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Connecting the Young: High School Graduates' Matching to First Jobs in Booms and Great Recessions

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Abstract

Using Swedish economy-wide data spanning across two deep recessions, we examine the relationship between labor market conditions and the role of social contacts in matching labor market entrants to employing firms. We use class-plant fixed-effects models to isolate the role of social contacts from paid work during high-school. One third of post-graduation matches are formed at establishments where youths worked during their studies. Furthermore, graduates are much more likely to match with sites to which adult coworkers from these jobs have relocated. These patterns are strikingly counter-cyclical. Contacts are much more important for job matching in deep recessions than in good times, suggesting that informal contacts and social networks are crucial determinants of matching patterns in bad times.

Keywords: job matching, social contacts, business cycle, young workers
JEL-codes: J01

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1 Introduction

A large and active empirical literature has asserted that informal hiring channels can provide firms with information about worker qualities (see e.g. Hensvik and Skans, 2016; Dustmann et al., 2016). Informal hiring channels mitigate the inherent uncertainty faced by recruiting firms (see e.g. Oyer and Schaefer, 2011) and thus reduce firm-level hiring costs (see e.g. Eliason et al., 2017, forthcoming). But the existence of informal hiring channels may also dampen aggregate labor market fluctuations if recessions provide firms with better opportunities to use informal hiring channels, as suggested by the Fujita and Moscarini (2013) analysis of recall hires across the cycle. This may be particularly important for labor market entrants since information problems are more severe for these groups (see e.g. Altonji and Pierret, 2001), and since entrants' labor market prospects tend to be particularly sensitive to business cycle fluctuations (Kahn, 2010; Oreopoulos et al., 2012). In this paper we present systematic evidence on the use of informal hires in the process where young workers are matched to their first post-graduation jobs using data that spans a 25-year period covering both booms and great recessions.

Most matching theories that allow for informal non-market matches suggest that firms should prefer to hire through informal channels, if given the choice. Montgomery (1991) and Casella and Hanaki (2006) show that firms have good reasons to prefer to hire based on private information about worker qualities as this implies that they do not have to pay the full market valuation of this information. Similarly, Dustmann et al (2016, on networks) and Fujita and Moscarini (2013) (2015, on recalls) argue that firms should prefer to hire through informal channels because they have superior private information about otherwise elusive idiosyncratic match quality (as in Jovanovic, 1979) in these cases. This suggests that firms may rely more heavily on informal hires during times of low tightness when workers' outside options are more limited. Cyclical variations in the share of informal hires may thus reinforce movements in labor market tightness as a source of countercyclical recruitment costs.

We use reduced form regressions to infer how the share of matches formed

through informal ties varies with the aggregate cycle. The previous literature is limited. Fujita and Moscarini (2013) show that the recall share of hires is countercyclical and Kramarz and Skans (2014) show that matching through family ties increases with unemployment. Hellerstein et al. (2015) show that US neighborhood networks became less valuable in the re-employment of displaced workers during the Great Recession reflecting reduced labor demand and a larger share of unemployed neighbors.

Our contribution to the literature is threefold: First, we are able to jointly study the two main channels of informal hires: direct contacts (through previous work experience from the hiring establishment) and indirect contacts (through social networks). The existing literature discussed above suggests that these channels may reduce hiring costs in a similar fashion. Second, we can identify the impact of these informal channels in a setting where confounding factors (such as cyclical patterns of sectoral mobility) are removed through the use of young entrants' classmates as a control group. In addition, we use multiple placebo-regressions to verify the interpretation. Third, we can decompose the relationship between hiring channels and business cycle movements into changes in the impact of hiring channels for a given set of agents and cyclical changes in the composition of connected agents on both the demand (firms) and supply (workers) side.

Our analysis focus on the role of jobs held while in high school ("summer jobs", for simplicity). 60 percent of the high school graduates in our sample work during high school, and since these jobs are the first connections young workers have with the labor market (besides the links through parents), they play (as we show) a significant role for the students' labor market entry after graduation. Summer jobs provide direct links to potential employers and indirect social links through former co-workers who have relocated to new establishments (see also Hensvik and Skans, 2014). This allows us to simultaneously study how the business cycle is related to the probability to return to a previous employer and the effect of social contacts within one unified framework.

We rely on graduation records of all Swedish high school students who graduated from vocational tracks between 1986 and 2010. The graduates

enter the market with a very precise vocational education (auto-mechanics, assistant nurses,...) but with very little on-the-job training or internships. The graduation data is linked to register data on demographic characteristics as well as a full characterization of all jobs held by graduates and their coworkers both before and after graduation. We analyze whether graduates with a (direct or indirect) link to a particular establishment are more likely to find a stable job there compared to their classmates and document how these pattens change with aggregate labor market conditions.

Our findings indicate that our measured informal ties are strong predictors of where graduates find their first stable jobs upon graduation. The effect is much more pronounced in deep recessions; we document a very strong and robust negative relationship between the size of our estimates and the youth employment rate at the yearly frequency. The impact of informal ties is twice as large in bad times as in good times. To validate our findings and account for preferential hiring patterns of workers from a specific plant, we use a set of placebo strategies relying on comparisons across establishments within firms and locations, as well as workers who move just before the start of a summer job spell. The results for these placebo exercises strongly support the interpretation that sorting patterns are causally related to the actual contacts, and, in particular, that the negative relationship to the business cycle is indeed a result of variations in the usefulness of young workers' social contacts in booms and recessions.

Our main results are very robust. They hold across all segments of the market and across both genders. We find similar results if we account for other changes over time by conditioning on time trends and quadratics.Results are also robust if we control for business cycle conditions when the links were formed, allow for delayed labor market entry, and use local instead of aggregate labor market conditions with and without time dummies.

To assess the role of selection on the supply or demand side we estimate models where we let the role of the contact vary with characteristics of the student (holding the summer job) and firm fixed effects as well as the business cycle. The results suggest that about one third of the effect is driven by demand side selection, i.e. firms that use informal hires more heavily are

also more likely to offer summer jobs in bad times. Despite including very rich worker-side characteristics in the models, we find no evidence of a corresponding systematic selection on the supply side. Finally we corroborate the interpretation in terms of match quality by showing that informal matches last longer, in particular if formed in bad times.

The paper is structured as follows: section 2 provides a background of the Swedish education system and labor market conditions. Section 3 describes the basic set-up of our empirical model. Section 4 describes the data, while results are presented in section 5. Section 6 includes the placebo estimations and robustness checks, section 7 shows results on match quality and section 8 concludes.

2 Background

2.1 The great Swedish recession and the recent recession

During the 1970s and 1980s, Sweden experienced a long period of low unemployment that came to an abrupt end in the early 1990s (for a detailed account, see e.g. Holmlund, 2003). A combination of a major budget deficit, a housing bubble, high inflation and a fixed exchange rate triggered the start of a recession that was characterized by an initially very high interest rate, rapidly falling house prices, a severe financial crisis and, subsequently, a substantial decline in public spending. As a consequence, unemployment rapidly increased from less than three to eleven percent. The effect on youth unemployment was even more pronounced as the rate increased from below six percent to over 25 percent in the midst of the recession. Recovery did not set in until employment picked up again in the very late 1990s. Unemployment fell to around six percent in 2001, which was however twice the average level prior to the recession. In some contrast, the Swedish labor market fared much better during the aftermath of the recent financial crisis. Once again, the economy was booming before the crisis although youth unemployment rates were now already much higher, but the labor market impact (as well

as the effect on public finances and housing prices) of the crisis was quite moderate by international standards. The (youth) unemployment rate rose from 6 (19) percent in 2007 to 9 (25) percent in 2011.[1].

2.2 Vocational high-school graduates

After nine years of compulsory schooling (at age 16), the nearly all students start high school.[2] In high school, students attend specific programs that are either vocational or academic. Half of the students attend vocational programs and most of these enter the labor market directly after graduation. Since we focus on the school-to-work transition, we only use vocational graduates in our main analysis (see also the robustness section).[3]

The vocational programs provide specific training into occupations such as "auto-mechanics", "business"', "childcare", "construction" or "electronics and computer science". Almost all of the training is done in class. The curriculum should, in principle, contain at least five weeks per year of on-site training with employers but schools can opt to do this part of the training on schools as well and many do so (see e.g. Skolverket, 2004).

3 Empirical model

In our analysis we examine the propensity that a graduate finds her first stable job at an establishment to which she is linked through jobs held during the last full year (January to December) before graduation in June the following year. We refer to these jobs as *summer jobs* for simplicity. We estimate the effects separately by graduation cohort and then, in a second step, relate the estimates to indicators of the business-cycle conditions at the time of labor market entry.

[1]The numbers are drawn from Statistics Sweden's linked series version 2015-10-27 during 1987-2004 and from their current series thereafter

[2]About 75-80% of a cohort finishes high school (Engdahl and Forslund, 2016).

