DISSERTATION SERIES 2020:1

Social networks and the school-to-work transition

Dagmar Müller

IFAU
INSTITUTE FOR EVALUATION OF LABOUR MARKET AND EDUCATION POLICY

The Institute for Evaluation of Labour Market and Education Policy (IFAU) is a research institute under the Swedish Ministry of Employment, situated in Uppsala. IFAU's objective is to promote, support and carry out scientific evaluations. The assignment includes: the effects of labour market and educational policies, studies of the functioning of the labour market and the labour market effects of social insurance policies. IFAU shall also disseminate its results so that they become accessible to different interested parties in Sweden and abroad.

IFAU also provides funding for research projects within its areas of interest. The deadline for applications is October 1 each year. Since the researchers at IFAU are mainly economists, researchers from other disciplines are encouraged to apply for funding.

IFAU is run by a Director-General. The institute has a scientific council, consisting of a chairman, the Director-General and five other members. Among other things, the scientific council proposes a decision for the allocation of research grants. A reference group including representatives for employer organizations and trade unions, as well as the ministries and authorities concerned is also connected to the institute.

Postal address: P O Box 513, 751 20 Uppsala
Visiting address: Kyrkogårdsgatan 6, Uppsala
Phone: +46 18 471 70 70
Fax: +46 18 471 70 71
ifau@ifau.uu.se
www.ifau.se

This doctoral dissertation was defended for the degree of Ph.D. in Economics at the Department of Economics, Uppsala University, on June 12th, 2020. **Essay II** has been published by IFAU as working paper 2020:17 and Swedish report 2020:18.

ISSN 1651-4149

Economic Studies 189

Dagmar Müller

Social Networks and the School-to-Work Transition

Department of Economics, Uppsala University

Visiting address: Kyrkogårdsgatan 10, Uppsala, Sweden
Postal address: Box 513, SE-751 20 Uppsala, Sweden
Telephone: +46 18 471 00 00
Telefax: +46 18 471 14 78
Internet: http://www.nek.uu.se/

ECONOMICS AT UPPSALA UNIVERSITY

The Department of Economics at Uppsala University has a long history. The first chair in Economics in the Nordic countries was instituted at Uppsala University in 1741.

The main focus of research at the department has varied over the years but has typically been oriented towards policy-relevant applied economics, including both theoretical and empirical studies. The currently most active areas of research can be grouped into six categories:

* Labour economics
* Public economics
* Macroeconomics
* Microeconometrics
* Environmental economics
* Housing and urban economics

Additional information about research in progress and published reports is given in our project catalogue. The catalogue can be ordered directly from the Department of Economics.

Dagmar Müller

Social Networks and the School-to-Work Transition

UPPSALA
UNIVERSITET

Dissertation presented at Uppsala University to be publicly examined in Hörsal 2, Ekonomikum, Kyrkogårdsgatan 10, Uppsala, Friday, 12 June 2020 at 14:15 for the degree of Doctor of Philosophy. The examination will be conducted in English. Faculty examiner: Associate Professor Albrecht Glitz (Universitat Pompeu Fabra Barcelona, Department of Economics and Business).

Abstract
Müller, D. 2020. Social Networks and the School-to-Work Transition. *Economic studies* 189. 146 pp. Uppsala: Department of Economics, Uppsala University. ISBN 978-91-506-2826-5.

Essay I: This paper studies the importance of market work during high school for graduates' school-to-work transition and career. Relying on Swedish linked employer-employee data, I show that such work provides students with an important job-search channel that some graduates are deprived of due to establishment closures that occur just prior to graduation and labor market entry. I identify the effects of the closures by comparing classmates from the same vocational tracks and find immediate and sizable negative effects on employment and earnings after graduation. The effects persist for up to 10 years, but are not permanent. Graduates who lose a connection in an industry that is relevant to their specialization in vocational school adjust by finding employment in less-relevant industries.

Essay II (with Lena Hensvik and Oskar Nordström Skans): Using Swedish economy-wide data spanning across two deep recessions, we examine how the role of social contacts in matching labor market entrants to employing firms changes with labor market conditions. We measure social contacts acquired during paid work while in high-school and rely on interacted class-plant fixed-effects models to isolate the effects of interest. 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 coworkers from these jobs have relocated. These patterns are strikingly counter-cyclical, suggesting that social contacts are crucial determinants of matching patterns in bad times.

Essay III: I provide empirical evidence on whether a longer education can causally reduce the reliance on social contacts in the transition from school-to-work as suggested by previous theoretical work. Relying on matched employer-employee data, I exploit a Swedish trial that generated exogenous variation in the length of vocational upper secondary education and analyze how this affected the use of parental job-search contacts. I find a negative and non-trivial impact on the probability of working at the same establishment as a parent during the early career, which is entirely driven by students with high-educated parents. For the group where the use of parental ties is most prevalent, students with low-educated parents, the reliance on parents appears resilient to policy-induced changes in the length of education.

Keywords: labor economics, social networks, social contacts, job search, job matching, young workers, school-to-work transition, youth labor market entry

Dagmar Müller, Department of Economics, Box 513, Uppsala University, SE-75120 Uppsala, Sweden. Units outside the University, Office of Labour Market Policy Evaluation, Box 513, Uppsala University, SE-75120 Uppsala, Sweden.

© Dagmar Müller 2020

ISSN 0283-7668
ISBN 978-91-506-2826-5
urn:nbn:se:uu:diva-408802 (http://urn.kb.se/resolve?urn=urn:nbn:se:uu:diva-408802)

Acknowledgments

I would like to thank the many people who contributed to this thesis. Whether you contributed directly by providing input on one of my papers or indirectly by making my life a bit richer, you have been essential in helping me reach this goal.

I would first like to thank Oskar Nordström Skans, my main supervisor, for his support, help, willingness to share his knowledge and all our discussions. Most of all, thank you for caring about my work and, crucially, about me as a person. It sounds so simple, but makes so much of a difference. Over a long period of time you have seen my ups and downs, seen me grow and at times get stuck. I am grateful for your ability to be direct, for pushing me (or giving me pep talks) when I doubted and for knowing when to wait and let me figure things out on my own.

I would also like to thank my co-supervisors (yes, plural!) Anna Sjögren and Lena Hensvik. Anna, for your integrity, your fearlessness of going against the stream and for acknowledging the advantages and shortcomings of the profession. I have always been able to count on your help and your honest opinion. Lena, for sharing your (Stata) knowledge, for the many times you helped me to put things into perspective, lunch meetings in Stockholm and for having an open ear when I needed one.

The time of my PhD was shared between the Department of Economics and the Institute for the Evaluation of Labour Market and Education Policy (IFAU) and this work has greatly benefitted from the discussions and input of great researchers at both institutions. However, I am tempted to believe that the informal conversations during fika with the people at IFAU have been just as important and I am grateful for the possibility of getting to know so many of you. I am particularly grateful for the help of Caroline Hall with the third chapter of this thesis and for the discussions with Erica Lindahl at IFAU; thank you for sharing your excitement about research with me. In addition, I would like to thank Björn Sandberg, Gunilla Berger and Olof Åslund for their continuous support. At the Department of Economics, I would particularly like to mention Tomas Guvå, Eva Mörk and Mikael Elinder and stress the importance of their early encouragement during the Master's program. I am also grateful for the funding from the Jan Wallander and Tom Hedelius Foundation for giving me the possibility to spend two semesters at the Institute for Policy Research at Northwestern University thanks to the kind invitation by David Figlio.

A special thanks is due to all the people who have shared the ups and downs over the course of the PhD program. Thanks to Mattias, Ylva and Vivika and my office mates Josefine, Olof, Mathias, Stefano, Gunnar, Anna and Lillit. I

am glad that you were there to share the fika breaks and office gymnastics as well. I am also extremely glad to have started the PhD program at the same time as Aino-Maija, Lucas, Maria, Paula, Henrik, Olle and Franklin. You are the very best cohort I could have wished for! Thanks for being so relaxed (and for all the pancakes). I was also fortunate enough to attend the job market in Rotterdam together with Maria, Jonas, André and Tamas. The ability to share the experience with you made all the difference.

Aino-Maija and Lucas, you have filled so many different roles over the past years (everything from being dance partners to sharing an office and even an apartment), but having you as friends has been the most important. I can't stress enough how lucky I feel to have met you. Cristina, thank you for sharing concerts and your thoughts; you are such an insightful and smart person. Stefano, the same applies to you (but I am also grateful beyond words for your Latex thesis template). Maria, thank you for being so open and such a great support during the last period of the PhD. Talking to you has made everything much more tolerable.

There are also all those people who reminded me that there is a life outside of academia: Lisa and Rakshith, for sharing and discovering the best Stockholm has to offer (especially swimming spots) and the Carlsson Feng family for sharing food and family life. I was also lucky enough to not only share an apartment, but also my thoughts with Mai and Constanze; thank you for having an open ear and your advice.

Die Göttinger Sieben have also been a continuous presence in my life and one that I value highly. Thank you Kathi, Linda, Luzie, Perschi, Sjard and Ute. I wish I would see you more often, but I always know that you are there.

Finally, I would like to thank my family for their support, love and acceptance of my choices over the years. I am grateful to my parents for teaching me the value of knowledge early on and for letting me choose my own path in life. Knowing that I have your support has enabled me to make those decisions. Thank you Lothar, Renate, Peter and Oma Edith for being there, your interest and, at times, welcome distractions (I mean you, Peter!).

And then there is, obviously, Robert. Thank you for being my biggest supporter; I know it hasn't always been easy. Most importantly, thank you for sharing your life with me and for the laughter.[1]

Uppsala, April 2020

Dagmar Müller

[1] Also, thanks for putting up with my dog obsession.

Contents

Introduction

The school-to-work transition is a defining moment in life, marking a major step towards adulthood and independence. However, despite all preparations culminating to this moment in time, finding a job can be particularly hard for young workers. With little work experience and fewer means to signal ability than more experienced workers, youths are at a disadvantage due to the intrinsic uncertainty that firms face about the productivity of young workers in their hiring decision.

The consequences of the resulting friction are reflected in youth unemployment rates that are often well above the ones for more tenured workers. Long-lasting scarring effects of initial joblessness manifest in terms of higher unemployment and earnings losses in adulthood (see e.g. Gregg, 2001; Burgess et al., 2003; Arulampalam, 2008; Neumark, 2002). Likewise, young workers are routinely hit hardest by business cycle fluctuations and suffer not only the immediate consequences, but are also negatively affected in terms of job finding and earnings for up to a decade into their careers (Raaum and Røed, 2006; Kahn, 2010; Oreopoulos et al., 2012). Hence, it seems that the circumstances surrounding how and when individuals enter the labor market are at least as defining as individual and societal investments for how today's young will fare in tomorrow's labor market, a prospect that we are forcefully reminded of in the light of the economic consequences of the current Covid-19 crisis which will likely produce yet another lost generation.

Given the importance of the period of labor market entry for career prospects in the long run, it is crucial to understand the mechanisms governing a successful school-to-work transition and the consequences when those mechanisms are not in place. While the question of how economic conditions affect youth labor market entry has recently received renewed attention, there is limited direct evidence for this exact point in the career on one of the most important job search channels that has been identified in the literature - *social contacts*. Estimates suggest that a third to one half of all jobs are allocated through social networks, making it possibly the most important job search channel (Ioannides and Loury, 2004). In fact, an extensive body of empirical research has confirmed the importance of social networks[2] and documented that the reliance on social contacts is more prevalent among younger workers (see e.g. Topa, 2011; Pellizzari, 2010). However, we know surprisingly

[2]For a detailed literature overview, see Topa (2011) and Oyer and Schaefer (2011) for a firm-side overview.

little about how access to social contacts affects graduates' chances of finding a first job and to what extent, and under what circumstances, initial usage matters over the course of graduates' careers.

Studying how graduates use social contacts to enter the labor market is valuable because it helps us to understand how job matching works in practice and in particular, whether such channels lead to more productive matches and better labor market outcomes. This also has implications for equality of opportunity if access to useful social connections differs among groups at an early career stage and if the reliance on social contacts is persistent and difficult to offset by alternative modes of entry.

In the three chapters of this thesis, I investigate various aspects of how social contacts matter for a successful school-to-work transition and long-term labor market outcomes. In all of them, I rely on extensive Swedish population registers and matched employer-employee data in order to identify the types of social networks that young workers tend to have ready access to even when other types of connections are scarce.

In the first two chapters, I focus on work-related networks in the form of former employers and co-workers from market work during high school. Recent papers (see e.g. Cingano and Rosolia, 2012; Glitz, 2017; Saygin et al., 2019; Glitz and Vejlin, 2019; Eliason et al., 2019) have emphasized the role of social contacts acquired in the labor market (mostly in the form of former co-workers). In the Swedish setting, they provide students with a crucial, and common, opportunity to form connections to potential employers: the vast majority of Swedish high school students has been employed prior to graduation.[3] Long vacations during the summer and a tradition of providing jobs aimed at young workers account for why working while in high school is so prevalent among this group. In fact, I show that employer contacts from high-school jobs account for almost a third of direct transitions into the labor market.

In the last chapter, I focus on the potential for policy-induced changes in the length of upper secondary education to offset the reliance on strong ties in the form of parents. Parental contacts are likely to be accessible to young workers for obvious reasons. Surveys name parents as one of the most commonly used types of job search contacts, and empirical evidence confirms that they are important for where young workers find employment (see Kramarz and Skans, 2014).

Another feature common to all chapters is the focus on graduates from vocational upper secondary education.[4] This group tends to enter the labor market directly after graduation and receives education towards a specific occupation in various vocational tracks, thus allowing for a cleaner comparison of labor market outcomes among students in the same tracks as in the first two chapters. Usage of social contacts is also generally higher as compared to for

[3] For a description, see chapter 1.

[4] In Sweden, this group accounts for roughly half of all graduates.

graduates from academic tracks, hence making the study of contacts for this group particularly relevant.

In the remainder of this introduction, I provide an overview of theories explaining why social contacts matter for job search. For the purpose of this thesis, I remain agnostic about the driving theoretical mechanisms, but give some examples of how they are relevant to the young. I proceed with a short summary of the three chapters in this thesis. Broadly speaking, the first two chapters analyze how important access to contacts from work during high school is for labor market outcomes both in the short and long run and whether the usefulness of these contacts varies with the business cycle. The final chapter approaches a slightly different and understudied question: whether more education can substitute for the importance of strong social ties in signaling ability during job search.

Why are social networks important?

On a theoretical level, the literature differentiates between two main mechanisms to explain the role of social networks for job search. The first strand of literature focuses on the role of social networks in transmitting information about job vacancies. This channel was put forward early on by Granovetter (1973) who emphasized the importance of the intensity of network connections in transmitting useful information about available jobs. Less intense connections ("weak ties") are more likely to convey new and unheard information as opposed to close connections ("strong ties") in the form of family and friends.

A formalized version of this information transmission mechanism is described by Calvó-Armengol and Jackson (2004, 2007) who model how social networks increase the arrival rate of job offers for unemployed workers: In each period, homogeneous agents are either employed (with a non-zero wage) or unemployed and receive random job offers. Unemployed agents accept the job offer, while employed agents pass the offer on to an unemployed agent in their network (as long as the offer does not dominate their current job), thus leading to faster transitions into employment.

The other types of models emphasize firm-side benefits of relying on social networks based on the premise that firms can reduce uncertainty in the hiring process through the use of referrals (as in Montgomery, 1991; Simon and Warner, 1992; Dustmann et al., 2016). According to Simon and Warner (1992), the use of referrals from random incumbent workers can reduce uncertainty about the productivity of the specific match for both workers and firms. According to this line of thought, hiring workers connected through the network can reduce uncertainty about how well a worker's abilities fit the needs of the firm, which allows for workers that are on average better matched.[5] In

[5]For extensions to this model, see e.g. Dustmann et al. (2016); Galenianos (2013).

Montgomery (1991), firms can mitigate adverse selection of new employees by hiring workers connected through productive incumbent workers. The idea is that an incumbent worker's productivity serves as signal of the productivity of connected workers. Due to assortative matching, high-ability workers are likely to be connected to other high-ability workers, thus reducing uncertainty about the quality of the potential hire.

The resilience of social networks for signaling the productivity of workers is explored in more detail in Casella and Hanaki (2008). The paper analyzes the potential of signals obtained in the market to substitute for the use of social networks. In an extension to the Montgomery model, workers can either signal their productivity through social networks or through obtaining a costly, but imperfect, signal in the education market. Despite the fact that the costly signal can transmit information more precisely, networks prove to be resilient in most cases since firms prefer to hire through social networks based on the access to privileged information at a low cost. The results imply that workers can abstain from obtaining costly signals (e.g. education) to signal productivity if they can use social contacts as a substitute.

Models that focus on how social contacts can reduce uncertainty in the hiring process can also point to why social networks might be of particular importance for the young. If firms face difficulties screening the abilities of young workers who are just about to start their careers due to a lack of available information (Altonji and Pierret, 2001), hiring through social contacts can mitigate some of that uncertainty. In addition, hiring firms might predominantly rely on hiring through referrals due to the value of privileged information obtained through this channel (Casella and Hanaki, 2008).

A third approach models hires through referrals as a means to reduce moral hazard (Heath, 2018). In this setup, firms pay their workers low initial wages so they can later raise wages as an incentive to exert effort. Due to minimum wage constraints, firms are prohibited to follow this strategy for lower-skilled workers. Instead, firms hire low-skilled workers through referrals of (mainly higher-skilled) incumbent workers who accept to forego wage increases if the referred worker performs poorly. As a result, referred workers are incentivized to exert effort in order to avoid wage penalties for both themselves and their contacts. This type of model is particularly relevant for strong ties, since the enforcement of implicit contracts should be more easily achieved among closely connected workers. The model also points to the importance of parental connections for young (and in particular, low-skilled) workers.

Usage of Social Contacts during the School-to-Work Transition

Notwithstanding the extensive empirical literature that has shown that usage of social contacts is higher among younger and low-educated workers (see e.g. Pellizzari, 2010; Corcoran et al., 1980; Datcher, 1983; Elliot, 1999), there is little direct (empirical) evidence on how the reliance on social contacts affects labor market entry (and thus career prospects for those lacking social contacts). A paper that focuses explicitly on graduates from different levels of schooling is Kramarz and Skans (2014) who provide a detailed empirical analysis of how different types of social contacts affect sorting patterns across students from the same class. The authors document that parental ties are an important predictor for post-graduation employment and that this channel is especially important for graduates with lower levels of schooling. They also show that graduates who find a job through their parents benefit in terms of longer job longevity and higher wage growth. In a similar setup, Hensvik and Skans (2014) show that job contacts from high school are predictive of where a cohort of Swedish students find a job after high school and reduce the amount of time it takes to enter employment.

Both papers also point towards economic benefits of job-finding through those channels. This is in contrast to the argument put forward in Bentolila et al. (2010) who highlight that job finding through contacts can indeed lead to mismatch and thus lower productivity if connections only extend towards occupations that do not match the worker's productive advantage.

In fact, the current literature falls short of providing enough evidence on how workers fare in the absence of connections, and we do not know how severe the consequences are for graduates who lack or are deprived of the possibility to rely on social contacts for job search. This fundamental question is the subject of the first chapter of this thesis.

In **Chapter 1: Lost Opportunities: Market Work, Establishment Closures and the Impact on Career Prospects**, I study how graduates are affected in the short and long run when they are deprived of a very important job-finding channel: employer contacts from market work during high school.

In the paper, causal identification relies on exogenous variation in access to employer connections that arises due to closures of establishments where students worked during high school and that occur just prior to graduation. The identifying variation stems from a comparison of students who lost an employer connection with other students from the same class and vocational track whose employer connection remained intact. This strategy allows me to remove several confounding factors.

The results show sizable negative effects of a lost employer connection on employment and earnings after graduation that persist for up to ten years, but are not permanent. Another contribution of the paper is the investigation of mechanisms behind the results. Graduates who lost a connection in an in-

dustry relevant to their occupational training from vocational school are more severely affected and are more likely to find employment in an industry which is less relevant to their vocational training, thereby leading to worse matches at least in the short run.

The implications of this are significant as they indicate that lost employer connections can act as an idiosyncratic shock with similarly long-lived effects as graduating under adverse labor market conditions. Thus, scarring may occur as a consequence of differences in access to productive social contacts at the time of graduation, even though it is important to note that the analyzed employer connections combine aspects of losing social contacts as well as firm-specific human capital.

The analysis also explores the question of whether the loss of labor market contacts can be replaced through relying on other types of social contacts. This seems to be the case indeed. Students who are affected by an establishment closure are more likely to find replacement jobs at their parents' workplace.

In **Chapter 2: Connecting the Young: High School Graduates' Matching to First Jobs in Booms and Great Recessions**, co-authored with Lena Hensvik and Oskar Nordström Skans, we focus on the same type of labor market contacts from work during high school as in the first chapter, but differentiate between "recall-type" direct links to employers and indirect contacts in the form of former co-workers who moved to other establishments.

While the first chapter dealt with the effects of idiosyncratic shocks, i.e. losing access to the opportunities associated with employer links, this chapter examines how the usefulness of social contacts for the matching of labor market entrants to firms changes during aggregate shocks. To this end, we analyze sorting patterns over a 25-year period including both booms and recessions. The main focus of the paper is a detailed analysis of how the reliance on social contacts for job matching varies with the business cycle.

We identify how the existence of direct and indirect contacts affects the probability to sort into a specific establishment by estimating *class-times-establishment* fixed effects models for each of the 25 cohorts in our data. This allows us to estimate the counterfactual probability of being hired by the same establishment in absence of a contact as the probability that classmates without a tie sort into the same establishment. In a second step, we relate the individual estimates for each cohort with business cycle indicators in the same year.

The results show the presence of a direct link is a strong and robust predictor of where graduates find a job after graduation. The impact exhibits a strong countercyclical pattern and is about twice as important during recessions. The patterns for indirect links are very similar, but much smaller in magnitude. We verify the interpretation of our results through estimation of different placebo strategies in order to account for the possibility that firms might prefer to hire workers from certain establishments and through using alternative sources of

identifying variation. We also let the effect of a contact vary with supply and demand side characteristics and find evidence that about a third of the business cycle variation in the usefulness of contacts is driven by the fact that firms that hire more through contacts are also more likely to offer jobs to high school students during economic downturns.

The patterns imply that firms rely relatively more on hiring both directly and indirectly connected workers during recessions when workers' outside options are worse and firms' market power is greater.

In **Chapter 3: Is more education a substitute for job search through social contacts?**, I shift focus to parental ties and analyze whether more (policy-induced) education can reduce the reliance on parents during job search.

While a negative correlation between level of education and the usage of social contacts is well-documented, there is uncertainty whether the relationship is causal. I provide evidence on whether more education can be used as a substitute to signal ability in lieu of social connections by using a Swedish trial that extended upper secondary vocational tracks from two to three years. I use exogenous variation in the length of vocational programs generated by the trial[6] to estimate the impact of an additional year of education on the probability to be (simultaneously) employed at the same establishment as a parent for up to 20 years after starting upper secondary school.

The results show that attending a longer vocational program has a negative and non-trivial impact on the probability to work at the same workplace as a parent in the beginning of the career that is driven by students with highly educated parents. In contrast, usage of parental contact is resilient among students with low-educated parents. To assess robustness, I use a set of placebo-like estimates to show that a decrease in the likelihood to work with a parent does not reflect a shift of students' preferences away from employment in parental industries. I also show that the estimated effects cannot be explained by changes in employment levels.

Overall, the results indicate that strong ties in the form of parents are remarkably resilient among students with low-educated parents. Incidentally, this is also the group that tends to rely on their parents the most. A potential interpretation of the patterns could be that general education is less informative as a signal of ability for employers in industries of low-educated parents as compared to employers of highly educated parents.

The results also have implications for some of the findings in the first chapter, in which I show that lost employer contacts lead to a higher reliance on parents, hence raising the question of whether it is likely that there is room for policy to fill the void for those students who cannot rely on their parents to

[6]The strategy follows Hall (2012) who analyzes the effects of the trial on enrollment in university studies and related outcomes.

find replacement jobs. For students with low-educated parents this is unlikely to be the case.

Concluding Remarks

In the three chapters of this thesis, I document that social contacts are an essential tool for young workers to gain footing in the labor market. In particular, I show that access to connections to the labor market established through work during high school has a causal impact on a successful school-to-work transition and that those connections are twice as important during recessions. In the Swedish context, job-finding through this channel is also quantitatively important, accounting for almost a third of direct labor market transitions.

The findings are crucial for our understanding of how the matching of labor market entrants to firms works in practice, but they are also highly relevant in the context of the current Covid-19 pandemic. There are already clear indications that summer jobs will be scarce as a consequence of the economic impact of the Covid-19 crisis, which will likely escalate young workers' struggle with the passage to the labor market during the crisis.

However, the results also imply that policies that foster employer contacts during high school can lead to a smoother transition into the labor market. The potential of such policies has not gone unnoticed, and prior to the outbreak of the Covid-19 pandemic, there has been expressed interest by governmental and regional commissions in Sweden to set up trials to evaluate the effects of randomly allocating both private and public sector summer jobs, sometimes with a focus on disadvantaged youths.

The implementation of such policies might prove challenging, but even more crucial during the economic fallout of the current Covid-19 crisis. In relation to the findings of this thesis, such employer contacts might be particularly useful in providing an alternative path into the labor market for students whose parents are also affected by the economic fallout and thus less useful as job-search contacts. Such policies could offer a crucial lifeline for students with low-educated parents when more education does not work as a substitute for parental contacts.

References

Altonji, J. G. and Pierret, C. R. (2001). Employer learning and statistical discrimination. *The Quarterly Journal of Economics*, 116(1):313–350.

Arulampalam, W. (2008). Is unemployment really scarring? Effects of unemployment experiences on wages. *The Economic Journal*, 111(475):F585–F606.

Bentolila, S., Michelacci, C., and Suarez, J. (2010). Social contacts and occupational choice. *Economica*, 77(305):20–45.

Burgess, S., Propper, C., Rees, H., and Shearer, A. (2003). The class of 1981: The effects of early career unemployment on subsequent unemployment experiences. *Labour Economics*, 10(3):291 – 309.

Calvó-Armengol, A. and Jackson, M. O. (2004). The effects of social networks on employment and inequality. *American Economic Review*, 94(3):426–454.

Calvó-Armengol, A. and Jackson, M. O. (2007). Networks in labor markets: Wage and employment dynamics and inequality. *Journal of Economic Theory*, 132(1):27 – 46.

Casella, A. and Hanaki, N. (2008). Information channels in labor markets: On the resilience of referral hiring. *Journal of Economic Behavior Organization*, 66(3):492 – 513.

Cingano, F. and Rosolia, A. (2012). People I know: Job search and social networks. *Journal of Labor Economics*, 30(2):291–332.

Corcoran, M., Datcher, L., and Duncan, G. (1980). Information and influence networks in labor markets. In Duncan, G. and Morgan, J., editors, *Five Thousand American Families: Patterns of Economic Progress.*, pages 1–37. Institute For Social Research, Ann Arbor.

Datcher, L. (1983). The impact of informal networks on quit behavior. *The Review of Economics and Statistics*, 65(3):491–495.

Dustmann, C., Glitz, A., Schönberg, U., and Brücker, H. (2016). Referral-based job search networks. *Review of Economic Studies*, 83(2):514–546.

Eliason, M., Hensvik, L., Kramarz, F., and Skans, O. N. (2019). Social connections and the sorting of workers to firms. IZA Discussion Paper 12323.

Elliot, J. (1999). Social isolation and labor market insulation: Network and neighborhood effects on less-educated urban workers. *The Sociological Quarterly*, 40(2):199–216.

Galenianos, M. (2013). Learning about match quality and the use of referrals. *Review of Economic Dynamics*, 16(4):668 – 690.

Glitz, A. (2017). Coworker networks in the labour market. *Labour Economics*, 44:218 – 230.

Glitz, A. and Vejlin, R. (2019). Learning through coworker referrals. IZA Discussion Paper No. 12270.

Granovetter, M. (1973). The strength of weak ties. *American Journal of Sociology*, 78:1360–1380.

Gregg, P. (2001). The impact of youth unemployment on adult unemployment in the NCDS. *The Economic Journal*, 111(475):626–653.

Hall, C. (2012). The effects of reducing tracking in upper secondary school: Evidence from a large-scale pilot scheme. *Journal of Human Resources*, 47(1):237–269.

Heath, R. (2018). Why do firms hire using referrals? Evidence from Bangladeshi garment factories. *Journal of Political Economy*, 126(4):1691–1746.

Hensvik, L. and Skans, O. N. (2014). Networks and youth labor market entry. *Nordic Economic Policy Review*, 1:81–118.

Hensvik, L. and Skans, O. N. (2016). Social networks, employee selection and labor market outcomes. *Journal of Labor Economics*, 34(4):825–867.

Ioannides, M. Y. and Datcher Loury, L. (2004). Job information, neighborhood effects and inequality. *Journal of Economic Literature*, 42(4):1056–1093.

Kahn, L. B. (2010). The long-term labor market consequences of graduating from college in a bad economy. *Labour Economics*, 17:303–316.

Kramarz, F. and Skans, O. N. (2014). When strong ties are strong: Networks and youth labour market entry. *Review of Economic Studies*, 81(3):1164–1200.

Montgomery, J. (1991). Social networks and labor-market outcomes: Toward an economic analysis. *The American Economic Review*, 81(5):1408–1418.

Neumark, D. (2002). Youth labor markets in the united states: Shopping around vs. staying put. *The Review of Economics and Statistics*, 84(3):462–482.

Oreopoulos, P., von Wachter, T., and Heisz, A. (2012). The short- and long-term career effects of graduating in a recession. *American Economic Journal: Applied Economics*, 4(1):1–29.

Oyer, P. and Schaefer, S. (2011). Personnel economics: Hiring and incentives. In Ashenfelter, O. and Card, D., editors, *Handbook of Labor Economics*, volume 4b, chapter 20, pages 1769–1823. Elsevier.

Pellizzari, M. (2010). Do friends and relatives help in getting a good job? *Industrial and Labor Relations Review*, 63(3):494–510.

Raaum, O. and Røed, K. (2006). Do business cycle conditions at the time of labor market entry affect future employment prospects? *The Review of Economics and Statistics*, 88(2):193–210.

Saygin, P. O., Weber, A., and Weynandt, M. A. (2019). Coworkers, networks, and job search outcomes. *ILR Review*.

Simon, C. J. and Warner, J. T. (1992). Matchmaker, matchmaker: The effect of old boy networks on job match quality, earnings, and tenure. *Journal of Labor Economics*, 10(3):306–330.

Topa, G. (2011). Labor markets and referrals. In Benhabib, J., Bisin, A., and Jackson, M. O., editors, *Handbook of Social Economics*, volume 1B, pages 1193–1221. Elsevier Science, Amsterdam.

1. Lost Opportunities: Market Work, Establishment Closures and the Impact on Career Prospects

Acknowledgments: I thank Oskar Nordström Skans, Anna Sjögren and Lena Hensvik for their guidance. For valuable comments and discussions I also thank Arizo Karimi, Kristiina Huttunen, J. Lucas Tilley, Björn Tyrefors, Daniel Waldenström, Alexander Willén and seminar audiences at IFAU, Uppsala University, UCLS, IFN, Lund University, IZA Bonn and IAB Nürnberg.

