to compete for career opportunities with untrained peers than with trained peers, who typically follow a different career path within the firm. Figure 1 provides empirical support for this assumption. It compares the career progression of trained and untrained workers who begin their first job in the same firm, occupation, and year, by tracking their positions in the firm's wage distribution, divided into deciles. Trained entrants start about 1 decile higher than untrained ones; after 10

Table 2—The Effects of Peer Characteristics in the First Job on Future Earnings and Wages

	Overall sample		Complex occupations	
	Earnings, rel. to mean, $t+5$	Log wage, $t+5$	Earnings, rel. to mean, $t+5$	Log wage, $t+5$
	(1)	(2)	(3)	(4)
Panel A. Overall peer quality				
Average quality of peers	−0.032	0.001	−0.015	0.016
	(0.005)	(0.004)	(0.014)	(0.007)
Average peer age	0.004	−0.001	0.004	−0.0002
	(0.001)	(0.0003)	(0.002)	(0.001)
Share of trained workers	0.164	0.019	0.101	0.028
	(0.014)	(0.007)	(0.036)	(0.015)
Panel B. Quality of untrained versus trained peers				
Quality of untrained peers	−0.036	−0.007	−0.053	−0.013
	(0.004)	(0.002)	(0.010)	(0.005)
Quality of trained peers	0.0005	0.007	0.037	0.019
	(0.004)	(0.002)	(0.010)	(0.005)
Average peer age	0.004	−0.001	0.003	−0.0002
	(0.001)	(0.0003)	(0.002)	(0.001)
Share of trained workers	0.174	0.029	0.088	0.036
	(0.020)	(0.009)	(0.044)	(0.017)
Number of observations	774,701	329,649	172,242	85,963

Notes: The table shows the effects of various characteristics of initial peers (i.e., coworkers in the same firm and occupation at labor market entry) on the earnings and wages of untrained workers five years after labor market entry. Average peer quality is standardized and measured by the average AKM wage fixed effects of coworkers. In panel A, peer quality is averaged over all peers; in panel B, we distinguish between untrained and trained peers. Earnings are set to zero for the nonemployed and are divided by mean earnings. All specifications control for initial occupation × cohort effects, initial firm × occupation effects, the coworker share of college-educated workers, the average quality of college-educated coworkers (in panel B), the coworker share of females, the wage in the initial job, age at labor market entry, and sex. Standard errors are clustered at the firm level.

Source: German social security records from the *Beschäftigtenhistorik* (BEH)

years, this gap widens to nearly 3 deciles, consistent with the view that trained and untrained workers follow distinct advancement paths within the firm.

IV. Results

A. *Identifying Knowledge Spillover and Competition Effects*

Baseline Results.—Table 2 presents our baseline results, focusing on labor market earnings and daily wages (in logs) five years after labor market entry. We impute zero earnings for nonemployed individuals, so the earnings measure captures both wage and employment effects. To ease interpretation, we normalize earnings by their sample mean.

In panel A, we pool trained and untrained peers, reflecting the typical specification in studies that ignore competition effects. We find no clear evidence of positive spillovers from higher-quality peers encountered in the first job. Average peer quality has a negative effect on earnings and no effect on wages five years later. When restricting the sample to complex occupations in columns 3 and 4, where the learning potential should be greater, peer quality positively affects wages but still has no effect on earnings. Taken at face value, these results suggest no consistent evidence of positive spillovers from peer quality among untrained young workers.

However, we observe positive spillover effects stemming from observed characteristics that are linked to the overall quality of the peer group. A 10 percentage point rise in the share of trained peers in the initial job results in a 1.6 percent increase in earnings 5 years later across all occupations and a 1 percent increase in earnings for complex occupations. Similarly, a 1-year increase in the average age of peers leads to a 0.4 percent increase in earnings in both samples. These positive effects are indicative of knowledge spillover and are unlikely to be contaminated by a competition effect, as inexperienced, untrained labor market entrants are not expected to compete with trained and older coworkers for career advancement.

In panel B, we distinguish peer quality by training status, as in equation (6). The effects now diverge clearly: In the overall sample, peer quality among untrained workers has a negative impact on both earnings and wages, while peer quality among trained workers has a positive effect on wages (though not on earnings). As shown in equation (4), the impact of untrained peer quality combines knowledge spillovers and competition. The negative overall effect implies that competition outweighs any positive spillovers. We refer to the effect of untrained peer quality as the competition effect for simplicity, even though it represents a lower bound. Similarly, we refer to the positive impact of trained peers as a knowledge spillover effect, though it may also include a (likely smaller) competition component.

Both patterns, and specifically knowledge spillover, become stronger when we focus on complex occupations.[21] For instance, while a 1 standard deviation increase in trained peer quality has little effect on earnings in the full sample, it raises earnings by 3.7 percent in complex occupations. This is consistent with the idea that learning opportunities and skill requirements are more pronounced in such settings. For these reasons, we focus the remainder of the paper on complex occupations.

Figure 2 shows how the positive spillover from trained peers and the negative spillover from untrained peers in complex occupations evolve. Both effects are persistent. If anything, the positive impact from trained peers (which primarily captures knowledge spillover) increases slightly over time, consistent with the idea that skills build on each other.

The figure also includes placebo estimates based on "noninteracting" peers: trained and untrained workers who were employed in the same firm and occupation but had already left before the focal worker joined. Reassuringly, the quality of these placebo peers does not significantly affect earnings, with estimates close to zero.[22]

To further investigate whether the negative effect of untrained peer quality reflects competition, we decompose peer quality by experience level for both trained and untrained peers (Table 3). We hypothesize that job starters are more likely to compete with similarly inexperienced coworkers than with more senior peers. The estimates confirm this for earnings; the negative effect is most substantial from untrained peers with up to 3 years of experience—a 1 standard deviation increase in their quality reduces earnings by 7.1 percent. In contrast, the reduction is only

[21] As shown in Supplemental Appendix Table D.7, this conclusion is robust to alternative definitions of complex occupations.