[3]Vocational graduates are eligible to go to college since 1994. With this reform, vocational programs were also extended from two to three years. However, the transition rates from vocational programs to higher education remain around 15 percent during the period we study.

We distinguish between two types of links. The first is the direct *return links* capturing the tie to the site where the summer job took place. The second type is the indirect *moved coworker links* capturing social contacts provided by co-workers from the summer jobs who relocated to other establishments before the entrant graduated. The return links should provide the same types of benefits as recalls, i.e. superior knowledge about firm-specific human capital whereas moved coworker links capture an important part of the graduates' labor-market related social networks.

We use an empirical model that aims to account for the (counterfactual) probability that the graduate would have been employed at the same plant even in the absence of a direct or indirect link. As this probability is likely to depend on how well a graduate's skills match the needs of a particular firm, we follow Kramarz and Skans (2014) and use classmates who received the exact same classroom training to estimate the counterfactual. A causal interpretation of our estimates rests on the assumption that there are small differences in how well graduates and classmates are matched to different plants. We return to tests of this assumption in the robustness section.

We estimate a linear probability model model where the outcome is that graduate i from class c is matched to establishment j after graduation using indicators of pre-existing links between graduates and establishments as the covariates of interest. The model is estimated on a sample that, in principle, includes all possible combinations of students and plants in the economy. However, the model includes a fixed effect for each combination of class and potential hiring plant and we are therefore able to exclude the combinations where none (or all) of the graduates from a class has a link to a particular plant as in

$$W_{icjt} = \gamma_{1t}\, Return\ Link_{icjt} + \gamma_{2t}\, MovedCoworker\ Link_{icjt} + \theta_{cjt} + \varepsilon_{icjt}, \quad (1)$$

where W_{icj} is an indicator that takes on the value 1 if graduate i from class c gets employed by plant j. All variables and parameters are indexed by t since we estimate the model separately for each graduation year. $Return\ Link_{icjt}$

and $MovedCoworker\,Link_{icjt}$ are dummies for the two types of links between graduate i from class c and plant j. θ_{cjt} represents a dummy for each possible combination of class and potential establishment. The parameters γ_t:s thus capture the excess propensities that graduates match with the linked establishments relative to classmates. ε_{icjt} is the error term that captures all other factors within a class that affect that graduate i starts working in plant j.[4]

Our ultimate interest lies in how the γ:s relate to labor market conditions and we therefore run the models separately for each graduation cohort in our sample (25 cohorts during 1986-2010). We then extract the estimates of interest and, in a second step, relate them to the labor market conditions at the time the match is formed. This second step is done non-parametrically in the form of graphs for the main analysis, and in a more compact linear-regressions form for robustness checks.

3.1 Identification issues

A potential concern to our identification is that it is not the social connections between graduates and former co-workers that cause sorting patterns, but shared characteristics of graduates and former co-workers. For instance, sorting of graduates and co-workers into the the same establishments could reflect that employers prefer to hire employees from a particular plant (if the previous place of work signals higher quality workers) or seek for individuals with specific skills that both graduates and co-workers acquired at the previous place of work. Thus, our estimates of interest may be biased if these concerns are valid and vary over the business cycle, e.g. if the sectoral mobility of graduates vary over the cycle. We will address the issue by studying the corresponding sorting patterns using two different types of "placebo" links: "coworkers" who moved *before* the summer job started (corresponding

[4]As in many treatment-effects settings our estimates are identified by the contrast between a well-defined treatment and a less well defined residual alternative. In our setting, the contrast between the matching through the measured set of links and all other forms of matches (which could involve other types of social ties). The estimated relationship to the business cycle will be biased towards zero if the use of other forms of social ties follow a similar business cycle pattern as the use of the summer job links.

to the moved coworker links) and "links" to other establishments with the same firm and location (corresponding to the actual return links).

Another possible concern is that associations between the importance of the links and the business cycle can be generated by systematic fluctuations in supply-side (graduates) or demand-side (firm) *composition*. Graduates who find in-school jobs during recessions might be different from those who work during school in years with higher employment. Likewise, employers that recruit during recessions might be different from employers that recruit when the economy is strong–the mere fact that a firm is willing to recruit during a great recession might indicate that it is an exceptional, high-performance, firm. Thus, our results may be influenced by changes in the composition of active agents instead of reflecting an increased importance of recalls or social networks during recessions for a given set of agents. In order to address this concern we estimate models that allow the *effect* of the pre-existing links to vary with supply side characteristics of the graduate and with the identity of the demand side agent (establishment fixed effects). We explain the details of the estimated model in the robustness section.

4 Data

4.1 Data sources

We use graduation records from Statistics Sweden covering all graduates from Swedish vocational high school programs between the years 1985-2010. As noted above, we focus on graduates from vocational tracks. "Classmates" are identified by an interaction of a school identifier and field-of-study code. In the event that a school has several classes within a field, we cannot separate between these, but our assumption is these different classes are trained similarly.[5] The key aspect of the "class" measure is that it allows us to control non-parametrically for unobserved skills shared between classmates as well as how these skills are valued at the time of market entry by each possible firm through the class-plant fixed effects.

[5]As shown below, most classes are of a reasonable size however.

The graduation records are linked with register data containing detailed information on demographic background characteristics as well as with an employer-employee data set covering the entire Swedish economy, which enables us to identify all jobs held by graduates in the years prior to and post graduation.

4.2 Pre-graduation employment and contacts

For each cohort of vocational track graduates, we identify all jobs that the graduates held in the year prior to graduation. We keep jobs that corresponded to at least two weeks of full time work.[6] We set this threshold to exclude very marginal jobs that only lasted few days or constituted jobs on one-time occasions. Most, but not all, of these jobs are set during summer and we refer to them as "summer jobs" for ease of exposition.

Next, we identify all of the graduates' co-workers from these summer jobs. We exclude co-workers below the age of 20 to ensure that classmates are not counted as contacts. To reduce measurement errors, we limit the analysis to summer-job plants with less than 100 employees in order to increase the likelihood that graduates actually interacted with their co-workers. As shown in the robustness section below, the results are not sensitive to variations in this threshold.

4.3 Post-graduation matches

We identify the place of work after graduation for graduates and for their contacts. The post-graduation employment status is measured in November; five months after graduation. With regard to graduates' employment, we will focus on the concept of "stable" jobs to assure that the employment level post-graduation is considerably greater as compared to the level sustained during high school. In accordance with Kramarz and Skans (2014), we define

[6]Measured by total earnings exceeding 0.5 times the monthly minimum wage. Sweden does not have a legislated minimum wage so it is proxied the 10th percentile of the actual wage distribution from 1997 and by the full-time wage of janitors until 1996 following Kramarz and Skans (2014) and others.

a job as stable if it lasts for at least four months during a calendar year and produces total annual earnings corresponding to at least three months of full time work (i.e. matches where annual earnings> 3*monthly minimum wage). If several employment spells satisfy our criteria, we focus on the match that generated the highest income in the given calendar year.

4.4 Summary statistics

Figure 1 shows the share of vocational track graduates with stable jobs upon graduation for 1986-2010. While up to 60 percent of graduates had a stable job upon graduation in the late 1980s, that share dropped to less than 15 percent in the midst of the great Swedish recession in the mid 1990s. Although employment started to recover in the second half of the nineties, post graduation employment never returned to pre-recession levels. The share of graduates with stable jobs stalled again in the early 2000s and during the recession in 2009, even though the effects on employment were far from as severe as during the early 1990s. We will use the share of post-graduation matches as our preferred measure of labor market conditions as it allows us to measure labor market performance in a consistent way throughout our sample period from 1986 onwards. However, our results are very similar if using youth unemployment numbers instead.[7]

[7]The youth unemployment numbers are less reliable across time due to a major data revisions in 1987 and 2005 and the fact that these unemployment numbers in later years are heavily influenced by students waiting for new jobs to start. See Skans (2009) for a detailed discussion.

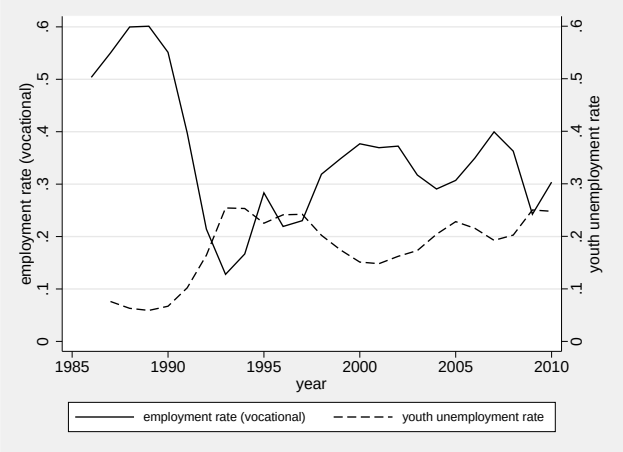

Figure 1: Share of vocational track graduates with stable job upon graduation, 1986-2010.