1.1 Introduction

The school-to-work transition is a crucial time for high school graduates as an unsuccessful transition can have a long-lasting impact on future career prospects. Several studies show strong evidence of a negative impact of graduating during poor aggregate labor market conditions. These effects are visible in terms of lower job finding rates, relatively more employment in lower-level occupations and at lower-paying employers and persistent effects on future earnings during a decade or more (Raaum and Røed, 2006; Kahn, 2010; Oreopoulos et al., 2012). Less is known about the causal impact of shocks that affect individuals' specific job-finding opportunities.[1] In this paper, I use Swedish register data to study how graduates are affected in the short and long run when they are deprived of a very important job-finding channel, market work during high school, due to a plant closure just prior to graduation.

Aiming for a smooth transition, young workers often rely on social connections and informal networks in order to find their first job (see, for instance, Ioannides and Loury, 2004; Topa, 2011). One important source of such connections is provided through direct ties to employers from paid work during high school.[2] I show that such *in-school jobs* are very common in Sweden, a country where vocational high schools provide little workplace training. In-school jobs provide one of the most commonly used paths into the labor market, accounting for about 30 percent of direct transitions into employment after high school (see also Hensvik et al., 2017).

In this paper, I document the causal effect of employer connections at the time of labor market entry for the school-to-work transition and long term labor market outcomes. The identification strategy exploits exogenous variation in the access to employer connections that arises due to closures of former in-school establishments that occur just prior to graduation. I rely on Swedish employer-employee data spanning over a 30-year period to identify all vocational graduates, their former in-school work establishments (i.e. employers hiring students for market work during the last year of high school) and whether those establishments closed prior to graduation.

In the empirical model, I use those closures to compare students who have a sustained link to a former work establishment with classmates in the same vocational track who lost this direct sustained link due to the closure of the former work establishment. A feature of this approach is that it allows me to remove several confounding factors through the use of classmates as a control group; these are not only from the same school, but also trained in the same vocational track and face the same (local) labor market conditions at the time of graduation.

[1] Exposure to early unemployment spells has a strong association with higher and persistent adult unemployment rates (Gregg, 2001; Burgess et al., 2003) and lower earnings (Arulampalam, 2008; Neumark, 2002; Skans, 2011).

[2] Another important source of connections is family networks (Kramarz and Skans, 2014).

It is important to note that the loss of employer links is foremost a loss of opportunities. Establishment closures later in the career are directly disruptive to the individual's life and have been shown to have lasting negative effects on employment, earnings, health and marital stability (see e.g. Jacobson et al., 1993; Stevens, 1997; Browning et al., 2006; Eliason and Storrie, 2006, 2009; Sullivan and von Wachter, 2009; Huttunen et al., 2011; Eliason, 2012). In contrast, graduates who are affected by the closure of an in-school work establishment lose the opportunity to find the first real job at a site where they already have firm-specific human capital and the benefits of social connections, both of which are factors that can shorten and simplify job search.

The results show that students who are affected by a closure of a former establishment do significantly worse upon labor market entry. Such students are less likely to find a stable job upon graduation and have substantial earnings losses as compared to classmates with intact employer links. The results matter for long term career prospects and point to sizable, albeit smaller negative effects on stable employment and earnings for up to a decade before fading out. The effects are thus highly persistent, but not permanent. These findings are particularly interesting since first jobs are generally only transitory in nature, even though they are less so when found through employer links from pre-graduation market work. It is noteworthy that the effects of losing an employer link are similarly long-lived (but less severe) as compared to effects of job loss due to an establishment closure later in life, which further emphasizes the importance of a successful school-to-work transition.

I verify the validity of my approach with a set of alternative specifications and robustness exercises. I show that my results are not driven by selection of students into closing establishments and corroborate that finding by using placebo-like closures of in-school establishments that occur after, instead of prior, to graduation. Reassuringly, I do not find an effect of such placebo closures on employment in the year of graduation. Since there is evidence that closures might be more common in certain industries (Eliason and Storrie, 2006; Browning et al., 2006), I show that the estimates hold when I compare classmates with in-school jobs in the same industry as an alternative sets of fixed effects. The results are also robust to reducing my sample to students with in-school jobs in non-seasonal industries only.

In addition, I provide novel evidence on potential mechanisms and show that parts of the negative effects of a closure are driven by the loss of links to employers that offer job opportunities in industries that are relevant with regard to graduates' specialization in vocational school. Market entrants who lose a connection to an employer in a relevant industry not only suffer from worse employment outcomes, but also adjust by shifting towards jobs in industries that are less-relevant to their specialization in vocational school, leading to worse matches at least in the short run. On another margin, I also show that affected students adjust through higher reliance on their parents during the job search process. In fact, the effects of a closure on employment are more severe

if students worked at the same establishments as a parent, i.e. in cases where students are deprived of this alternative channel to find replacement jobs.

The paper contributes to several strands of literature, particularly with regard to the use of social contacts and networks. While there is ample and compelling evidence of the importance of social networks for job finding (see e.g. Montgomery, 1991; Bayer et al., 2008; Beaman and Magruder, 2012; Kramarz and Skans, 2014; Dustmann et al., 2016), I add to the scarce literature that provides causal estimates based on exogenous variation in access to networks. There are previous papers that have used plant or establishment closures to identify causal effects of networks, albeit with very different approaches. For instance, Eliason et al. (2018) use establishment closures in the network of a firm's incumbent workers as supply shocks directed towards the incumbent's firm and analyze the subsequent effects on firm outcomes. The other papers focus on the importance of network characteristics. Cingano and Rosolia (2012) use plant closures to show that an increase in the employment rate among former co-workers decreases unemployment duration. In a similar approach, Glitz (2017) uses the displacement rate among former co-workers as exogenous variation in network strength to analyze the effects on individuals' re-employment and wages. Saygin et al. (2019) extend a similar analysis by showing that firm-side hiring probabilities are higher for displaced workers who are connected through a former co-worker.

In contrast, I focus on variation in access to employer connections for a population that might be particularly prone to suffer long-lasting consequences in the absence of employer connections since previous research suggests that networks are particularly important for young and inexperienced workers (see e.g. Kramarz and Skans, 2014) and additionally provide new evidence on how those workers compensate for the loss of some type of contacts through others.

The paper also contributes to the literature on the returns to in-school and in-college jobs by discussing an additional channel through which in-school jobs might matter for labor market outcomes. Most of the existing literature focuses on the skill-enhancing aspect of in-school work and its effect on subsequent earnings (see Carr et al., 1996; Ruhm, 1997; Light, 2001; Hotz et al., 2002; Häkkinen, 2006). While early studies (see Carr et al., 1996; Ruhm, 1997; Light, 2001) consistently show that in-school work is associated with substantial and lasting positive effects on labor market outcomes, later papers such as Häkkinen (2006) or Hotz et al. (2002) put greater focus on assessing the robustness of this association. Häkkinen (2006) addresses possible selection into in-school work by instrumenting work experience with local unemployment rates and finds that the effect of work experience increases earnings immediately after labor market entry, but does not find any long term effects. The absence of long run effects is also in line with Hotz et al. (2002), who use a discrete choice model to account for selection.

The paper is structured as follows: Section 1.2 provides a discussion of the institutional background, followed by the empirical model in section 1.3 and

a detailed description of the data in section 1.4. Short term results, validation exercises and robustness checks are presented in section 1.5. In section 1.6, I provide long term effects on labor market outcomes and discuss mechanisms. Section 1.7 concludes.

1.2 Institutional background

1.2.1 The Swedish school system

Following nine years of compulsory school, Swedish students can choose to enroll in upper secondary education, which is divided into several academic and vocational tracks. Students can apply to specific programs such as "child-care", "construction" or "business" based on their grade point average from compulsory school.[3] The vast majority of students enrolls in upper secondary education with roughly half of a cohort opting for academic tracks and the other half for vocational tracks and about 85% of a cohort graduates from high school. Vocational programs provide training for occupations such as construction worker, electrician or care worker with a limited amount of on-site training with employers that usually amounts to five weeks per year. In contrast to central Europe, there are no large scale apprenticeship programs in Sweden. The majority of vocational students enters the labor market upon graduation, while graduates from academic programs tend to enroll in college or university.

Today, academic and vocational programs are three-years long, however up to 1994, vocational programs were limited to two years. For cohorts graduating from 1995 onwards, the extension of vocational programs meant that vocational graduates were put on par with academic graduates with regard to eligibility for university admission. This did however not change the fact that the vast majority of vocational students enters the labor market directly after graduation and that transition rates to university remain low. Since I focus on the school-to-work transition, I limit the analysis to students in vocational tracks.

1.2.2 In-school market work

It is very common that Swedish students work during upper secondary school and the majority of those jobs are found through the ordinary labor market as opposed to jobs that are arranged through cooperation of schools and employers. In the analysis, I refer to market jobs during the year prior to graduation as in-school jobs. Most of the in-school work experience is gathered throughout the two months of summer vacation, but in my analysis I include any

[3]Students who enroll in vocational programs have on average a somewhat lower compulsory GPA.

job that generated positive earnings during the twelve months prior to graduation (which takes place in June). While most in-school jobs are acquired through the regular labor market, municipalities also provide some summer jobs. These municipality-provided jobs are short-term (usually artificial) jobs that pay little and often provide few contacts to real employment opportunities (see Alam et al., 2015). Due to their special nature, I exclude jobs provided by municipalities in my analysis.[4]

My period of analysis also includes some of the most turbulent times for the Swedish economy. While unemployment rates were low throughout most of the 1970s and 1980s, Sweden was hit by a major recession in the early 1990s that led to soaring unemployment (see e.g. Holmlund, 2003). Particularly young workers were hit and faced unemployment rates of up to 25 percent during the peak of the recession. Whilst recovery set in by the end of the 1990s, employment never reached pre-recession levels again.

In contrast, the financial crisis in the late 2000s led to a comparatively moderate increase in youth unemployment rates, albeit from much higher levels.

1.3 Empirical model

1.3.1 Main model

My goal is to identify the short and long run labor market effects of losing the opportunities associated with an intact in-school (market work) employer connection at labor market entry. The identifying variation in my empirical model stems from closures of establishments that employed graduates and that closed within the twelve months prior to graduation. As such, I compare graduates from the same graduation cohort and class (defined as attending the same school and vocational track) that only differ in access to an employer link upon graduation due to an establishment closure.

I estimate the following baseline model:

$$Outcome_{ic(t+n)} = \mu_{ct} + \beta\, plant\, closure_{ict} + \delta X_i + \epsilon_{ict}, \qquad (1.1)$$

where μ_{ct} are class x graduation year fixed effects. X_i are individual characteristics including age, gender, grade rank from high school and immigrant background. Additionally, the vector includes average monthly in-school job earnings in $t-1$ to proxy work experience and to make sure that any effect is not driven by differences in the amount of work experience that was acquired prior to labor market entry. β captures the effect of losing one's direct connection to a former in-school employer due to an establishment closure between $t-1$ and t. I distinguish between short and long run outcomes defined as: $Outcome_{ic(t+n)}$ with $n = 0$ measures (1) the probability of having a (stable)

[4]Note that results hold even when I include jobs provided by municipalities.

job in graduation year t and (2) earnings. $Outcome_{ic(t+n)}$ measures (1) and (2) in each year $t+n$ after graduation for up to 15 years.

I keep all students who had any positive earnings from a job in the year prior to graduation. Class fixed effects are included, such that the identifying variation occurs between classmates. A feature of this approach is that peers from the same class are identified by a common school and vocational track identifier, implying that classmates do not only resemble each other in terms of location and school environment, but also in their choice of a prospective future career path and subsequent specialized education in that field. Examples of such specialization are fields such as "auto-mechanics" or "care work". Another advantage of using graduation class fixed effects is that this approach mitigates concerns that labor market outcomes are affected by differences in the national or local unemployment rate by the time of graduation since I only compare classmates who graduate in the same calendar year and who therefore face the same labor market conditions at the time of graduation as well as each subsequent year in the analysis.

A causal interpretation relies on the assumption that there are no other factors that simultaneously affect graduates' propensity to work at an establishment that closes and that affect employment outcomes. I interpret an establishment closure as an exogenous shock to students' opportunities. To be precise, this includes both the loss of an employer link as well as the fact that students may have acquired (firm-specific) human capital during the in-school job that is no longer of relevance.

1.3.2 Threats to identification & robustness

A possible concern could be sorting into closing establishments by student quality; e.g. high school students who are employed by establishments that will eventually shut down are more likely to have certain characteristics that also lead to worse employment outcomes. Also, closures do not occur randomly across all establishment sizes and industries. I address this issue early on by the discussion of the balance tests in section 5. To make sure that my results are not driven by selection of the establishment type that closes, I will show that any such sorting patterns are negligent in size and disappear once I take into account establishment characteristics. I show that limiting my variation to classmates within the same earnings tercile and/or industry of in-school work establishment confirms my findings and does not impact the magnitude or significance of my main results.

I confirm that sorting does not appear to be a concern by using a second approach that uses future closures of former in-school establishments that occur in year $t+n$ after graduation to test for systematic differences of students with jobs at establishments that close and those that remain in business.

I also provide evidence for the robustness of my estimates by focusing on non-seasonal industries only and by using different restrictions on establishment size.

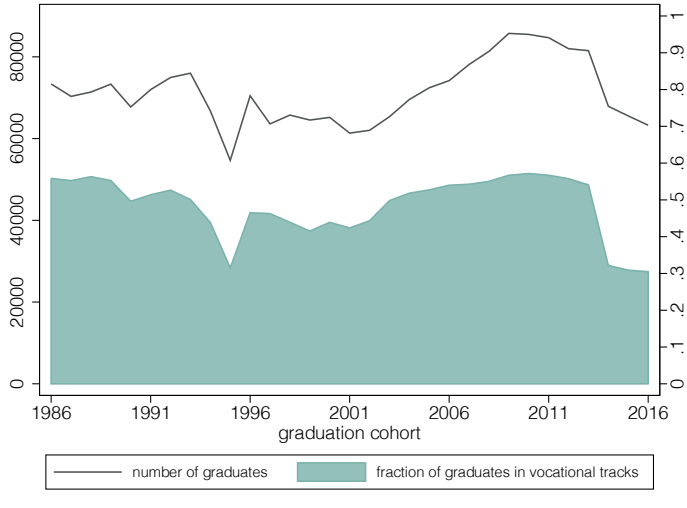
1.4 Data and description

1.4.1 Data sources

In the analysis, I use matched employer-employee register data from Statistics Sweden. The graduate population of interest is defined by the Upper Secondary School graduation register, which entails information on all students who graduated from upper secondary school each year. The register allows me to identify my sample of all vocational students (at age 18 or 19) who graduate in a given year.

Figure 1 shows the number of high school graduates for each year as well as the share of graduates in vocational tracks. During my period of analysis from 1986-2015, 2,147,307 students graduate from upper secondary school of which 1,066,715 graduate from vocational tracks.

Figure 1. Fraction of vocational graduates



Notes: Sample of all students graduating in a given year.

The graduation registers are linked with register data containing information on demographic characteristics as well as with matched employer-employee data covering Sweden's entire working age population (aged 16-69). This data includes detailed information on individuals' earnings received from employment as well as the time period each employment spell lasted. I use this data set to identify whether and where graduates have work experience from

an in-school job in the year prior to graduation. I keep all jobs that generated positive earnings.

1.4.2 Post-graduation employment

A main concern is to identify students' employment outcomes after graduation. As in Kramarz and Skans (2014) and Hensvik et al. (2017), I focus on the concept of "stable jobs" in order to capture a level of labor market attachment that clearly exceeds that of in-school jobs. In the register data, post-graduation employment status is measured 5 months after graduation, i.e. in November of the same year. I identify stable jobs as employment spells that lasted for at least four months during a calendar year including the month of November and that generated total earnings of at least the equivalent of three times the monthly minimum wage as defined by the 10th percentile of the wage distribution.[5] If there are several spells that exceed that threshold, I keep the spell that generated the highest earnings.

1.4.3 In-school market jobs

Figure 2 shows the share of graduates with any in-school job experience. Between 60 and 90 percent of all (vocational) students obtained some work experience. However, given that I include jobs that generated very little income that is not surprising. Still, when applying the restriction of only including jobs that generated at least $0.5\times$ the monthly minimum wage (as measured by the 10th percentile of the wage distribution), the levels decrease to 40 to 80 percent, but follow the same pattern over time.[6]

The importance of in-school jobs for labor market entry is reflected in figure 3. Around 15 percent of all vocational students find their first job at a former in-school job establishment. When I account for direct transitions only, the number increases to about 30 percent (see figure A.1), which corresponds to the recall share of more experienced displaced workers (see e.g. Fujita and Moscarini, 2017).[7]

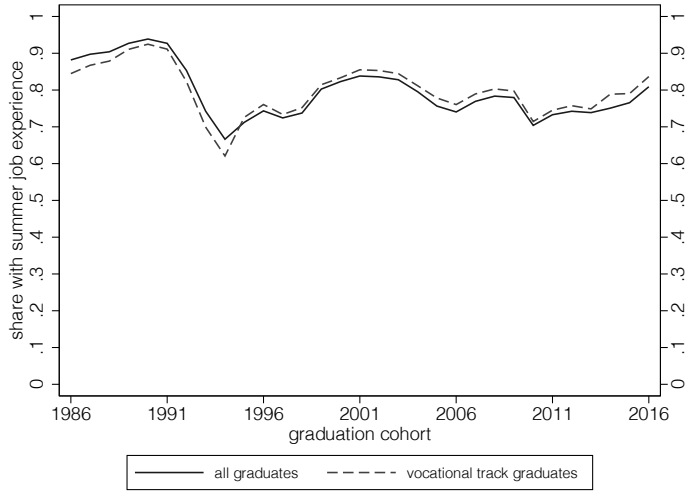
However, are these jobs leading to future careers within the same firm or are they stepping stone jobs from which individuals tend to move on quickly?

[5]This time series is obtained from Lönestrukturstatistiken, see Statistics Sweden `http://www.statistikdatabasen.scb.se`.

[6]Main results are very similar when I apply this restriction.

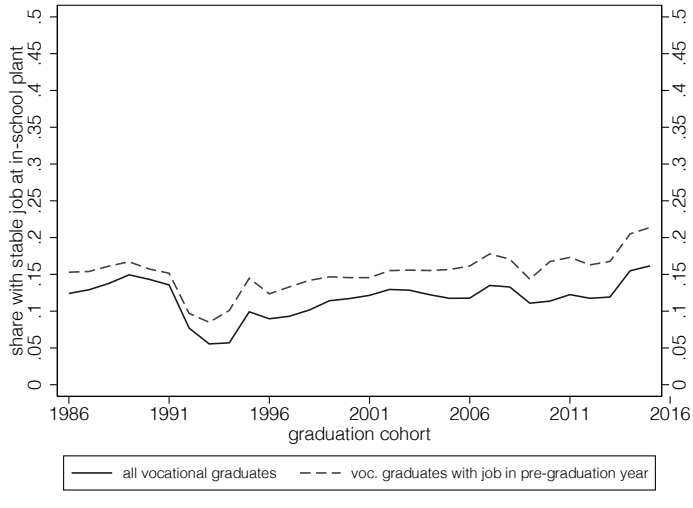
[7]Fujita and Moscarini (2017) find that 30 percent of separated workers in the US, including those that leave the labor force, are recalled to their last employer. Shares are even higher for those that separate into unemployment. The share of graduates returning to their previous in-school employer in Sweden is even higher if I relax restrictions on the length and earnings of the post-graduation employment spell, implying that an even higher share returns to their previous employer if I include very short and low-paying employment spells.

Figure 2. Fraction with in-school job experience



Notes: In-school job experience includes all graduates with positive earnings from a job in the year before graduation.

Figure 3. Fraction returning to in-school work establishment after graduation

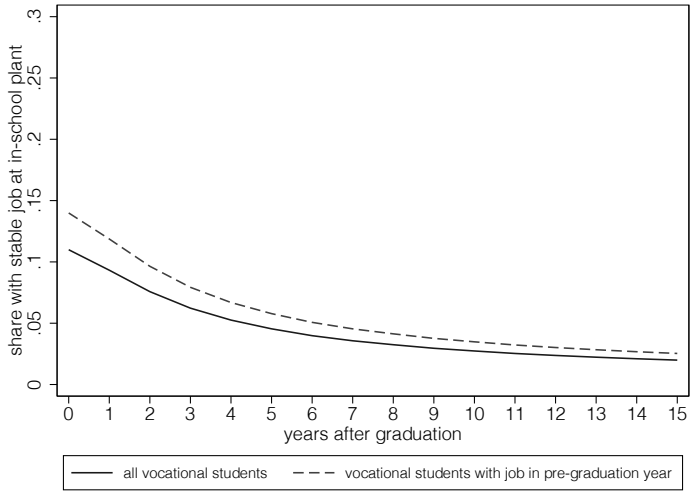


Notes: In-school job experience includes all graduates with positive earnings from a job in the year before graduation. Share returning to in-school job establishment refers to the share of all vocational graduates/vocational graduates with in-school jobs that find their first employment after graduation at a former in-school establishment.

Figure 4 shows the share of each graduation cohort that is employed at a former in-school establishment for up to 15 years after graduation. In-school jobs

seem to be short term jobs that are most important during the first few years after graduation. Five years after graduation, the share working for a former employer decreases with two thirds and levels out at around 4 percent after 15 years.

Figure 4. Fraction of vocational track graduates working at in-school establishment



Notes: Sample: Vocational graduates 1986-2001. In-school job experience includes all graduates with positive earnings from a job in the year before graduation. Share returning to in-school job plant refers to the share of all vocational graduates/vocational graduates with in-school jobs that is employed at a former in-school establishment in year t after graduation.

From the figure, it may appear that those jobs are stepping stone jobs that last for only a short period of time, but they are in fact less short term than jobs that are found through other channels at the same career stage (see also Hensvik et al. (2017) for a similar analysis). Table 1 shows the estimation results of a simple regression comparing the probability of remaining with the first employer after graduation for students from the same class who worked for the employer already prior to graduation and for those who did not. The probability to stay with the first employer is notably higher for students who had worked for the same employer even prior to graduation. Estimates remain higher even ten years after labor market entry, amounting to half of the mean outcome. An implication is that jobs found through in-school employers provide graduates with better long term prospects (unless they would lock students into jobs that do not match their qualifications, but as shown in section 6.2, results do not suggest that such lock-in effects are present).

Table 1. *First jobs at former in-school employers last longer*

Probability of staying in first job in	(1) t+3	(2) t+5	(3) t+10
Former in-school employer	0.031***	0.027***	0.023***
	(0.001)	(0.001)	(0.001)
Female	-0.015***	-0.013***	-0.009***
	(0.001)	(0.001)	(0.001)
Age	-0.001	-0.001	-0.002*
	(0.002)	(0.002)	(0.001)
Immigrant background	-0.022***	-0.016***	-0.012***
	(0.001)	(0.001)	(0.001)
Grade percentile rank	0.005***	0.004***	0.002
	(0.002)	(0.001)	(0.001)
Mean outcome	0.089	0.065	0.042
Observations	654,588	596,307	448,363
R-squared	0.182	0.155	0.126
controls	yes	yes	yes
class FE	yes	yes	yes

Notes: *** p<0.01, ** p<0.05. Robust standard errors clustered on classes. t refers to the year of graduation. Sample includes all vocational track graduates from 1986-2016 with an in-school job that generated positive earnings in the pre-graduation year. Immigrant background is an indicator variable for whether either parent is born outside of Sweden.

1.4.4 Defining establishment closures

Students who lose their connections to a former employer right before entry into the labor market are left with fewer opportunities than their peers with an intact connection. This notion captures both the loss of firm-specific human capital that was acquired on the job as well as social contacts to the previous employer. In order to capture a true loss of this type of opportunities as consequence of a closure, I want to capture closures of established employers that resulted in the ceasing (and not transfer) of business activities and that occurred close to the graduation date of any former student employees and thus impacted students' possibilities to establish ties to new employers after the closure, but prior to graduation.

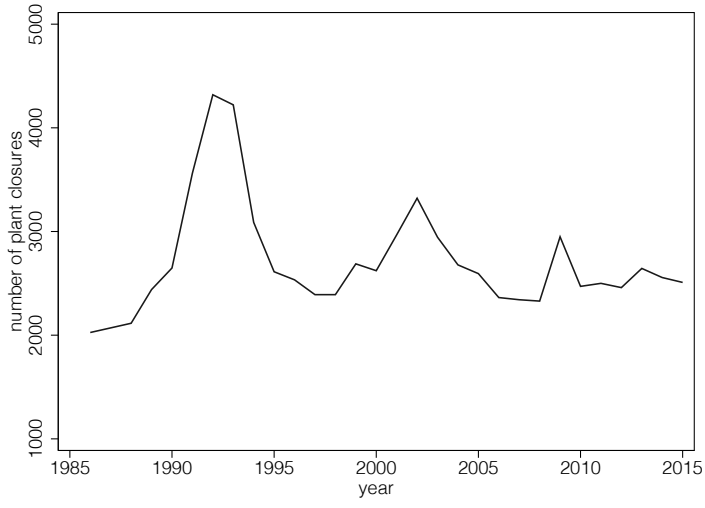
I impose three restriction in order to define what constitutes an establishment closure. First, I identify all cases in the employer-employee register where an establishment's identifier disappears between the year prior to graduation $t-1$ and the year following graduation $t+1$. Second, closures should occur between October in the year prior to graduation and June in the graduation year (i.e. the last employment spell at a closing establishment should end within nine months prior to graduation in June). Third, no more than 30

percent of the stable workforce[8] at the ceasing establishment should be found at another establishment in year t+1. I apply this restriction following Hethey-Maier and Schmieder (2013) and Eliason et al. (2018) in order to capture a closure instead of mergers or dispersals. Application of this rule also leads to the exclusion of closures of establishments with fewer than four employees.[9]

Workers are deprived of the opportunities that arise from in-school work if they were employed at an establishment that satisfies the above three criteria.

Figure 5 shows the number of establishment closures for each year during my analysis period.[10] The bulk of the variation in establishment closures occurs during the time period during the first half of the 1990s when Sweden was hit by a severe recession. During the rest of the time period, the number of closures has been relatively stable with around 2000-3000 establishments closing each year.

Figure 5. Number of establishment closures



Notes: The graph shows the number of establishment closures in the economy in a given year.

Figure 6 shows the share of all vocational graduates that has been affected by the closure of a former in-school job establishment (conditional on having positive income from an in-school job). Generally, between 1 and 1.5 percent of (vocational) graduates were affected by a closure by the time they graduated

[8] Stable workforce refers to those employees who had a continuous employment spell at the closing establishment that meets the criteria of what constitutes a stable job. Thus, summer workers or short term substitutes are not counted as part of the stable workforce.

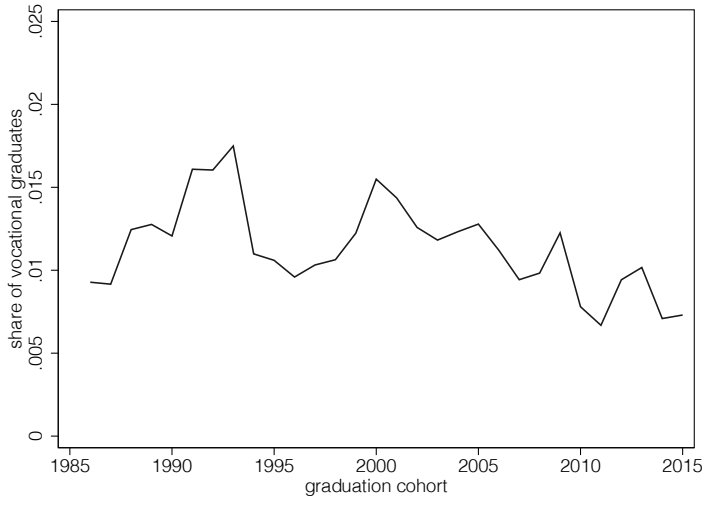
[9] I verify that my results hold when I use different cutoffs for establishment size (see section 5.2.4).

[10] Since I only include closures that allow me to identify an establishment with a distinct physical location, public sector jobs are excluded.

and fluctuations mirror the changes in the number of closures in the previous figure. While the share might seem small, it should capture the cases where opportunities were in fact lost and should be relevant for a larger segment than just these 1-1.5 percent.

Notably, the share of affected students was highest during the recession in the early 1990s. It also increased during the recession in 2008, but has followed a slight downward trend since the 2000s.

Figure 6. Share of graduates affected by in-school establishment closures



Notes: Sample: all graduates with a summer job. The graph shows the share of graduates who had a summer job at a plant that closed in graduation year t.

1.4.5 Summary statistics

Table 1 shows summary statistics for the analysis sample. The statistics are displayed separately for graduates with an in-school job at an establishment that did not close and graduates who were employed by establishments that closed just prior to graduation. The panels are fairly balanced even though women and graduates with an immigration background are slightly over-represented among graduates at closing in-school job establishments. Students who were affected by a a closure also attend somewhat larger classes. Students who do not have connections to a former employer due to a closure have notably lower employment rates upon graduation.

With regard to establishment characteristics, we see that closing establishments are, in line with the literature, smaller in terms of number of employees (see Eliason and Storrie, 2006). Likewise, the income that an in-school job at a closing establishment generates in the pre-graduation year is slightly smaller,

Table 2. *Descriptives by closure status of in-school establishment*

	(1) closure=0		(2) closure=1	
	mean	sd	mean	sd
Female	0.449	0.497	0.486	0.500
Grade percentile rank	0.510	0.286	0.499	0.287
Age	18.749	0.434	18.722	0.448
Immigrant mother	0.058	0.233	0.086	0.281
Immigrant father	0.073	0.260	0.111	0.314
Immigrant background	0.087	0.281	0.127	0.333
Class size	33.607	32.563	37.458	35.743
Income from in-school job	16716.721	15867.748	16045.982	15289.515
Average monthly wage	3411.056	3988.789	2750.823	2959.082
Size of in-school establishment	314.392	1124.876	246.761	3078.196
Stable job after graduation	0.414	0.493	0.353	0.478
Observations	701356		7876	

Notes: The analysis sample consists of all vocational graduates 1986-2015 with some positive earnings from a job in the year prior to graduation and non-missing values for the controls.

even though this might simply reflect the fact that in-school jobs are cut short by the establishment closure.

I provide balance tests for the baseline model and different specifications in the next section.

1.5 Short term results

1.5.1 Main results

In this section, I examine the short run consequences for labor market entry for students who lost the opportunities from previous employer connections due to an establishment closure. I estimate the effect of a closure on the probability of finding a stable job in the year of graduation by gradually introducing the baseline model (1) and several modifications.

Table 3 shows the estimation results. I start out by estimating the model using cohort fixed effects only. Column (1) shows the estimated impact of an establishment closure on the probability of finding a stable job. In this setup, a closure is related to a 5.4 percentage points decrease in the probability to find employment in the graduation year.