[22] Confidence intervals for placebo peer quality are omitted from the figure for clarity. All placebo effects are statistically insignificant.

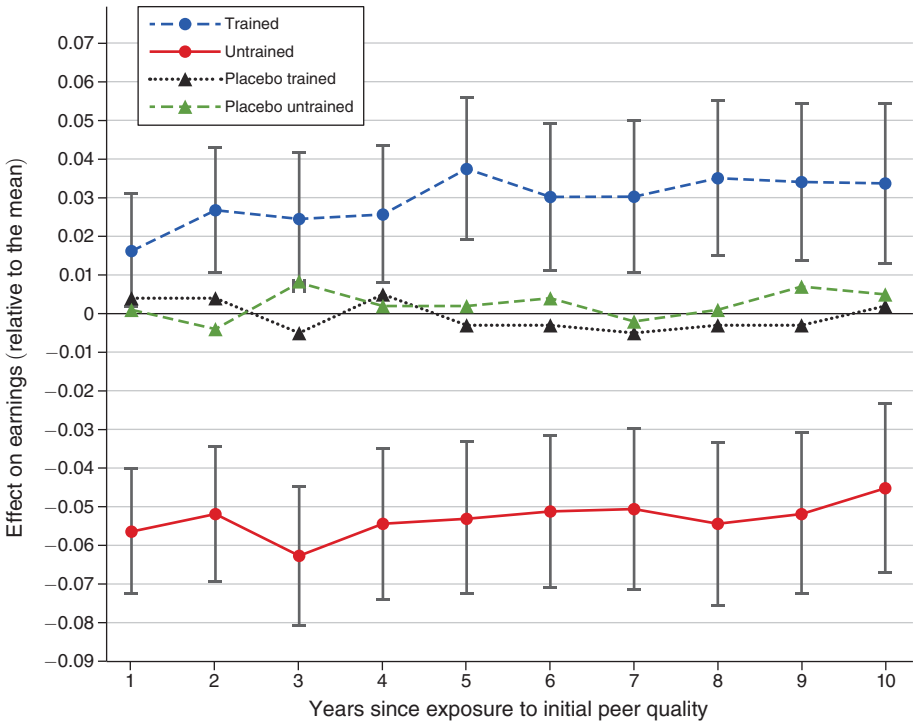


FIGURE 2. THE EFFECTS OF THE QUALITY OF TRAINED AND UNTRAINED PEERS IN THE FIRST JOB ON EARNINGS OVER TIME (COMPLEX OCCUPATIONS)

Notes: The figure plots the effects of (standardized) initial peer quality (at labor market entry) of actual and placebo coworkers on earnings in complex occupations over time, up to 10 years after labor market entry, distinguishing between peer quality of trained and untrained coworkers. Peers are coworkers in the same initial firm and occupation. Placebo peers are made up of workers who were in the same peer group two years before the focal worker joined but left the peer group the year before the focal worker joined, precluding any overlap with the focal worker. Average peer quality is standardized and measured by the average AKM wage fixed effects of coworkers; a 1 unit increase in standardized average peer quality corresponds to an increase in average peer quality of approximately 20 percent. The regressions control for initial occupation × cohort effects, initial firm × occupation effects, the coworker shares of trained and college-educated workers, the average quality of college-educated coworkers, the coworker share of females, the wage in the initial job, age at labor market entry, and sex. Earnings are set to zero for the nonemployed and are divided by mean earnings. Confidence intervals are based on standard errors clustered at the firm level. Confidence intervals for the placebo peer quality are not shown to keep the figure clearly legible. All placebo effects are statistically insignificant.

Source: German social security records from the *Beschäftigtenhistorik* (BEH)

1.8 percent and is statistically insignificant for untrained peers with at least 9 years of experience.

Spillover effects from trained peers are relatively stable across experience levels, suggesting largely homogeneous knowledge spillovers. No clear pattern is expected for these peers; while job starters may find learning easier from younger peers, more experienced workers may possess greater knowledge.

Robustness Checks.—The pattern of negative spillovers from untrained peers and positive spillovers from trained peers is highly robust, as shown in Table 4. For reference, row (i) presents our baseline estimates. In row (ii), we flexibly control for initial firm size. Row (iii) adds initial-firm-class-by-year-of-entry fixed effects,

TABLE 3—THE EFFECTS OF THE QUALITY OF UNTRAINED AND TRAINED PEERS BROKEN DOWN BY PEER EXPERIENCE (COMPLEX OCCUPATIONS)

	Earnings, rel. to mean, $t+5$				Log wages, $t+5$			
		By peers' years of experience				By peers' years of experience		
	Baseline	≤ 3 yrs	4–9 yrs	>9 yrs	Baseline	≤ 3 yrs	4–9 yrs	>9 yrs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Quality of untrained peers	−0.058	−0.071	−0.032	−0.018	−0.012	−0.015	−0.011	−0.012
	(0.010)	(0.011)	(0.013)	(0.013)	(0.005)	(0.006)	(0.006)	(0.006)
Quality of trained peers	0.03	0.027	0.035	0.036	0.019	0.017	0.02	0.02
	(0.009)	(0.010)	(0.011)	(0.011)	(0.005)	(0.005)	(0.005)	(0.005)
Number of observations	172,242		172,242		85,963		85,963	

Notes: The table shows the effects of the (standardized) quality of initial peers (i.e., untrained and trained coworkers in the same firm and occupation at labor market entry) on untrained job starters' earnings and wages five years after labor market entry. Peer quality is measured by the average AKM wage fixed effects of coworkers, broken down by the training status and labor market experience of the peers. All specifications control for initial occupation × cohort effects, initial firm × occupation effects, the average coworker age, the coworker shares of trained and college-educated workers, the average quality of college-educated coworkers, the coworker share of females, the wage in the initial job, age at labor market entry, and sex. Earnings are set to zero for the nonemployed and are divided by mean earnings. Standard errors are clustered at the firm level.