Summary statistics for vocational track graduates are displayed in Table 1. We split the sample in three bins depending on business-cycle conditions at the time of graduation. Between 41 and 46 percent of vocational track graduates are women. The interaction of school identifier and field-of-study seems to provide a fair measure of a class as the average graduate has 34 to 37 classmates. Unsurprisingly, the share of students with summer jobs is higher in years with low unemployment. The 73 percent of graduates who were employed in the year before graduation in good times can be contrasted with 54 percent during high unemployment years. As expected, most summer jobs generated very low total annual earnings.

Table 1: Descriptive Statistics

	Low unemployment		Medium unemployment		High unemployment	
Variable	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
All graduates:						
Age	18.17	.38	18.93	.26	18.69	.46
Female	.41	.49	.46	.50	.45	.50
Grade percentile rank	.49	.29	.50	.29	.49	.29
No of class mates	36.64	31.49	33.87	35.66	34.68	29.11
In-school jobs	.73	.44	.58	.49	.54	.50
Observations	194,171		474,958		217,647	
Graduates with summer jobs:						
Age	18.19	.39	18.91	.28	18.73	.44
Female	.41	.49	.46	.50	.45	.50
Grade percentile rank	.50	.29	.52	.28	.52	.28
Avg monthly income from in-school job (SEK)	2,351	2,029	4,297	4,448	3,771	3,669
No of class mates	37.60	32.62	33.20	34.82	35.27	29.69
Observations	142,638		273,392		117,289	
Graduates with summer jobs (analysis sample)*:						
Age	18.18	.38	18.93	.26	18.83	.37
Female	.34	.47	.40	.49	.44	.50
Grade percentile rank	.43	.27	.42	.25	.44	.26
Avg monthly income from in-school job (SEK)	1,997	1,680	3,884	3,508	3,538	3,162
No of contacts	24.24	20.63	25.38	21.19	24.29	20.64
Employment rate of contacts	.96	.07	.93	.10	.91	.11
No of connected establishments	8.97	7.96	9.30	8.42	8.16	7.56
Observations	52,930		81,574		21,766	

Descriptive statistics for used data sets. Data are drawn from the IFAU data base.
*Analysis sample only includes graduates with in-school jobs at plants with less than 100 employees and stable job upon graduation. Low unemployment years include years 1986-1990. Medium unemployment years include years 1991, 1995, 1998-2008, 2010. High unemployment years include years 1992-1994, 1996-1997, 2009.

The bottom of the table displays summary statistics for our (preferred) used sample. The sample only includes graduates with both summer jobs and a stable jobs upon graduation.[8] Notably, there are no signs of systematic differences in ability across the cycle as the average grade percentile rank does not differ between the low, medium and high unemployment years samples. However, the share of women during low unemployment years is lower in the used sample reflecting the lower sensitivity to the business cycle among female graduates and a shift towards academic tracks among females after the 1980s boom. On average, graduates in the analysis sample are linked to about 9 establishments through their summer job experience.

[8]In the robustness section we will estimate models that relax this restriction.

5 Results

5.1 Main results

Estimation results based on equation 1 are displayed in Figure 2 and 3. Figure 2 captures the excess propensity that a graduate returns to find her first stable job at establishment of the summer job, whereas Figure 3 measures the excess propensity to match with an establishment to which the graduate is linked through a summer-job coworker who moved.

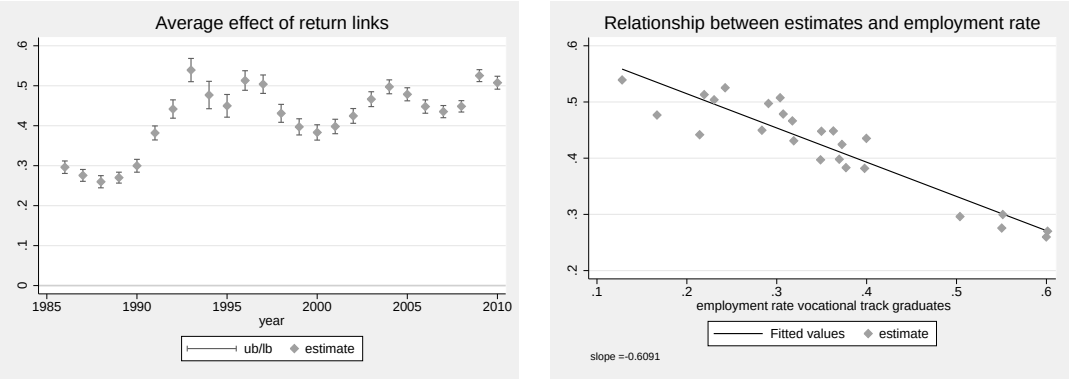

Figure 2: Results for return links. *Left:* Estimates from the sorting model, by year. Effects are for each contact. *Right:* Relationship between estimated effect of return links and employment rate.

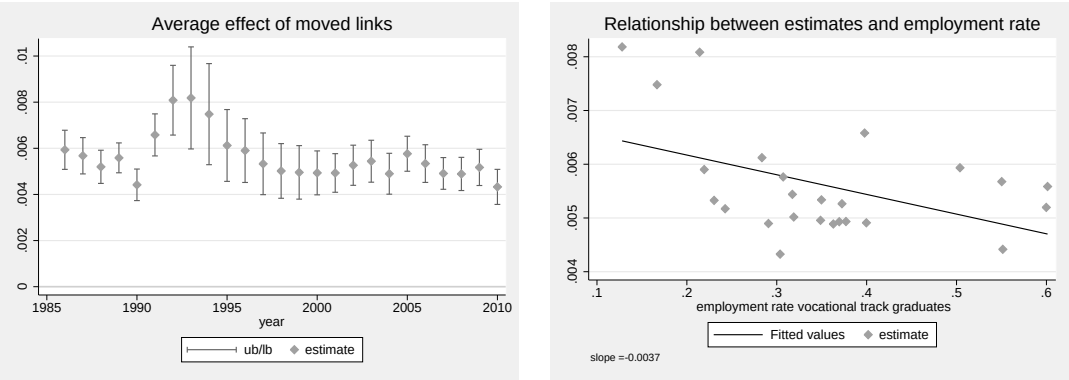

Figure 3: Results for moved co-worker links. *Left:* Estimates from the sorting model, by year. Effects are for each contact. *Right:* Relationship between estimated effect of moved co-worker links and employment rate.

The left-side panel of Figure 2 shows the estimates of $\gamma_1 : s$ for each year of the analysis. The magnitudes of the estimates have a strikingly countercyclical pattern as highlighted in the right-side panel of Figure 2 which explicitly illustrates the relationship between the size of the estimates and the post-graduation employment rate. When employment levels were at their lowest in the midst of the great Swedish recession in the 1990s, the magnitude of the estimates is roughly twice as large as in the most extreme boom years (54 vs. 26 percentage points). Likewise, we see a decrease in the magnitude of the estimates coincides with the recovery of the labor market in the late 1990s. Estimates are larger again when the worldwide financial crisis became apparent around 2009. For completeness, we show the slope of the fitted regression from the right side panel of Figure 2 in Table 2, column(1), top panel. We obtain the estimate from linear regressions of the estimated γ_{1t}:s and γ_{2t}:s in Equation(1) on the demeaned graduation employment rate. The estimated constant (0.42) thus capture the importance of return links with average business cycle conditions and the estimated slope indicates that an increase in the employment rate of one percentage point leads to a decrease of 0.61 percentage points in the propensity that a graduate finds her first stable job at a summer job establishment.

Turning to the estimates for the importance of moved coworker links displayed in Figure 3 we see a similar pattern. Point estimates are positive and significant, but for obvious reasons noticeably smaller in magnitude than for the return links; the estimated impact of each link is between 0.43 to 0.82 percentage points depending on the year. But since the estimates reflect the effect of each *single* moved coworker link and the average graduate has links to eight different establishments through these links, the total effect of these links is substantial. These links were also clearly more predictive during the great Swedish recession and there is a strong negative relationship between the estimates and post-graduation employment rates as displayed in the right panel of Figure 3. The slope of the fitted regression line displayed in Table 2, column (2), bottom panel is -0.0037 and the constant (reflecting the effect during average conditions) is 0.0056.