However, cohort fixed effects do not account for regional differences in background and labor market characteristics. In columns (2)-(4), I therefore use class fixed effects to estimate the model, which allows me to compare students who obtained the same education directed towards a specific profession. The impact of a closure is now associated with a 3.5 percentage point decrease in the probability of finding a stable job and is reduced to 2.7 percent-

age points after I include individual and establishment level controls. Note that column (3) corresponds to the baseline model set out in the empirical model section. All controls are highly significant and of the expected sign. Students with higher monthly earnings are more likely to find stable employment after graduation. The entire reduction in the size of the estimate that occurs between columns (2) and (3) is driven by the inclusion of the control for average monthly log earnings, while individual background characteristics or plant size controls do not appear to matter, suggesting that the class fixed effect do a good job in controlling for background characteristics. However, the effect is still substantial and accounts for about 7 percent in relation to the mean. It corresponds to half of the estimated effect of having an immigrant background, which is one of the most important factors explaining variation in employment rates.

In column (4), I allow for more flexible firm level controls in terms of logged earnings and plant size. The results suggest that the functional form does not matter and the estimate is almost identical.

As mentioned earlier, a concern is that results may be driven by differences in the industry of the in-school establishments. This concern is addressed in columns (5) and (6), which re-estimates the baseline model (with and without controls) with class-industry fixed effects. The identifying variation occurs between classmates with an in-school job at an establishment in the same 2-digit industry. The estimated effects are remarkably similar to the ones obtained using class fixed effects only and thus confirm the validity of the baseline model. Closures become slightly more important for short term employment prospects with an estimated impact of 3.1 percentage points after including the controls; implying that post-graduation employment rates are higher among students with in-school jobs in industries that are affected by more closures. While the results are still precisely estimated, this approach is of course much more demanding of the data and the standard errors for this specification almost double.

In column (7), I go one step further. Since average monthly earnings is driving the reduction in the estimate, I narrow my comparison group to students in the same earnings tercile within a given class and the same in-school job industry. While this specification might be too demanding of the data, it does reassuringly yield a significant and even slightly larger estimate of the effect, but at the cost of less precision.

The estimates are remarkably consistent throughout a number of specifications and imply that graduates who lost an employer connection due to the closure of a former in-school job establishment have a notably less successful school-to-work transition than their classmates. Table 4 illustrates that the negative effect on employment corresponds to the share of graduates that cannot compensate the loss of a re-employment opportunity by finding employment in a new establishment. Column (2) displays the effect of an establishment closure on the probability of working at a former in-school establishment upon

Table 3. *Main results: Effect on stable employment in graduation year*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Closure	-0.054***	-0.035***	-0.027***	-0.029***	-0.035***	-0.031***	-0.037**
	(0.005)	(0.006)	(0.006)	(0.006)	(0.011)	(0.011)	(0.018)
Female			-0.016***	-0.017***		-0.013***	-0.012**
			(0.002)	(0.002)		(0.004)	(0.006)
Age			0.036***	0.036***		0.040***	0.041***
			(0.003)	(0.003)		(0.005)	(0.008)
Immigrant background			-0.057***	-0.057***		-0.056***	-0.054***
			(0.002)	(0.002)		(0.004)	(0.006)
Grade percentile rank			0.041***	0.041***		0.037***	0.033***
			(0.003)	(0.003)		(0.005)	(0.007)
log monthly earnings			0.046***	0.077***		0.053***	0.041***
			(0.001)	(0.004)		(0.001)	(0.003)
log plantsize			-0.013***	0.007***		-0.002**	-0.001
			(0.000)	(0.002)		(0.001)	(0.002)
sq log monthly earnings				-0.002***			
				(0.000)			
sq log plantsize				-0.002***			
				(0.000)			
Mean outcome	0.41	0.41	0.41	0.41	0.41	0.41	0.41
Fixed effects	**cohort**	**class**	**class**	**class**	**class-industry**	**class-industry**	**class-ind. earn. tercile**
Observations	709,232	709,232	709,232	709,232	709,003	709,003	709,003
R-squared	0.054	0.233	0.245	0.245	0.604	0.610	0.759

Notes: *** p<0.01, ** p<0.05. Robust standard errors clustered on classes. Sample includes all vocational track graduates from 1986-2015 with a job that generated positive earnings in the pre-graduation year. Monthly earnings refers to the average monthly earnings from the pre-graduation year at the in-school job establishment. Immigrant background is an indicator variable for whether either parents is born outside of Sweden. The model in column (1) is estimated using cohort-fixed effects, columns (2)-(4) using class fixed effects, columns (5)-(6) using class-industry fixed effects and column (7) class-industry-earnings tercile fixed effects.

graduation. I estimate the baseline model laid out in section 2 with the new outcome of having a stable job at the previous in-school establishment. Graduates who were affected by a closure are on average 13 percentage points less likely to be stably employed by a former in-school establishment.

In column 3, I change the outcome to having a stable job at an establishment that is not the in-school establishment. A closure increases the probability of working for a new employer by 10.5 percentage points, implying that about 80 percent of the effect in column (2) is driven by replacement jobs and the other 20 percent by non-employment, which corresponds to the main effect in column 1 (compare to table 3, column 3).

Table 4. *Composition of main effect*

	(1) Stable job	(2) Stable job at in-school est.	(3) Stable job at non-in-school est.
Closure	-0.027***	-0.131***	0.105***
	(0.006)	(0.002)	(0.006)
Mean outcome	0.41	0.15	0.26
Controls	yes	yes	yes
class FE	yes	yes	yes
Observations	709,232	709,232	709,232
R-squared	0.150	0.198	0.096

Notes: *** p<0.01, ** p<0.05. Robust standard errors clustered on classes. Sample includes all vocational track graduates from 1986-2015 with an in-school job that generated positive earnings in the pre-graduation year. Controls include sex, immigrant background, grade percentile rank, average monthly log earnings and log plantsize.

1.5.2 Validation exercises & robustness checks

Balance tests

A main concern is that student sorting into closing establishments is not random and that students who are employed at such establishments have worse labor market outcomes for other reasons.

Table 5 estimates the differences in characteristics of table 1 using the baseline model with class-fixed effects and the controls as dependent variable. While differences in individual characteristics are precisely estimated, they are tiny: having worked in an establishment that closes decreases the grade rank by 1.8 percentiles. The probability to have an immigration background increases with two percentage points for students whose in-school workplace was affected by a closure.

Table 5. *Balance tests*

	(1) class FE-model	(2) class-industry FE-model	(3) class-industry-tercile FE-model
Female	0.002	-0.013	-0.014
	(0.005)	(0.009)	(0.016)
Age	0.009***	0.014**	0.016
	(0.003)	(0.007)	(0.011)
Immigrant background	0.023***	0.010	0.012
	(0.004)	(0.008)	(0.013)
Grade percentile rank	-0.018***	-0.021***	-0.020
	(0.003)	(0.006)	(0.010)
Log avg monthly wage	-0.175***	-0.062**	-0.043
	(0.015)	(0.028)	(0.024)
Log plant size	-0.141***	0.029	0.000
	(0.018)	(0.030)	(0.047)
Observations	709,232	709,003	709,003

Notes: *** p<0.01, ** p<0.05. Note: Robust standard errors clustered on classes. The table shows the results of using the control variables of the preferred model from equation (1) as outcome variables using the indicated set of fixed effects.

The covariates that do matter for the magnitude of the estimated effect of a closure are the control for the characteristics of the closing establishments, mainly average monthly earnings and size of the establishment (measured as the number of employees). In table A.1, I gradually introduce first individual controls and in a second step average monthly earnings and establishment size in both the class FE and the class-industry FE model. While the introduction of the individual's background characteristics do not matter for the size of the estimate, we see that the reduction in the magnitude is indeed driven by the earnings and size controls, indicating that selection in individual characteristics is of little concern in practice.

For re-assurance, I also provide balance tests for the model using class-industry fixed effects in the second column of table 5. The estimates in column

(2) show that differences in immigration background are driven by differences in the industries of closing firms and not significantly different by closure status of the establishment, thus further mitigating concerns about selection.

In the third column, I provide the same balance test, while making use of the fact that monthly earnings do matter. In this setup, I only use variation within in-school establishment industry among classmates in the same earnings tercile, which however leads to a loss of precision. None of the covariates are significantly different by closure status. Reassuringly, using those class-industry-earnings tercile fixed effects to estimate the effect on having a stable job in the year of graduation provided significant and even slightly larger estimates of the main effect (see table 3, column 7).[11] As a whole, the above tests show that selection is highly unlikely to be driving the main results.

Placebos

I corroborate that conclusion that selection is unlikely to be driving the results by employing an alternative strategy using placebo closures. The idea is that immediate employment outcomes upon graduation should not be affected by closures of former in-school establishments that occur in the future. We would expect that such placebo closures should have no, or at least a smaller impact, on the probability to find stable employment after graduation.

It is of course possible that a future closure in the years following graduation might already diminish students' opportunities to some extent before it occurs. However, even in that case we would expect smaller effects. Any sizable negative effect of such placebos would suggest that students are negatively selected into ceasing establishments.

In practice, I identify in-school establishments that close during the three years following graduation. I then estimate the effects of such future placebo closures on the probability of having a stable job during the graduation year.

Table A.1 shows the effect of such placebo closures during the first three years after graduation. In all cases, the estimates are close to zero (compare to the baseline in table 3, column (4)) and not statistically significant, suggesting that there is no evidence for sorting of worse-performing students into ceasing establishments. Note also that the results are not due to lack of statistical power.

[11] Note also that average earnings are closely correlated with the size of the establishment. Using variation between classmates with an in-school job in the same industry and establishment size category provides similar balance tests and estimates of the effect on stable employment as using earnings terciles.

Table 6. *Placebos: Effect of closure in t+i on stable employment in graduation year*

	(1)	(2)	(3)
Placebo closure in $t+1$	0.009		
	(0.006)		
Placebo closure in $t+2$		0.008	
		(0.007)	
Placebo closure in $t+3$			0.009
			(0.007)
Mean outcome	0.41	0.41	0.41
Additional controls	yes	yes	yes
class FE	yes	yes	yes
Observations	674,149	647,176	618,795
R-squared	0.244	0.244	0.244

Notes: *** p<0.01, ** p<0.05. Robust standard errors clustered on classes. t refers to the year of graduation. Placebo analysis uses maximum years available after restricting on plants closing in future years.

Removing Seasonal Industries

A caveat to the interpretation of the results is the fact that the nature of in-school jobs varies widely. In the analysis, I do not make a distinction between the relevance of in-school jobs for future career prospects, implying that certain types of jobs are included that have little potential for providing future opportunities of finding a stable job. This might particularly apply to jobs in seasonal industries, such as ice cream vendors or farm workers during harvesting season.

As a robustness check, I re-run the baseline model from table 3 after excluding jobs in mainly seasonal industries. In order to avoid arbitrariness in the definition of seasonal industries, I use a data-driven approach instead of manually excluding industries. I first calculate the length of all students' employment spells within the same industry to arrive at the share of employment spells within a given industry that only last throughout the three months of summer. If the industry share of seasonal spells is larger than a given cutoff, I define those industries as seasonal and exclude them from the analysis.

Table 7 shows the results of this exercise. The different columns correspond to different cutoffs with regard to the share out of all jobs that only occur during the summer season. Using the definition in column (1), the point estimate is as good as identical to the one obtained in the baseline model. Applying a more restrictive definition of a seasonal industry in columns (2) and (3), the point estimates are highly significant and larger than previously, hence imply-

Table 7. *Removing jobs in seasonal industries*

	Excluding industries with $> X\%$ seasonal spells		
	$>70\%$	$>50\%$	$>30\%$
	(1)	(2)	(3)
Outcome: stable employment in graduation year			
Closure	-0.026***	-0.031***	-0.037***
	(0.006)	(0.006)	(0.007)
Female	-0.016***	-0.017***	-0.016***
	(0.002)	(0.002)	(0.003)
Age	0.037***	0.040***	0.050***
	(0.003)	(0.003)	(0.004)
Immigrant background	-0.058***	-0.058***	-0.061***
	(0.002)	(0.002)	(0.003)
Grade percentile rank	0.041***	0.042***	0.055***
	(0.003)	(0.003)	(0.003)
log avg monthly earnings	0.048***	0.055***	0.068***
	(0.001)	(0.001)	(0.001)
log plantsize	-0.011***	-0.008***	-0.004***
	(0.000)	(0.000)	(0.001)
Observations	675,328	594,993	393,340
R-squared	0.248	0.257	0.275
class FE	yes	yes	yes

Notes: *** p<0.01, ** p<0.05. Robust standard errors clustered on classes. Sample includes all vocational track graduates from 1986-2015 with an in-school job that generated positive earnings in the pre-graduation year and that worked in seasonal industries. Seasonal industries are defined as those industries, in which more than the indicated share of employment spells occurs during the three months long summer season. Average monthly earnings is the average monthly earnings from the pre-graduation year at the in-school job plant. Immigrant background is an indicator variable for whether either parents is born outside of Sweden.

ing that establishment closures might have an even greater impact in industries that are not subject to strong seasonality.

Restrictions on establishment size

Due to the rule that not more than 30 percent of the workforce should be employed at the same new establishment following a closure, I exclude establishments with less than four employees from the analysis. I confirm that other size restriction on establishments as commonly applied in the literature do not affect the validity of the results.

Table 8. *Restrictions on establishment size*

Excluding establishments with	< 5 employees		< 10 employees	
	(1)	(2)	(3)	(4)
Outcome: stable employment in graduation year				
Closure	-0.028***	-0.032***	-0.030***	-0.028**
	(0.006)	(0.011)	(0.006)	(0.012)
Female	-0.015***	-0.013***	-0.008***	-0.006
	(0.002)	(0.004)	(0.002)	(0.004)
Age	0.037***	0.042***	0.037***	0.043***
	(0.003)	(0.006)	(0.003)	(0.006)
Immigrant background	-0.057***	-0.056***	-0.057***	-0.056***
	(0.002)	(0.004)	(0.002)	(0.004)
Grade percentile rank	0.040***	0.036***	0.035***	0.029***
	(0.003)	(0.005)	(0.003)	(0.005)
log avg monthly earnings	0.046***	0.054***	0.046***	0.055***
	(0.001)	(0.001)	(0.001)	(0.001)
log plantsize	-0.014***	-0.003***	-0.017***	-0.007***
	(0.000)	(0.001)	(0.000)	(0.001)
class FE	yes	no	yes	no
class-industry FE	no	yes	no	yes
Observations	692,020	691,824	619,479	619,359
R-squared	0.246	0.613	0.254	0.623

Notes: *** p<0.01, ** p<0.05. Robust standard errors clustered on classes. Sample includes all vocational track graduates from 1986-2015 with a job that generated positive earnings in the pre-graduation year at an establishment with at least four (columns 1-2) or 10 (columns (3-4) employees. Average monthly wage is the average wage earned in the pre-graduation year at the in-school job plant. Immigrant background is an indicator variable for whether either parents is born outside of Sweden.

I use two cutoffs and exclude establishments with less than (1) five or (2) 10 employees following the more recent literature on plant closures that typically excludes very small establishments using a cutoff of 10 employees or fewer (see, for instance, Eliason and Storrie, 2009; Eliason et al., 2018; Hethey-Maier and Schmieder, 2013; Huttunen et al., 2011).

Table 8 re-estimates the main model using both class and class-industry fixed effects, while excluding establishments with less than five (columns 1-2) or less than ten (columns 3-4) employees. In all cases, the results confirm the validity of the main results and the estimates are indeed very similar to the ones obtained in table 3, although the effect is slightly larger using the class fixed effects model as opposed to the class-industry fixed effects model when applying the cutoff of at least 10 employees.

1.5.3 Heterogeneous effects

Previous research has suggested that the benefits of social contacts vary by individuals' characteristics, providing evidence that social contacts might be more important for the less-educated and men (see, for instance Pellizzari, 2010; Corcoran et al., 1980; Datcher, 1983; Elliot, 1999). Consequently, the impact of an establishment closure has the potential to vary across individuals' characteristics. Likewise, Hensvik et al. (2017) have shown that employer contacts are more predictive of where young workers start their careers during recessions.

I test for heterogeneous results in those dimensions by using a pooled regression with separate fixed effects and covariates for each gender, grade quartiles, parents' highest education background, low vs high unemployment years and establishment size. The pooled regressions are estimated using both class fixed effect and class-industry fixed effects. Overall, the results suggest (with few exceptions) little evidence of systematic heterogeneity and results are therefore confined to the appendix.

Using the class fixed effects model, the effects are very similar to the baseline estimate for almost all groups (see tables A.2 and A.3) and there are no significant differences by gender, grade quartiles or unemployment rates. However, the effects for students with highly educated parents (as determined by the parent with the highest education level) appear insulated (see panel C). The results indicate that the negative employment effect is driven by students whose parents have at most finished high school, while there is no effect for students whose parents attained at least some post high school education. A possible interpretation could be that employment of students with highly-educated parents is not as affected by a closure since this group of students is more likely to obtain more education after graduation instead of entering the labor market.

Turning to the demand side, there are no significant differences by the size of the in-school work establishment (see panel E), even though the results are indicative that losing a connection matters more for students employed at an in-school establishment with less than 50 employees.

Note also that estimates are greater for low-grade and students with high school educated parents when using class-industry fixed effects (see appendix, tables A.4 and A.5). In line with the literature, the magnitude of the effect is larger for low-grade students (relative to students in the middle of the grade distribution) and during times of high unemployment. The fully interacted model does however come at the cost of precision and does not allow to statistically differentiate between the estimates.

Table A.6 (and A.7, using class-industry fixed effects) shows heterogeneous results for some of the most common fields of specialization in vocational

tracks[12] as the usefulness of employer links might vary across the field of specialization. A caveat to the interpretation of this approach is, of course, that there might be differences across tracks in the degree to which students find in-school jobs in an industry that is relevant to their field of specialization and thus results have to be interpreted with caution.

While limiting the sample to the students in a specific track leads to less precision, the results still indicate that the loss of an employer links has a negative effect on employment regardless of the chosen vocational track, but is larger in magnitude for students in construction, automechanics and electronics. Overall, the impression is that the effects are fairly homogenous across the dimensions in which I have the power to test for differences, with the one exception of socio-economic background where children of high educated parents appear insulated from the effects.

1.6 Long term results and mechanisms

1.6.1 Long term results

The previous section established that immediate employment rates are lower for graduates who are affected by a closure of an in-school job establishment. Most of those jobs are only used as stepping stones to ease the transition into the labor market, but do seldom lead to long-lasting careers. As such, one could expect the effects of such establishment closures to be temporary. At the same time, the scarring literature suggest that even short unemployment spells at the beginning of one's career can have a lasting impact for up to a decade.

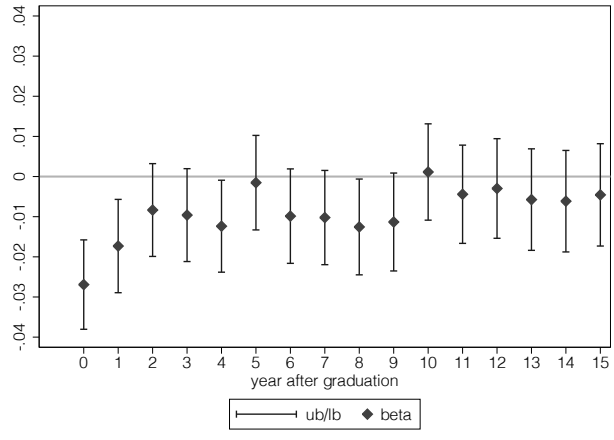
Figure 7 shows that the latter is indeed the case with regard to lost opportunities from establishment closures. The figures show the estimated effect of an establishment closure on the probability of having a stable job (a) and log earnings (b) for each of the 15 years following graduation.

The effects on stable employment are strongest immediately after graduation. By the second year after graduation, they are reduced by more than half in magnitude, but persist for about a decade before they fade out.

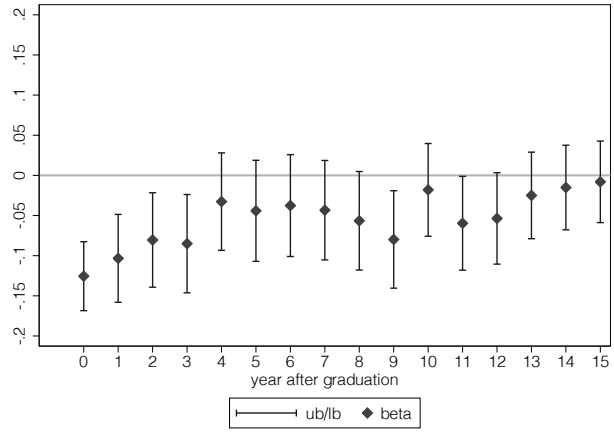
The reduction in stable employment shares comes hand-in-hand with earnings losses (see figure 7, b) . Immediately upon graduation, students who worked for an establishment that closed prior to graduation suffer from earnings losses of around 13 percent. In a similar pattern as for the stable employment outcome, losses in the subsequent years are notably smaller, but still sizable with up to 10 percent during the first three years and around or less than

[12] Since the analysis focuses on private sector in-school jobs as I could not identify a specific physical work establishments for students who were employed by municipalities, I exclude two of the most common vocational tracks (health care and childcare) that typically lead to employment by a municipality.

Figure 7. Long term effects of closure in year t+i after graduation



(a) Effect on having a stable job



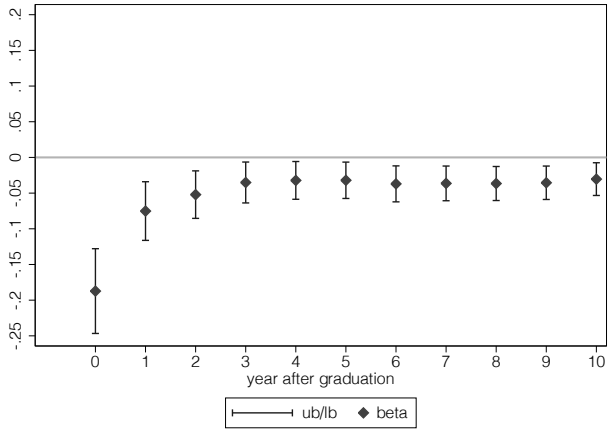
(b) Effect on log earnings

Notes: Sample includes all vocational track graduates from 1986-2001 with an in-school job that generated positive earnings in the pre-graduation year and that can be followed for the indicated number of years after graduation. Estimates correspond to estimating the model in column (3) in table 3 and are displayed with the corresponding 95% confidence intervals. Standard errors are clustered on classes.

5 percent thereafter. Earnings losses are persistent for around a decade before they level out.[13] Students who were affected by the closure prior to graduating

[13]For completeness, I estimate the effect of a closure over a 10-year follow-up period using a balanced sample as well (see figure A.2), which reduces my original sample by about a third. The immediate effect on finding stable employment is slightly smaller in the sample with 10-year follow-up period (around .023 as compared to .027), but larger thereafter. The effects on

Figure 8. Long term effect of closure on accumulated earnings



Notes: Sample includes all vocational track graduates from 1986-2005 with an in-school job that generated positive earnings in the pre-graduation year and that can be followed for the indicated number of years after graduation. Estimates correspond to estimating the model in column (3) in table 3 and are displayed with the corresponding 95% confidence intervals. Standard errors are clustered on classes.

have on average 3 percent lower accumulated earnings over the entire 10-year period following graduation than their peers (see figure 8).

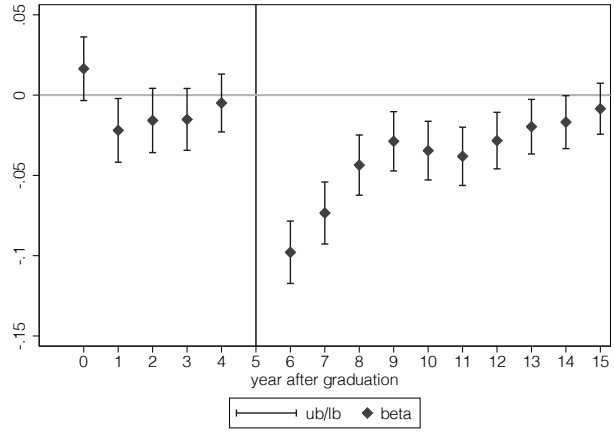
The long-term effects are even large in relation to the consequences of suffering from job loss due to an establishment closure later in life. Using the same population of graduates, figure 9 shows the long term effects of such a closure on stable employment and earnings. Since the loss of an actual job is arguably much more disruptive, we would expect the impact to be greater than that related to the missed opportunities due to the closure of an in-school establishment.

As opposed to many previous papers that have focused on the effect of plant closures for male and more tenured workers, I focus on the same sample of graduates (as in the main analysis) five years into their careers as a relevant comparison group, without placing restrictions on gender, tenure or labor market attachment at the time the closure occurs. A comparison of the effects of a closure of a former in-school establishment and a closure of a workplace five years after graduation shows that the estimated effects are indeed much larger in the latter case (albeit in relation to considerably higher employment rates five years after graduation) and particularly with regard to stable employment. Effects on employment and earnings persist for a similar time period as the ef-

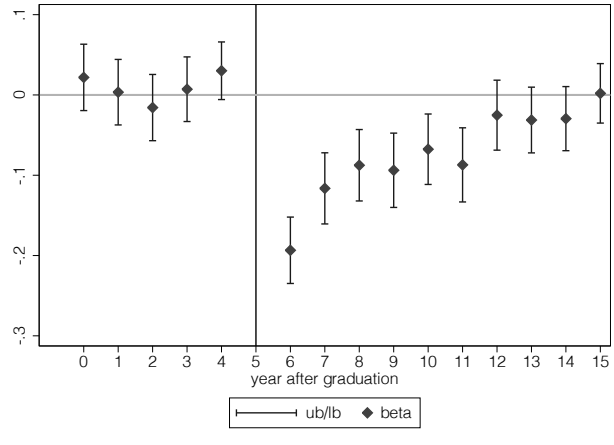
log earnings are also a bit larger in magnitude, but confirm the general pattern and persistence of the effects in both cases.

fects from an in-school establishment closure, but decline more quickly before they level off.

Figure 9. Effects of job loss due to an establishment closure later in life



(a) Effect on having a stable job



(b) Effect on log earnings

Notes: Sample consists of all graduates with a summer job 1986-2001. Estimates correspond to estimating the main model using a closure in year 5 after graduation and are displayed with the corresponding 95% confidence intervals. Standard errors are clustered on classes.

The results thus reinforce the picture that the negative effects from lost opportunities of a closure prior to graduation take notably long to subside completely and can even be considered large given that only a third of students is making use of their opportunities in the baseline.

1.6.2 Match quality and industry adjustments

So far I have shown that graduates who lose an employer link just prior to entering the labor market have worse employment outcomes. We would expect the importance of those links to vary dependent on whether students worked in an industry that was related to the vocational track or not. As the data does not allow to directly identify industries that correspond to a given vocational track, I rely on a statistical criteria instead of manually matching vocational tracks and industries in order to avoid arbitrariness in measuring whether the industry of an in-school establishment is related to a vocational track. For students in each vocational track, I identify the two most common industries that the graduates worked in in five years after graduation. In-school jobs are defined as relevant with respect to the vocational track if the in-school establishment operated in either of these two most common industries.

Table 9 shows the results from a pooled regression with separate fixed effects and covariates for students working in an industry related and unrelated to the vocational track they are enrolled in.[14] The effect of a closure on stable employment is negative in both cases, but four times larger if the industry of the in-school job was related to the vocational track, indicating that the majority of the effect is driven by closures in relevant industries (panel A). Connections to establishments in other industries appear to be less helpful in finding stable employment; likely because they only provide employer links to industries with worse career prospects given the field of specialization and therefore less desirable job options. After ten years, the employment effect subsides regardless of whether the in-school job was in an industry related to the field of specialization.

The results show that the closure of a former in-school establishment matters more if the industry of the establishment is related to the field of specialization of the affected student. Students who lose the link to an employer in a relevant industry do not only lose the chance of re-employment at the same establishment, but might also be at a disadvantage in entering another establishment in a relevant industry and might instead be forced to consider jobs in industries with worse career prospects in order to avoid unemployment. The closure of an in-school establishment could thus shift students from the industry of the in-school establishment towards different industries.

Table 10 shows the effect of a closure on the probability of being employed in an 2-digit industry other than the 2-digit industry of the in-school establishment directly upon graduation and ten years after graduation using the full sample in column (1) and the sample of graduates from 1986-2005 that I can follow for a 10-year period in (column 2).[15] The model is estimated using class-industry fixed effects, indicating that students in the same track and with

[14] Table A.8 shows the results using class-industry fixed effects.

[15] Estimating the immediate effect in (column 1) for the sample 1986-2005 leads to an estimated effect of 0.019* (0.010) of a closure.

Table 9. *By relevance of in-school job*

	non-relevant in-school industry (1)	relevant in-school industry (2)	Difference (2)-(1) (3)
A. Stable job in t			
Closure	-0.012*	-0.049***	-0.038***
	(0.007)	(0.012)	(0.014)
Mean outcome	0.385	0.480	
Additional controls	yes		
Class FE	yes		
Observations	709,232		
R-squared	0.299		
B. Stable job in t+10			
Closure	0.005	0.002	-0.002
	(0.008)	(0.013)	(0.015)
Mean outcome	0.771	0.783	
Additional controls	yes		
Class FE	yes		
Observations	474,241		
R-squared	0.299		

Notes: *** p<0.01, ** p<0.05, * p<0.10. Robust standard errors clustered on classes. t indicates graduation year. Results are from a pooled regression with separate fixed effects for in-school jobs in industries that are/ are not related to the vocational track field. Lincom is used to calculate estimates and test for differences. Sample includes all vocational track graduates from 1986 to 2015 (Panel A)/2005 (Panel B) with a job that generated positive earnings in the pre-graduation year.

an in-school job within the *same* industry are 1.5 percentage points more likely (significant at the ten percent level) to switch industries upon graduation if they were affected by a closure (see panel A). The entirety of the short run effect is driven by industry adjustments of graduates who had an in-school job in an industry relevant to their field of specialization and the effect is about a third in relation to the mean. Adverse effects of a closure are thus magnified for graduates who lost a link to an employer in a relevant industry; not only are they less likely to be employed in the short run, but they also end up in worse matches if they find a stable job.[16]

[16] Short term effects are larger when conditioning on employment status, but less well identified (see table A.9). About half of employed graduates find their first stable job in an industry different from the one of the in-school job.

Table 10. *Industry adjustments*

	all	non-relevant in-school industry	relevant in-school industry	Difference (3)-(2)
	(1)	(2)	(3)	(4)
A. Stable job in different industry in t				
Closure	0.015*	0.004	0.029***	0.025
	(0.008)	(0.013)	(0.011)	(0.017)
Mean outcome	0.185	0.226	0.091	
Additional controls	yes	yes		
Class-industry FE	yes	yes		
Observations	709,001	709,001		
R-squared	0.663	0.663		
B. Stable job in different industry in t+10				
Closure	0.022*	0.021	0.025	0.004
	(0.013)	(0.017)	(0.021)	(0.027)
Mean outcome	0.611	0.674	0.439	
Additional controls	yes	yes		
Class-industry FE	yes	yes		
Observations	448,134	448,134		
R-squared	0.559	0.559		

Notes: *** p<0.01, ** p<0.05, * p<0.10. Robust standard errors clustered on classes. t indicates graduation year. Results in columns 2-3 are from a pooled regression with separate fixed effects for in-school jobs in industries that are/ are not related to the vocational track field. Lincom is used to calculate estimates and test for differences. Sample includes all vocational track graduates with a job that generated positive earnings in the pre-graduation year. Sample includes graduates from 1986 to 2015 (panel A) and 1986-2005 (panel B). In panel B, the sample is also restricted to cases in which I can identify a consistent industry ten years later.