Source: German social security records from the *Beschäftigtenhistorik* (BEH)

TABLE 4—ROBUSTNESS OF THE EFFECTS OF THE QUALITY OF UNTRAINED AND TRAINED PEERS IN THE FIRST JOB (COMPLEX OCCUPATIONS)

	Earnings, rel. to mean, $t+5$		Log wage, $t+5$	
	Untrained peers	Trained peers	Untrained peers	Trained peers
(i) Baseline	−0.053	0.037	−0.013	0.019
	(0.010)	(0.010)	(0.005)	(0.005)
(ii) Control for 8 firm size categories of intial firm	−0.054	0.037	−0.013	0.018
	(0.010)	(0.010)	(0.005)	(0.005)
(iii) Add firm-class by cohort FE	−0.056	0.034	−0.017	0.016
	(0.010)	(0.010)	(0.005)	(0.005)
(iv) Add triannual peer group FE	−0.05	0.018	−0.028	0.017
	(0.017)	(0.015)	(0.011)	(0.009)
(v) Drop large groups with >50 peers	−0.053	0.029	−0.02	0.012
	(0.011)	(0.010)	(0.006)	(0.005)
(vi) Starting age ≥ 20	−0.081	0.033	−0.026	0.021
	(0.016)	(0.014)	(0.009)	(0.008)
(vii) Peers with FE estimated on 4 or more observations	−0.05	0.041	−0.012	0.019
	(0.010)	(0.010)	(0.005)	(0.005)
(viii) Fixed effects shrunk	−0.05	0.039	−0.01	0.018
	(0.011)	(0.010)	(0.005)	(0.004)
(ix) Over last 3 cohorts, 1996–1998	−0.095	0.034	−0.039	0.009
	(0.051)	(0.043)	(0.028)	(0.040)

Notes: The table reports several robustness checks for the baseline effects of the (standardized) quality of initial peers (i.e., untrained and trained coworkers in the same firm and occupation at labor market entry) on the earnings and wages of untrained workers five years after labor market entry. Earnings are set to zero for the nonemployed and are divided by mean earnings. All specifications control for initial occupation × cohort effects, initial firm × occupation effects, the average coworker age, the coworker shares of trained and college-educated workers, the average quality of college-educated coworkers, the coworker share of females, the wage in the initial job, age at labor market entry, and sex. Earnings are set to zero for the nonemployed and are divided by mean earnings. Standard errors are clustered at the firm level.

Source: German social security records from the *Beschäftigtenhistorik* (BEH)

where firm classes are defined by the ventiles of the preestimated firm fixed effect distribution, interacted with entry-year dummies, capturing time-varying shocks at

the firm-class level. In row (iv), we allow the initial-firm-by-occupation fixed effect (δ_{jo}) to vary across three-year intervals—a demanding specification that restricts identification to comparisons among job starters entering the same firm-occupation within a short window. In row (v), we exclude peer groups with more than 50 members, where knowledge sharing or competition may be less well-defined.[23] Across these specifications, estimates remain very similar.

Row (vi) excludes job starters under age 20, who may be students in temporary jobs. While these jobs can offer learning opportunities, competition is likely weaker since these workers have not yet begun full labor market careers. Consistent with this, negative spillovers from untrained peers become slightly stronger, while positive spillovers from trained peers remain essentially unchanged.

Because peer quality is constructed from preestimated worker fixed effects, measurement error could attenuate estimated coefficients. This concern is greater when fixed effects are based on short work histories. In row (vii), we drop coworkers whose fixed effects are based on three or fewer observations; estimates remain nearly identical, suggesting that this does not drive our results.

In row (viii), we remove excess sampling variation from the worker fixed effects by shrinking them toward their sample mean in proportion to the signal-noise ratio implied by the sampling error in the first-step AKM regression of equation (8).[24] Estimates using these shrunk fixed effects are virtually unchanged.

In principle, the quality of untrained focal workers could affect their coworkers' estimated wage fixed effects through reverse peer effects—a potential reflection problem. However, this concern is unlikely in our setting, as focal workers are untrained job starters with no prior work experience. To further assess this, we restrict the sample to untrained entrants from the last three years of the observation period (1996–1998). Since worker fixed effects are estimated over 1984–1998, these later cohorts could influence peer fixed effects for at most 3 years, compared to up to 15 years for 1984 entrants. If our estimates were driven by reflection, we would expect substantially smaller effects in these cohorts. As shown in row (ix), this is not the case.[25]

Alternative Rank Strategy.—As an alternative approach, we estimate spillover effects from combined trained and untrained peers, conditioning on the focal worker's rank within the peer group, as in equation (7). Columns 1 and 3 of Table 5 replicate the findings from panel A of Table 2. As before, we find no consistent pattern; peer quality has a mildly negative effect on future earnings and a mildly positive effect on wages. However, once we condition on rank, the effects of peer quality become substantially stronger. A 1 standard deviation increase in peer quality raises earnings and wages 5 years after entry by 7 percent and 2.6 percent, respectively. These results underscore the importance of conditioning on rank in uncovering

[23] Results continue to hold when further restricting to smaller peer groups.

[24] We run 200 bootstrap iterations of the first-step AKM model (equation (8)), clustering at the firm level, to obtain standard errors for the worker fixed effects. Using these, we shrink the fixed effects following equation (15) in Walters (2024). To generate the estimates in row (viii), we recompute peer quality using the shrunk fixed effects and reestimate our baseline regression.

[25] This robustness check also addresses the related concern that common peer group–cohort shocks to future wage growth could be absorbed into coworkers' AKM wage effects when these are estimated using future periods.