Table 2: Main results and robustness checks

	(1) Baseline	(2) Time Trend	(3) Time Trend^2	(4) Job t or t+1	(5) Youth unemployment
Return links:					
Effect of BC (employment rate in t)	-0.6091***	-0.5198***	-0.5438***		
	(0.0468)	(0.0364)	(0.0446)		
Effect of BC (employment rate t & $t+1$)				-0.5459***	
				(0.0540)	
Effect of youth unemployment rate					1.2020***
					(0.0790)
Constant	0.4219***	0.3727***	0.3880***	0.3857***	0.4272***
	(0.0063)	(0.0085)	(0.0148)	(0.0062)	(0.0052)
Observations	25	25	25	24	24
R-squared	0.865	0.956	0.959	0.861	0.905
Time trend	No	Yes	Yes	No	No
Time trend squared	No	No	Yes	No	No
Moved links:					
Effect of BC (employment rate in t)	-0.0037**	-0.0061***	-0.0072***		
	(0.0017)	(0.0012)	(0.0012)		
Effect of BC (employment rate t & $t+1$)				-0.0054**	
				(0.0020)	
Effect of youth unemployment rate					0.0030
					(0.0032)
Constant	0.0056***	0.0070***	0.0076***	0.0058***	0.0056***
	(0.0002)	(0.0003)	(0.0004)	(0.0002)	(0.0002)
Observations	25	25	25	24	24
R-squared	0.215	0.703	0.737	0.378	0.036
Time trend	No	Yes	Yes	No	No
Time trend squared	No	No	Yes	No	No

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Column 1-4: The estimated constant is for the average graduation employment rate within the sample.
Column 5: The estimated constant is for the average youth unemployment rate within the sample.

6 Robustness, heterogeneity, placebo and selection

We next turn to assess the robustness of the main results. In order to be able to present a large set of small and large variations in an efficient way, we now focus on showing estimates of the association between the estimated effects of links and our measure of business cycle conditions (i.e. the post-graduation employment rate for the most part) in table format. For the most relevant exercises we also show the results as figures in the appendix.

6.1 Robustness

We have re-estimated the model accounting for a general time trend in the second step (Table 2, column 2) and a quadratic in time (column 3) to remove any influence of other potential changes over time. Reassuringly, the countercyclical relationship with the business cycle is stable for return links and becomes even stronger for the moved links if accounting for secular time trends.

Our main model only uses "stable job" matches during the first year after graduation. But a possible concern is that some males will be prevented from satisfying this criteria because of (usually, 7 months long) military training. Below we show results by gender but as a more direct test, we have included all graduates who found a stable jobs in any of the first *two* years after graduation instead (column 4). The estimated effect for return links decreases slightly, while the effect for moved links increases somewhat.

We have also used an alternative measure of the business cycle. We rely on the most consistent series of LFS-based youth (and adult)unemployment rates we could find (Column 5).[9] Since we use the unemployment rate instead of the employment rate, the slope is now positive but the effects have an equally strong association to the business cycle. In Section 6.4 below we show estimates from further variations, including using local unemployment rates instead and accounting for the state of the cycle when the summer job was formed.

In appendix figures A.1 and A.2, we also show the results obtained when we extend the sample to include graduates who did not have a summer job and when we focus on academic track graduates instead. Also when focusing on these two cases, the relationship remains strongly countercyclical.

[9]Statistics Sweden has corrected the old series in an attempt to make them comparable across a major data revision in 2005. The series cover 1987-2010 which implies that we lose one year.

6.2 Heterogeneous effects

In order to further assess the role of male participation in the military we have estimated the main model separately for men and women (Table A.1, columns 2 and 3). The results are quite similar although the return links appear more closely related to the cycle for women and the moved coworker links more so for men.

Our main data restriction is to include all contacts from summer jobs with less than 100 employees. When we change this threshold to 50 or 20 in columns 4 and 5, the estimated effect of the business cycle is slightly smaller, but still negative.

Finally, we address the possibility that the observed importance of links could be driven by estimates within a particular segment of the labor market. We identify the six most common fields that graduates specialize in during high school and estimate specification (1) for each of these. Estimates are displayed in Table A.2. There is a strong negative relationship between the estimates and the employment rate indicating that the results are not driven by a particular industry. Important to note is also that the average impact of links captured by the constant is very similar across industries.

6.3 Placebo links

A potential concern is that the sorting patterns that we observe are based on factors other than the personal interactions between graduates and former co-workers. A shared employment history could explain the observed pattern if employers prefer to hire employees from specific establishments or seek after individuals with specific skills that both graduates and former co-workers have acquired at their previous place of work. In order to test whether any of these explanations could have generated the observed patterns, we define two sets of placebo links.

The first type of placebos is aimed at mimicking the role of the return links using *other establishments within the same firm* as the actual summer job establishment. The advantage of this definition is that graduates and placebo co-workers are subject to the same time-varying firm-level factors.

The second placebo aims to mimic the the moved coworker links. Here we use workers who were employed at the summer job establishment, but who *moved the year before the student started* to work there. This should ensure that there was no first-order (on-site) interaction between graduates and these placebo co-workers. The workers' joint employment history allows us to assess whether any other (constant) factors related to the relationship between the summer-job establishment and the linked establishment can explain our main results for moved coworker links.

Notably, the first placebo strategy is only meaningful for firms with multiple sites. In addition, in the second strategy we only make use of graduates who worked for less than two years at their summer job establishments.[10] We therefore start by re-estimating the baseline model within each of these samples. The results, shown to the left in Figures 4 and 5 are similar to the main estimates.

The right hand side of Figures 4 and 5 illustrates the relationship between the two types of placebo estimates and the employment rate of vocational track graduates (Table A.3 provides the results in table format). The placebo estimates can be contrasted with the baseline estimates on the left. The estimated placebo constants (i.e. the effects under average business cycle conditions) are considerably closer to zero than the baseline estimates (one tenth or less). Moreover, the slope of the fitted regression line suggests, if anything, a *positive* relationship between the propensity to find a job at a placebo coworker's plant and the employment rate. This suggests that these alternative placebo links, although in a technical sense defined according to a protocol that is near identical to the actual links, capture a matching technology that is more closely related to market matches (hence, procyclical).

[10]The restriction is imposed because we focus on placebo links that must have worked in the plant prior to the graduate.

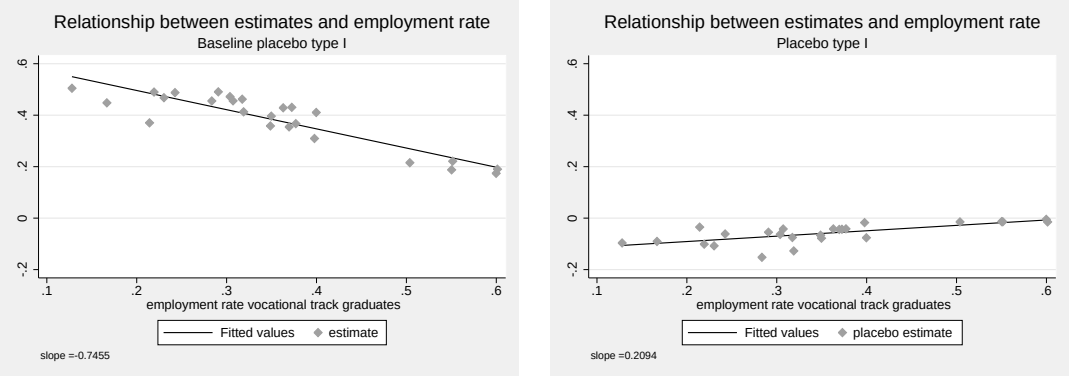

Figure 4: Placebo type I: Relationship between estimates and employment rate. *Left:* Baseline. *Right:* Placebo type I.

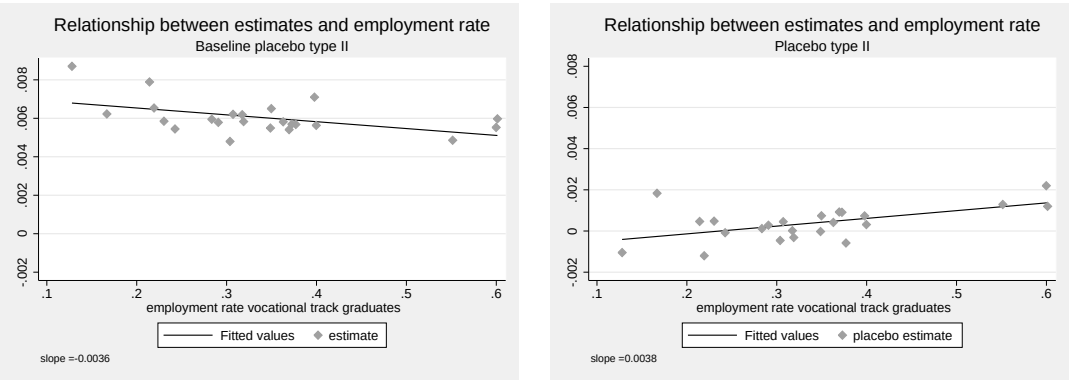

Figure 5: Placebo type II: Relationship between estimates and employment rate. *Left:* Baseline. *Right:* Placebo type II.

6.4 Supply and demand side selection

A potential concern for the comparability of our estimates over the business cycle is that either the composition of graduates who had a summer job and/or the composition of firms that offer summer jobs varies across the cycle.

Graduates who find summer jobs during recessions might be systematically different from the ones who find summer jobs when unemployment is low. Likewise, employers who recruit during recessions might differ from the

ones that recruit in good times. For instance, the fact that firms can afford to hire during recessions might in itself be a signal of high quality. Such firms might be more prone to using social contacts in order to reduce search cost and uncertainty in the hiring decision than the average firm.