In fact, industry adjustments are not only temporary, but persist even ten years after graduation (see panel B), though inference by relevance of the in-school job industry is limited due to the smaller sample size for the 10 year follow-up period.

1.6.3 Replacement contacts

I have shown that graduates who lost the opportunities provided by an intact employer connection at the time of graduation have worse employment outcomes in the short run as compared to their their peers with an intact connection. However, the negative employment effect might be partly offset if graduates are able to replace missed opportunities from the in-school establishment closure with alternative methods of job finding. A potential strategy could be to substitute lost employer connections with greater reliance on other existing social contacts. Social contacts that might be readily available for young workers are parents and other family members or displaced co-workers from the in-school establishment who have found another job following the closure of their former work establishment. In a first step, I will focus on parental links, whose importance for the school-to work transition has been documented by Kramarz and Skans (2014), showing that students who find their first job through their parents enter the labor market faster and remain longer in their jobs while experiencing faster wage growth.

I start out by letting the effect of a closure vary by the presence of a parent at the in-school establishment. In table 11, I extend model (1) by adding an interaction between closure status and having a parent present at the in-school establishment and examine the effects on short and long run employment and accumulated earnings after ten years.

Even though I measure the effect of a parent who is present at the in-school establishment as opposed to at the first job after graduation, the general effects of working at the same establishment as a parent are well in line with Kramarz and Skans (2014). Graduates who worked with one of their parents prior to graduation fare better with respect to short and long run employment outcomes as well as accumulated earnings.

The negative effects of a closure on finding a stable job in the year of graduation are indeed magnified by the presence of a parent at the in-school establishment and are in fact three times as large as the baseline effect. However the presence of a parent at the closing plant matters only in the beginning of the career and does not seem to magnify the effects in the long run.

In table 12, I explore whether the larger employment effects can be explained by the fact that students are deprived of a possible channel to replace the lost job opportunity through finding employment with a parent.

I revisit the results in table 4, showing how a closure affects the probability of having a stable job at the previous in-school establishment (see column 1) and at an establishment other than the in-school establishment.

As already shown, graduates who were affected by a closure are on average 13 percentage points less likely to be stably employed by a former in-school establishment. About 80 percent of the effect in column (1) is driven by replacement jobs (and the other 20 percent by non-employment). In column 3, I check whether those replacement jobs occur at a parent's work establishment by estimating the baseline model and defining a dummy variable as the

Table 11. *Parental contacts*

VARIABLES	(1) Stable job in t	(2) Stable job in t+10	(3) Accumulated earnings t+10
Closure	-0.021***	0.003	-0.023*
	(0.006)	(0.007)	(0.012)
Closure*parent at job	-0.048***	-0.010	-0.033
	(0.017)	(0.018)	(0.034)
Parent at in-school job	0.011***	0.012***	0.040***
	(0.001)	(0.002)	(0.003)
Female	-0.015***	-0.123***	-0.283***
	(0.002)	(0.002)	(0.004)
Age	0.036***	-0.019***	-0.062***
	(0.003)	(0.003)	(0.005)
Immigrant background	-0.056***	-0.072***	-0.209***
	(0.002)	(0.003)	(0.005)
Grade percentile rank	0.041***	0.077***	0.179***
	(0.003)	(0.003)	(0.005)
log avg monthly earnings	0.046***	0.011***	0.051***
	(0.001)	(0.001)	(0.001)
log plantsize	-0.013***	-0.005***	-0.013***
	(0.000)	(0.000)	(0.001)
Observations	709,232	474,241	448,363
R-squared	0.245	0.127	0.290
class FE	yes	yes	yes
SE clustered on	classes	classes	classes

Notes: *** p<0.01, ** p<0.05, * p<0.10. Robust standard errors clustered on classes. Sample includes all vocational track graduates from 1986-2015 (column 1) or 1986-2005 (columns 2-3) with an in-school job that generated positive earnings in the pre-graduation year. Average monthly earnings refers to the average earnings in the pre-graduation year at the in-school establishment. Immigrant background is an indicator variable for whether either parent is born outside of Sweden. Closure*parent at job is an interaction between the closure dummy and the dummy for whether a parent is present at the in-school job establishment.

outcome that takes on the value 1 if a graduate works at the same establishment as one of the parents (and other than the in-school establishment) upon graduation. A closure is associated with a 1.4 percentage point increase in the probability to simultaneously work with either parent, which corresponds to about five percent of replacement jobs. This suggests that graduates rely in part on their parents to substitute for the loss of employer connections, sug-

Table 12. *Alternative job finding channels*

	(1) Stable job at in-school est.	(2) Stable job at non-in-school est.	(3) Stable job at new plant with parent
Closure	-0.131***	0.105***	0.014***
	(0.002)	(0.006)	(0.002)
Mean outcome	0.153	0.260	0.020
class FE	yes	yes	yes
Observations	709,232	709,232	709,232
R-squared	0.150	0.198	0.096

Notes: *** p<0.01, ** p<0.05. Robust standard errors clustered on classes. Sample includes all vocational track graduates from 1986-2015 with an in-school job that generated positive earnings in the pre-graduation year. Average earnings is the average monthly earnings from the pre-graduation year at the in-school establishment. Immigrant background is an indicator variable for whether either parent is born outside of Sweden.

gesting that the effects of a closure are magnified by the presence of a parent because students are deprived of a possible alternative channel to find a job after the loss of a direct employer link.

1.7 Conclusion

In this article, I examine the impact of losing the connection to a former in-school employer just prior to labor market entry. I contribute to the the literature on social connections and informal networks by exploiting exogenous variation in such connections that is generated by closures of former in-school establishments that occur just prior to labor market entry and show that graduates' short term employment outcomes are negatively affected if they entered the labor market without an intact employer connection.

A conclusion is that idiosyncratic job search opportunities have scarring effects that mimic those arising from poor aggregate labor market conditions at the time of market entry. There are sizable effects on both stable employment and earnings for up to a decade. As such, the longevity of the effects is well in line with effects of entering the labor market during a recession (see Kahn, 2010; Oreopoulos et al., 2012).

The results may seem surprising with regard to the transitory nature of first jobs as the loss of a job that usually is only a stepping stone should likely not yield grave consequences in the long run. However, graduates who find their first jobs at a former in-school establishment seem to be better matched to their

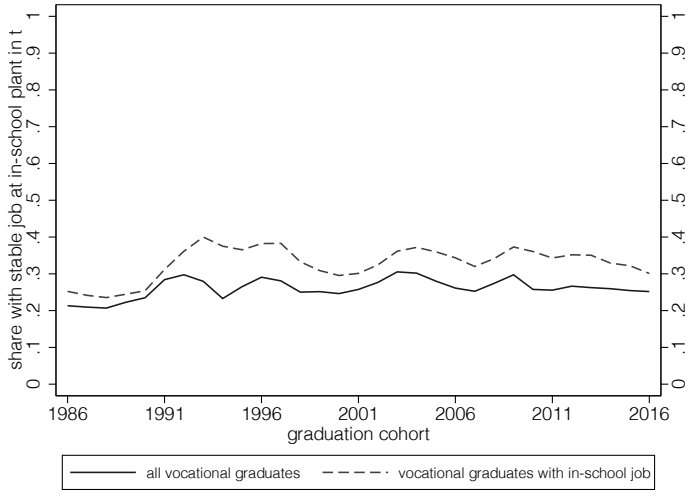
employers as they are more likely to remain there longer than graduates who find their first job through other means.

The negative effects of losing an employer link as the consequence of a closure are driven by the loss of links to employment opportunities in industries that are relevant to the field of specialization in vocational school. Students who lose such relevant links suffer the consequences twofold: not only do they miss out on re-employment opportunities, but, upon the loss of such a opportunity, they are also more likely to adjust by shifting to first jobs in other less-relevant industries.

From a policy perspective, it is important to determine to what extent alternative channels of job-finding can alleviate or even out the absence of employer links. I provide evidence for interplay between different job search channels, which may suggest that institutionalized job search support, for instance through schools or other government agencies, may be particularly useful for students without retained contacts from market work experience and in particular if the family network is weak.

Appendix

Figure A.1. Direct transitions: Fraction returning to in-school work establishment after graduation



Notes: In-school job experience includes all graduates with positive earnings from a job in the year before graduation. Share returning to in-school establishment refers to the share of all vocational graduates/vocational graduates with in-school jobs that find their first employment after graduation at a former in-school establishment **conditional on having a stable job after graduation**.

Table A.1. *Gradual introduction of controls*

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome: Effect of closure on having stable job in t						
Closure	-0.035***	-0.033***	-0.027***	-0.035***	-0.034***	-0.031***
	(0.006)	(0.006)	(0.006)	(0.011)	(0.011)	(0.011)
Female		-0.021***	-0.016***		-0.017***	-0.013***
		(0.002)	(0.002)		(0.004)	(0.004)
Age		0.044***	0.036***		0.049***	0.040***
		(0.003)	(0.003)		(0.005)	(0.005)
Immigrant background		-0.064***	-0.057***		-0.059***	-0.056***
		(0.002)	(0.002)		(0.004)	(0.004)
Grade percentile rank		0.052***	0.041***		0.048***	0.037***
		(0.003)	(0.003)		(0.005)	(0.005)
log avg monthly earnings			0.046***			0.053***
			(0.001)			(0.001)
log plantsize			-0.013***			-0.002**
			(0.000)			(0.001)
Mean outcome	0.41	0.41	0.41	0.41	0.41	0.41
Observations	709,232	709,232	709,232	709,003	709,003	709,003
class FE	**yes**	**yes**	**yes**	no	no	no
class-industry FE	no	no	no	**yes**	**yes**	**yes**
R-squared	0.233	0.236	0.245	0.604	0.605	0.610

Notes: *** p<0.01, ** p<0.05. Robust standard errors clustered on classes. Sample includes all vocational track graduates from 1986-2015 with a job that generated positive earnings in the pre-graduation year. Average monthly earnings is the average earnings in the pre-graduation year at the in-school job plant. Immigrant background is an indicator variable for whether either parents is born outside of Sweden. The model in column (1)-(3) is estimated using class-fixed effects and in columns (4)-(6) using class-industry fixed effects.

Table A.2. *Heterogeneous Effects: Effect of closure on having stable job in graduation year*

	(1)	(2)	(3)
A. By gender:	Men	Women	Difference
Closure	-0.028***	-0.022***	0.006
	(0.008)	(0.008)	(0.012)
Mean outcome	0.443	0.377	
B. By grade quartiles:	Lowest	Highest	Difference
Closure	-0.022*	-0.026**	-0.004
	(0.013)	(0.013)	(0.018)
Mean outcome	0.406	0.375	
C. By parents' education:	Compulsory	High School	Difference
Closure	-0.038	-0.034***	0.005
	(0.023)	(0.008)	(0.024)
Mean outcome	0.493	0.434	
	Compulsory	Post high school	Difference
Closure	-0.038	0.004	0.042*
	(0.023)	(0.012)	(0.025)
Mean outcome	0.493	0.352	

Notes: *** p<0.01, ** p<0.05, * p<0.10. Robust standard errors clustered on classes. Results are from a pooled regression with separate fixed effects and covariates for men and women (panel A), grade quartiles (panel B) and parents' highest education level (panel C) respectively. Lincom is used to calculate estimates and test for differences. Sample includes all vocational track graduates from 1986-2015 with a job that generated positive earnings in the pre-graduation year. Parents' highest education level is determined by the parent with the highest level of education.

Table A.3. *Heterogeneous Effects: Effect of closure on having stable job in graduation year*

D. By unemployment rates:	Low UR	High UR	Difference
Closure	-0.026***	-0.027***	-0.002
	(0.009)	(0.007)	(0.011)
Mean outcome	0.486	0.356	
E. By establishment size:	Less than 50	50-100	Difference
Closure	-0.037***	-0.033*	0.004
	(0.009)	(0.017)	(0.019)
Mean outcome	0.423	0.416	
	Less than 50	More than 100	Difference
Closure	-0.037***	-0.025**	0.012
	(0.009)	(0.012)	(0.015)
Mean outcome	0.423	0.400	

Notes: *** p<0.01, ** p<0.05, * p<0.10. Robust standard errors clustered on classes. Results are from a pooled regression with separate fixed effects and covariates for high and low unemployment years (panel D) and establishment size respectively. Lincom is used to calculate estimates and test for differences. Sample includes all vocational track graduates from 1986-2015 with a job that generated positive earnings in the pre-graduation year.

Table A.4. *Heterogeneous Effects: Effect of closure on having stable job in graduation year*

	(1)	(2)	(3)
A. By gender:	Men	Women	Difference
Closure	-0.038**	-0.032*	0.006
	(0.019)	(0.017)	(0.025)
Mean outcome	0.443	0.377	
B. By grade quartiles:	Lowest	Highest	Difference
Closure	-0.043	-0.017	0.026
	(0.038)	(0.031)	(0.049)
Mean outcome	0.407	0.375	
C. By parents' education:	Compulsory	High School	Difference
Closure	0.004	-0.048**	-0.052
	(0.062)	(0.022)	(0.065)
Mean outcome	0.493	0.434	
	Compulsory	Post high school	Difference
Closure	0.004	-0.004	-0.008
	(0.062)	(0.028)	(0.068)
Mean outcome	0.493	0.352	

Notes: *** p<0.01, ** p<0.05, * p<0.10. Robust standard errors clustered on classes. Results are from a pooled regression with separate class-industry fixed effects and covariates for men and women (panel A), grade quartiles (panel B), parents' highest education level (panel C). Lincom is used to calculate estimates and test for differences. Sample includes all vocational track graduates from 1986-2015 with a job that generated positive earnings in the pre-graduation year. Parents' highest education level is determined by the parent with the highest level of education.

Table A.5. *Heterogeneous Effects: Effect of closure on having stable job in graduation year*

D. By unemployment rates:	Low UR	High UR	Difference
Closure	-0.027*	-0.034**	-0.007
	(0.016)	(0.015)	(0.022)
Mean outcome	0.486	0.356	
E. By establishment size:	Less than 50	50-100	Difference
Closure	-0.052***	-0.028	0.023
	(0.019)	(0.043)	(0.047)
Mean outcome	0.423	0.416	
	Less than 50	More than 100	Difference
Closure	-0.052***	-0.022	0.030
	(0.019)	(0.030)	(0.036)
Mean outcome	0.423	0.400	

Notes: *** p<0.01, ** p<0.05, * p<0.10 Robust standard errors clustered on classes. Results are from a pooled regression with separate class-industry fixed effects and covariates for high and low unemployment years (panel D) and establishment size (panel E) respectively. Lincom is used to calculate estimates and test for differences. Sample includes all vocational track graduates from 1986-2015 with a job that generated positive earnings in the pre-graduation year.

Table A.6. *Heterogeneous effects: by vocational track specialization*

	(1) Construction	(2) Electricity	(3) Hotel/restaurant	(4) Business	(5) Auto	(6) Machinery	(7) Electronics
Closure	-0.036*	-0.009	-0.018	-0.034**	-0.099***	-0.004	-0.052**
	(0.020)	(0.028)	(0.015)	(0.017)	(0.027)	(0.029)	(0.022)
Female	-0.133***	-0.021	-0.006	-0.012***	-0.116***	-0.053***	0.005
	(0.011)	(0.017)	(0.005)	(0.005)	(0.016)	(0.011)	(0.011)
Age	-0.044***	0.012	0.036***	0.046***	-0.064***	-0.027**	0.013
	(0.009)	(0.011)	(0.011)	(0.008)	(0.012)	(0.012)	(0.010)
Immigrant background	-0.060***	-0.078***	-0.037***	-0.074***	-0.054***	-0.062***	-0.044***
	(0.009)	(0.009)	(0.007)	(0.006)	(0.011)	(0.010)	(0.008)
Grade percentile rank	0.245***	0.068***	0.185***	0.059***	0.137***	0.006	-0.110***
	(0.009)	(0.011)	(0.009)	(0.008)	(0.012)	(0.011)	(0.010)
log monthly earnings	0.035***	0.034***	0.056***	0.061***	0.033***	0.030***	0.035***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
log plantsize	-0.016***	-0.018***	-0.001	-0.014***	-0.017***	-0.012***	-0.013***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)
Observations	59,981	48,788	54,477	64,514	38,543	45,397	46,872
R-squared	0.254	0.232	0.134	0.168	0.265	0.273	0.201
class FE	yes	yes	yes	yes	yes	yes	yes

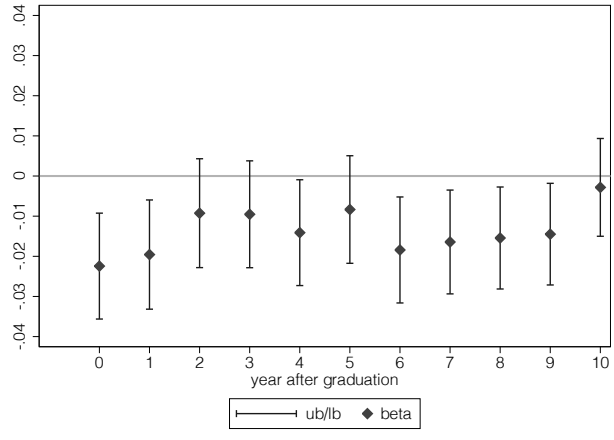
Notes: *** p<0.01, ** p<0.05, * p<0.10. Robust standard errors clustered on classes. Sample includes all graduates from 1986-2015 who attended the indicated vocational track and with a job that generated positive earnings in the pre-graduation year. Average monthly earnings is the average monthly earnings in the pre-graduation year at the in-school job plant. Immigrant background is an indicator variable for whether either parents is born outside of Sweden. The model is estimated using class-fixed effects.

Table A.7. *Heterogeneous effects: by vocational track specialization*

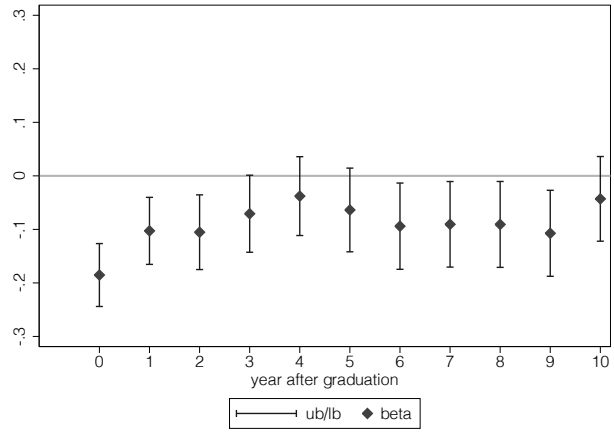
	(1) Construction	(2) Electricity	(3) Hotel/restaurant	(4) Business	(5) Auto	(6) Machinery	(7) Electronics
Closure	-0.076**	0.021	-0.016	-0.101***	-0.159**	0.027	-0.090
	(0.031)	(0.064)	(0.021)	(0.031)	(0.066)	(0.066)	(0.063)
Female	-0.137***	-0.014	-0.010	-0.016*	-0.108**	-0.025	0.004
	(0.017)	(0.035)	(0.008)	(0.008)	(0.042)	(0.018)	(0.030)
Age	-0.057***	0.007	0.026*	0.049***	-0.067**	-0.009	0.013
	(0.013)	(0.024)	(0.015)	(0.014)	(0.030)	(0.023)	(0.027)
Immigrant background	-0.059***	-0.079***	-0.034***	-0.071***	-0.061**	-0.065***	-0.025
	(0.013)	(0.020)	(0.011)	(0.011)	(0.026)	(0.018)	(0.021)
Grade percentile rank	0.240***	0.076***	0.180***	0.052***	0.100***	-0.022	-0.115***
	(0.013)	(0.025)	(0.013)	(0.014)	(0.029)	(0.022)	(0.025)
log monthly earnings	0.036***	0.038***	0.064***	0.074***	0.039***	0.031***	0.035***
	(0.004)	(0.005)	(0.003)	(0.004)	(0.007)	(0.006)	(0.006)
log plantsize	-0.004	-0.011**	0.009***	0.005	-0.007	-0.011**	-0.005
	(0.003)	(0.005)	(0.003)	(0.003)	(0.006)	(0.005)	(0.005)
Observations	59,960	48,780	54,467	64,462	38,524	45,375	46,859
R-squared	0.535	0.660	0.433	0.543	0.715	0.651	0.707
controls	yes	yes	yes	yes	yes	yes	yes
class FE	no	no	no	no	no	no	no
industry x class FE	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.10. Robust standard errors clustered on classes. Sample includes all graduates from 1986-2015 who attended the indicated vocational track and with a job that generated positive earnings in the pre-graduation year. Average monthly earnings is the average monthly earnings in the pre-graduation year at the in-school job plant. Immigrant background is an indicator variable for whether either parents is born outside of Sweden. The model is estimated using class-industry fixed effects.

Figure A.2. Long term effects of closure in year t+i after graduation



(a) Effect on having a stable job



(b) Effect on log earnings

Notes: Sample includes all vocational track graduates from 1986-2005 with an in-school job that generated positive earnings in the pre-graduation year and that can be followed for the indicated number of years after graduation. Estimates correspond to estimating the model in column (3) in table 3. Standard errors are clustered on classes.

Table A.8. *By relevance of in-school job*

	non-relevant in-school industry (1)	relevant in-school industry (2)	Difference (2)-(1) (3)
A. Stable job in t			
Closure	-0.019	-0.045***	-0.026
	(0.014)	(0.016)	(0.022)
Mean outcome	0.385	0.480	
Additional controls	yes		
industry x class FE	yes		
Observations	709,001		
R-squared	0.606		
B. Stable job in t+10			
Closure	0.015	0.002	-0.013
	(0.016)	(0.018)	(0.024)
Mean outcome	0.771	0.783	
Additional controls	yes		
industry x class FE	yes		
Observations	474,012		
R-squared	0.523		

Notes: *** p<0.01, ** p<0.05, * p<0.10. Robust standard errors clustered on classes. Results are from a pooled regression with separate fixed effects for in-school jobs in industries that are/ are not related to the vocational track field. Lincom is used to calculate estimates and test for differences. Sample includes all vocational track graduates from 1986-2015 with a job that generated positive earnings in the pre-graduation year. Average earnings is the average monthly earnings in the pre-graduation year at the in-school establishment. Immigrant background is an indicator variable for whether either parent is born outside of Sweden.

Table A.9. *Industry adjustments, alternative samples*

	(1) Stable job in t in diff industry	(2) Stable job in t+10 in diff industry
Closure	0.091***	0.026**
	(0.022)	(0.013)
Female	-0.005	-0.007
	(0.006)	(0.005)
Age	0.005	0.008
	(0.006)	(0.005)
Immigrant background	-0.003	0.013**
	(0.007)	(0.005)
Grade percentile rank	-0.012*	0.011**
	(0.007)	(0.006)
log avg monthly wage	-0.047***	-0.014***
	(0.002)	(0.002)
log plantsize	-0.002	0.005***
	(0.002)	(0.001)
Mean	0.479	0.832
Observations	273,830	329,034
R-squared	0.815	0.626
class-industry FE	yes	yes
Sample	All with stable job	Until 2005 & stable job in t+10

Notes: *** p<0.01, ** p<0.05, * p<0.10. Robust standard errors clustered on classes. Sample includes all vocational track graduates from 1986-2015 (column 1)/1986-2005 (column 2) with a job that generated positive earnings in the pre-graduation year and with a stable job in the outcome year. Average earnings is the average monthly earnings in the pre-graduation year at the in-school establishment. Immigrant background is an indicator variable for whether either parent is born outside of Sweden.

References

Alam, M., Carling, K., and Nääs, O. (2015). The program and treatment effect of summer jobs on girls' post-schooling incomes. *Evaluation review*, 39(3):339–359.

Arulampalam, W. (2008). Is unemployment really scarring? Effects of unemployment experiences on wages. *The Economic Journal*, 111(475):F585–F606.

Bayer, P., Ross, S., and Topa, G. (2008). Place of work and place of residence: Informal hiring networks and labor market outcome. *Journal of Political Economy*, 116(6):1150–1196.

Beaman, L. and Magruder, J. (2012). Who gets the job referral? Evidence from a social networks experiment. *American Economic Review*, 102(7):3574–3593.

Browning, M., Moller Dano, A., and Heinesen, E. (2006). Job displacement and stress-related health outcomes. *Health Economics*, 15(10):1061–1075.

Burgess, S., Propper, C., Rees, H., and Shearer, A. (2003). The class of 1981: The effects of early career unemployment on subsequent unemployment experiences. *Labour Economics*, 10(3):291 – 309.

Carr, R. V., Wright, J. D., and Brody, C. J. (1996). Effects of high school work experience a decade later: Evidence from the national longitudinal survey. *Sociology of Education*, 69(1):66–81.

Cingano, F. and Rosolia, A. (2012). People I know: Job search and social networks. *Journal of Labor Economics*, 30(2):291–332.

Corcoran, M., Datcher, L., and Duncan, G. (1980). Information and influence networks in labor markets. In Duncan, G. and Morgan, J., editors, *Five Thousand American Families: Patterns of Economic Progress.*, pages 1–37. Institute For Social Research, Ann Arbor.

Datcher, L. (1983). The impact of informal networks on quit behavior. *The Review of Economics and Statistics*, 65(3):491–495.

Dustmann, C., Glitz, A., Schönberg, U., and Brücker, H. (2016). Referral-based job search networks. *Review of Economic Studies*, 83(2):514–546.

Eliason, M. (2012). Lost jobs, broken marriages. *Journal of Population Economics*, 25(4):1365–1397.

Eliason, M., Hensvik, L., Kramarz, F., and Skans, O. N. (2018). The causal impact of social connections on firms' outcomes. mimeo, Uppsala University.

Eliason, M. and Storrie, D. (2006). Lasting or latent scars? Swedish evidence on the long-term effects of job displacement. *Journal of Labor Economics*, 24(4):831–856.

Eliason, M. and Storrie, D. (2009). Job loss is bad for your health – Swedish evidence on cause-specific hospitalization following involuntary job loss. *Social Science & Medicine*, 68(8):1396 – 1406.

Elliot, J. (1999). Social isolation and labor market insulation: Network and neighborhood effects on less-educated urban workers. *The Sociological Quarterly*, 40(2):199–216.

Fujita, S. and Moscarini, G. (2017). Recall and unemployment. *American Economic Review*, 107(12):3875–3916.

Glitz, A. (2017). Coworker networks in the labour market. *Labour Economics*, 44:218 – 230.

Gregg, P. (2001). The impact of youth unemployment on adult unemployment in the NCDS. *The Economic Journal*, 111(475):626–653.

Häkkinen, I. (2006). Working while enrolled in a university: Does it pay? *Labour Economics*, 13:167–189.

Hensvik, L., Müller, D., and Skans, O. N. (2017). Connecting the young: High school graduates' matching to first jobs in booms and great recessions. IFAU Working Paper 2017:2.

Hethey-Maier, T. and Schmieder, J. F. (2013). Does the use of worker flows improve the analysis of establishment turnover? Evidence from German administrative data. *Schmollers Jahrbuch: Journal of Applied Social Science Studies*, 133(4):477–510.

Holmlund, B. (2003). The rise and fall of Swedish unemployment. CESifo Working Paper No. 918.

Hotz, V. J., Xu, L. C., Tienda, M., and Ahituv, A. (2002). Are there returns to the wages of young men from working while in school? *The Review of Economics and Statistics*, 84(2):221–236.

Huttunen, K., Møen, J., and Salvanes, K. G. (2011). How destructive is creative destruction? Effects of job loss on job mobility, withdrawal and income. *Journal of the European Economic Association*, 9(5):840–870.

Ioannides, M. Y. and Loury, L. D. (2004). Job information, neighborhood effects and inequality. *Journal of Economic Literature*, 42(4):1056–1093.

Jacobson, L. S., LaLonde, R. J., and Sullivan, D. G. (1993). Earnings losses of displaced workers. *The American Economic Review*, 83(4):685–709.

Kahn, L. B. (2010). The long-term labor market consequences of graduating from college in a bad economy. *Labour Economics*, 17:303–316.

Kramarz, F. and Skans, O. N. (2014). When strong ties are strong: Networks and youth labour market entry. *Review of Economic Studies*, 81(3):1164–1200.

Light, A. (2001). In-school work experience and the returns to schooling. *Journal of Labor Economics*, 19(1):65–93.

Montgomery, J. (1991). Social networks and labor-market outcomes: Toward an economic analysis. *The American Economic Review*, 81(5):1408–1418.

Neumark, D. (2002). Youth labor markets in the United States: Shopping around vs. staying put. *The Review of Economics and Statistics*, 84(3):462–482.

Oreopoulos, P., von Wachter, T., and Heisz, A. (2012). The short- and long-term career effects of graduating in a recession. *American Economic Journal: Applied Economics*, 4(1):1–29.

Pellizzari, M. (2010). Do friends and relatives help in getting a good job? *Industrial and Labor Relations Review*, 63(3):494–510.

Raaum, O. and Røed, K. (2006). Do business cycle conditions at the time of labor market entry affect future employment prospects? *The Review of Economics and Statistics*, 88(2):193–210.

Ruhm, C. J. (1997). Is high school employment consumption or investment? *Journal of Labor Economics*, 15(4):735–776.

Saygin, P. O., Weber, A., and Weynandt, M. A. (2019). Coworkers, networks, and job search outcomes. *ILR Review*.

Skans, O. N. (2011). Scarring effects of the first labor market experience. IZA Discussion Paper No. 5565.

Stevens, A. H. (1997). Persistent effects of job displacement: The importance of multiple job losses. *Journal of Labor Economics*, 15(1, Part 1):165–188.

Sullivan, D. and von Wachter, T. (2009). Job displacement and mortality: An analysis using administrative data. *The Quarterly Journal of Economics*, 124(3):1265–1306.

Topa, G. (2011). Labor markets and referrals. In Benhabib, J., Bisin, A., and Jackson, M. O., editors, *Handbook of Social Economics*, volume 1B, pages 1193–1221. Elsevier Science, Amsterdam.

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

with Lena Hensvik and Oskar Nordström Skans

Acknowledgments: We thank David Figlio, Chris Huckfeldt, Kirabo Jackson, Francis Kramarz, Rafael Lalive, Kevin Lang, David Neumark, Martin Nybom, Anna Sjögren and seminar audiences at UCL, IFAU, UCLS, the 2016 EEA-meeting and the 2016 Swedish Economists Meeting for useful comments and Forte for financial support.

2.1 Introduction

The transition between school and work is generally perceived as a formative stage in young workers' careers, and it is a well documented fact that young workers' labor market prospects tend to be highly sensitive to business cycle fluctuations (Kahn, 2010; Oreopoulos et al., 2012). It is also well-understood that young workers frequently rely on social and informal contacts of various forms to smooth the transition into the labor market (see e.g. Kramarz and Skans, 2014; Bentolila et al., 2010). However, we have very little systematic evidence on how young workers' use of social connections in the search for first jobs changes across the cycle. In this paper, we add to the literature by studying connections acquired through paid work during high school and we show that this is a quantitatively very important transition-path into the labor market. Our main contribution to the literature is, however, to document how the connections are used in the matching of young workers to their first post-graduation jobs across the cycle. To this end, we use Swedish data that spans a 25-year period, which includes both booms and great recessions.