TABLE 5—THE EFFECTS OF PEER QUALITY IN THE FIRST JOB ON FUTURE LABOR MARKET OUTCOMES WHEN CONTROLLING FOR RANK (COMPLEX OCCUPATIONS)

	Earnings, relative to mean, $t+5$		Log wages, $t+5$	
	(1)	(2)	(3)	(4)
Average peer quality	−0.015	0.07	0.016	0.026
	(0.014)	(0.014)	(0.007)	(0.008)
Rank in peer group		0.157		0.017
		(0.007)		(0.003)
Number of observations	172,242	172,242	85,963	85,963

Notes: The table shows the effect of the (standardized) quality of initial peers (i.e., coworkers in the same firm and occupation at labor market entry) on the earnings and wages of untrained workers five years after labor market entry, with and without controlling for the rank of untrained workers among their initial peers. Average peer quality is measured as the average AKM wage fixed effect. Average peer quality is standardized; a 1 unit increase in standardized peer quality corresponds to an increase of approximately 20 percent. The untrained worker's rank among their initial peers is based on residualized wages net of experience to capture current performance relative to expectations based on seniority. Rank is standardized; a 1 unit increase in standardized rank corresponds to an increase in rank of about 30 percentiles. Earnings are set to zero for the nonemployed and are divided by mean earnings. All specifications control for initial occupation × cohort effects, initial firm × occupation effects, the average coworker age, the coworker shares of trained and college-educated workers, the coworker share of females, the wage in the initial job, age at labor market entry, and sex. Standard errors are clustered at the firm level.

Source: German social security records from the *Beschäftigtenhistorik* (BEH)

knowledge spillover effects that may otherwise be overshadowed by competition. The estimates derived from the rank strategy are stronger than the positive spillovers from trained peers in our baseline strategy, possibly indicating that the latter underestimates the true knowledge spillover.

We also find that rank in the first job has a strong and lasting impact on labor market outcomes. A 1 standard deviation increase in rank, equivalent to moving roughly 30 percentiles up in the peer group's wage distribution, increases earnings by 15.7 percent and wages by 1.7 percent after 5 years. This aligns with the view that a better relative position among peers confers an advantage in within-firm competition for promotion and retention, improving long-term outcomes.[26] As we show in Section IVD (Table 8), this pattern also holds across all occupations.

B. *Anatomy of the Competition Effect*

To better understand the mechanisms behind the competition effect, we examine how the quality of untrained incumbents affects employment and job mobility among untrained labor market entrants (panel A, Table 6). A 1 standard deviation increase in untrained peer quality reduces the probability of employment 5 years later by 2.1 percentage points, suggesting that part of the earnings effect in Table 2 operates through the extensive employment margin. Moreover, workers exposed to higher-quality untrained peers are more likely to be employed in a different firm

[26] Interestingly, the rank effect on earnings is substantially larger than the effect on wages. In additional results (not shown in the table), we find that a 1 standard deviation increase in rank raises the probability of employment 5 years after entry by 6 percentage points, which helps explain the stronger effect on earnings. Since separation from the initial firm is a key reason for nonemployment, this suggests that the competition effect may be driven, at least in part, by competition for retention.

Table 6—The Effects of the Quality of Untrained Peers in the First Job: Mechanisms (Complex Occupations)

	Employed (1)	Moved firm (2)	Moved occupation (3)
Panel A. Mobility, $t+5$			
Quality of untrained peers	−0.021	0.028	0.029
	(0.005)	(0.009)	(0.009)
Number of observations	172,242	85,963	85,963
	All	Stayers	Movers
Panel B. log wages, $t+5$, movers versus stayers			
Quality of untrained peers	−0.013	−0.008	−0.007
	(0.005)	(0.006)	(0.008)
Number of observations	85,963	47,003	38,960

	Move to $\geq$ fifth decile (1)	Change in the occupational hierarchy (2)	Change in occupational log mean wage (3)	Wage growth rel. to coworkers $\geq$ 15pp (4)
Panel C. Promotion outcomes (*stayers*)				
Quality of untrained peers	−0.024	−2.201	−0.004	0.007
	(0.014)	(1.038)	(0.002)	(0.015)
Number of observations	47,003	46,984	46,984	47,003

	Peers in own occupation (1)	Peers in other occupations (2)
Panel D. Earnings, $t+5$, *peers in the same versus different occupations*		
Quality of untrained peers	−0.053	0.005
	(0.010)	(0.007)
Number of observations	172,242	172,242

	Earnings, $t+5$	Log wages, $t+5$
Panel E. Earnings, $t+5$, *high versus low "promotion" prize* (*wage spread in firm-occupation*)		
Quality of untrained peers	−0.019	−0.009
× below-median promotion prize	(0.013)	(0.006)
Quality of untrained peers	−0.074	−0.015
× above-median promotion prize	(0.011)	(0.006)
Number of observations	172,242	85,963

Notes: The table shows the effects of the (standardized) quality of initial untrained peers (i.e., untrained coworkers in the same firm and occupation at labor market entry) on various career outcomes of untrained workers five years after labor market entry. Peer quality of untrained peers is measured by the average AKM wage fixed effects of untrained coworkers. Earnings are set to zero for the nonemployed and are divided by mean earnings. All specifications control for initial occupation × cohort effects, initial firm × occupation effects, the average coworker age, the coworker shares of trained and college-educated workers, the average quality of trained and college-educated coworkers, the coworker share of females, the wage in the initial job, age at labor market entry, and sex. Column 1 in panel C additionally controls for the worker's starting decile in the firm wage distribution. Promotions in panel C are proxied as follows: a dummy variable for whether the worker has moved to at least the fifth decline in the firm's wage distribution (calculated excluding college graduates) by the fifth year since labor market entry (column 1); the absolute change in the worker's occupational ranking between year 5 and labor market entry, where occupations are ranked by the average occupational wage in a reference year (column 2); the worker's change in the occupational log mean wage measured in a reference year between year 5 and labor market entry (column 3); a dummy variable for whether the worker has experienced annual wage growth exceeding the average wage growth of coworkers in the firm by at least 15 percentage points at least once over the 5 years since labor market entry (column 4). The sample in panel C is restricted to untrained workers who have remained employed in their initial firm for at least five years. Standard errors are clustered at the firm level.In panel E, the promotion prize is proxied by the ninetieth–tenth percentile wage gap of untrained workers in the firm-occupation cell.