In order to assess whether selection can account for part of the countercyclical relationship between our estimates and the employment rate, we adjust our model and let the effect of links (focusing on return links for computational reasons) vary with characteristics of the firm and worker. We thus estimate:

$$W_{icj} = \theta_{c,j} + [\delta^i X_i + \rho_j + \beta\, Year_c + \gamma^{BC} E_c]\, ReturnLink_{i,j} + \varepsilon_{i,c,j} \qquad (2)$$

where we have *interacted the effect of the links* with observed characteristics of graduates (X_i), a firm fixed effect (ρ_j), a time trend and the business cycle (E_c). The individual characteristics include grade rank, gender, immigration background and total earnings from the summer jobs. The parameter of interest is γ^{BC} which captures how the effect of return-links varies with the business cycle, conditional on the other aspects included in the model.

In order to be able to estimate the model, we need to transform the data to remove the class-plant fixed effects $\theta_{c,j}$. Here, we follow the procedure in Kramarz and Skans (2014). More specifically, we compute for each class-plant combination, the difference between the fraction of graduates who found a stable job at plant j through a return link and the fraction of graduates with a stable job at plant j without a return link:

$$G_{cj} = \frac{\sum_{ic} W_{ij} * Link_{i,j}}{\sum_{ic} Link_{i,j}} - \frac{\sum_{ic} W_{ij} * (1 - Link_{i,j})}{\sum_{ic} (1 - Link_{i,j})}$$

G_{cj} captures the difference between the non-market transition rate and the market transition rate within connected class-plant pairs. This difference varies for the connected firms over time, which implies that we can relate it to the business cycle using the following estimable equation:

$$G_{cj} = \delta^i \bar{X}_i + \rho_j + Year_c + \gamma^{BC} E_{c,j} + u_{cj} \qquad (3)$$

where E(u)=0 if the original model was correctly specified (see Kramarz and Skans, 2014) and $\bar{X}_i$ is the average characteristics among those with a link.

The estimation results of equation (4) are displayed in Table 3. Column (1) recaps the baseline effect of the business cycle from Table 2. Columns (2)-(6) show the estimates of γ^{BC} using various sets of control variables. We show the results in two panels, panel A treats all class-plant pairs as single observations, clustering standard errors at the plant level. Panel B instead collapses the data by averaging the data at the year level–this plays a very limited role in practice, however. We first estimate the model without including individual and firm-fixed effects in column (2). The estimate of the effect of the employment rate is very close to our baseline estimate of the slope, as expected. Including firm-specific link-effects in column (3) reduces the estimate by about a third, indicating that there is varying selection across the cycle on the demand side. On the other hand, the estimate in column (4) is unaltered by the inclusion of individual characteristics. Likewise, including both individual characteristics and firm fixed effects simultaneously (column 5) produces estimates very similar to Column (3), i.e. two thirds of the baseline. Column (6) shows the estimate when we include individual characteristics, firm fixed effects and a time trend, again with little changes in the results from Column (3).

Table 3: Demand and supply side selection and the business cycle (BC)

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	**Estimated BC-effect from KS-model**				
	(slope)	Base	ρ	X	X and ρ	Time, X and ρ
Estimate (BC)	-0.609***	-0.687***	-0.482***	-0.665***	-0.483***	-0.544***
	(0.0468)	(0.0111)	(0.0212)	(0.0109)	(0.0212)	(0.0239)
N		119,221	119,221	119,119	119,119	119,119
Estimate (BC)	-0.609***	-0.620***	-0.551***	-0.648***	-0.599***	-0.589***
	(0.0468)	(0.0534)	(0.0462)	(0.0995)	(0.0502)	(0.0481)
N		25	25	25	25	25
Model accounts for:						
Firm-specific effect of link	-	No	Yes	No	Yes	Yes
X-specific effect of link	-	No	No	Yes	Yes	Yes
Trend in effect of link	-	No	No	No	No	Yes

Notes: Robust standard errors clustered on plants. Column (1) shows the main estimate for return links from column 1 in table 2. Column (2) shows the corresponding estimated effect of the business cycle from the KS-Model. In columns (3)-(6) we gradually introduce additional controls: individual student characteristics, plant fixed-effects and a time trend. Panel A shows the estimates using full data, while Panel B shows the estimates based on data collapsed to the yearly level

Overall, the results thus suggest that selection on the employer side account for about one third of the variation in the effect of having a link over the business cycle; however given our controls grade rank, gender, immigration background and earnings from in-school work, there is no evidence for selection on the supply side.

To substantiate this even further, Table 4 (column 2) adds a control for the business cycle during the year when the link was formed (i.e. the year of the summer job). Despite the obvious correlation between the employment rate in the two years, the impact of the business cycle during the graduation year remain very stable. Column (3) replaces the aggregate cycle with an, identically calculated, county-level cycle indicator and the results remain stable and Column (4) adds year dummies to the model with reduces the estimate, but with a large and significant remaining effect. Thus, the countercyclical usefulness of informal links in the matching process survives even when we let the effect of links vary with important individual observed characteristics such as grades and the employment intensity, non-parametrically with firm identifiers and non-parametrically with year dummies.

Table 4: Using additional employment variation

	(1)	(2)	(3)	(4)
	Baseline	Lagged empl. rate	Local empl. rate	Local empl. rate
	X and ρ	X and ρ	X and ρ	X and ρ
Estimate (BC)	-0.483***	-0.420***		
	(0.0212)	(0.0348)		
Lagged BC		-0.086**		
		(0.0362)		
County empl. rate			-0.498***	-0.192**
			(0.0213)	(0.0756)
Observations	119,219	112,723	115,909	115,909
Plant fixed effects	Yes	Yes	Yes	Yes
Student X:s	Yes	Yes	Yes	Yes
Year dummies	No	No	No	Yes

Notes: Robust standard errors clustered on plants. Column (1) shows the estimate from specification 5, table 7 (controlling for student characteristics and plant fixed effects). Column (2) includes the lagged employment rate as additional control. In columns (3) and (4) the local county employment rate is used instead of the national employment rate. The county employment rate is calculated as the share of graduates in county c that finds a stable job within the year of graduation.

7 Match quality

Our analysis has shown that informal ties are more predictive of matching patterns during recessions. We argued initially that the usefulness of these ties are likely to be associated with superior knowledge about match quality. In order to assess if the linked matches we observe really are of better quality, we have examined the relationship to future tenure as in much of the existing literature.

To this end, we estimate the effect of matching through a return link or a moving coworker link on the quality of the match, captured by γ_1 and γ_2 below. As before, we then relate the size of these estimates to the business cycle. Our measure of match quality, MQ, is a dummy that takes on the value 1 if graduate i still works in plant j 2,4 and 6 years after graduation.

$$MQ_{icj} = \gamma_1 \, Return \, Link_{icj} + \gamma_2 \, Moved \, Link_{icj} + \lambda_{c/j} + \mathbf{X}_i\beta + \varepsilon_{icj}, \quad (4)$$

$\mathbf{X}_i$ is a vector of individual characteristics controlling for pre-match earnings, grades and gender and $\lambda_{c/j}$ are class or plant fixed effects (the effects are very similar). We display the results in Table 5. The first thing to note is that matches mediated by in-school contacts are of better average quality: graduates who found their first stable job in the plant where they held their summer job are 26 percentage points more likely to remain employed there after two years compared to graduates who found their first stable job through the market. A similar pattern is found for graduates who found the job through a moving link, although as before this effect is weaker. The difference in match quality dissipates somewhat over time (columns 2-3), but even after six years about one third of the initial difference remains.

Columns (1)-(6), row (1) show the association between the impact on match quality and the business cycle. These results suggests that matching with the summer-job-plant is relatively better in terms of match quality during recessions while matching via moved links is superior to market matches irrespective of the business cycle conditions. To highlight the patterns over time in good and bad times, Figure 6 shows the predicted effect of links of the probability of staying separately for high and low employment years, defined as in Table 1 (corresponds to model (4) with class fixed-effects).

Table 5: Match Quality

	(1) Stay in t+2	(2) Stay in t+4	(3) Stay in t+6	(4) Stay in t+2	(5) Stay in t+4	(6) Stay in t+6
Return links:						
Effect of business cycle (slope)	-0.3905***	-0.2165***	-0.1286***	-0.3913***	-0.2169***	-0.1288***
	(0.0298)	(0.0114)	(0.0076)	(0.0298)	(0.0115)	(0.0077)
Constant	0.2623***	0.1487***	0.0931***	0.2625***	0.1488***	0.0931***
	(0.0054)	(0.0031)	(0.0017)	(0.0054)	(0.0031)	(0.0017)
Observations	23	21	19	23	21	19
R-squared	0.808	0.822	0.878	0.809	0.823	0.878
Class FE	Yes	Yes	Yes	No	No	No
Plant FE	No	No	No	Yes	Yes	Yes
Moved links:						
Effect of business cycle (slope)	0.0008	0.0008	0.0011	0.0002	0.0003	0.0006
	(0.0016)	(0.0008)	(0.0007)	(0.0016)	(0.0009)	(0.0007)
Constant	0.0037***	0.0020***	0.0011***	0.0042***	0.0023***	0.0014***
	(0.0002)	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0001)
Observations	23	21	19	23	21	19
R-squared	0.020	0.073	0.188	0.001	0.014	0.054
Class FE	Yes	Yes	Yes	No	No	No
Plant FE	No	No	No	Yes	Yes	Yes

Robust standard errors clustered on classes in parentheses
*** p<0.01, ** p<0.05, * p<0.1
The estimated constant is for the average graduation employment rate within the sample.