One of the key advantages of informal hiring channels from the firms' perspectives is that connections seem to mitigate the inherent uncertainty faced by recruiting firms and thus reduce firm-level hiring costs (see e.g. Oyer and Schaefer, 2011; Hensvik and Skans, 2016; Dustmann et al., 2016). But the existence of informal hiring channels may also dampen aggregate labor market fluctuations if recessions provide firms with better opportunities to use their networks. This may be particularly important for labor market entrants since information asymmetries appear more severe for these groups (see e.g. Altonji and Pierret, 2001; Jovanovic, 1979) and since their labor market prospects tend to be particularly sensitive to business cycle fluctuations (Kahn, 2010; Oreopoulos et al., 2012).

Indeed, most of the theoretical work on informal non-market matches suggests that firms should prefer to hire through informal channels if given the choice, which suggests that contacts should be used more in recessions when competition for labor is lower. For example, Montgomery (1991) and Casella and Hanaki (2006) show that firms should prefer to hire based on private information about workers' qualities (if they can) since they do not have to pay the full market valuation of this information. Similarly, Dustmann et al. (2016) (on networks) and Fujita and Moscarini (2017) (on recalls) argue that firms should prefer to hire through informal channels because these channels give them superior private information about otherwise elusive aspects of idiosyncratic match quality (as in Jovanovic, 1979). A parallel literature on apprenticeship training (Acemoglu and Pischke, 1998, 1999a,b) makes very similar arguments. These studies emphasize that firms can exploit the monopsony power provided by private information about pre-existing skills and general training generated within such programs. An insight from this literature is that with high unemployment, the monopsony power of the firm is even larger,

suggesting that firms should rely more heavily on informal hires during times of low tightness when workers' outside options are more limited.

On the empirical side, evidence (on cyclicality) is much more scarce. Fujita and Moscarini (2017) show that the recall share of hires is counter-cyclical and Kramarz and Skans (2014) show results indicating that matching through family ties is more prevalent when unemployment is high. 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.

In this paper, we use reduced-form regressions to infer how the share of matches formed through informal ties generated through market work during high school varies with the aggregate cycle. Our contribution to the literature is threefold: First, we are able to jointly study the two main channels of informal hires: "recall-type" direct links (through previous work experience from the hiring establishment) and indirect links (through social networks). The existing literature discussed above suggests that these channels may reduce hiring costs in a similar fashion, but they are rarely studied together. 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 can use multiple placebo-type regressions to verify our interpretation. Third, we can decompose the relationship between hiring channels and business cycle movements into changes in effect sizes for a given type of agents, and cyclical changes in the composition of connected agents on both the demand (firms) and supply (workers) side.

Our analysis focuses on the role of market-work 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, 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 co-workers from the summer jobs who relocate to new establishments before graduation. 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.

The aim of the paper is to measure how the use of these two sets of informal contacts (direct and indirect) vary with the business cycle. At the same time, we need to account for changes across the cycle in the counterfactual probability that the very same graduate would have been hired by a connected employer even without the social tie. We use the employment patterns of classmates with different summer jobs to measure the counterfactual allocation.[1] In

[1]The strategy follows Kramarz and Skans (2014) although they analyzed parental contacts and did not explicitly focus on the role of the cycle.

practice, we estimate statistical models where we regress indicators of social contacts and class-plant fixed effects on the probability that the graduate finds the first job with each specific employer. We then relate the ensuing estimates to indicators of the business cycle conditions at the time of graduation.

Our used data include the records of all graduates from Swedish vocational high school tracks between 1986 and 2010.[2] The graduates enter the labor 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 co-workers both before and after graduation.

Our findings indicate that connections acquired during summer jobs are strong predictors of where vocational students find their first stable jobs after graduation. The impact is particularly strong and robust for the direct links (i.e. returning to the summer job establishment), and the results clearly point in the same direction for the indirect links (i.e. through a moved worker), although in the latter case with less statistical power in some of the robustness exercises. On average, the impact of each indirect connection provided by a co-worker who moved before graduation is 1/75 of the impact of the link to the actual workplace when using our preferred specification (0.56 vs. 42 percentage points). Both effects are much more pronounced in deep recessions. Our preferred specification suggests that the impact of informal ties is twice as large in bad times (0.54 for return hires and 0.008 for each connecting co-worker who moved) as in good times (0.26 and 0.004). To validate our findings and account for preferential hiring patterns of workers from a specific establishment, we use a set of placebo strategies using instead *other establishments within the same firm and location* (instead of the actual establishments) and *co-workers who move just before the start of a summer job spell* (instead of those moving just after). The results for these placebo exercises strongly support the interpretation that our main 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 also remain robust when we (instead of using the class-fixed effects strategy) use our placebo variation as an alternative source of identification, i.e. when comparing summer job establishments to other establishments within the same firm for the same graduate, or when comparing the probability that graduates find employment with co-workers who moved after the summer job to co-workers who moved *before* the summer job (again, for the same graduate).

[2]Hensvik and Skans (2014) provide a policy oriented survey article on social contacts and youth labor market entry. In that article, we briefly illustrated the role of summer jobs with similar data as we use here, but focusing only on graduates during 2006, i.e. without discussing the business cycle.

For the direct links, our main results hold across all segments of the market and across both genders, but some of the sub-sample estimates are insignificant when studying the indirect "return" links due to lack of statistical power. We find similar results (for both types of connections) if we account for other changes over time by conditioning on time trends and time quadratics. All results are also robust if we allow for delayed labor market entry, or if we remove those that had summer jobs where their parents worked. Estimates remain negative if we use local instead of aggregate labor market conditions.

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), firm fixed effects, and the business cycle. The results suggest that about one third of the business cycle variation in the usefulness of contacts 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 show that matches acquired through these social connections last longer (in particular, if formed during recessions) and are associated with persistent earnings gains of about 5 percent. Jointly, these results indicate that the matches formed through these channels are of high quality compared to market matches.

Overall, our results imply that firms hire more connected young workers in times when unemployment is high, workers have poor outside options, and the firms therefore have greater market power. This pattern holds most strongly for connections involving information about firm-specific human capital. But we find similar patterns for the moved contacts where information is acquired in the context of interactions at *another* firm and thus relate to general, rather than firm-specific, human capital. The fact that firms rely more heavily on both of these types of connections when hiring in recessions suggests that firms use their monopsony power to exploit private information about both specific and general human capital in situations when workers have few outside options. High unemployment may also induce firms to train summer-job employees in more general skills, as in the apprenticeship model of Acemoglu and Pischke (1998), making these workers more attractive to firms with private knowledge about the training through direct or indirect links. This may amplify any effects arising from the learning-process about pre-existing skills, in particular if training and ability are complementary as in Acemoglu and Pischke (1998).

The paper is structured as follows: section 2.2 provides a background of the Swedish education system and labor market conditions. Section 2.3 describes the basic set-up of our empirical model and the data. Main results, placebo estimates, robustness checks, the analysis of demand and supply side selection, as well as an analysis of employment duration and earnings are presented in section 2.4. Section 2.5 concludes.

2.2 Background

2.2.1 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 unemployment rate rose from 6 percent in 2007 to 9 percent in 2011, while the youth unemployment rate rose from 19 to 25 percent during the same period.[3]

2.2.2 Vocational high-school graduates

After nine years of compulsory schooling (at age 16), nearly all students start high school.[4] 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).[5]

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

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

[5]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.

2.2.3 Working while in school

In Sweden, summer jobs for vocational students are allocated at the regular labor market with little interventions from government agencies. There is no large scale policy corresponding to the massive Federal Work-Study program for US college students (see Scott-Clayton and Minaya, 2016, for an evaluation). The one exception is specific summer jobs provided at the local authorities (municipalities). These are typically very short and low-paying summer jobs of last resort that are intended to ensure that youths with few social contacts get access to some work experience during the summer (see Alam et al., 2015). The demand for summer job staff instead mostly arises as a function of generous vacation laws and concentrated holiday habits. All Swedish employees are granted at least 5 weeks of fully paid vacation each year. They have the legal right to take out at least 4 of these weeks as consecutive vacation during the period June to August. Traditionally, these vacation periods have been very concentrated during the "industrial holiday" occurring in the 5 weeks covering July. In addition, collective agreements often make additional vacation provisions (in particular for workers above age 40) and parents with children under the age of 8 are able to use saved parental leave entitlements to prolong their holidays. Together with obvious seasonal jobs, this generates substantial demand for temporary workers during the vacation period around July. Finally, we note that collective agreements often include generous options for employers to pay lower wages for workers until they turn 18 or 19 (Forslund et al., 2012). One likely motivation for these provisions is to increase the use of summer jobs as stepping stones into the labor market.[6]

2.3 Data and empirical methods

2.3.1 Overview

Below, we first describe our data sources and how we construct the used data before turning to the details of our main empirical model in Section 2.3.3. As a preview, note the following five features of the model that may be useful for understanding our data description:

1. Data is analysed in the form of *dyads* where one observation is a combination of a graduate (i) and an establishment (j).
2. Our models exploit variation within graduation classes.
3. Our two key explanatory variables, taking the value 0 or 1 for each (i, j) observation, are:
    - whether or not graduate i was employed by establishment j during high school

[6] At the time of writing, the Swedish parliament is discussing a potential payroll tax reduction for workers below 18 in order to further increase the prevalence of summer jobs.

- whether or not a former co-worker of graduate i's job during high school has moved into establishment j before i graduated.

4. The outcome of interest is the event that graduate i finds post-graduation employment at establishment j.
5. Our final interest lies in how the use of links changes across the business cycle.

Below, we describe each of these steps in turn, before turning to the empirical model and summary statistics.

2.3.2 Data construction

Graduates and establishments

We use graduation records from Statistics Sweden covering all graduates from Swedish vocational high school programs between the years 1985-2010. We focus on graduates from vocational high school tracks as described in the background section above.

The graduation records are linked to an employer-employee data set covering the entire Swedish economy. This enables us to identify all jobs held by graduates in the years prior to graduation and to relate these to post-graduation employment patterns. In addition, we can link the data to registers on detailed demographic background characteristics of graduates and other workers. The data include annual earnings paid by the employer to the specific worker as well as indicators for the first and last remunerated month during the year.

Defining classes

"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 since we use the "class" measure to control non-parametrically for the types of specific skills that are shared between classmates, it is sufficient to assume that these different classes are trained similarly.[7] This is discussed in more detail in Section 2.3.3 below.

Graduate-establishment links from market work during high school

Our objects of analysis are links that arise from market work during the last year of high school. Graduates are directly linked to establishments where they worked before graduating. Furthermore, by tracking the mobility patterns of all people each graduate worked with during high school, we find a set of other establishments to which each graduate has indirect links through co-workers who moved.

We identify all jobs held during the last full year (January to December) at high school. Graduation takes place in June the following year. We keep jobs

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

with annual earnings that correspond to at least two weeks of full time work.[8] We set this threshold to exclude very marginal jobs that only lasted a 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.

This allows us to define two indicator variables for pairs of graduates and establishments (our dyads):

- $Return\,Link_{ij}$ taking the value one if graduate i was employed by establishment j during the last year of high school.
- $Moved\,Link_{ij}$ taking the value one if someone (aged 20+) that graduate i worked with during high school has moved into establishment j before i graduated.

Stable jobs after graduation

We identify the place of work after graduation for graduates. Here, we place a higher bar for what we count as employment than we did for the summer jobs to assure that we are not including low-intensive jobs of the same nature as the work performed while in school. In accordance with Kramarz and Skans (2014), we thus focus on stable jobs defined as a job that covers November (five months after graduation), lasts for at least four months during a calendar year, and is paid a total sum of annual earnings corresponding to at least three months of full time work (i.e. matches where annual earnings$> 3\times$monthly minimum wage). If several employment spells satisfy our criteria, we focus on the match that generated the highest income during the given calendar year.

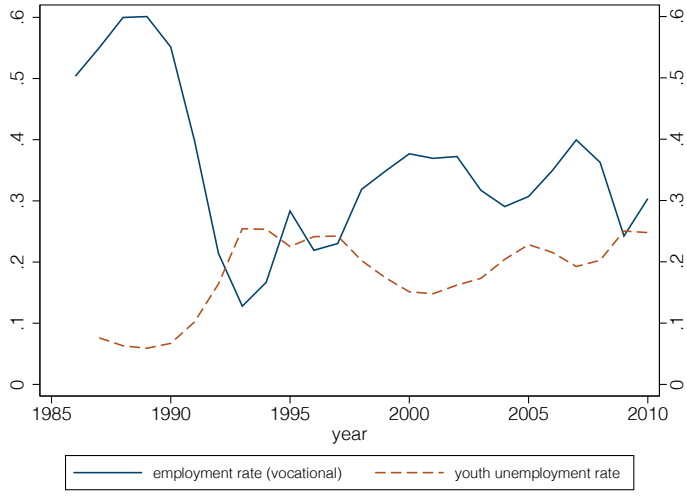
The business cycle

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 dramatically 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.

[8]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.

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.[9]

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



2.3.3 Empirical model

Our aim is to investigate how the incidence of informal hires among market entrants varies across the business cycle while accounting for changes in the counterfactual probability that a graduate would find employment at the same establishment by chance. We use classmates to approximate the counterfactual hiring patterns and we now turn to the details of how this is done in practice.

Our empirical model uses dyadic data where each observation is a combination of a graduate (i) and an establishment j. Each graduate may start her career in any existing establishment in the economy and our outcome variable, the indicator W_{ijct}, takes the value one if graduate i starts her career with employer j. The subscript ct indicates the class and graduation year. Our model includes the covariates of interest $Return\,Link_{ijct}$ (the graduate had a summer job at j) and $Moved\,Links_{ijct}$ (a co-worker from the graduate's summer job have moved into j before graduation).

In order to identify the effects of interest, we need to account for the counterfactual probability that the graduate would have entered the linked estab-

[9]The youth unemployment numbers are less reliable across time due to 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.

lishment even in the absence of a link. Our baseline strategy is to follow Kramarz and Skans (2014) and use the interaction of establishment identity and graduation class to estimate the counterfactual probability of entering that very establishment (see the robustness section for alternative identification strategies). We therefore include a full set of *class-times-establishment* fixed effects (θ_{jct}).

Our ultimate interest lies in how the impact of links varies with aggregate labor market conditions and we therefore estimate our model separately for each of the 25 graduation cohorts ($t = 1986$ to 2010) in our sample. Thus, we index all variables and parameters by *t*. This leaves us with the following model:

$$W_{ijct} = \gamma_{1t}\, Return\, Link_{ijct} + \gamma_{2t}\, Moved\, Link_{ijct} + \theta_{jct} + \varepsilon_{ijct}. \qquad (2.1)$$

where ε_{ijct} is an error term capturing all other factors within a class that affect the probability that graduate i starts working in establishment j.[10] The parameters of interest are the time-varying γ_t's, capturing the excess probability that graduates match with a linked establishment relative to the classmates' probability of entering the same establishment in a given year.

When interpreting the results, we note that the return links resemble recalls and apprenticeships and provide similar benefits as these to the hiring firms, i.e. firm-specific human capital and first-hand knowledge of how the graduate performs within the firm-specific context on top of any other benefits arising from social networks. In contrast, moved links capture a purer impact of social networks established at a workplace.

We estimate equation 2.1 year-by-year as a set of linear probability models and extract the estimates of interest. In a second step we relate the estimates 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.

The sample

Conceptually, our sample can be considered as if it includes one observation for each possible combination of a graduate and an establishment in the economy every year. There are, however, around 10 billion such possible combinations each year, most of which build on irrelevant geographical and occupational combinations. Fortunately, we only need to include data points which can contribute to identification. This implies that we only need to use

[10] 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.

segments of the data where there is variation in at least one of the *Link*-variables within the fixed effects, i.e. graduate-establishment combinations where someone within the graduate's class is linked to the specific establishment. In terms of potential sample size, it is obvious that most of the (billions of) potential graduate-establishment combinations are excluded by this restriction. Importantly, however, our sample construction does *not* remove any of the possibly identifying dyads, i.e. dyads where there are links in the first place.[11] This stands in contrast to traditional fixed effects settings where the fixed effects may soak up much of the potentially relevant variation. The sample-restriction is instead more akin to selecting a reasonable control group to the treated dyads as we retain all observations that can provide us with any substantial insights, but instead remove graduate-establishment combinations where neither the graduate nor any of his or her classmates are linked.

We impose two additional restrictions on our baseline sample, both of which may seem limiting in principle, but in practice turn out to have very little importance. First, since we are interested in understanding the share of matches that are formed due to the links, we only include graduates who do find a stable job in our baseline analysis. This implies that we exclude all graduates without any positive outcome. Second, since graduates without links do not help us identify the impact of links, we only include graduates with summer jobs in our baseline analysis. The excluded observations could in principle have helped us to pin down the counterfactual hiring patterns (i.e. the fixed effects), but we choose to exclude them since we conjecture that graduates without summer jobs may provide a worse counterfactual for the connected workers job-finding patterns than other graduates who had a summer job. Importantly, however, our results are insensitive to relaxing either of these two restrictions as we show in the robustness analysis.

Identification issues, placebos, within-worker identification and interactions with supply and demand side characteristics

The fixed effects used in our empirical model account for all interactions between classes and establishments. The interaction is crucial because it accounts for how well the graduate's skill-set overlaps with the needs of that specific employer (see also Kramarz and Skans, 2014, for a similar discussion). Our concern is not that workers with links to, e.g. establishment j, are better in general (in which case they could go into *any* establishment) but that workers with links to j have skills or attributes that make them more likely to enter into that exact establishment (j) even without the links.[12] We use classes

[11] In principle, all graduates in a class could be linked to one establishment, which would make these links useless for identification. However, this is not a relevant concern in practice.

[12] This is also the reason for why we do not include individual covariates that could capture within-class heterogeneity. Any covariate must be interacted with characteristics of j to play any role in practice.

because graduates from the same class have training in the same fields and enter the same local labor market at the same point in time.

Threats to our identification arise if graduates would be more likely to find employment at the linked establishments than their classmates, even without the links. For instance, concerning the moved links, this would be the case if both graduates and moved co-workers acquired skills at the previous workplace that are valued by a narrow subset of other establishments. Our estimates of interest may be biased if these concerns are valid and vary over the business cycle, e.g. if mobility patterns become narrower in booms or recessions. We will address this concern by studying the corresponding sorting patterns using two different types of "placebo" links: "co-workers" who moved *before* the summer job started (corresponding to the moved co-worker links) and "links" to other establishments with the same firm and location (corresponding to the actual return links). In section 2.4.2 we present results from these "placebo" exercises.

The logic underlying the placebo regressions can also be used to derive within-worker identification strategies.[13] Here, we do not rely on class-establishment fixed effects at all but instead *i)* compare the probability of finding employment with an actual co-worker who left the establishment after the summer job (actual link) to the probability of finding employment with someone who left before the summer job (counterfactual) for a given individual and *ii)* compare the probability of finding a stable job at the actual summer-job establishments (actual link) to the probability of finding a stable job at another establishments within the same location and firm (counterfactual) for a given individual. These strategies deal with all forms of individual unobserved heterogeneity as it is estimated with worker fixed effects. A disadvantage is that the models only can be identified for individuals where we observe co-worker exits both before and after the summer job (for the first strategy) and for graduates with summer jobs in multi-establishment firms (for the second strategy). Results are very similar to the baseline, however, as discussed in the robustness section.

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

[13] In terms of technicalities, this identification draws on insights from Eliason et al. (2019).

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.

2.3.4 Summary statistics

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. The overall share of graduates from vocational tracks is fairly stable across time, except for a dip during the transition from 2 to 3 year tracks (see Appendix figure B.2). 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 (see also Appendix figure B.1). 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. Despite this however, graduates with and without summer jobs appear fairly similar across the cycle, both in terms of gender and compulsory school grades (Panel B). As expected, most summer jobs generated very low total annual earnings.

Panel C displays summary statistics for the sample of graduates with both a summer job and a stable job upon graduation, i.e. the sample we focus on for most of what we do.[14] Notably, the table shows 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. In Appendix B we show more detailed descriptive statistics on the year-to-year evolution of the share of vocational graduates, the fraction of vocational school graduates and the fraction of those with a summer job who return to the same establishment upon graduation.

[14] As we show in the robustness section, our results do not change if we use broader samples including those without summer jobs or those who do not find stable employment at all.

Table 1. *Descriptive Statistics*

	(1) Low unemployment		(2) Medium unemployment		(3) High unemployment	
	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
Panel A: 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	
Panel B: 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 summer 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	
Panel C: Graduates with summer jobs and stable job upon graduation (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 summer 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	

Notes: 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.

2.4 Results

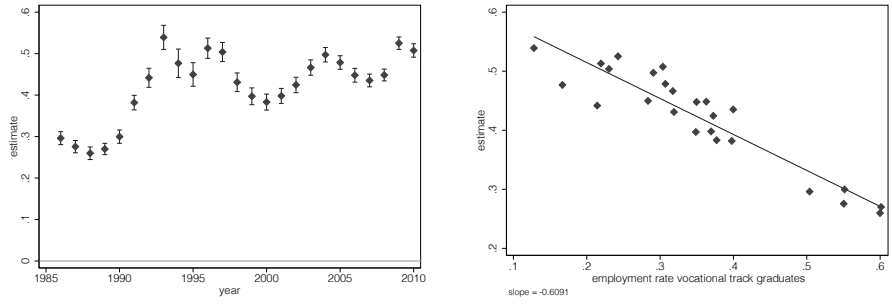
2.4.1 Main results

Estimation results for the γ_{1t}'s and γ_{2t}'s in Equation 2.1 are displayed in Figure 2 and Figure 3. Figure 2 captures the excess propensity that a graduate returns to find her first stable job at the 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 co-worker who moved.

The left-side panel of Figure 2 shows the estimates of $\gamma_1' s$ for each year of the analysis. Overall, they suggest that our measured connections are strong predictors of where graduates find their first stable jobs upon graduation: a graduate is about 30-50 percentage points more likely to return to the summer job establishment compared to classmates. In addition, the magnitudes of the estimates have a strikingly counter-cyclical pattern. This is highlighted in the right-side panel of Figure 2 which illustrates the relationship between the size of the estimates and the post-graduation employment rate (for completeness, Table 2 below shows that the constant and slope of the fitted regression line is statistically significant). 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 that 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.

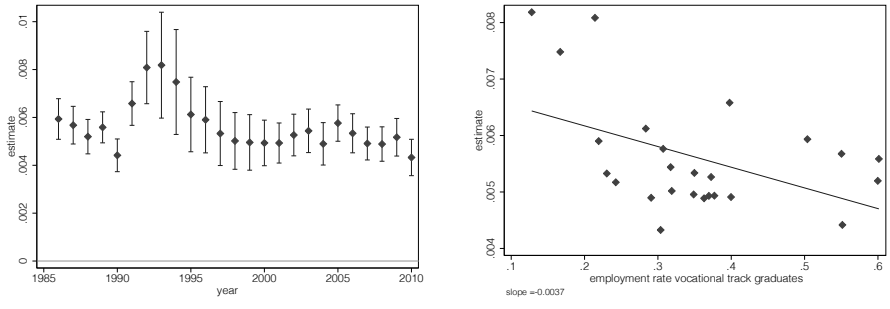
Turning to the estimates for the importance of moved co-worker links displayed in Figure 3 we see a similar pattern. Point estimates are positive and significant, but for obvious reasons noticeably smaller in magnitude than for the return links; the estimated impact of each link is between 0.43 to 0.82 percentage points depending on the year. But since the estimates reflect the effect of each *single* moved co-worker 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.

Figure 2. Results for return links



Notes: The left hand side figure shows the γ_{1t}'s and corresponding 95% confidence intervals from Equation 2.1. The right hand side shows the relationship between these estimates and the employment rate of vocational track graduates.

Figure 3. Results for moved co-worker links



Notes: The left hand side figure shows the γ_{2t}'s and corresponding 95% confidence intervals from Equation 2.1. The right hand side shows the relationship between these estimates and the employment rate of vocational track graduates.

2.4.2 "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. As will be obvious, these are watered-down versions of the actual links (thus not "pure" placebos) and may therefore pick up some aspects of social connections in their own right. However, the point of contrasting our main estimates to these placebo-type indicators is the idea that they should

be equally affected by confounding factors such as within-class variations in skills.

The first type of placebos uses *other establishments within the same firm* and area as the actual summer job establishment in order to mimic the role of the return links but without the actual connections provided on site. The advantage of this definition is that graduates and placebo co-workers are subject to the same time-varying firm-level factors. Note that we zoom in on firms with multiple establishments in the same location because the probability of entering any establishment at random locations in Sweden will be close to zero by definition (thus, the placebo will mechanically "look good" without this restriction). Instead, we here rely on those firms which run multiple operations within the same municipality (think of H&M stores, grocery chains, pharmacy chains, fast food chains and so forth), see also Kramarz and Skans (2014) and Hensvik and Skans (2016) for similar placebos.

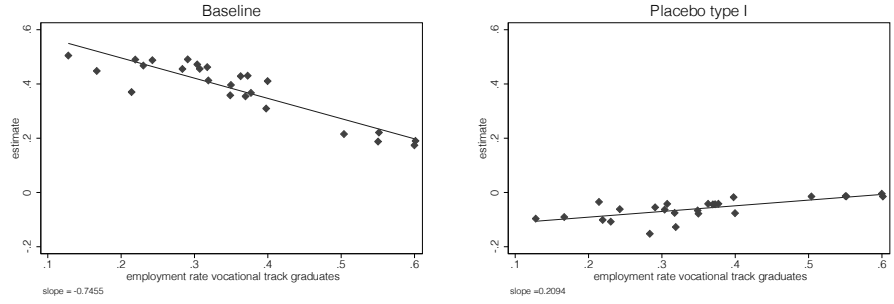
The second placebo uses workers who were employed at the summer job establishment, but who *moved the year before the student started* to work there, in order to mimic the actual moved co-worker links. This should ensure that there was no first-order (on-site) interaction between graduates and these 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 co-worker links.

Notably, the first placebo strategy is only meaningful for firms with multiple sites in the same area. In addition, in the second strategy we only make use of graduates who worked for less than two years at their summer job establishments.[15] 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.1 provides the results in table format). The placebo estimates can be contrasted with the baseline estimates on the left. Reassuringly, the estimated placebo constants (i.e. the effects under average business cycle conditions) are one tenth or less than the baseline link-estimates. Moreover, the slope of the fitted regression line suggests, if anything, a *positive* relationship between the propensity to find a job at a placebo co-worker'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, pro-cyclical).

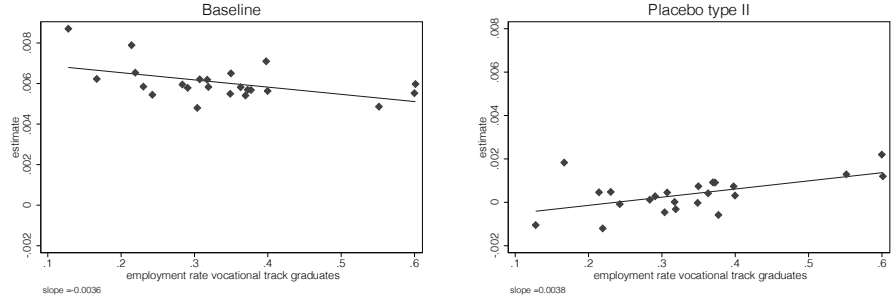
[15]The restriction is imposed because we focus on placebo links that must have worked in the plant prior to the graduate.

Figure 4. Placebo results (type I)



Notes: The left figure shows the relationship between the baseline estimates corresponding to γ_{1t}'s from Equation 2.1 and the employment rate of vocational track graduates for the sample restricted to multi-plant firms. The right figure shows the relationship between the placebo type I estimates and the employment rate.

Figure 5. Placebo results (type II)



Notes: The left figure shows the relationship between the baseline estimates corresponding to γ_{2t}'s from Equation 2.1 and the employment rate of vocational track graduates for the sample restricted to graduates with summer jobs of a duration of maximal two years. The right figure shows the relationship between the placebo type II estimates and the employment rate.

These placebos can also be used to devise alternative identification strategies. To this end, we have estimated alternative models where we rely on the placebos (instead of classmates) to provide us with the counterfactual. Here, we estimate the probability of starting to work within the summer-job establishment relative to other local establishments in the same firm (corresponding to Placebo I) and the probability of working with a moved previous co-worker relative to the probability of working with someone who left the same establishment before the summer job (corresponding to Placebo II). Thus, these regressions no longer rely on classmates for identification but instead construct the fixed effects from combinations of *individual* and broader "contact space" defined from all establishments with either a placebo contact or a true contact. We then estimate the difference between true contacts and placebo contacts for a given individual in all cases when both are observed. Reassuringly, the estimates are robust to this alternative strategy (see appendix figures A.1 and

A.2). An important aspect of this identification strategy is that it fully relies on variation within individual. Thus, these estimates cannot be confounded by unobserved heterogeneity across workers.

2.4.3 Robustness and heterogeneous effects

We next turn to assess the robustness of the main results. In order to be able to present a large set of variations in an efficient way, we show 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. Throughout, we de-mean the business cycle indicators so that the constants refer to the effect during average business cycle conditions. For the most crucial exercises, we also show corresponding figures in the appendix.

We first show the slope of the fitted regression from the right side panel of Figure 2 in Table 2, column 1, panel A. We obtain the estimate from linear regressions of the estimated γ_{1t}'s and γ_{2t}'s in Equation 2.1 on the demeaned graduation employment rate. The estimated constant (0.42) thus captures the importance of return links at 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.

Estimates for Moved Links corresponding to Figure 3 are displayed in Table 2, column 1, panel B. The relationship to the business cycle is -0.0037 and the constant (reflecting the effect during average conditions) is 0.0056.

Next we turn to robustness by first accounting for a general time trend and a quadratic in time (Table 2, columns 2 and 3) to remove any influence of other potential changes over time that may be correlated with our business cycle indicators. Reassuringly, the negative relationship with the business cycle is stable for return links and the association becomes even stronger for the moved links if accounting for these secular time trends.

Our main model only uses "stable job" matches during the year of 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 return to gender-specific estimates (military service is not relevant for females) 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). This also reduces the sensitivity to concerns about sample selection. The estimated effect for return links decreases slightly, while the effect for moved links increases somewhat.