Source: German social security records from the *Beschäftigtenhistorik* (BEH)

or occupation after five years, consistent with the idea that competition raises the likelihood of nonretention or voluntary exit due to limited advancement prospects. Our alternative rank strategy corroborates these findings (panel A, Supplemental Appendix Table D.2), where the rank coefficient, interpreted as an inverse competition effect, shows similar patterns.

In panel B, we further investigate the effects of the quality of untrained peers on wages separately for workers who remain employed at their initial firm (stayers) and workers who have moved to another firm (movers). We find similar (and statistically insignificant) effects for both groups.

Panel C examines whether exposure to high-quality, untrained peers affects the promotion chances of those still employed at their initial firm after five years. We use several proxies for promotion: reaching at least the fifth decile of the firm's wage distribution (achieved by 29 percent of workers); moving to a higher-ranked occupation based on a time-constant wage-based occupation ranking; changes in the average occupational wage; and having experienced at least one instance of unusually high wage growth, exceeding that of their coworkers by at least 15 percentage points. Except for the latter proxy, the results generally show that a higher quality of untrained peers reduces the probability of promotion. In terms of magnitude, a 1 standard deviation increase in the quality of untrained peers leads to a reduction in the rungs of the occupational ladder of approximately 2, which reduces average occupational wages by 0.4 percent, accounting for half of the negative wage effect for stayers of 0.8 percent in panel B.

In panel D, we test for spillovers from untrained coworkers in other occupations within the same firm. We find no evidence of such effects, reinforcing the idea that competition is localized within occupations.

Finally, panel E provides suggestive evidence that the intensity of the competition effect increases with the size of the "promotion prize," as predicted by our model (equation (4)). Using the wage gap between the ninetieth and tenth percentiles of untrained workers in the firm-occupation cell as a proxy for the promotion prize, we find that the negative effect of untrained peer quality on earnings and (to a lesser extent) wages is stronger in cells with above-median wage spreads.

C. *Knowledge Spillover and Its Mechanisms*

We next examine in more detail how exposure to high-quality, trained peers shapes the careers of untrained labor market entrants. Panel A of Table 7 shows that a higher quality of trained peers in the initial job increases the likelihood of being employed five years later, indicating that part of the earnings effect observed in Table 2 operates through the employment margin. However, trained peer quality does not significantly affect the probability of switching firms or occupations.

Turning to wage effects (panel B), we find that trained peer quality raises wages five years later even when controlling for trained peer quality at $t + 5$ (column 2). This suggests the effect is not purely driven by mechanical correlation with current peer quality. Columns 3 and 4 further split the sample into stayers (those still at the initial firm) and movers. While both groups benefit, the effect tends to be larger for movers. This indicates that much of the benefit reflects a long-lasting advantage realized in other firms, consistent with transferable knowledge spillovers. Results

Table 7—The Effects of the Quality of Trained Peers in the First Job: Mechanisms (Complex Occupations)

	Employed (1)	Moved firm (2)	Moved occupation (3)	
Panel A. Mobility, $t+5$				
Quality of trained peers	0.011	−0.01	−0.013	
	(0.005)	(0.009)	(0.009)	
Number of observations	172,242	85,963	85,963	
	Baseline (1)	Controlling for quality of peers in $t+5$ (2)	Stayer (3)	Mover (4)
Panel B. log wages, $t+5$, *movers versus stayers*				
Quality of trained peers	0.019	0.013	0.016	0.03
	(0.005)	(0.004)	(0.006)	(0.007)
Number of observations	85,963	85,963	47,003	38,960
	Peer quality, $t+5$ (1)	Firm fixed effect, $t+5$ (2)	Log firm size, $t+5$ (3)	Ever referred by $t+5$ (4)
Panel C. Mediating outcomes				
Quality of trained peers	0.045	0.007	0.044	0.002
	(0.013)	(0.003)	(0.027)	(0.002)
Number of observations	85,963	85,607	85,963	172,242
	Peers in own occupation (1)	Peers in other occupations (2)		
Panel D. Earnings, $t+5$, *peers in the same versus different occupations*				
Quality of trained peers	0.037	−0.002		
	(0.010)	(0.009)		
Number of observations	172,242	172,242		
	Earnings, $t+5$ (1)	Log wages, $t+5$ (2)	Log wages, $t+5$, movers (3)	
Panel E. High versus low "learning opportunities"				
Quality of trained peers	0.048	0.041	0.054	
× high learning opportunities	(0.019)	(0.009)	(0.015)	
Quality of trained peers	0.035	0.016	0.026	
× low learning opportunities	(0.010)	(0.005)	(0.008)	
Number of observations	172,242	85,963	38,960	

Notes: The table shows the effects of the (standardized) quality of initial trained peers (i.e., trained coworkers in the same firm and occupation at labor market entry) on various career outcomes of untrained workers five years after labor market entry. The quality of trained peers is measured by the average AKM wage fixed effects of trained coworkers. All specifications control for initial occupation × cohort effects, initial firm × occupation effects, average coworker age, the coworker shares of trained and college-educated workers, the average quality of untrained and college-educated coworkers, the coworker share of females, the wage in the initial job, age at labor market entry, and sex. Earnings are set to zero for the nonemployed and are divided by mean earnings. In panel E, learning opportunities are defined as low or high depending on whether the ratio of the number of experienced trained workers to the number of untrained workers in the peer group is smaller or greater than 1. Here, a dummy variable indicating a peer group with low learning opportunities is added as a control variable. Standard errors are clustered at the firm level.