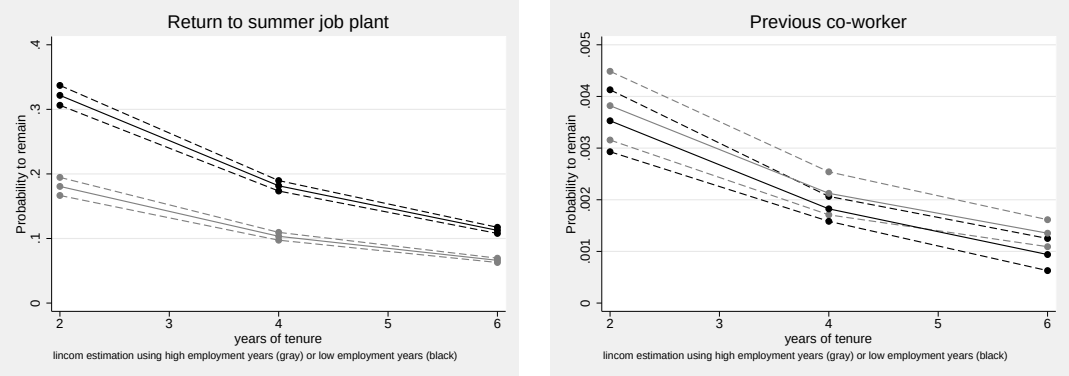

Figure 6: Difference in length of employment between graduates employed through contacts and those without contacts. Difference in high employment years in gray and low employment years in black. *Left:* Return links. *Right:* Moved co-worker links.

8 Conclusion

Our results show that informal hiring channels are more important in the job matching process during recessions than in booms. The predictive power of both direct and indirect links are much stronger in bad times than in good times. The effect size is twice as large in bad times as in good times. This result holds across all segments of the market and across both genders. It is robust to accounting for time trends (and quadratics), to accounting for important characteristics of the entrant, to allowing for delayed market entry, to models that account for business cycle conditions when the links where formed, and to using local rather than aggregate labor market conditions with and without time dummies. Transitions into other establishments within the linked establishment's firm or towards establishments where workers who left just before the summer jobs started are if anything pro-cyclical instead. Thus, we conclude that the countercyclical patterns we document are large in magnitude, general in nature, and robust to large alterations of the statistical model. We also document that part of the effects are driven by demand-side selection. Summer jobs offers during recessions are more likely to come from firms that rely on informal hire regardless of business cycle conditions.

Overall, our results are consistent with a view that informal hires are preferred from a firm perspective because of lower screening costs and better information about worker or match quality. Thus, the ties are used more prevalently when tightness is low and connected workers therefore have worse outside options.

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A Additional results

Table A.1: Heterogeneity by gender, size of summer-job establishment

	(1)	(2)	(3)	(4)	(5)
		By gender:		By establishment size:	
	Baseline	Women	Men	<50 employees	<20 employees
Return links:					
Effect of business cycle (slope)	-0.6091***	-0.7754***	-0.5293***	-0.5518***	-0.5306***
	(0.0468)	(0.0881)	(0.0290)	(0.0522)	(0.0503)
Constant	0.4219***	0.4080***	0.4286***	0.4198***	0.4206***
	(0.0063)	(0.0115)	(0.0047)	(0.0069)	(0.0068)
Observations	25	25	25	25	25
R-squared	0.865	0.758	0.895	0.815	0.808
Moved links:					
Effect of business cycle (slope)	-0.0037**	-0.0021	-0.0067**	-0.0024	-0.0025
	(0.0017)	(0.0017)	(0.0027)	(0.0016)	(0.0021)
Constant	0.0056***	0.0050***	0.0062***	0.0061***	0.0098***
	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0004)
Observations	25	25	25	25	25
R-squared	0.215	0.095	0.268	0.116	0.035

Robust standard errors clustered on classes in parentheses
*** p<0.01, ** p<0.05, * p<0.1
The estimated constant is for the average graduation employment rate within the sample.

Table A.2: Heterogeneous effects: by field

	(1) Business	(2) Childcare	(3) Healthcare	(4) Construction	(5) Electronics	(6) Hotel
Return links:						
Effect of business cycle (slope)	-0.7128***	-0.9540***	-0.9522***	-0.3307***	-0.7385***	-0.6748***
	(0.0453)	(0.1357)	(0.1011)	(0.0467)	(0.0724)	(0.0598)
Constant	0.4795***	0.4022***	0.3416***	0.4893***	0.4481***	0.3661***
	(0.0069)	(0.0163)	(0.0129)	(0.0078)	(0.0119)	(0.0070)
Observations	25	25	25	25	25	25
R-squared	0.879	0.702	0.789	0.555	0.725	0.864
Moved links:						
Effect of business cycle (slope)	-0.0065***	-0.0078***	-0.0012	-0.0067*	-0.0031	-0.0041
	(0.0021)	(0.0018)	(0.0025)	(0.0034)	(0.0053)	(0.0031)
Constant	0.0055***	0.0052***	0.0048***	0.0083***	0.0063***	0.0040***
	(0.0003)	(0.0002)	(0.0003)	(0.0004)	(0.0006)	(0.0003)
Observations	25	25	25	25	25	25
R-squared	0.249	0.412	0.010	0.142	0.019	0.123

Robust standard errors clustered on classes in parentheses
*** p<0.01, ** p<0.05, * p<0.1
The estimated constant is for the average graduation employment rate within the sample.
Column (5): Electronics and computer science. Column (6): Hotel and restaurant.

Table A.3: Placebo Results

	(1) Baseline	(2) Placebo
Placebo I (only multi-plant firms):		
Effect of business cycle (slope)	-0.7455***	0.2094***
	(0.0742)	(0.0297)
Constant	0.3825***	-0.0591***
	(0.0096)	(0.0056)
Observations	25	25
R-squared	0.807	0.487
Placebo II (co-workers left before summer job started):		
Effect of business cycle (slope)	-0.0036*	0.0038**
	(0.0017)	(0.0015)
Constant	0.0060***	0.0004***
	(0.0002)	(0.0001)
Observations	23	23
R-squared	0.243	0.308

Robust standard errors clustered on classes in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The estimated constant is for the average graduation employment rate within the sample. Baseline placebo I restricts the sample to multi-plant firms only. Baseline placebo II restricts the sample to graduates with summer jobs of a duration of maximal two years.

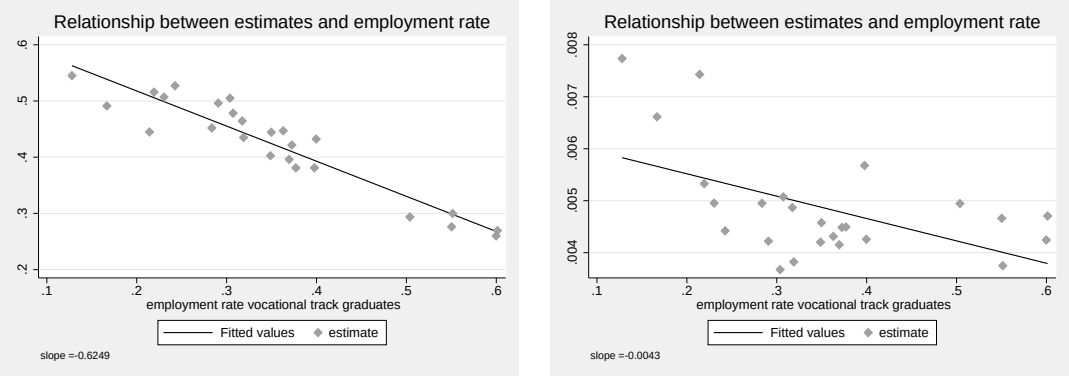


Figure A.1: Including graduates both **with and without summer jobs**: Relationship between estimates and employment rate. *Left:* Return links. *Right:* Moved co-worker links.