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
Panel A. 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 in 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

Continue to next page

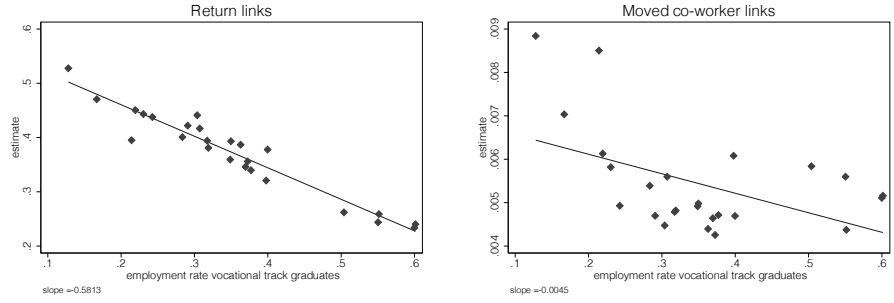
Notes: *** p<0.01, ** p<0.05, * p<0.1. Column (1)/columns (2)-(4) shows the results from regressing the yearly estimates from model 1/extended model 1 as specified in the column heading on the employment rate in t. The estimated constant is for the average graduation employment rate within the sample. Column (5) shows the results from regressing the yearly estimates from model 1 on the youth unemployment rate in t. The estimated constant is for the average youth unemployment rate within the sample. The yearly estimates are obtained from estimating model 1 separately for each graduation cohort from 1985-2010 for the sample of all vocational track students with a summer job in $t-1$ and a stable job in t. Standard errors in model (1) are clustered on classes.

Table 2 - continued from previous page

	(1) Baseline	(2) Time Trend	(3) Time Trend^2	(4) Job t or $t+1$	(5) Youth unemployment
Panel B. 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

Notes: *** p<0.01, ** p<0.05, * p<0.1. Column (1)/columns (2)-(4) shows the results from regressing the yearly estimates from model 1/extended model 1 as specified in the column heading on the employment rate in t. The estimated constant is for the average graduation employment rate within the sample. Column (5) shows the results from regressing the yearly estimates from model 1 on the youth unemployment rate in t. The estimated constant is for the average youth unemployment rate within the sample. The yearly estimates are obtained from estimating model 1 separately for each graduation cohort from 1985-2010 for the sample of all vocational track students with a summer job in $t-1$ and a stable job in t. Standard errors in model (1) are clustered on classes.

Figure 6. Including graduates **without stable job upon graduation**



Notes: The left (right) figure shows the relationship between the estimates corresponding to γ_{1t}'s (γ_{2t}'s) from Equation 2.1 and the employment rate of vocational track graduates for the sample including graduates without a stable job upon graduation.

In Figure 6, we show results obtained when we extend the sample to also include *all graduates who did not find a stable job upon graduation*. Reassuringly, the relationships remains strongly counter-cyclical. In the appendix we show further results obtained when we extend the sample to also include i) *graduates who did not have a summer job* (Figure A.3) and *graduates from academic tracks* (Figure A.4) with similar results.

Since Kramarz and Skans (2014) show that parental contacts are an important stepping stone into the labor market we have also re-estimated the models using data where we exclude summer jobs where parents were employed; the results are robust (see appendix table A.2).[16]

Our main model uses post-graduation employment rates as the business cycle indicator. In column 5 of Table 2 we instead use the most consistent series of survey-based youth unemployment rates we could find.[17] 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.[18]

Next, 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 2.1 for each of these. Estimates are displayed in Table 3.

[16]The interaction with the business cycle is unchanged even though the impact of links during average business cycle conditions is lowered (see the constant) by one quarter when parental links are removed.

[17]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.

[18]In Section 2.4.4 below we show estimates from further variations, including using local employment rates instead and accounting for the state of the cycle when the summer job was formed.

Table 3. *Heterogeneous effects: by field*

	(1) Business	(2) Childcare	(3) Healthcare	(4) Construction	(5) Electronics	(6) Hotel
Panel A. 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
Panel B. 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

Notes: *** p<0.01, ** p<0.05, * p<0.1. Column (1)-(6) shows the results from regressing the yearly estimates from model 1 on the employment rate in t. The estimated constant is for the average graduation employment rate within the sample. The yearly estimates are obtained from estimating model 1 separately for each graduation cohort and field from 1985-2010 for the sample of all vocational track students with a summer job in $t-1$ and a stable job in t. Standard errors in model (1) are clustered on classes. Column (5): Electronics and computer science. Column (6): Hotel and restaurant.

Table 4. *Heterogeneity by gender, size of summer-job establishment*

	(1)	(2)	(3)	(4)	(5)
	All	By gender:		By plant size:	
	Baseline	Women	Men	<50 empl.	<20 empl.
Panel A. 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
Panel B. 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

Notes: *** p<0.01, ** p<0.05, * p<0.1. Column (1)-(5) shows the results from regressing the yearly estimates from model 1 on the employment rate in t. The estimated constant is for the average graduation employment rate within the sample. The yearly estimates are obtained from estimating model 1 separately for each graduation cohort (column 1) or combination of graduation cohort and gender (column 2 and 3) or summer job plants with less than 50/20 employees (column 4 and 5) from 1985-2010 for the sample of all vocational track students with a summer job in $t-1$ and a stable job in t. Standard errors in model (1) are clustered on classes.

The average impact of links (captured by the constant) is very similar across industries. There is also a strong negative relationship with the graduation employment rate in all of these for the return links. The point estimates remain negative across all segments for moved links, although some of these estimates are statistically insignificant. Overall, however, we interpret the results of the table as indicating that our baseline results are not driven by any one particular industry.

In Table 4 we show results for women (column 2) and men (column 3). The results are quite similar although the return links appear more closely related to the cycle for women whereas moved co-worker links are more closely related to the cycle for men. Both matter equally during average conditions as indicated by the estimated constant. The fact that results arise also for females reinforces the impression that interactions with military service are not crucial. Our main data restriction is to include all contacts from summer jobs with less than 100 employees but in columns 4 and 5 of the same table we change this threshold to 50 or 20 and the estimated effects of the business cycle are only slightly smaller and remain negative.

2.4.4 Supply and demand side selection

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

$$W_{ijct} = \theta_{jct} + [\delta^i X_i + \rho_j + \beta Year_t + \gamma^{BC} E_t] \, ReturnLink_{ijct} + \varepsilon_{ijct} \quad (2.2)$$

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_t). 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 θ_{jct}. 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:

$$G_{jct} = \frac{\sum_{ict} W_{ijct} * Link_{ijct}}{\sum_{ict} Link_{ijct}} - \frac{\sum_{ict} W_{ijct} * (1 - Link_{ijct})}{\sum_{ic}(1 - Link_{ijct})}$$

G_{cj} captures the difference in hiring rates between connected and unconnected observations within each class-plant pair. 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:

$$G_{jct} = \delta^i \bar{X}_i + \rho_j + \beta, Year_t + \gamma^{BC} E_t + u_{jct} \tag{2.3}$$

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 2.3 are displayed in Table 5. 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 characteristics 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 (see 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.

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 for grade rank, gender, immigration background and earnings from in-school work, there is no evidence for selection on the supply side.

Table 5. *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
			Panel A: Full data			
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
			Panel B: Aggregated data			
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: *** p<0.01, ** p<0.05, * p<0.1. 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.

To substantiate this even further, Table 6 (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 remains 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, which reduces the estimate, but with a large and significant remaining effect. Thus, the counter-cyclical 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 6. *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:*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered on plants. Column (1) shows the estimate from specification 5, table 5 (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.

2.4.5 Beyond sorting

In this subsection we have collected the results from a set of descriptive regressions that describe how various labor market outcomes are related to connections across the cycle for young workers. The regressions are somewhat less well identified in nature than the analysis above and should therefore perhaps best be considered as descriptive regressions. A causal analysis of these processes needs to be left as a topic of interest for future research.

We first show a description of how match duration and earnings trajectories differ between youths who found their first stable jobs through moved/return links and those who found them without use of such links. Then, we turn to the association between having a summer job or not (and the "quality" of this job) with the cycle.

Links, match quality and earnings trajectories

Our analysis has shown that informal ties are more predictive of matching patterns during recessions. We argued initially that the usefulness of these ties could 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 and earnings as in much of the existing literature.

To this end, we estimate the effect of matching through a return link or a moving co-worker link on the quality of the match, captured by parameters γ_{1t} and γ_{2t}. As before, we also relate the sizes of these estimates to the business cycle. As outcomes, we use indicator variables that take on the value 1 if graduate i still works in plant j 2,4 and 6 years after graduation as well as measures of log earnings at the same time intervals since graduation. Denoting these outcomes by y, we estimate:

$$y_{icjt} = \gamma_{1t}\, Return\, Link_{icjt} + \gamma_{2t}\, Moved\, Link_{icjt} + \lambda_{ct} + \mathbf{X}_{it}\beta_t + \varepsilon_{icjt}, \quad (2.4)$$

where $\mathbf{X}_{it}$ is a vector of individual characteristics controlling for pre-match earnings, grades and gender and λ_{ct} are class fixed effects.

We display the results in Table 7. The results show that matches mediated by our measured contacts last longer on average: graduates who found their first stable job in the plant where they held their summer job are 16 percentage points more likely to remain employed at the same site after two years compared to graduates who found their first stable job through the market under average business cycle conditions. A similar pattern is found for graduates who found the job through a moving link, although as before this effect is weaker (10 percentage points). The difference in match quality dissipates somewhat over time (columns 2-3), but even after six years about half to one third of the initial difference remains. The results further suggests that matching through links is relatively better in terms of match quality during recessions at least in the short run. To highlight the patterns over time in good and bad times, Appendix figure A.13 shows the predicted effect of links on the probability of staying during high- and low employment years, defined as in Table 1. The graphs clearly highlights that short-run job-stability during recessions is largest for matches formed through social contacts.

Table 7. *Match Duration and Earnings*

	(1) Stay in $t+2$	(2) Stay in $t+4$	(3) Stay in $t+6$	(4) Log earnings in $t+2$	(5) Log earnings in $t+4$	(6) Log earnings in $t+6$
Panel A. Return links						
Effect of business cycle (slope)	-0.1296***	-0.0087	0.0112	0.1552*	0.2601***	0.1203**
	(0.0297)	(0.0270)	(0.0254)	(0.0888)	(0.0667)	(0.0507)
Constant	0.1646***	0.1225***	0.0891***	0.0499***	0.0544***	0.0382***
	(0.0041)	(0.0035)	(0.0030)	(0.0111)	(0.0078)	(0.0064)
Observations	23	21	19	23	21	19
R-squared	0.431	0.006	0.015	0.139	0.505	0.275
Class FE	Yes	Yes	Yes	Yes	Yes	Yes
Panel B. Moved links						
Effect of business cycle (slope)	-0.0826*	-0.0173	0.0127	-0.0065	-0.1000	0.0084
	(0.0423)	(0.0255)	(0.0213)	(0.0668)	(0.1229)	(0.0925)
Constant	0.0977***	0.0595***	0.0385***	0.0556***	0.0518***	0.0468***
	(0.0068)	(0.0037)	(0.0042)	(0.0113)	(0.0145)	(0.0131)
Observations	23	21	19	23	21	19
R-squared	0.107	0.020	0.011	0.000	0.044	0.001
Class FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Column (1)-(6) shows the results from regressing the yearly estimates from model 4 on the employment rate in t. The estimated constant is for the average graduation employment rate within the sample. The yearly estimates are obtained from estimating model 4 separately for each graduation cohort from 1985-2008 (columns 1 & 4)/1985-2006 (columns 2 & 5)/1985-2004 (columns 3 & 6) for the sample of all vocational track students with a summer job in $t-1$ and a stable job in t. Standard errors in model (4) are clustered on classes.

Turning to earnings, we see that graduates who use links to find their first job under average business cycle conditions have much higher earnings during the coming 6 years (see the constant). For the return links these benefits are strongly procyclical. One possible interpretation along the lines of Acemoglu and Pischke (1998) is that firms are required to share the rents from the informational advantage during times when the market is tight and workers have better outside options. For Moved links, benefits are of equal magnitudes during average market conditions but we find no statistically significant association to the business cycle. Again, we show the predicted effects during high- and low-employment years in the appendix (Figure A.14).

Summer jobs and job finding across the cycle

Here we document associations between having summer jobs and the rate at which graduates find stable jobs during the year of graduation. We separately study the *extensive margin*, i.e. having a summer job or not, and the quality of these jobs i.e. the *intensive margin*, for those with summer jobs.

We first estimate the association between an indicator variable taking the value one for graduates who had a summer job (SJ_{ic}) and an indicator variable taking the value one if finding a stable job during the graduation year E_{ic} and relate the patterns of estimates to the cycle. To this end, we estimate the following class-fixed effects model:

$$W_{ict} = \gamma_t^{SJ} SJ_{ict} + \lambda_{ct} + \mathbf{X}_{it}\beta_t + \varepsilon_{ict}, \tag{2.5}$$

$\mathbf{X}_{it}$ is a vector of individual characteristics controlling for high school grades and gender and λ_{ct} are class fixed effects. We display the results in Table 8. Youths with summer jobs are much more likely to rapidly find a stable job than classmates with similar grades. The advantage during average business cycle conditions is 14 percentage points (see the constant). We do, however, not find any significant differences in this association across the cycle. Taken at face value, these results, together with those of our core analysis, thus suggests that our measured links are more decisive in allocating graduates across employers in bad times than in good times, but equally useful in terms of protecting against non-employment regardless of the cycle. However, we would like to caution against drawing too firm conclusions in this direction since the precision of the interaction estimates of Table 8 is far from ideal. It should also be reiterated that the associations displayed in Table 8 capture the joint impact of changes in causal effects and the composition of students finding summer jobs, which is a concern if there is business cycle heterogeneity in ability levels between those with and without summer job that is not fully captured by grades and class fixed effects.[19]

[19] In contrast, the sorting models of our core analysis only need to be concerned about match-specific advantages among those with summer jobs, see Section 2.3.3.

Table 8. *Job finding across the cycle*

	(1)	(2)	(3)
	Impact of having a summer job	Quality of summer jobs	
		Hires in t	Separations in t
Effect of BC	0.0404	-0.0000359	-0.0000761*
	(0.0441)	(0.000025)	(0.0000391)
Constant	0.1411***	0.000000	0.0000445**
	(0.0044)	(0.000000)	(0.0000184)
Outcome	Finding a stable job in t	Finding a stable job in t	Finding a stable job in t
Sample	All graduates	With summer job	With summer job
Class fixed effects	Yes	Yes	Yes
Summer job earnings	–	Yes	Yes
Summer job est. size	–	Yes	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered on classes. The estimated constant is for the average graduation employment rate within the sample. The sample consists of all vocational students (column 1) and all vocational students with a summer job (irrespective of plant size) in column (2) and (3). The mean number of hires and separations among summer jobs plants is 6.36 and 4.87 respectively.

Next, we turn to the intensive margin. Here we reestimate the model for the sample with $SJ_{ic} = 1$ and instead focus on variables measuring aspects of the quality of the summer job. We use two indicators of quality: *Total hiring* and *Total separations* between t-1 and t (i.e. graduation year) at the summer job establishment. We control for size of the summer job establishment in t-1 and the summer job earnings of the graduate alongside grades, gender and class fixed effects. Note that we can control for earnings here (and thus are able to handle more of the heterogeneity), but not in column 1, since the intensive-margin analysis only use cases with $SJ_{ic} = 1$. The first intensive-margin analysis compares classmates with summer jobs at sites with different graduation-year hiring needs whereas the second intensive-margin analysis makes a similar comparison but instead focusing on variations across classmates in the total number of workers separating from the summer-job establishment. Note that separations may be bad because they indicate a low hiring need, but they may also be good since they imply that the outflow of potentially useful contacts that end up at other firms is increased. The results, however, shows that post graduation job finding is largely unrelated to both hiring and separations, regardless of business cycle conditions. For hires, all estimates are tiny and statistically insignificant. For separations, results are also small in nature, but statistically significant. Point estimates suggest that a massive separation of 100 workers from the summer-job establishment is associated with an increase in job-finding of 0.5 percentage points under average business cycle conditions. This positive estimate from separating contacts is larger in bad times, but again, with very moderately sized estimates.

2.5 Conclusions

Our results show that informal hiring channels are more important for young graduates' job matching process during recessions than during boom years. The predictive power of both direct and indirect links is much stronger in bad times than 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 counter-cyclical patterns we document are large in magnitude, general in nature, and robust to large alterations of the statistical model, in particular for return links.[20] 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, the results show that firms hire more connected young workers in times when young workers struggle to find employment. When these (young) workers have poor outside options, and the firms therefore could be presumed to have greater market power, informal channels appear to become more influential. This is not an obvious result, a priori, since firms should have a longer list of market applicants as well during recession years. Interestingly, the patterns are well in line with results on recalls in Fujita and Moscarini (2017). The results hold most strongly for the return links which are tied to firm-specific human capital, as in the recall case, but also for the results for moved links where information is acquired in the context of one firm and carried over to another through an employee. The fact that the results appear in both cases suggests that firms are more likely to exploit private information about both specific (return links) and general (moved links) human capital when workers' outside options are dreary.

Our results and analysis are set in the context of summer jobs and these jobs are typically allocated through market mechanisms. However, there do exist explicit policies for promoting summer jobs in some countries and settings (see e.g. Scott-Clayton and Minaya, 2016; Alam et al., 2015). In terms of such policies, our results suggest that they may be particularly useful as direct entry ports into real jobs during bad times. Our results may also contribute to the understanding of the mechanics of apprenticeships policies. A feature noted in the literature on apprenticeships (Acemoglu and Pischke, 1998, 1999a,b) is that firms may be happy to provide their apprentices with general skills because they retain an informational advantage over outside firms. Such

[20] The few cases when the results are sensitive all relate to the moved links.

mechanisms may reinforce the learning component discussed above if firms are willing to invest more in their summer-job staff during bad times when the workers' outside options are worse. Interpreted through the lens of this literature, our results further suggest that the training firms should be weary of exits by other co-workers as these may provide information leakage into other competing firms, thus potentially reducing the monopsony power of the original employers.

Appendix

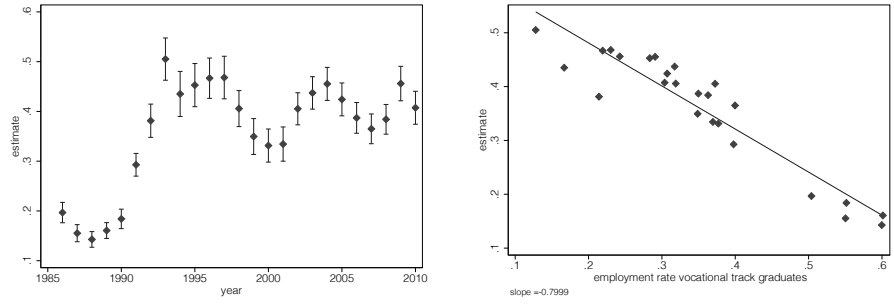
Additional results

Table A.1. *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

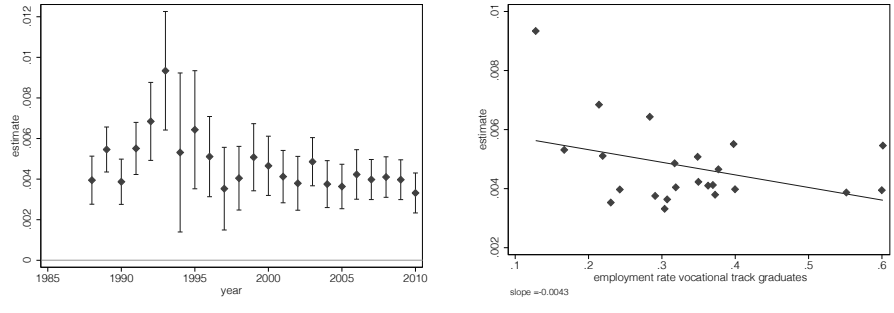
Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered on classes in parentheses. The estimated constant is for the average graduation employment rate within the sample. Baseline placebo I restricts the sample to multi-plant firms. Baseline placebo II restricts the sample to graduates with summer jobs of a duration of maximal two years.

Figure A.1. Using Placebo I as counterfactual: Returning to same plant vs. another plant in same firm



Notes: The left hand side figure shows the estimates and corresponding 95% confidence intervals from an alternative identification strategy using placebo (type I) as counterfactual (see section 2.4.2), thus estimating the probability of starting to work within the summer-job establishment relative to other local establishments in the same firm. The right hand side shows the relationship between these estimates and the employment rate of vocational track graduates.

Figure A.2. Using Placebo II as counterfactual: Moving to same plant as former co-worker vs. moving to the plant of co-workers who left before the summer job started



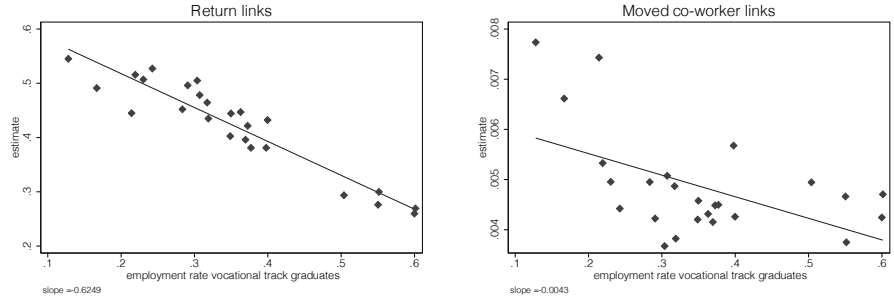
Notes: The left hand side figure shows the estimates and corresponding 95% confidence intervals from an alternative identification strategy using placebo (type II) as counterfactual (see section 2.4.2), thus estimating the probability of working with a moved previous co-worker relative to the probability of working with someone who left the same establishment before the summer job started. The right hand side shows the relationship between these estimates and the employment rate of vocational track graduates.

Table A.2. *Removing parents*

	(1) Baseline	(2) Without Parents
Return links:		
Effect of business cycle (slope)	-0.6091***	-0.5489***
	(0.0468)	(0.0352)
Constant	0.4219***	0.3467***
	(0.0017)	(0.0049)
Observations	25	25
R-squared	0.865	0.897
Moved links:		
Effect of business cycle (slope)	-0.0037*	-0.0030**
	(0.0017)	(0.0015)
Constant	0.0056***	0.0046***
	(0.0002)	(0.0001)
Observations	25	25
R-squared	0.215	0.212

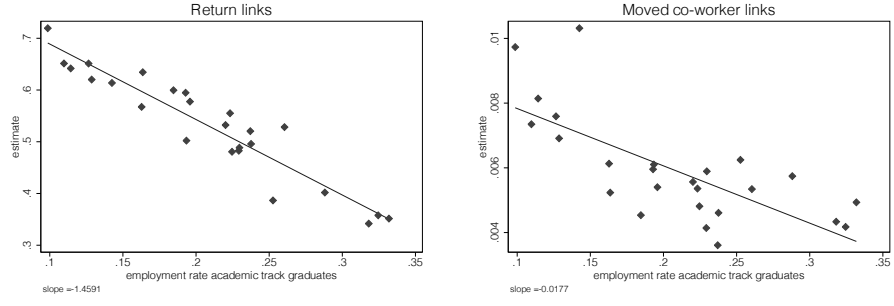
Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered on classes in parentheses. The estimated constant is for the average graduation employment rate within the sample.

Figure A.3. Including graduates both **with and without summer jobs**



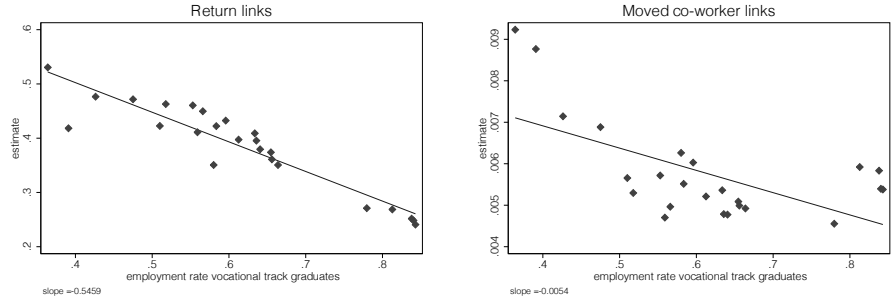
Notes: The left (right) figure shows the relationship between the estimates corresponding to the γ_{1t}'s (γ_{2t}'s) from Equation 2.1 and the employment rate of vocational track graduates for the sample including graduates both with and without summer jobs.

Figure A.4. Academic track graduates



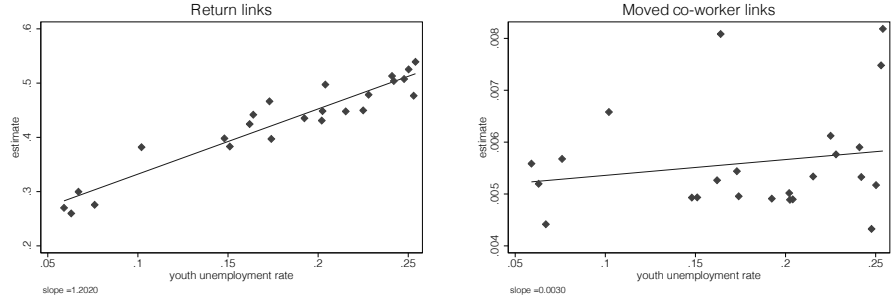
Notes: The left (right) figure shows the relationship between the estimates corresponding to the γ_{1t}'s (γ_{2t}'s) from Equation 2.1 and the employment rate of vocational track graduates for the sample of academic track graduates.

Figure A.5. Graduates with a **stable job in t and t+1**



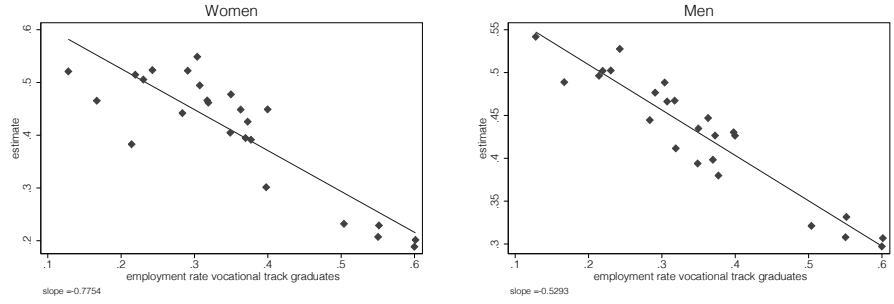
Notes: The left (right) figure shows the relationship between the estimates corresponding to the γ_{1t}'s (γ_{2t}'s) from Equation 2.1 and the employment rate of vocational track graduates for the sample of graduates with a stable job in either t or t+1.

Figure A.6. Youth unemployment rate



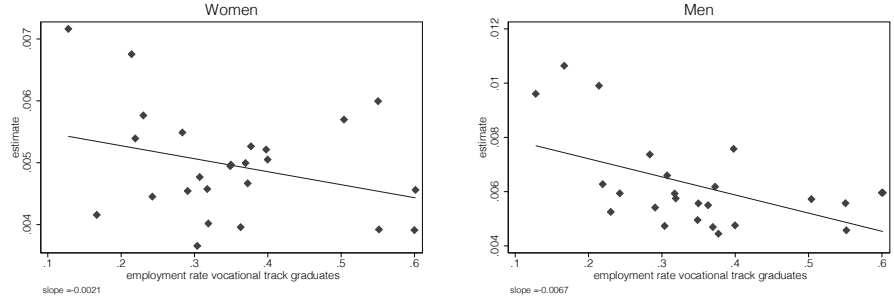
Notes: The left (right) figure shows the relationship between the estimates corresponding to the γ_{1t}'s (γ_{2t}'s) from Equation 2.1 and the official youth unemployment rate.

Figure A.7. Return links, by gender



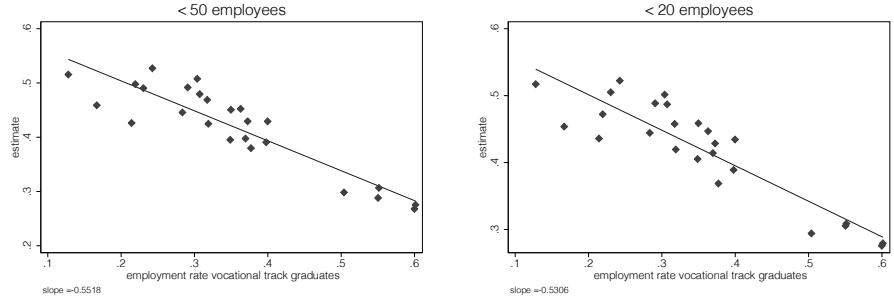
Notes: The figures show the relationship between the estimates corresponding to the γ_{1t}'s from Equation 2.1 and the employment rate of vocational track graduates for the sample restricted to women (left) or men (right).

Figure A.8. Moved co-worker links, by gender



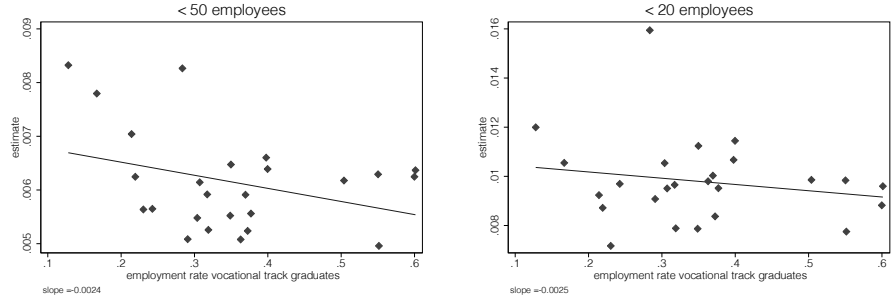
Notes: The figures show the relationship between the estimates corresponding to the γ_{2t}'s from Equation 2.1 and the employment rate of vocational track graduates for the sample restricted to women (left) or men (right).

Figure A.9. Return links, by firm size



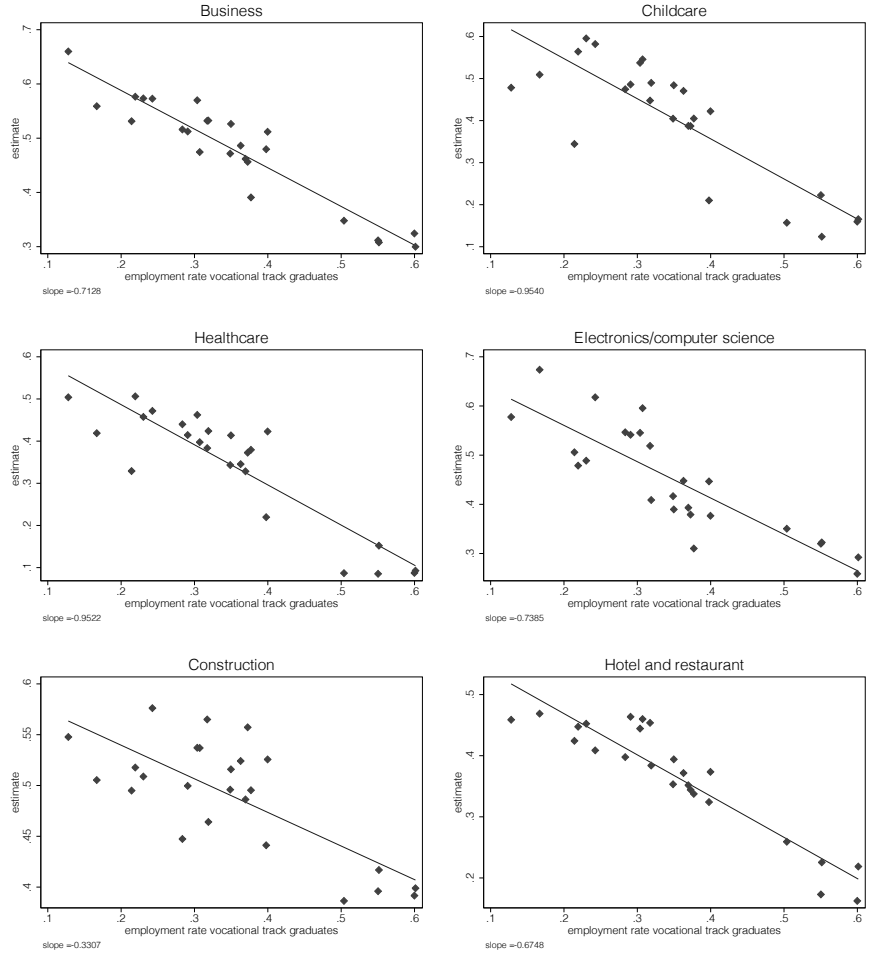
Notes: The figures show the relationship between the estimates corresponding to the γ_{2t}'s from Equation 2.1 and the employment rate of vocational track graduates for the sample restricted to firms with less than 50 employees (left) or less than 20 employees (right).