Source: German social security records from the *Beschäftigtenhistorik* (BEH)

TABLE 8—THE EFFECTS OF PEER QUALITY IN THE FIRST JOB ON FUTURE LABOR MARKET OUTCOMES WHEN CONTROLLING FOR RANK: THREE GROUPS OF LABOR MARKET ENTRANTS (ALL OCCUPATIONS)

	Earnings, rel. to mean, $t+5$		Log wage, $t+5$	
	(1)	(2)	(3)	(4)
Panel A. Untrained				
Average peer quality	−0.032	0.027	0.001	0.01
	(0.005)	(0.005)	(0.004)	(0.004)
Rank in peer group		0.131		0.017
		(0.003)		(0.002)
Number of observations	774,701	774,701	329,649	329,649
Panel B. Trained				
Average peer quality	−0.008	0.061	−0.004	0.036
	(0.002)	(0.002)	(0.001)	(0.001)
Rank in peer group		0.196		0.105
		(0.001)		(0.000)
Number of observations	2,211,011	2,211,011	1,552,053	1,552,053
Panel C. College graduates				
Average peer quality	−0.011	0.012	−0.007	0.008
	(0.006)	(0.006)	(0.003)	(0.003)
Rank in peer group		0.044		0.028
		(0.002)		(0.001)
Number of observations	1,265,753	1,265,753	777,948	777,948

Notes: The table shows the effects of (standardized) initial peer quality (at labor market entry) on earnings and wages five years after labor market entry (completion of the apprenticeship program) for three groups of labor market entrants: untrained workers (in all occupations; panel A), trained workers who completed an apprenticeship program (panel B), and college-educated workers (panel C). Results are reported with and without controlling for the entrant's standardized rank within the initial peer group (to capture competition effects). For untrained and college-educated workers, initial peers are coworkers in the same firm and occupation in their first job at labor market entry. For trained workers, initial peers refer to their coworkers during apprenticeship training. Average peer quality is standardized and measured by the average AKM wage fixed effects of coworkers. For untrained and college-educated workers, their rank among initial peers is based on residualized wages net of experience to capture current performance relative to expectations based on seniority. Since the wages of apprentices in training are not comparable to the wages of regularly employed coworkers, we compute the rank of apprentices based on AKM wage fixed effects rather than current wages. Earnings are set to zero for the nonemployed and are divided by mean earnings. All specifications control for initial occupation × cohort effects, initial firm × occupation effects, average coworker age, coworker shares of trained and college-educated workers, the coworker share of females, the wage in the initial job, age at labor market entry, and sex. Standard errors are clustered at the firm level.

Source: German social security records from the *Beschäftigtenhistorik* (BEH)

from our alternative rank strategy (Supplemental Appendix Table D.3) support this conclusion.

In panel C, we investigate a range of additional outcomes that could mediate the effect of initial peer quality on future wages. Exposure to higher-quality trained peers increases subsequent peer quality by 4.5 percent of a standard deviation, the employing firm fixed effects by 0.7 percent, and firm size by 4.4 percent.[27] These findings suggest that exposure to high-quality, trained peers facilitates the transition of untrained workers into larger, better-paying firms with higher-quality coworkers. We find no effect on the probability of referral, measured as joining a firm where an

[27] As the regression conditions on the initial-firm-by-initial-occupation fixed effect, the effect on future firm fixed effect is driven entirely by workers who move to another firm.

initial coworker previously moved, suggesting that network-based job searching is not a key driver of the mover advantage.

We report a tentative mediation analysis in Supplemental Appendix C, estimating the extent to which the wage effect (from column 1, panel B) can be explained by these mediators. Together, they account for 65 percent of the effect, with future firm and peer quality being the main channels. We interpret this descriptively, as the causal interpretation depends on strong assumptions.

Panel D explores whether knowledge spills over from trained peers in different occupations. Our findings indicate no evidence of such effects, thus reinforcing the idea that knowledge spillovers are confined within three-digit occupations.

Panel E examines whether the positive spillover from trained peers is stronger in peer groups with greater "learning opportunities." We classify peer groups based on the ratio of experienced trained workers (with at least 3 years of experience) to untrained workers: Groups above 1 are considered high opportunity (42 percent of complex occupation peer groups), and those below 1 are considered low opportunity. In low-opportunity groups, competition for learning opportunities may be greater, potentially limiting knowledge spillovers. We find suggestive evidence consistent with this: A 1 standard deviation increase in the quality of trained peers raises wages by 4.1 percent in high-opportunity groups, compared to 1.6 percent in low-opportunity groups. Similar differences between high- and low-learning groups arise when we focus on workers who have moved to a new firm, suggesting that the skills learned through knowledge spillover are transferable across firms.[28]

D. *Other Labor Market Entrants*

So far, our analysis has focused on untrained workers without postsecondary education. In Table 8, we extend the investigation to trained labor market entrants who completed an apprenticeship (focusing on peer exposure during firm-based apprenticeship training) and to entrants with a college degree (focusing on peer exposure during their first job after graduation from college). We employ the alternative rank strategy and a sample that covers all apprenticeship and entry occupations. For untrained workers, results across all occupations closely mirror those for complex occupations in Table 5; peer quality in the first job negatively affects earnings (and does not affect wages) five years later when rank is not controlled for. However, once we condition on rank, positive spillovers emerge, and rank itself has a strong positive effect on both earnings and wages (panel A).