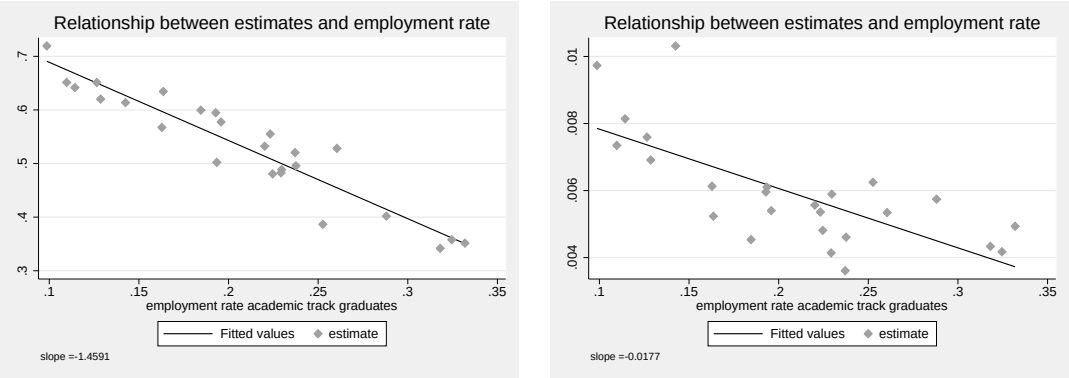


Figure A.2: Academic track graduates: Relationship between estimates from main model estimated on sample of **academic track graduates** and employment rate of academic track graduates. *Left:* Return links. *Right:* Moved co-worker links.

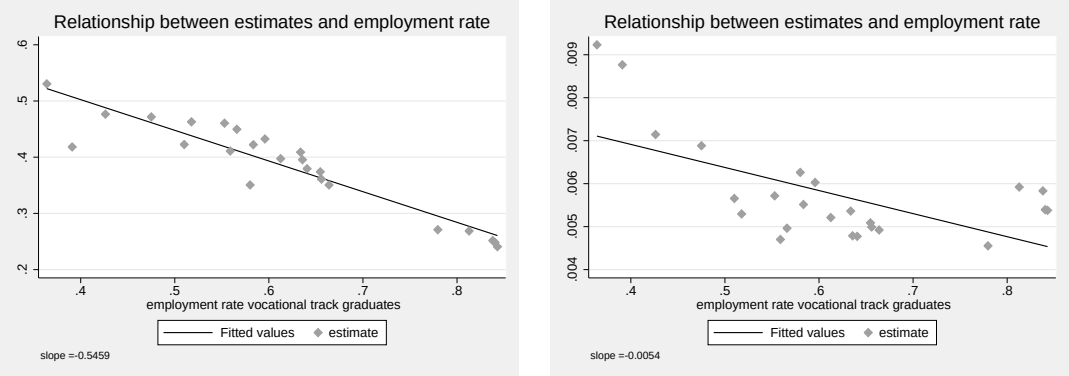


Figure A.3: **Stable job in t and t+1**: Relationship between estimates and employment rate. *Left:* Return links. *Right:* Moved co-worker links.

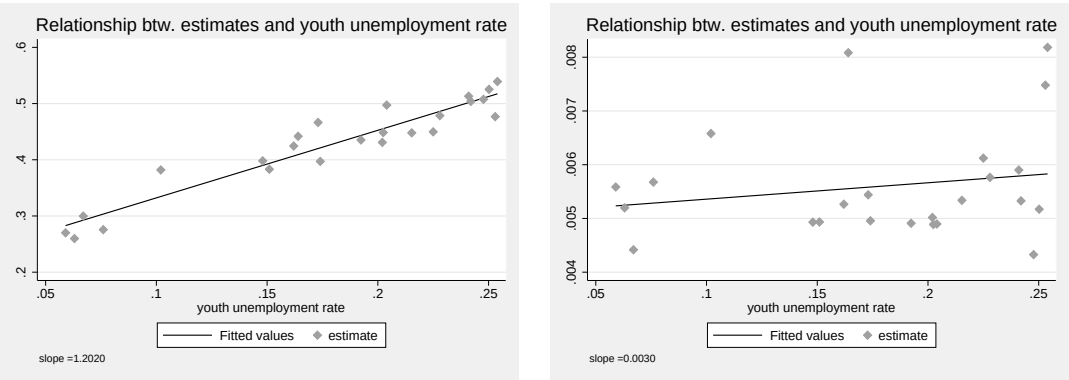
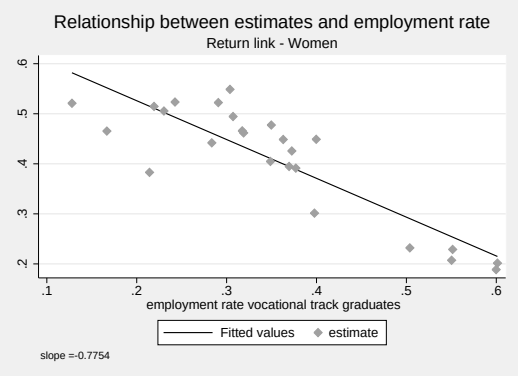


Figure A.4: Youth unemployment rate: Relationship between estimates and **official youth unemployment rate**. *Left:* Return links. *Right:* Moved co-worker links.



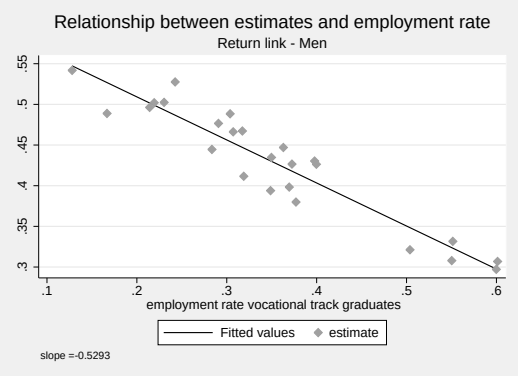
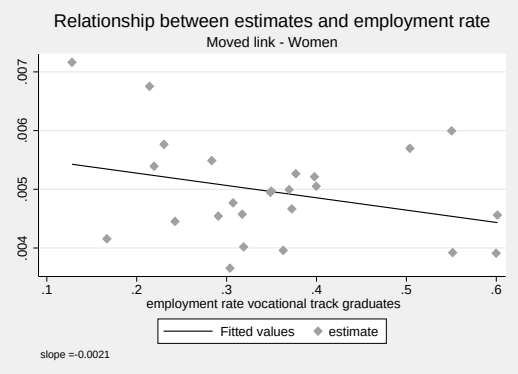


Figure A.5: Relationship between estimated effect of **return links and employment rate, by gender**



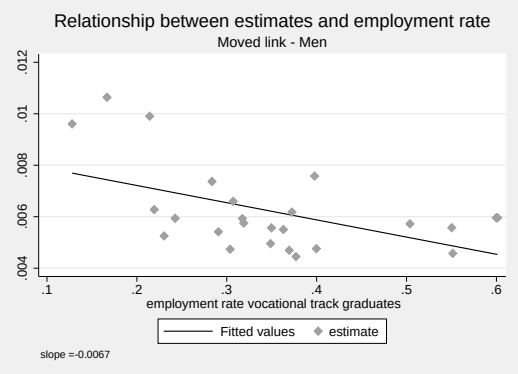


Figure A.6: Relationship between estimated effect of **moved co-worker links and employment rate, by gender**

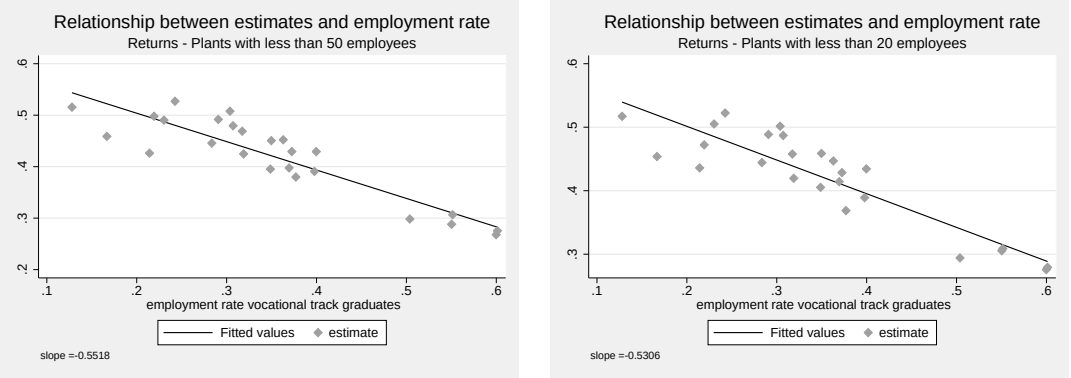


Figure A.7: Relationship between estimated effect of **return links and employment rate, by firm size**