Figure A.10. Moved co-worker links, by firm size



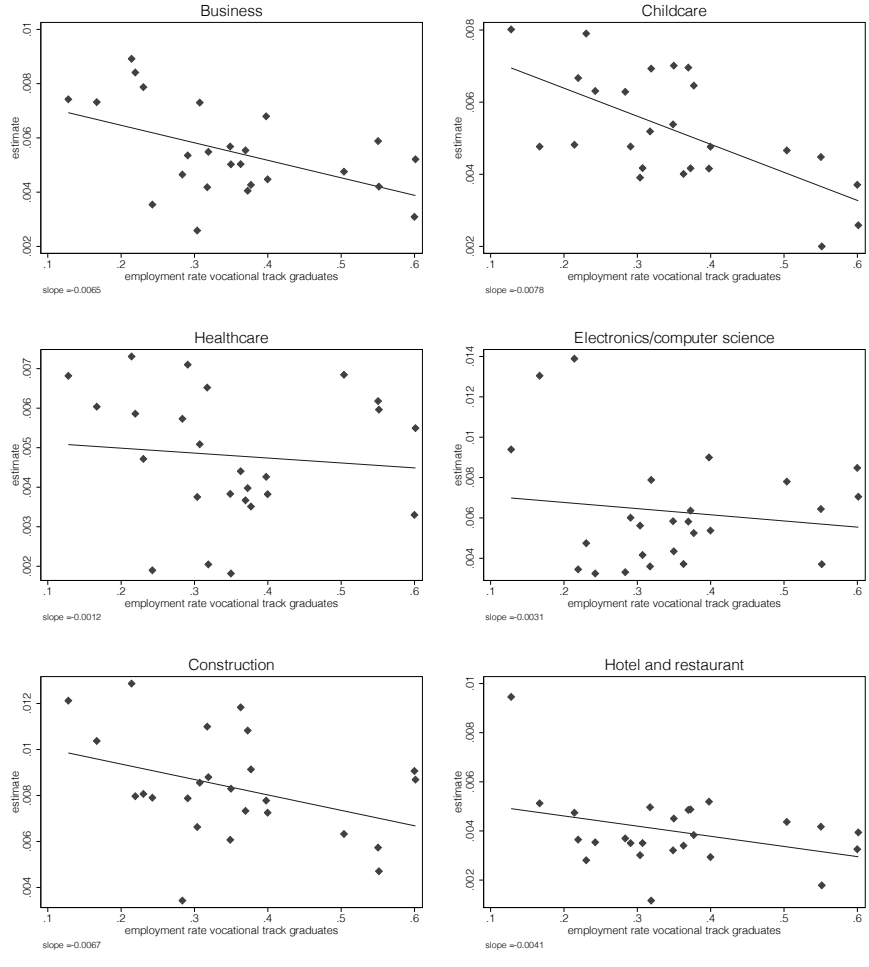
Notes: The figures show the relationship between the estimates corresponding to the γ_{2t}'s from Equation 2.1 and the employment rate of vocational track graduates for the sample restricted to firms with less than 50 employees (left) or less than 20 employees (right).

Figure A.11. Return links, by most common sectors



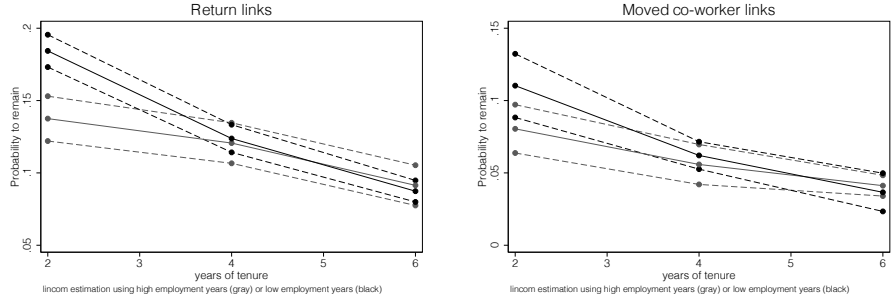
Notes: The figures show the relationship between the estimates corresponding to the γ_{1t}'s from Equation 2.1 and the employment rate of vocational track graduates for the sample restricted to the most common sectors of vocational track graduates.

Figure A.12. Moved co-worker links, by most common sectors



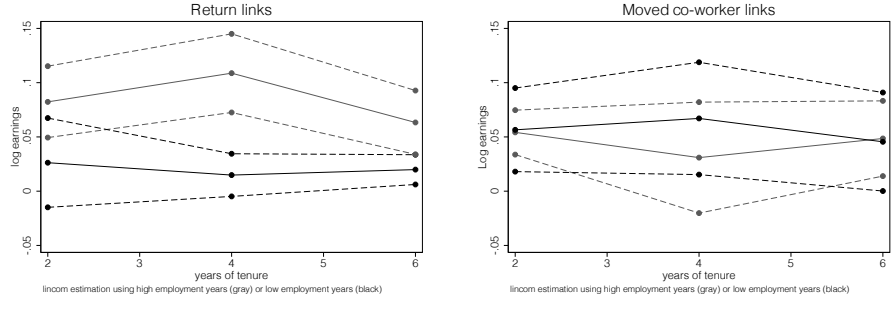
Notes: The figures show the relationship between the estimates corresponding to the γ_{2t}'s from Equation 2.1 and the employment rate of vocational track graduates for the sample restricted to the most common sectors of vocational track graduates.

Figure A.13. Difference in length of employment between graduates employed through contacts and those without contacts



Notes: The figures show the 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.

Figure A.14. Difference in log earnings between graduates employed through contacts and those without contacts



Notes: The figures show the difference in log earnings 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.

Supplementary description

Figure B.1. Fraction of vocational students with summer jobs and relationship with business cycle

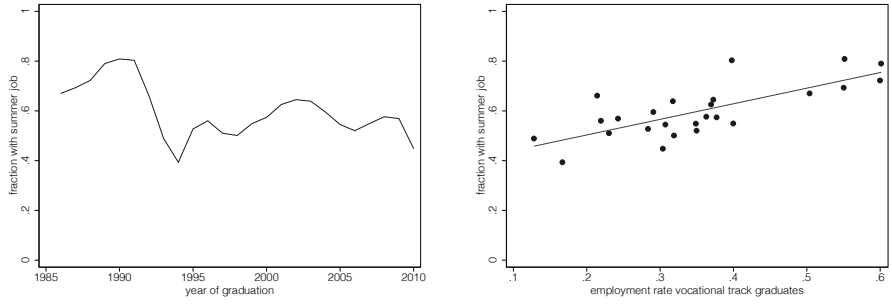
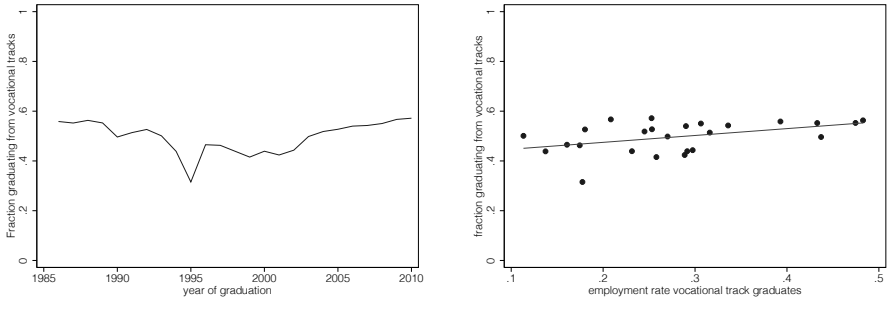
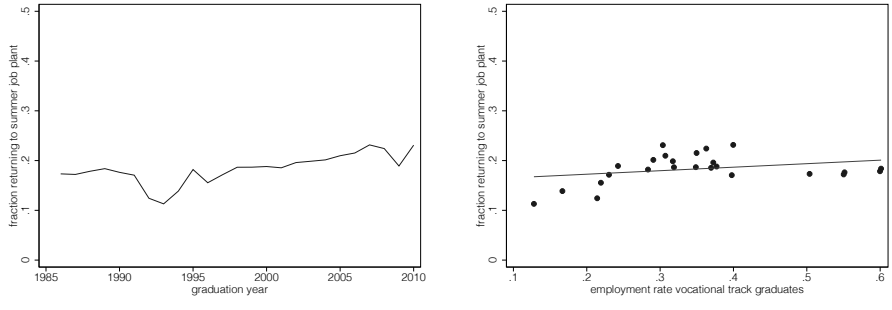


Figure B.2. Fraction vocational track graduates and relationship with business cycle



Notes: The glitch in 1995 is due to a prolongation of tracks that reduced the graduation rates in that year.

Figure B.3. Fraction among graduates with summer jobs recalled to same plant and relationship with business cycle



References

Acemoglu, D. and Pischke, J.-S. (1998). Why do firms train? Theory and evidence. *The Quarterly Journal of Economics*, 113(1):79–119.

Acemoglu, D. and Pischke, J.-S. (1999a). Beyond Becker: Training in imperfect labour markets. *The Economic Journal*, 109(453):112–142.

Acemoglu, D. and Pischke, J.-S. (1999b). The structure of wages and investment in general training. *Journal of Political Economy*, 107(3):539–572.

Alam, M., Carling, K., and Nääs, O. (2015). The program and treatment effect of summer jobs on girls' post-schooling incomes. *Evaluation review*, 39(3):339–359.

Altonji, J. G. and Pierret, C. R. (2001). Employer learning and statistical discrimination. *The Quarterly Journal of Economics*, 116(1):313–350.

Bentolila, S., Michelacci, C., and Suarez, J. (2010). Social contacts and occupational choice. *Economica*, 77(305):20–45.

Casella, A. and Hanaki, N. (2006). Why personal ties cannot be bought. *American Economic Review*, 96(2):261–264.

Dustmann, C., Glitz, A., Schönberg, U., and Brücker, H. (2016). Referral-based job search networks. *Review of Economic Studies*, 83(2):514–546.

Eliason, M., Hensvik, L., Kramarz, F., and Skans, O. N. (2019). Social connections and the sorting of workers to firms. IZA Discussion Paper No. 12323.

Engdahl, M. and Forslund, A. (2016). En förlorad generation? Om ungas etablering på arbetsmarknaden. IFAU Rapport 2016:1.

Forslund, A., Hensvik, L., Skans, O. N., and Westerberg, A. (2012). *Kollektivavtalen och ungdomarnas faktiska begynnelselöner*. Institutet för arbetsmarknads- och utbildningspolitisk utvärdering (IFAU).

Fujita, S. and Moscarini, G. (2017). Recall and unemployment. *American Economic Review*, 107(12):3875–3916.

Hellerstein, J. K., Kutzbach, M. J., and Neumark, D. (2015). Labor market networks and recovery from mass layoffs before, during, and after the great recession. NBER Working Paper 21262.

Hensvik, L. and Skans, O. N. (2014). Networks and youth labor market entry. *Nordic Economic Policy Review*, 1:81–118.

Hensvik, L. and Skans, O. N. (2016). Social networks, employee selection and labor market outcomes. *Journal of Labor Economics*, 34(4):825–867.

Holmlund, B. (2003). The rise and fall of Swedish unemployment. CESifo Working Paper No. 918.

Jovanovic, B. (1979). Job matching and the theory of turnover. *Journal of Political Economy*, 87(5):972–990.

Kahn, L. B. (2010). The long-term labor market consequences of graduating from college in a bad economy. *Labour Economics*, 17:303–316.

Kramarz, F. and Skans, O. N. (2014). When strong ties are strong: Networks and youth labour market entry. *Review of Economic Studies*, 81(3):1164–1200.

Montgomery, J. D. (1991). Social networks and labor-market outcomes: Toward an economic analysis. *The American Economic Review*, 81(5):1408–1418.

Oreopoulos, P., von Wachter, T., and Heisz, A. (2012). The short- and long-term career effects of graduating in a recession. *American Economic Journal: Applied Economics*, 4(1):1–29.

Oyer, P. and Schaefer, S. (2011). Personnel economics: Hiring and incentives. In Ashenfelter, O. and Card, D., editors, *Handbook of Labor Economics*, volume 4b, chapter 20. Elsevier.

Scott-Clayton, J. and Minaya, V. (2016). Should student employment be subsidized? Conditional counterfactuals and the outcomes of work-study participation. *Economics of Education Review*, 52:1–18.

Skans, O. N. (2009). *Varför är den svenska ungdomsarbetslösheten så hög?* Finanspolitiska rådet.

Skolverket (2004). Arbetsplats för lärande - om samverkan mellan skola och arbetsliv. Skolverket.

3. Is More Education a Substitute for Job Search through Social Contacts?

Acknowledgments: I thank Caroline Hall, Lena Hensvik, Anna Sjögren, Oskar Nordström Skans, Helena Holmlund, Per-Anders Edin, Kristiina Huttunen, Peter Fredriksson, Francis Kramarz, Jan Sauermann and Judith Hellerstein as well as seminar audiences at the Institute for Policy Research at Northwestern University, UCLS, the IZA summer school 2019 and EALE 2019 for useful comments.

3.1 Introduction

The transition into the labor market is a a crucial, albeit difficult, step for many young workers. Information problems are more common for this group since previous work experience is often limited and work references are few, leading to a higher degree of uncertainty about worker quality (Altonji and Pierret, 2001; Fredriksson et al., 2018). Previous research has shown that social contacts are a widely-used channel for addressing this uncertainty since they can convey information between employers and potential hires in the matching process (Montgomery, 1991; Dustmann et al., 2016; Hensvik and Skans, 2016).[1] Social contacts seem to be particularly important for workers with lower socio-economic status and education levels as well as for blue-collar workers (see e.g. Pellizzari, 2010; Corcoran et al., 1980; Datcher, 1983; Elliot, 1999). Furthermore, theory suggests that education and social connections can be substituted as signals of ability. However, to the best of my knowledge, there exists no evidence on whether the empirical association is causal or related to confounding attributes. Even though the importance of social contacts as a means to substitute for a lack of information is well known, there is only limited knowledge about the extent to which education can function as substitute for social contacts.

The aim of this paper is to address this question and provide evidence on whether a longer upper secondary education can replace the need for social contacts in the job search process of Swedish graduates from vocational upper-secondary school tracks. Since young workers do not typically have a broad network of contacts when they enter the labor market, I focus on contacts in the form of the graduates' own parents. Kramarz and Skans (2014) show that parents have proven to be of particular importance in the process of securing a job when other types of connections are scarce. The main alternative would be to rely on workplace contacts acquired through summer jobs during school, but these contacts are clearly endogenous to the duration of studies, and I therefore focus on parental contacts.[2]

Relying on Swedish register data, I provide the first study of the consequences of more education on the extent to which young worker rely on informal contacts for their placement in the labor market by exploiting a large

[1] There is a large empirical literature analyzing the importance of different types of social networks for job search, see for instance Glitz (2017); Hensvik et al. (2017) for labor market networks (co-workers, employers), Kramarz and Skans (2014) for family members, Dustmann et al. (2016); Munshi (2003) for ethnic networks or Bayer et al. (2008) for neighbors among others. For a general summary of the importance of social contacts, see Topa (2011) and Oyer and Schaefer (2011) for a firm-side overview of how accessing social networks can reduce uncertainty and thus hiring costs.

[2] Müller (2020) shows that Swedish high school graduates compensate the loss of employer links at the time of labor market entry by finding replacement jobs at the same establishment as a parent, while students who cannot rely on their parents to find a replacement job are more negatively effected in terms of employment.

scale trial that took place in Sweden in the late 1980s and prolonged upper secondary education from two to three years. The trial created exogenous variation across municipalities and student cohorts in the extent to which longer vocational tracks were available. As in Hall (2012), I use this variation to instrument whether a student received an additional year of upper secondary education. I then analyze whether attending an additional year of upper secondary school affected the probability to simultaneously work at the same establishment as a parent for up to 20 year after starting vocational school.

Notably, the trial coincided with a severe recession that started in the early 1990s and peaked by the mid-1990s, so that students who participated in the trial graduated under worse business cycle conditions than their peers in shorter tracks. In the empirical model, I remove potential correlation with the business cycle by comparing outcomes for students in the same year after enrollment and let the effect of prolonged education vary with the business cycle to allow for the possibility that benefits of more education might not materialize until after the end of the recession.[3]

My results indicate that the average impact of a longer education on the use of parental job-search contacts is negative during the early career. The point estimate is non-trivial in magnitude (a 1 percentage point reduction relative to a mean of 7 percentage points) but the estimate is statistically imprecise (p-value of 0.1). The apparent reduction does not appear to arise because students move away from parents' industries in general. In fact, the converse appears to be the case: In order to assess the robustness of my estimates, I show that students become somewhat more likely to work in other establishments in their parents' industries if they attend a longer vocational track. Additionally, and in order to better understand the impact of parental contacts, I also present novel results on the overall effect of prolonged vocational tracks on employment and show that changes in employment levels cannot account for the estimated effects.[4]

Notably, the patterns diverge by parental background. Even though children of low-educated parents use parental contacts more on average, I find no negative effect of a longer education for this group. Instead, the overall effect is entirely driven by a very large and statistically significant effect within the group of vocational students with well-educated parents. The results thus suggest that parental contacts and education are substitutes for students with highly-educated parents, while youths whose parents have finished at most

[3]For instance, Hensvik et al. (2017) examine the relationship between business cycles and the use of social contacts for job matching of labor market entrants. They find that social contacts are more important for job matching during recessions, indicating that an effect of prolonged education on the use of parental contacts might appear with a delay.

[4]Previously, Hall (2012) finds that the trial did not increase earnings, or the probability of having positive earnings. In contrast, I analyze the effects of more education on a more substantial level of stable employment. In addition, my empirical model allows me to let the effect of attending a longer education to vary with the business cycle.

compulsory school seem to rely more on parents even after the reform. Hence, the results suggest that the reliance on parental ties appears to be remarkably resilient among students with low-educated parents.

The paper relates to the literature on the importance of parents for young workers' job search. For example, Kramarz and Skans (2014) show that 11.5% of Swedish graduates from upper secondary school find their first stable job at a plant where their parent is employed. The authors use linked employer-employee data to show that parental ties are an important predictor of where graduates from different levels of schooling find their first stable job. The effects are larger for youths with lower levels of education and for those with lower grades. Furthermore, graduates who find jobs through their parents find jobs faster, but at a lower entry wage, which is however made up by higher subsequent wage growth. For Canada, Corak and Piraino (2011) find that 40% of a cohort of young men have at some point worked for an employer who also employed their father. While the authors show that this is mainly due to young workers who find their first jobs at their father's employer, there are still 6–9% of individuals who have their main job as adults at their father's (previous) main employer. Magruder (2010) proxies parental networks by geographic proximity of the father. He finds that when parental industries are growing, sons are more likely to be employed if their fathers live close by, indicating that fathers serve as network connections for their sons (but not daughters) in South Africa.

A conclusion of these studies is that parental contacts matter for where young workers find employment. This has implications for intergenerational mobility if opportunities in the labor market for young workers from different socio-economic backgrounds are partly determined by the access to potential employers that is provided through their parents.

The paper also contributes to a strand of literature that is concerned with how reliance on social contacts differs by demographic groups. The literature on social contacts generally agrees that networks are more important for workers with lower socio-economic status or less education even though studies generally cannot establish a causal relationship between the two (see, for instance Pellizzari, 2010; Corcoran et al., 1980; Datcher, 1983; Elliot, 1999). However, if a higher level or different type of education can provide a reliable signal for worker ability, uncertainty might be lower for those with a longer or more specific education and thus weaken the reliance on parents or other contacts. The existence of such a relationship suggests that education policy can enhance social mobility by reducing inequality in the access to employers through social networks, but my study is the first to provide causal evidence on this issue.

A theoretical foundation is provided by Casella and Hanaki (2008) who develop a model to test if, and under which conditions, signals obtained in the (education) market can reduce the reliance on contacts. They predict that this is possible under certain parameter restrictions, even though the reliance on

networks proves to be remarkably resilient in most cases, as indeed I find for the children of low-educated parents. As Casella and Hanaki (2008) argue, the reliance on contacts is more resilient the less precisely the signal (in this case, one more year of education) captures the skills required for the job at hand. In terms of the diverging patterns by parental background, this could reflect that more general education is a less precise and thus less informative signal for workers in industries that low-educated parents are typically employed in, but more informative in the industries of highly educated parents.

The paper is structured as follows: Section 3.2 gives an overview of the institutional background of the Swedish upper secondary school system and describes the reform in detail, while section 3.3 sets up the empirical strategy. Section 3.4 includes a description of the data, followed by the empirical results and robustness analysis in section 3.5. The last section concludes.

3.2 Institutional background

3.2.1 The Swedish school system

The Swedish upper secondary school system underwent a major reform in the 1990s that led to adjustments in the content and length of the vocational tracks, a change to a course-based program structure as well as a new grading system and curriculum. While the period of study in this paper predates the changes that came with the reform of the 1990s, I am exploiting changes in the vocational tracks during a trial period for some suggested aspects of the reform.

However, some general features of the Swedish school system were unaltered by the reform and remain unaffected over time. Following the nine years of compulsory school, students can choose to enroll in upper secondary education, which is divided into several academic and vocational tracks. Students who opt into vocational upper secondary education can apply to specific training programs such as "childcare", "construction" or "business" based on their grades from compulsory school. The vast majority of students enrolls in upper secondary education[5] with roughly half of a cohort opting for academic tracks and the other half for vocational tracks. For cohorts that started upper secondary education prior to the reform, academic tracks were 3 years long and could be chosen in preparation of higher education, while vocational tracks had less academic content and were only two years long. After the parliament voted in favor of the school reform in 1991, vocational tracks were extended to three years for those starting after 1992 in an attempt to facilitate transitions from upper secondary school to higher education. The extension went hand-

[5] According to Holmlund et al. (2014), during the period of study from 1986-1993, more than 90 percent of a birth cohort started upper secondary education and about 80% graduated before age 22.

in-hand with a broadening of the curriculum and led to the inclusion of more academic subjects, which granted students who graduated from those longer vocational tracks basic eligibility for university studies (see Holmlund et al., 2014). Prior to the reform, students could only attain university eligibility by graduating from academic tracks or by complementing their vocational studies with academic subjects.

3.2.2 Introduction of the trial period

Leading up to the reform in the 1990s, the decision was taken to implement an extensive nation-wide trial period during which vocational 3-year tracks were gradually introduced in a growing number of municipalities for the cohort starting upper secondary school between 1987 and 1990 (Holmlund et al., 2014). In line with the changes of the 1991 reform, students who enrolled in vocational tracks during the trial period were also exposed to a higher academic content and obtained eligibility for university studies. In addition to Swedish, which was the only general academic subject in 2-year tracks, the new 3-year tracks also included English, Social Sciences and electives such as Maths.

The extended tracks were also supposed to provide more on-the-job training with regional employers during the third year. Starting with the 1988 cohort, the goal was that ten percent of the education during the first two years and sixty percent during the third year should take place in workplaces.[6] In practice, the amount of workplace training generally increased during the third year, but not all schools met the third year goal, implying that the actual share of workplace training was often considerably lower than 60% in some tracks and municipalities (National Board of Education, 1990; SOU, 1989b, 1990). The introduction of the trial coincided with one of the most turbulent periods on the Swedish labor market. The failure to offer the intended amount of workplace training was thus in part due to the worsening economic climate in 1991.

The decision of the allocation of trial slots was taken by the National Board of Education.[7] The objectives of the roll-out were to ensure that each track should eventually have the same share of 3-year slots, while creating cross-municipality variation in trial intensity. As a results, a large share of vocational tracks was extended in some municipalities, whereas other municipalities only had few prolonged tracks. Further objectives were to create variation in participating municipalities with regard to industry and demographic structure (SOU, 1989a).

[6] Since the scope of the trial was small in 1987, there was no increase in workplace training.

[7] While municipalities had to apply if they wanted to be part of the trial, all municipalities opted into participating.

The trial did not increase the total number of available slots in vocational tracks. Instead, it converted existing classes in 2-year tracks into classes in 3-year tracks. Municipalities which were part of the trial could then increase the share of available 3-year tracks and slots in the following year of the trial. Consequently, both the number of municipalities that participated as well as the intensity of participation (as measured by the share of available 3-year tracks) increased during the trial period. The scope of the trial was substantial and by the end of it, about a fifth of all slots in vocational tracks had been converted to 3-year tracks. The number of available slots in 3-year tracks increased from just 500 in 1987 to 6,000 in 1988, 10,000 in 1989 and 11,200 in 1990. (Hall, 2012; SOU, 1989a,b, 1990).

In most municipalities, both 2- and 3-year tracks were available and, in some cases, both a 2- or 3-year version of the same type of track. Hence, some students could decide whether they wanted to attend a longer track depending on the municipality of residence and, to a lesser extent, neighboring municipalities if their own municipality did not offer vocational tracks. In many instances however, municipality participation in the trial and which tracks were affected, was decided after compulsory school leavers had to apply for upper secondary school. Hence, there was limited scope for students to actually choose the length of upper secondary school by applying to a school in a neighboring municipality (SOU, 1989a, 1990).

Consequences of the trial

The direct effects of the trial have been studied previously. Hall (2012) examines the effects of the reform on pursuing tertiary education and finds that contrary to its intention, the reform did not increase university enrollment or graduation.[8] Likewise, Hall (2013) analyzes whether having attended the longer vocational education reduced the risk for future unemployment during recession, but does not find any effect. Grönqvist and Hall (2011) use the same trial to investigate the effects of the reform on male and female fertility rates and find that while there was no effect on male fertility rates, female rates were lower among those who attended a 3-year rather than a 2-year program. A recent study that examines the effects of the reform on crime found a reduction in property crime amongst students who attended a vocational 3-year track (Grönqvist et al., 2015).

[8]Hall (2012) also finds evidence that low-performing students were more likely to drop out of upper secondary school as a consequence of the trial, which is, however, not corroborated by Holmlund et al. (2014).

3.2.3 The Swedish recession in the 1990s

Following a booming economy in the late 1980s, Sweden was hit by a severe recession in the early 1990s which lead to a financial crisis, a sharp decline in public spending and a subsequent soaring of the unemployment rate (see Holmlund, 2003). At the peak of the recession in the second part of the 1990s, unemployment had increased to eleven percent compared to less than three percent prior to the crisis. Young workers were affected by even higher unemployment, peaking at around 25 percent in the mid-1990s. As a consequence, students from the same cohort who attend a 3-year rather than a 2-year track graduated under systematically worse business cycle conditions. Recovery set in during the end of the decade as the unemployment rate fell to around six percent in 2001. Notably, unemployment stagnated at around twice the levels prior to the recession.[9]

3.3 Empirical model

In this paper, I study the effect of attending a longer vocational track on the reliance on parental contacts in the job-search process. In order to assess the impact of the trial, I start from the following equation:

$$Outcome_{icm} = \gamma_c + \mu_m + \beta Long\,track_{icm} + \delta X_i + \epsilon_{icm} \qquad (3.1)$$

where the subscripts *i*, *c* and *m* refer to individual *i* from cohort *c* (starting upper secondary school in the same year) in municipality *m*. γ_c and μ_m are cohort and municipality of residence fixed effects respectively and X_i is a vector of individual controls including sex, grade percentile rank from compulsory school, immigration background of the individual and parents and parents' highest education level. $Outcome_{icm}$ measures various outcomes, the main one being a dummy for whether graduate i was employed at the same establishment as a parent in a specific year $\tau = 3, 4, ..., 20$ after starting upper secondary school. Other outcomes include: an indicator that takes on the value one if individual i had (1) a stable job in the same 5-digit industry sector as a parent, (2) had a stable job, (3) was studying, as well as log earnings from employment in year τ after starting upper secondary school. The regression is run for each outcome year $\tau = 3, ..., 20$ separately. The parameter of interest is β, which captures the effect of starting a more general 3-year vocational track as compared to a 2-year track on outcomes (1)-(3).

Since students from the same cohort who attend a 2-year track enter the labor market one year earlier than their peers in 3-year tracks, the estimated effect captures the effect of attaining more education relative to potentially more labor market experience. However, one might be concerned that OLS estimates may be biased if selection into 2-year vocational tracks or the more

[9]The numbers are drawn from Statistic Sweden series 1987-2004, version 2015-10-27.

general 3-year vocational tracks is not random. To overcome this issue, I follow Hall (2012, 2013) and Grönqvist and Hall (2011) and use the introduction of the trial, which was described in the previous section, to identify a source of exogenous variation in the length of the track that students attended. As noted before, the setup of the trial led to a situation which created variation in the extent to which 3-year tracks were available to students from different municipalities and cohorts. I will exploit this variation in the share of available 3-year tracks (dependent on cohort and municipality of residence) as an instrument for whether an individual enrolled in a 2- or 3-year vocational track. In order to minimize the possibility that students moved to another municipality based on whether 2- or 3-year tracks were available, municipality of residence is measured as the municipality in which a student lived in the year prior to starting upper secondary education.[10] Standard errors are clustered on the level at which the instrument variation occurs, namely at the municipality $\times$ year level.

One potential concern is the fact that students who attend a three year track graduate under systematically worse business cycle conditions than students who attended a shorter 2-year track due to the recession that hit Sweden in the early 1990s. For this reason, my analysis of the outcomes is based on the same year after enrollment (and not graduation) within each cohort, thus removing potential correlation with the business cycle. Nonetheless, effects may still differ with the business cycle in case that graduates only benefit from longer education in years when unemployment is low, so that an effect of more education might only materialize after the end of the recession. I assess whether the effect of attending a longer track varies with the business cycle by including an interaction of the de-meaned national unemployment rate with the dummy indicating whether a student attended a 3-year track. To be able to do this, I pool my data across year τ after starting upper secondary school, which provides me with a data set with τ observations per individual. In this data set, I can introduce an interaction between the unemployment rate and track lengths that allows me to let the effect of track lengths vary over the business cycle. Note also the equivalence between model (3.1) and model (3.2) (before the interaction of track lengths and unemployment rate is added): both strategies yield the same parameter estimates of β_τ, but model (3.2) allows me to use a single regression to estimate the same 18 parameter estimates β_τ as from the 18 different regressions in model 3.1.