The patterns are similar for both trained and college-educated entrants, though the magnitudes differ. Exposure to high-quality peers during apprenticeship training yields notably positive effects, pointing toward the effectiveness of Germany's structured formal apprenticeship system in facilitating knowledge transfer (panel B). Rank effects are also more pronounced, indicating that an apprentice's

[28] If workers also compete for these learning opportunities, such competition may constitute one channel of the competition effect captured by the quality of untrained peers. We investigate this in Supplemental Appendix Table D.8, which reports interactions between untrained peer quality and indicators for high versus low learning opportunities. The results are mixed. For wages, we find some evidence that the competition effect is stronger—that is, the coefficient on untrained peer quality is more negative—in groups with high learning opportunities. For earnings, however, the pattern is reversed, with the heterogeneous effects pointing in the opposite direction.

relative position among coworkers plays an important role in their career advancement. Both effects are more minor in magnitude for entrants with a college degree (panel C).

These findings highlight that knowledge spillovers may be obscured if the counteracting effects of competition are ignored. They also suggest that trained and untrained workers benefit more from peer learning than college graduates and that peer learning is particularly important when enrolled in apprenticeship programs. Finally, since untrained and trained workers rarely work alongside college-educated peers in their first job, the results imply that spillovers primarily stem from trained coworkers with apprenticeship backgrounds, rather than those with university degrees.[29]

E. *Distinguishing Effects by Gender*

Both competition and knowledge spillover effects may vary by gender. A large body of lab and field evidence suggests that women are generally less willing to compete or to sort into competitive environments (Gneezy, Niederle, and Rustichini 2003; Niederle and Vesterlund 2011; Flory, Leibbrandt, and List 2015). Men and women also differ in promotion aspirations (Azmat, Cuñat, and Henry 2020), negotiation behavior (Azmat and Petrongolo 2014), and exposure to gender-based biases in promotion processes (Benson, Li, and Shue 2023; Sarsons, Gërxhani, Reuben, and Schram 2021; Blackaby, Booth, and Frank 2005; Haegele 2024, 2026). Collectively, this literature suggests that women are less likely to enter promotion contests and face lower expected returns. Accordingly, an increase in the quality of potential competitors should affect women's career progression less than men's.

Consistent with this hypothesis, Table 9 shows that untrained peer quality has a more negative effect on male than female job starters (panels A and B, column 1). A 1 standard deviation increase in the quality of untrained peers reduces men's earnings 5 years later by 6.4 percent, compared to 2.5 percent for women—a statistically significant difference at the 10 percent level. These differences are not explained by occupational sorting; they persist when regressions for men (women) are reweighted to match the occupational distribution of women (men) (column 2).[30]

Interestingly, the negative competition effect for both genders appears driven primarily by same-gender peers (columns 3 and 4). One possible interpretation is that employers view same-gender peers as closer substitutes; alternatively, firms may apply explicit or implicit gender quotas in promotion or retention decisions.[31]

[29] For untrained workers, only 2 percent of coworkers in their first job hold a college degree. The share is also low for apprentices in training (5 percent). This contrasts with the experience of labor market entrants with a college degree: 45 percent of coworkers in their first job hold a college degree. These statistics highlight the limited exposure of trained and untrained labor market entrants to university-educated coworkers at the beginning of their careers.

[30] The respective regression weight for a worker of a given sex in occupation j is the sample share of occupation j in the opposite-sex sample divided by the corresponding share in the own-sex sample.

[31] Existing research suggests that men and women may not be perfect substitutes in the workplace (De Giorgi, Paccagnella, and Pellizzari 2015). This imperfect substitutability may stem from gender differences in task preferences (Gelblum 2020) or aptitudes (Baker and Cornelson 2018). Relatedly, the presence of explicit or implicit gender quotas may influence the dynamics of promotions. Azmat and Boring (2020, Table 2) report that among large firms in Germany, 84 percent implement basic gender diversity policies, and 33 percent set quantitative gender targets.

Table 9—The Effects of the Quality of Untrained and Trained Peers in the First Job on Future Earnings: Men versus Women (Complex Occupations)

	All peers (1)	Weighted for female occupations (2)	Male peers (3)	Female peers (4)
Panel A. Male untrained labor market entrants				
Quality untrained peers	−0.064	−0.041	−0.065	−0.019
	(0.015)	(0.030)	(0.015)	(0.027)
Quality trained peers	0.063	0.073	0.06	0.039
	(0.015)	(0.025)	(0.015)	(0.026)
Number of observations		95,707		
	All peers (1)	Weighted for male occupations (2)	Male peers (3)	Female peers (4)
Panel B. Female untrained labor market entrants				
Quality untrained peers	−0.025	−0.028	0.008	−0.041
	(0.014)	(0.024)	(0.021)	(0.016)
Quality trained peers	0.011	−0.022	0.012	0.028
	(0.013)	(0.019)	(0.014)	(0.015)
Number of observations		76,535		

Notes: The table shows the effects of the quality of initial peers (i.e., untrained and trained coworkers in the same firm and occupation at labor market entry) on the earnings of untrained workers five years after labor market entry. Results are shown separately for male (panel A) and female (panel B) untrained workers. Columns 3 and 4 additionally break down peer quality by the gender of the peers. Average peer quality is standardized and measured by the average AKM wage fixed effects of coworkers. Earnings are set to zero for the nonemployed and are divided by mean earnings. All specifications control for initial occupation × cohort effects, initial firm × occupation effects, average coworker age, the coworker shares of trained and college-educated workers, the average quality of college-educated coworkers, the coworker share of females, the wage in the initial job, and age at labor market entry. Standard errors are clustered at the firm level.