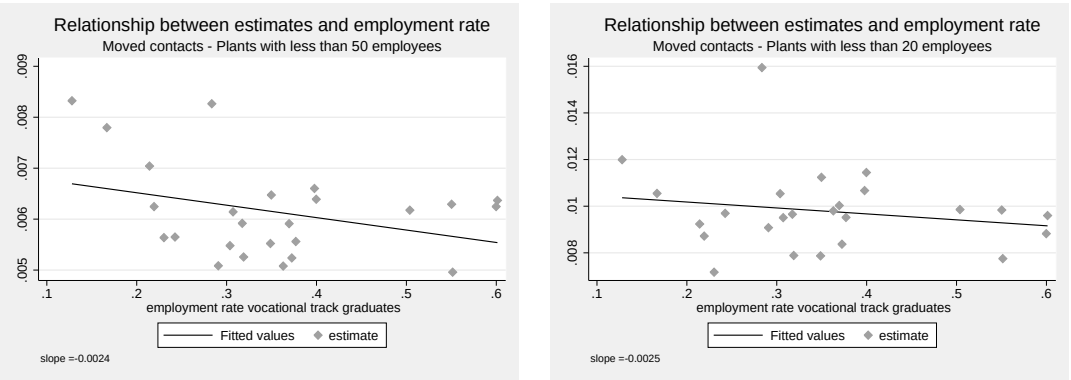


Figure A.8: Relationship between estimated effect of **moved co-worker links and employment rate, by firm size**

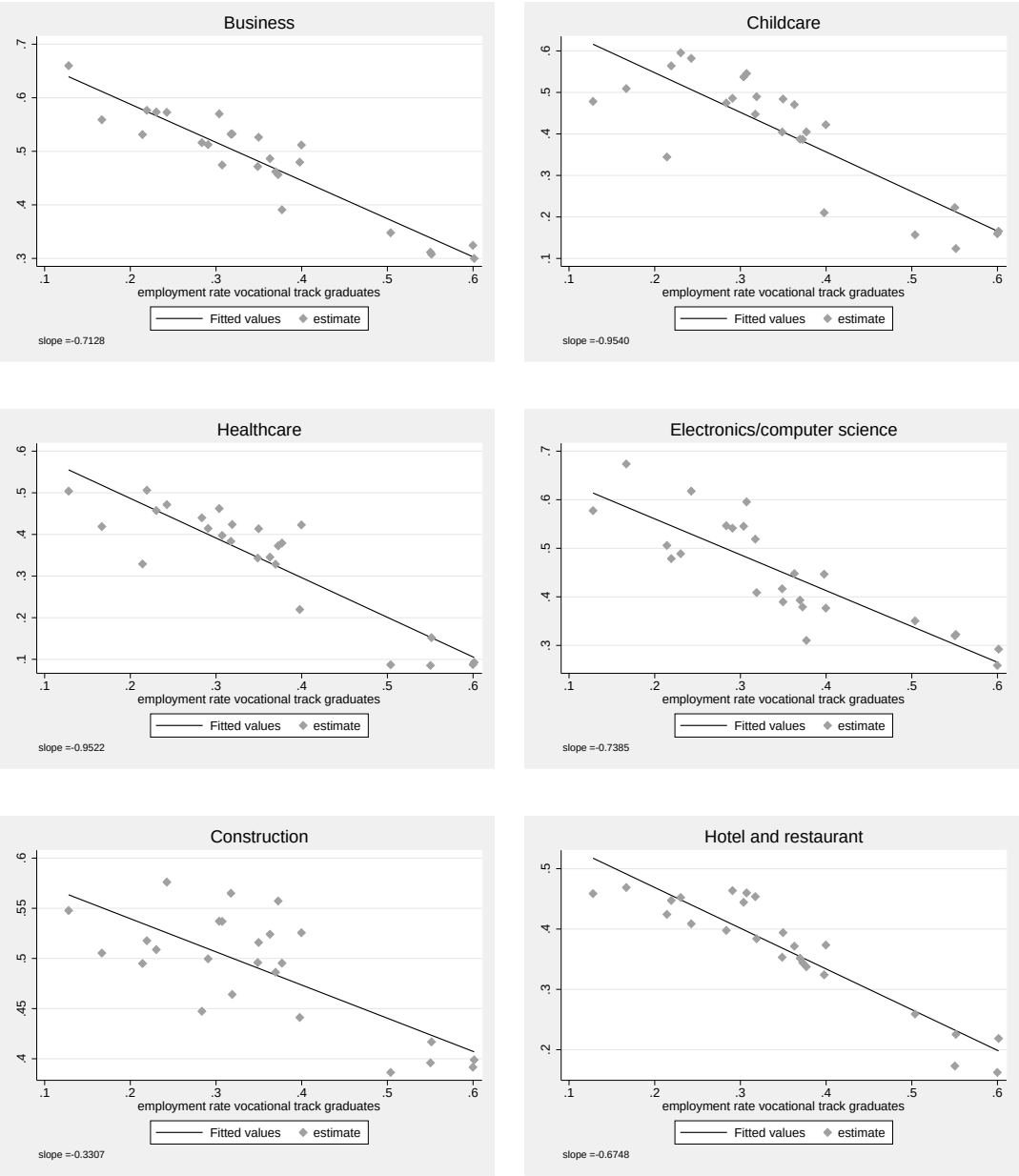


Figure A.9: Relationship between estimated effect of **return links and employment rate by most common sectors**

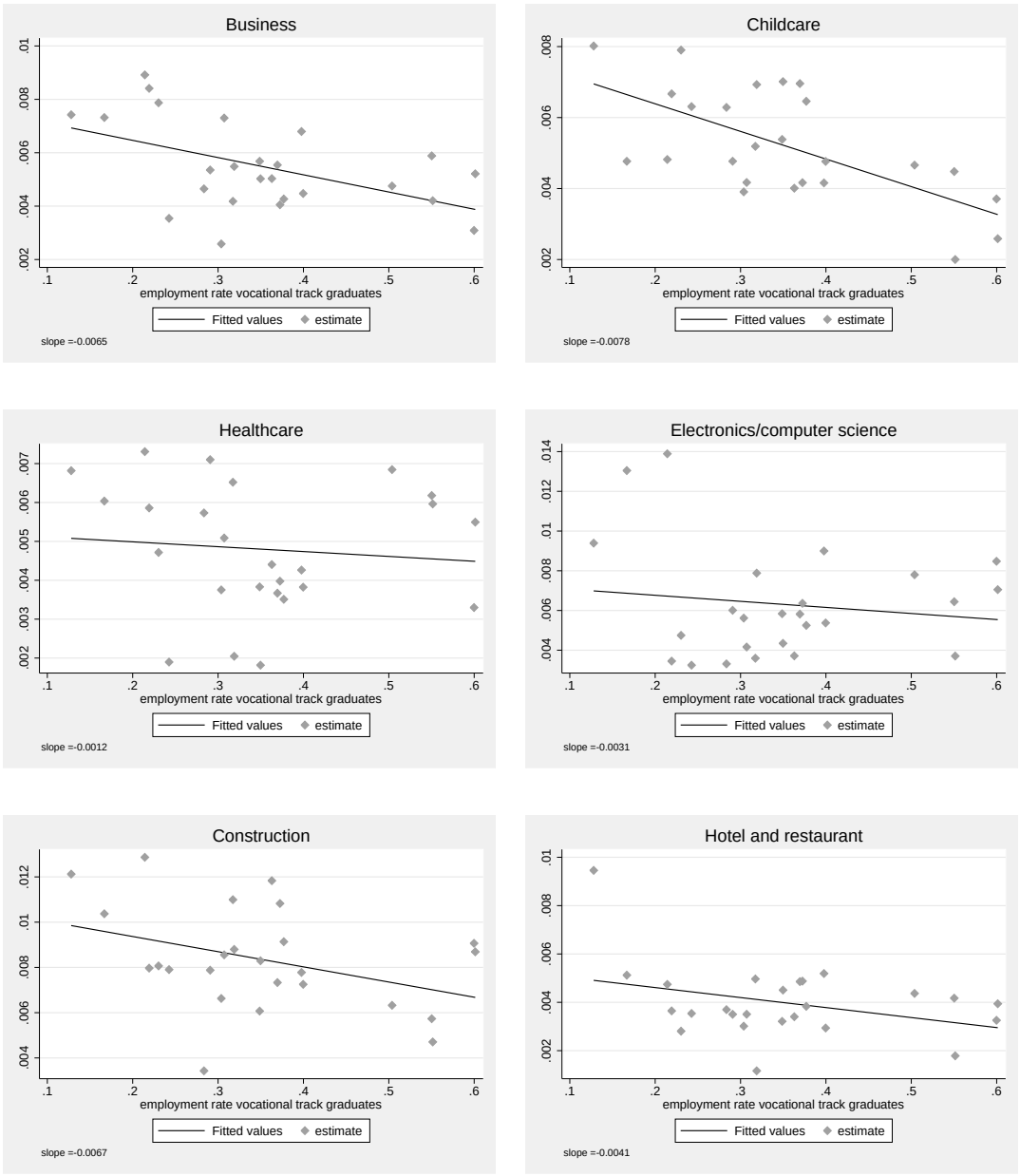


Figure A.10: Relationship between estimated effect of **moved co-worker links and employment rate by most common sectors**