Source: German social security records from the *Beschäftigtenhistorik* (BEH)

As with competition effects, knowledge spillovers may also differ between male and female labor market entrants, potentially due to differences in workplace interactions. Women are more likely than men to take career breaks following childbirth, contributing to well-documented child penalties in earnings (Adda, Dustmann, and Stevens 2017; Kleven et al. 2019). These career "resets" may reduce the persistence of benefits from early knowledge spillovers for women. Additionally, if workplace interactions are shaped by homophily—where same-gender coworkers are more likely to engage with one another due to gender-based segregation in occupations, teams, mentoring, or social networks—then knowledge spillovers may be more substantial among same-gender peers than between opposite-gender peers.

The findings in Table 9 show that positive spillover effects from trained peers are concentrated among men. For women, these effects are small and statistically insignificant (panel B, column 1), suggesting that men benefit more from knowledge spillovers. These differences are not primarily due to differential occupational sorting between men and women, as they persist when controlling for the occupational structure of men and women (column 2). There is also evidence that spillovers are stronger among same-gender peers for both sexes, consistent with Messina, Sanz-de-Galdeano, and Terskaya (forthcoming), who find much stronger same-gender than opposite-gender spillovers in the Brazilian labor market.

Table 10 explores potential mechanisms behind the stronger spillovers for men. For male entrants, the quality of trained peers in the first job has a positive

Table 10—The Effects of the Quality of Trained Peers in the First Job on Future Labor Market Outcomes by Gender: Mechanisms (Complex Occupations)

	Male	Female
	(1)	(2)
Employment, $t + 5$	0.018	0.003
	(0.007)	(0.008)
Actual experience, $t + 5$	0.074	−0.014
	(0.027)	(0.030)
Peer quality, $t + 5$	0.063	0.04
	(0.017)	(0.021)
Firm fixed effect, $t + 5$	0.013	0.000
	(0.004)	(0.004)
Log firm size, $t + 5$	0.065	−0.016
	(0.038)	(0.044)
Log wages, $t + 5$	0.029	0.013
	(0.006)	(0.008)
Log wages, $t + 5$, stayers	0.018	0.024
	(0.008)	(0.009)
Log wages, $t + 5$, movers	0.046	0.002
	(0.010)	(0.015)

Notes: The table shows the effects of (standardized) quality of trained initial peers (i.e., trained coworkers in the same firm and occupation at labor market entry) on the career outcomes of untrained workers five years after labor market entry. Results are shown separately for male and female untrained workers. Average peer quality is standardized and measured by the average AKM wage fixed effects of coworkers. All specifications control for initial occupation × cohort effects, initial firm × occupation effects, the average coworker age, the coworker shares of trained and college-educated workers, the average quality of untrained and college-educated coworkers, the coworker share of females, and the wage in the initial job. Standard errors are clustered at the firm level.

Source: German social security records from the *Beschäftigtenhistorik* (BEH)

impact on employment, experience, peer and firm quality (measured via firm fixed effects), and firm size five years later. These effects are largely absent for women. Strikingly, both men and women benefit similarly from high-quality trained peers if they remain with their initial firm. However, among workers who switch firms, only men experience gains from initial peer quality. This suggests that women primarily benefit from trained peers when staying in the same firm, while men also gain through mobility to larger, better-paying firms.

This finding aligns with prior research showing that job and occupational mobility contribute more to men's wage growth than to women's (e.g., Loprest 1992; Fitzenberger and Kunze 2005; Hirsch, Schank, and Schnabel 2010; Petrongolo and Ronchi 2020). Our alternative rank strategy supports this interpretation, revealing more substantial competition effects for men (Supplemental Appendix Table D.2, panel B) and greater spillover-driven mobility to better-paying firms (Supplemental Appendix Table D.4).

V. Discussion and Conclusions

In this paper, we argue that workplace knowledge spillovers may go undetected if one ignores the counteracting effects of workplace competition from high-productivity peers. We address this challenge with an identification strategy based on the premise that competition is most intense among coworkers with similar qualifications and career stages but unlikely between those on different career paths.

We leverage an institutional feature of the German labor market, where trained (apprenticeship-completers) and untrained workers frequently work side by side in the same occupations but are likely on distinct career trajectories.

We find robust evidence of persistent knowledge spillovers, alongside competition effects of similar magnitude. Exposure to high-quality untrained peers significantly reduces the future earnings and wages of untrained labor market entrants, consistent with competition among peers on similar career paths. These effects are strongest among workers at similar career stages and more pronounced for men, aligning with evidence that men are more willing to engage in and benefit from workplace competition.

In contrast, consistent with knowledge spillovers, early exposure to high-quality trained peers increases future wages and earnings, especially in complex occupations. Spillover effects are also stronger for men than for women. Women benefit primarily when they remain with their initial firm. At the same time, men also gain through job mobility, suggesting that men more fully capitalize on the transferable human capital they acquire from trained peers. We corroborate these findings using an alternative identification strategy that conditions on both absolute peer quality (to capture knowledge spillovers) and ordinal rank (to capture competition).

Applying this rank-based strategy, we further show that knowledge spillover and competition effects are particularly pronounced for trained workers during apprenticeship training, consistent with such programs being conducive to knowledge spillover but also increasing competition. For college graduates, the same patterns hold but are smaller in magnitude.

Our findings carry important implications for research and policy. We document significant knowledge spillovers that stem largely from trained peers with apprenticeship backgrounds. If the higher quality of trained peers reflects productivity increases due to training (as opposed to positive selection into training programs), our results suggest that vocational and apprenticeship programs generate positive externalities, implying that the social return to apprenticeship training likely exceeds the private return. This also suggests that studies using untrained workers as a comparison group may underestimate the true private returns to such training (see Wolter and Ryan 2011 for a summary of the returns to apprenticeship training found in the literature).

Finally, our analysis provides new insights into gender differences in competition. While existing studies typically rely on lab settings or specific contexts such as sports or individual firms, we demonstrate in a broader labor market context that men experience more substantial competition effects than women. This supports the view that women are less inclined to enter competitive environments and gain less from within-firm competition for promotions.

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