[10] Another potential threat to identification would arise if the introduction of vocational 3-year tracks led students to enroll in academic tracks rather than vocational tracks. Hall (2012) finds however no evidence for such a pattern.

Thus, I estimate the following model:

$$\begin{aligned} Outcome_{icm\tau} =& \gamma_{c\tau} + \mu_{m\tau} + \sum_{\tau=3}^{20} \beta_\tau Long\,track_{icm} + \sum_{\tau=3}^{20} \delta_\tau X_i \\ &+ \zeta UR_{it(=c+\tau)} \times Long\,track_{icm} + \epsilon_{icm\tau} \end{aligned} \tag{3.2}$$

To mimic equation (1), all variables in this data set are fully interacted with dummies for each year τ after starting upper secondary education. In this setup, the parameter β_τ captures the effect of attending a 3-year track vs a 2-year track at average business cycle conditions (that is, the average unemployment rate). The one difference to equation (3.1) is that equation (3.2) adds an interaction between the outcome-year unemployment rate and length of education, which is instrumented by an interaction of the outcome-year unemployment rate and the share of available 3-year tracks in a given municipality. The effect of whether attending an additional year of education depends on the business cycle is thus captured by parameter ζ.

3.3.1 Interpretation of effects

While the longer vocational tracks contained more academic subjects, there was also an increase in the amount of on-the-job training as compared to the original 2-year programs. The bulk of that increase occurred during the third year of the respective program at which point 60% of the school year was to be allocated to workplace training. In practice, this was not always achieved, and programs (such as, for instance, construction and healthcare) that already had strong ties to employers were more successful than others in providing workplace training. Unfortunately, there is no data available regarding where students received workplace training since students are not considered (and thus registered) as employed. Note also that the estimated effect of attending a longer track should be interpreted relative to the counterfactual of one year of more potential work experience (since students from the same cohort who attend a 2-year track enter the labor market one year earlier than their peers).

3.4 Data

The data used in the analysis stems from matched employer-employee data and registers from Statistics Sweden. The population of interest is defined by the Upper Secondary School application register, which entails information on all students who applied for upper secondary school each year. The register allows identification of a sample of all students below the age of 18 who apply to a vocational track directly from compulsory school. For all students, I

identify the location, type and length of the track they enrolled in as well as their GPA from compulsory school and municipality of residence in the year prior to starting upper secondary school.[11]

The application register is also used to identify the type and length of available tracks in different municipalities, which makes it possible to calculate the instrument, e.g. the trial intensity as measured by the share of vocational tracks in each municipality that are 3-years long (instead of 2 years). The instrument is calculated for all municipalities that offered vocational tracks during the trial period, which is the case for about 70 percent of the 284 municipalities at that time in Sweden. Municipalities that do not offer vocational tracks are typically small and excluded from my sample. The data from the application registers are matched with population registers containing information on individual and parental background characteristics, such as age, gender, immigrant background and highest education level of parents. "Compulsory" refers to those students whose parents have at most finished compulsory education, while "tertiary" refers to students who have at least one parent with some tertiary education.

Table 1. *Descriptives: Students in upper secondary education 1987-1990 by municipal share of prolonged vocational tracks*

	Share of 3-year tracks in municipality			
	Low		High	
	mean	sd	mean	sd
Enrolled in 3-year track	0.053	0.225	0.262	0.440
Avg share of 3-year tracks	0.047	0.055	0.322	0.179
Grade percentile rank	0.469	0.286	0.468	0.285
GPA from compulsory school	2.867	0.524	2.862	0.517
Female	0.398	0.489	0.396	0.489
Immigrant background	0.012	0.107	0.015	0.122
Parents with immigrant background	0.031	0.174	0.038	0.191
Parents' highest education level:				
Compulsory	0.298	0.457	0.255	0.436
Upper secondary	0.549	0.498	0.569	0.495
Post upper secondary	0.153	0.360	0.176	0.381
Observations	71567		48047	

Notes: Municipalities are defined as having a high share of 3-year tracks if the share of prolonged tracks is above the average of 0.15. The sample includes all students who applied for vocational tracks directly after finishing compulsory school in all municipalities that offered vocational tracks.

[11] The municipality of residence is measured during the last year of compulsory school in order to avoid the possibility that some students move to another municipality following the completion of compulsory school in response to the type and lengths of tracks that are available in their new municipality.

The final sample consists of $119,614$ individuals in 193 municipalities in four enrollment cohorts. Table A.1 in the appendix shows summary statistics for the whole sample, while Table 1 splits ups the sample by students in municipalities with below and above average shares of 3-year tracks. In both groups, students are very similar with regard to observed characteristics, even though the share of students with parents with at most compulsory schooling is slightly larger in municipalities with below average share of long vocational tracks, while the share of those with tertiary-educated parents is smaller.

Table A.2 shows the most common tracks separately for students with compulsory-educated parents and tertiary-educated parents. Among those with compulsory-educated parents, tracks specializing in industry and transport and vehicle engineering are more common than for those with tertiary-educated parents, while the reverse is true for electrical engineering.[12]

3.4.1 Working with a parent

I use matched employer-employee data covering Sweden's entire working age population (aged 16-69) to determine the establishments in which graduates and their parents work for up to twenty year after starting upper secondary school. The data includes annual earnings from a specific job spell as well as information on which months the individuals are employed. Establishments are identified by a unique combination of firm and workplace, which allows me to identify the physical establishment at which individuals worked. I use the data to identify whether and where graduates had a stable job in the 20 years following starting upper secondary school. In order to make sure that I capture a minimum level of labor market attachment and not just some small temporary job. I follow Kramarz and Skans (2014) who define a stable job as one that lasted at least four months during a calendar year and that generated total earnings of at least the equivalent of three times the monthly minimum wage as defined by the 10th percentile of the wage distribution.[13]

The main outcome of interest is whether an individual had a job simultaneously at the same establishment as a parent in a given calendar year. I create a dummy taking on the value one if both the student and at least one of their parents is registered at the same establishment during year τ after enrolling in upper secondary school.

[12] For robustness, I re-estimate my main results after excluding the above-mentioned tracks. Even with this restriction, the results are very similar and, if anything, larger in magnitude.

[13] This time series is obtained from Lönestrukturstatistiken, see Statistics Sweden `http://www.statistikdatabasen.scb.se`.

3.4.2 Labor market outcomes

Since attending an additional year of education might affect the choice of industry that students opt into, I also check whether there is evidence that an additional year of education alters the probability of working in the same sector as a parent in general. As such it is possible that a reduction in the probability to work with a parent does not reflect a reduced reliance on parental contacts, but instead that industries in which parents typically work are less attractive as a consequence of the reform. I construct a measure for working in the same industry as either parent by taking a similar approach as above. I identify the 5-digit industry in which both the graduates' as well as their parents' employer operates in. Since I want to capture whether the effects are driven by changes in the attractiveness of an industry, the measure excludes those who work at the same plant as a parent, which by default would mean working in the same industry.

In order to interpret the effects on working with a parent, I also analyze whether any changes in the probability to work with a parent simply reflect that students who were affected by the trial were also affected in terms of their probability to be employed and to pursue post-upper secondary education. I estimate whether graduates were stably employed during each of the 20 year following starting upper secondary school using the definition of a stable job above.

Hall (2012) does not find an effect of attending an additional year of education on the probability of enrolling in university or having positive earnings. She only finds a significant negative effect on earnings during the second and third year after starting upper secondary school (i.e. a mechanical effect since students in longer tracks have typically not entered the labor market yet). As opposed to Hall (2012), I estimate the effects on labor market and education outcomes relying on slightly different outcomes using model (2) and thus taking into account that potential effects might only manifest after the recession. For completeness, I also report results for the main sample on whether students are in education in the appendix. [14]

[14] This assessed by whether they receive any amount of study grants, which all students can receive who are above 16 years of age and are either in school, university or any other types of further education. In order to qualify for study grants, students need to study at least half-time and pass a certain amount of credits each semester.

3.5 Results

3.5.1 First stage

This section investigates whether the share of available 3-year tracks in a student's municipality of residence can be used as an instrument for whether students attain two or three years of vocational education on the upper secondary level.

Table 2. *First Stage Results*

Outcome	Attending a 3-year track	
	(1)	(2)
Trial intensity (instrument)	0.688***	0.688***
	(0.0355)	(0.0354)
Grade percentile rank		0.036***
		(0.008)
Female		-0.016**
		(0.007)
Immigrant background		0.014
		(0.012)
Parents with immigrant background		-0.004
		(0.005)
Parents' highest education level:		
Compulsory		*ref.*
Upper secondary		0.007***
		(0.002)
Post upper secondary		0.026***
		(0.003)
F-Statistic on instrument	375.15	376.12
Observations	119,614	119,614
R-squared	0.192	0.194
SE clustered on municipality*year	yes	yes
cohort FE	yes	yes
Municipality FE	yes	yes

Notes: Sample includes all vocational students enrolled in municipalities that participated in the trial between 1987-1990. */**/*** denotes significance at the 10/5/1 percent level respectively. Robust standard errors in parentheses allowing for clustering at the municipality×year level.

Table 2 shows the results from the first stage regression that replicates Hall (2012). Both regressions include cohort and municipality of residence fixed effects. Column 1 shows estimates when a dummy for attending a 3-year vocational track is regressed on the instrument. Additionally, column (2) includes individual characteristics, such as sex, grade percentile rank from compulsory school and immigrant background as well as parental characteristics regarding their highest education level and immigration background. The coefficients on the instrument are all statistically significant at the 1 percent level and the size of the estimate is robust to the introduction of individual and parental characteristics. To be precise, a 10 percentage point increase in the share of 3-year tracks in one's municipality of residence increases the probability of attending a 3-year track with roughly 7 percentage points. The interpretation of the F-static (see last row in table) further assures that the share of available 3-year tracks is a sufficiently strong instrument and that the coefficient for the instrument is not zero for any of the specifications.

3.5.2 Main results

I next turn to the main results. Table 3 shows the results for the main outcome "work in the same establishment as either parent" in column 1. For ease of exhibition, the results of model 3.2 for the different outcomes will henceforth be discussed in table form displaying the average immediate (year 3), short term (years 4-9), medium term (years 10-14) and long term (years 15-20) effect.[15]

After discussing the main results, I will investigate whether the results could be explained by changes in industry preferences or employment that could have arisen as a consequence of attending an additional year of education. Thus, I will relate the estimated effect on the probability to work with a parent to the results in columns (3) and (5) which show the results for working in the same 5-digit industry (but not plant) as a parent and stable employment respectively.

Table 3 shows the average effect over the time periods indicated in each row. Generally, attending an additional year of education seems to have a negative short run impact on the probability to work with one's parent. The negative effect in year 3 is expected since students who attend a three year track have not yet entered the labor market. The average effect during years 4-9 is weakly significant (at the ten percent level) and amounts to a one percentage point reduction in the probability of being employed at the same establishment as a parent; a sizable effect in relation to the mean outcome over the same time period. However, the estimates are not very precise. Not surprisingly, long run effects are close to zero, which is in line with the fact that we would expect parents to be most important during the early stages of a career.

[15] In practice, I estimate parameters β_τ from model 2 for $\tau = 3, 4, ...20$ and use the lincom command in Stata to estimate the mean effects over the indicated time period.

Table 3. *Main Results, full sample*

Outcome	Same establishment as parent		Same 5-digit sector as parent		Stable job	
	(1)	(2)	(3)	(4)	(5)	(6)
	Estimates, by years	Mean dep. variable	Estimates, by years	Mean dep. variable	Estimates, by years	Mean dep. variable
Year 3	-0.023*	0.070	-0.002	0.019	-0.011***	0.531
	(0.013)		(0.006)		(0.033)	
Year 4-9	-0.010*	0.072	0.005*	0.021	-0.028**	0.596
	(0.006)		(0.003)		(0.013)	
Year 10-14	0.004	0.061	0.004	0.023	0.030***	0.734
	(0.006)		(.003)		(0.009)	
Year 15-20	-0.005	0.047	-0.001	0.020	0.012*	0.785
	(0.004)		(0.002)		(0.007)	
Obs.	2078532		2078532		2078532	

Notes: */**/*** denotes significance at the 10/5/1 percent level respectively. Sample includes all vocational students enrolled in municipalities that participated in the trial between 1987-1990. Estimates and standard errors are calculated with lincom as average effects of the IV estimates β_τ of model 3.2 for the indicated years. Mean of the outcome variables is displayed for the indicated years. Robust standard errors allow for clustering at the municipality$\times$year level.

It is possible that the decrease in the probability to work with a parent reflects that students who attend an additional year of education are less likely to sort into the same industries as their parents. In that case, the estimates in column (1) would reflect that students who are exposed to more education alter their occupational choices. In column (3), I display a set of placebo-like estimates by defining the outcome as working in the same narrow 5-digit industry, but not establishment, as one's parent. If more education led to more intergenerational mobility in terms of what industries students work in, we would even expect to see a decrease in the estimates in column (3). However, there is no indication that attending a longer track has reduced the probability to sort into the same industries as parent, implying that the, albeit imprecise, effect during year 4-9 in column (1) is not driven by changes in industry preferences away from following in a parents' footsteps. Indeed the estimated impact on other jobs in the same industry is positive (and marginally significant).

Another caveat to the interpretation of the effects in column (1) would be if the additional year of education affects students' employment prospects, in which case any effects on the probability to work with a parent could be driven by overall changes in employment levels. Even though Hall (2012) does not find any significant effect on log earnings or the probability of having positive earnings[16] after year 3, I estimate the effect of an additional year of education on stable employment (and for completeness on earnings and the probability to study in table A.3 in the appendix). The main difference as opposed to Hall (2012) is that using model (2), I measure any effects at the average unemployment rate. Another difference is that I use the more restrictive definition of a stable job (instead of positive earnings) in order to assess the effect on employment.

The results are displayed in column (5). The negative effect in year 3 is to some extent mechanical since students who attend a third year of upper secondary education are less likely to have time to work and are obviously more likely to study (as the first stage holds and as confirmed in column (1) in table A.3).

However, I find evidence for a moderate negative effect on stable employment during years 4-9 after starting upper secondary school, implying that an additional year of potential labor market experience is valued more relative to an additional year of education, while the reverse seems to be true later during the career. Attending a longer track leads to a 2.8 percentage point reduction in the probability of having a stable job during years 4-9, but a positive effect of roughly the same magnitude later on. Note that in relation to the mean, the negative effect in years 4-9 is considerably smaller than the effect on working with a parent, suggesting that changes in employment can account for around a third of the effect of interest.

[16]There is imprecise evidence of a negative effect on the probability of having positive earnings in few years.

3.5.3 Heterogeneous effects

Parental background

As I have shown in the previous section, there is evidence that an additional year of education can reduce the reliance on parents for all vocational students (albeit imprecise). I next turn to heterogeneous results by education level of the graduates' parents. We know from previous research that the characteristics of the connected contacts can be important for the productivity of the link. In fact, parental ties in particular, are more likely to lead to recruitment by the parents' employer for lower educated parents (Kramarz and Skans, 2014). This is the case regardless of the education level of the child (though effects are smaller with increasing education level), indicating that the use of parental contacts is persistent among this group. However, could more education (within the same education level) be a way of breaking this link?

Table 4, shows heterogeneous results by education level of the parents. "Compulsory" refers to students whose parents have at most finished compulsory school, while "upper secondary" and "tertiary" refers to cases in which at least one parent has the indicated level of education. In line with the literature, we can see that the average share of students who work together with their parents is higher when parents have at most finished upper secondary school as compared to tertiary education (see means in columns (2),(4) and (6)).

As a results of evaluating differences in the impact of more education, two patterns emerge. Students with tertiary-educated parents are significantly less likely to find a job at the exact same plant as a parent during the earlier stages of their career, while there is a positive average effect for students with compulsory-educated parents during years 10-14 after starting upper secondary school. For all other students, an additional year of education does not seem to have any effect once students typically enter the labor market four years after starting upper secondary school.

For students with highly-educated parents, there is an average decrease in the probability to work with a parent of 4.8 percentage points during years 4-9 after starting upper secondary school. During this time period, the effects are large and can explain about 90 percent of the variation in the mean outcome. During the later stages of the career, the effects disappear.

In contrast, students are more likely to rely on their compulsory-educated parents to find a job at the same employer despite having obtained an additional year of education. Interestingly, the effect first materializes later in the career with an average increase of 2.7 percentage points during years 10-14.

A conclusion based on these results is that the use of parental contacts seems to be persistent among the group with low-educated parents regardless, while the additional year of education seems to equip students with tertiary-educated parents better to enter the labor market without their parents' help.

Table 4. *Effect on working with a parent, by parents' highest level of education*

Outcome	Same establishment as parent					
	Compulsory		Upper secondary		Tertiary	
	(1) Estimates, by years	(2) Mean dep. variable	(3) Estimates, by years	(4) Mean dep. variable	(5) Estimates, by years	(6) Mean dep. variable
Year 3	0.017	0.079	-0.031*	0.071	-0.074***	0.051
	(0.027)		(0.017)		(0.027)	
Year 4-9	0.004	0.078	-0.011	0.074	-0.048***	0.053
	(0.012)		(0.008)		(0.012)	
Year 10-14	0.027***	0.065	-0.006	0.064	-0.010	0.044
	(0.010)		(0.006)		(0.009)	
Year 15-20	0.013	0.047	0.005	0.050	-0.010	0.038
	(0.008)		(0.005)		(0.007)	
Obs	581742		1158948		337842	

Notes: */**/*** denotes significance at the 10/5/1 percent level respectively. Sample includes all vocational students enrolled in municipalities that participated in the trial between 1987-1990. "Compulsory" refers to students whose parents have at most finished compulsory education, "Upper secondary" to students whose parents have obtained at least some upper secondary education (but no tertiary education) and "Tertiary" to students have at least one parent with some tertiary education. Estimates and standard errors are calculated with lincom as average effects of the IV estimates β_τ of model 3.2 for the indicated years. Mean of the outcome variables is displayed for the indicated years. Robust standard errors allow for clustering at the municipality$\times$year level.

Table 5 shows the effects on working in the same 5-digit industry and stable employment split up by education level of the parents. For students with compulsory-educated parents, there is no evidence that more upper secondary education has affected the probability to work in the parental industry (but not plant). For students with tertiary-educated parents, the pattern that emerges in the long run indicates that this group of students is more likely to work in the same 5-digit industry sector as their parents (even after excluding those who work in a parent's plant) even up to 20 years after they started upper secondary school. Note that if the outcome working in the same 5-digit sector also includes jobs found at either parent's plant, the effects are close to zero.

An interpretation could be that this group of students sorts into the same industries as their parents regardless of whether they attended an additional year of education or not, but that additional schooling reduces the extent to which they rely on their parents to find a job by broadening their access to other employers in the parental industry.

An additional year of education does not seem to affect the probability of having a stable job for students with tertiary-educated parents (column 7), whereas students with low-educated parents are on average less likely to have a stable job during years 4-9 after starting upper secondary school. In the following time period (years 10-14), positive effects in terms of stable employment materialize for this group of students, indicating that experience is valued more relative to more education in the beginning of the career. A look at how studying is affected for this group (see additional results in table A.5) shows that they enter the labor market later after spending more time in education (both from and after upper secondary school).[17]

[17] Attending a longer track leads to higher employment rates later in life for those with low-educated parents. Delayed labor market entry also points to a potential explanation to why the increase in the probability to work with a parent occurs first during years 10-14 after starting upper secondary school and can also account for part (but not all) of the increase in the probability to find a job with a parent. Compare the effect relative to the mean outcome $0.027/0.065 = 0.42$ to the employment effect relative to the outcome mean: $0.096/0.735 = 0.13$.

Table 5. *Robustness, by parents' highest level of education*

Outcome	Same 5-digit sector				Stable job			
Parental ed.	Compulsory		Tertiary		Compulsory		Tertiary	
	(1) Estimates, by years	(2) Mean dep. variable	(3) Estimates, by years	(4) Mean dep. variable	(5) Estimates, by years	(6) Mean dep. variable	(7) Estimates, by years	(8) Mean dep. variable
Year 3	0.004	0.017	-0.007	0.020	-0.098**	0.571	-0.039	0.477
	(0.010)		(0.015)		(0.045)		(0.054)	
Year 4-9	0.007	0.019	0.001	0.024	-0.068***	0.611	-0.020	0.564
	(0.005)		(0.008)		(0.021)		(0.026)	
Year 10-14	0.003	0.020	0.031***	0.027	0.096***	0.735	-0.011	0.723
	(0.005)		(0.008)		(0.016)		(0.020)	
Year 15-20	0.001	0.016	0.025***	0.024	0.050***	0.781	-0.007	0.784
	(0.004)		(0.007)		(0.013)		(0.017)	
Obs	581742		337842		581742		337842	

Notes: */**/*** denotes significance at the 10/5/1 percent level respectively. Sample includes all vocational students enrolled in municipalities that participated in the trial between 1987-1990. "Compulsory" refers to students whose parents have at most finished compulsory education, "Upper secondary" to students whose parents have obtained at least some upper secondary education (but no tertiary education) and "Tertiary" to students have at least one parent with some tertiary education. Estimates and standard errors are calculated with lincom as average effects of the IV estimates β_τ of model 3.2 for the indicated years. Mean of the outcome variables is displayed for the indicated years. Robust standard errors allow for clustering at the municipality$\times$year level.

Student characteristics

On the supply side, workers with a weak position (lower education, low-paying occupations) tend to rely more on social contacts during job search (Elliot, 1999; Pellizzari, 2010). Kramarz and Skans (2014) find that this is in particular the case for labor market entrants with lower grades. As a consequence, academically weak students might benefit relatively more if attending a longer track provides a more accurate signal of ability for this group.

In table 6, I test whether there are heterogenous effects by grade quartiles from compulsory school. As opposed to the results for parental level of education, the results show no clear gradient.[18] Instead, both students in the top and the bottom grade quartile are less likely to work with their parents as a response of attending a 3-year track, while there is an increase for students in the middle of the grade distribution in the long run. The negative average effects for bottom and top students during years 4-9 are substantial in size and in relation to the outcome mean, suggesting that labor market entry is less reliant on their parents.

The results in table 7 confirm again that the results are not due to the fact that students are less likely to sort into parental industries per se. However, a small part of the negative effect for low-grade students can be accounted for by lower employment rates in the short run. Note also that Hall (2012) found that students with a low GPA from compulsory school, and to a lesser extent low-educated parents, had a higher probability of not completing upper secondary school if they enrolled in a prolonged vocational track. Even so, the first stage holds for even for those groups, implying that the monotonicity assumption should still hold.

[18]Note also that there is only partial overlap between students with compulsory-educated parents and low-grade students. Thus, the pattern for low-grade students in terms of employment in a parent's plant is not in line with that for students with low-educated parents.

Table 6. *Effect on working with a parent, by grade quartile*

Outcome	Same establishment as parent					
	Lowest GQ		Middle GQ		Highest GQ	
	(1) Estimates, by years	(2) Mean dep. variable	(3) Estimates, by years	(4) Mean dep. variable	(5) Estimates, by years	(6) Mean dep. variable
Year 3	-0.033	0.073	-0.014	0.072	-0.039*	0.062
	(0.025)		(0.016)		(0.022)	
Year 4-9	-0.045***	0.075	0.013	0.075	-0.030***	0.060
	(0.012)		(0.009)		(0.011)	
Year 10-14	-0.015	0.066	0.018**	0.064	-0.003	0.049
	(0.010)		(0.007)		(0.009)	
Year 15-20	-0.002	0.049	0.007***	0.050	-0.007***	0.038
	(0.003)		(0.002)		(0.002)	
Obs	525384		1081584		471564	

Notes: */**/*** denotes significance at the 10/5/1 percent level respectively. Sample includes all vocational students enrolled in municipalities that participated in the trial between 1987-1990. Grade quartiles are defined by cohort. Estimates and standard errors are calculated with lincom as average effects of the IV estimates β_τ of model 3.2 for the indicated years. Mean of the outcome variables is displayed for the indicated years. Robust standard errors in parentheses allowing for clustering at the municipality$\times$year level.

Table 7. *Robustness, by lowest and highest grade quartiles*

Outcome	Same 5-digit sector				Stable job			
	Lowest GQ		Highest GQ		Lowest GQ		Highest GQ	
	(1) Estimates, by years	(2) Mean dep. variable	(3) Estimates, by years	(4) Mean dep. variable	(5) Estimates, by years	(6) Mean dep. variable	(7) Estimates, by years	(8) Mean dep. variable
Year 3	-0.011	0.014	-0.010	0.023	-0.106**	0.500	-0.048	0.552
	(0.009)		(0.013)		(0.043)		(0.049)	
Year 4-9	-0.002	0.017	0.007	0.025	-0.063***	0.544	-0.012	0.628
	(0.005)		(0.007)		(0.022)		(0.024)	
Year 10-14	0.001	0.020	0.014**	0.026	0.067***	0.709	0.045***	0.742
	(0.005)		(0.006)		(0.018)		(0.017)	
Year 15-20	0.001	0.018	0.002	0.023	0.012**	0.750	0.000	0.804
	(0.001)		(0.002)		(0.005)		(0.005)	
Obs	525384		471564		525384		471564	

Notes: */**/*** denotes significance at the 10/5/1 percent level respectively. Sample includes all vocational students enrolled in municipalities that participated in the trial between 1987-1990. Grade quartiles are defined by cohort. Estimates and standard errors are calculated with lincom as average effects of the IV estimates β_τ of model 3.2 for the indicated years. Mean of the outcome variables is displayed for the indicated years. Robust standard errors allow for clustering at the municipality×year level.

I next turn to discussing the results by gender of the students. Typically, the Swedish labor market for men and women is quite segregated, which is already reflected in different track choices during upper secondary school. Hence, the importance of contacts, and to which extent they can be affected by a longer upper secondary education, may differ by gender.

However, the results in table 8 show no evidence of gender differences with regard to working in the same plant as a parent. Instead, the results mirror the average effects found for the full sample in table 3.[19] Note also that men are generally more likely to work with their parents (see column 4) as compared to women (column 2).

Table 8. *Effect on working with a parent, by gender*

Outcome	Same establishment as parent			
	Women		Men	
	(1) Estimates, by years	(2) Mean dep. variable	(3) Estimates, by years	(4) Mean dep. variable
Year 3	-0.023	0.058	-0.023	0.078
	(0.019)		(0.017)	
Year 4-9	-0.010	0.050	-0.010	0.087
	(0.010)		(0.007)	
Year 10-14	0.006	0.037	-0.006	0.077
	(0.007)		(0.007)	
Year 15-20	0.001	0.030	0.009*	0.059
	(0.005)		(0.005)	
Obs	835830		1242702	

Notes: */**/*** denotes significance at the 10/5/1 percent level respectively. Sample includes all vocational students enrolled in municipalities that participated in the trial between 1987-1990. Estimates and standard errors are calculated with lincom as average effects of the IV estimates β_τ of model 3.2 for the indicated years. Mean of the outcome variables is displayed for the indicated years. Robust standard errors allow for clustering at the municipality×year level.

3.6 Conclusion

I examine whether more upper secondary education replaces the need for social contacts in the job search process. I investigate this question by focusing on parental contacts which have proven to be important for where young workers find their first job. The overall reliance on these contacts is particularly

[19] Results for working in the same industry as a parent and stable employment are displayed in table A.6 in the appendix.

prevalent among children of low-educated parents. While theory suggests that education and social connections could be substituted as signals of ability, little is known about whether education policy can be designed so as to reduce the reliance on contacts and thus improve social mobility.

By exploiting a policy pilot that extended upper secondary education, I find that students who receive an additional year of education are less likely to find employment with their parents during the beginning of their career. I find the reverse impact regarding employment in other jobs in the same sector as the parent, suggesting that the results are unlikely to be driven by more general sectoral employment patterns.

The patterns diverge by the background of the students: Students with tertiary-school educated parents respond to a longer upper secondary education by reducing the reliance on their parents. The opposite is the case for students with low-educated parents who do not appear to respond at all in this dimension. Thus, more education appears to function as a substitute for social contacts for some groups, but not among students from lower-educated backgrounds. For these students, the reliance on family ties is already higher in general and appears to be very resilient to policy-induced variations in the length of education. In cases where these students cannot rely on their parents during job search, there is little reason to suspect that policies directed towards increasing education can fill the void.

Appendix

Table A.1. *Descriptive Statistics, full sample*

	mean	sd	min	max
Enrolled in 3-year track	0.137	0.344	0	1
Avg share of 3-year tracks	0.158	0.181	0	1
Grade percentile rank	0.469	0.286	0	1
GPA from compulsory school	2.865	0.521	1	5
Female	0.397	0.489	0	1
Immigrant background	0.013	0.113	0	1
Parents with immigrant background	0.034	0.181	0	1
Parents' highest education level:				
Compulsory	0.281	0.449	0	1
Upper secondary	0.557	0.497	0	1
Post upper secondary	0.162	0.369	0	1
Observations	119614			

Notes: Sample includes all vocational students enrolled in municipalities that participated in the trial between 1987-1990.

Table A.2. *Tracks by parents' highest education level*

	(1) Compulsory			(2) Tertiary		
	Obs	pct	cumpct	Obs	pct	cumpct
Construction	3937	11.06	11.06	1928	9.79	9.79
Electrical engineering	3823	10.74	21.80	3311	16.82	26.61
Transport & Vehicle engineering	3970	11.15	32.96	1452	7.38	33.99
Business & Services	6998	19.66	52.62	3664	18.61	52.60
Industry	5278	14.83	67.45	1781	9.05	61.64
Food manufacturing & restaurant	2242	6.30	73.75	1545	7.85	69.49
Use of natural resources	1423	4.00	77.74	979	4.97	74.47
Health care and caring services	6415	18.02	95.77	4189	21.28	95.74
Process technology	452	1.27	97.04	185	0.94	96.68
Textile & clothing manufacturing	426	1.20	98.24	285	1.45	98.13
Wood technology	628	1.76	100.00	368	1.87	100.00
Total	35592	100.00		19687	100.00	

Notes: Sample includes vocational students enrolled in municipalities that participated in the trial between 1987-1990. "Compulsory" refers to students whose parents have at most compulsory schooling, while "Tertiary" indicates that students have at least one parent with tertiary education. Only vocational tracks that existed both as 2- and 3-year tracks are included.

Table A.3. *Effect on log earnings and studying, full sample*

Outcome	Log earnings		Studying	
	(1) Estimates, by years	(2) Mean dep. variable	(3) Estimates, by years	(4) Mean dep. variable
Year 3	-0.312***	10.63	0.506***	0.381
	(0.087)		(0.028)	
Year 4-9	-0.041	11.058	0.094***	0.166
	(0.035)		(0.011)	
Year 10-14	0.065***	11.71	-0.027***	0.083
	(0.024)		(0.006)	
Year 15-20	0.030*	12.08	-0.003	0.049
	(0.017)		(0.004)	
Obs.	2078532		2078532	

Notes: */**/*** denotes significance at the 10/5/1 percent level respectively. Sample includes all vocational students enrolled in municipalities that participated in the trial between 1987-1990. Estimates and standard errors are calculated with lincom as average effects of the IV estimates β_τ of model 3.2 for the indicated years. Mean of the outcome variables is displayed for the indicated years. Robust standard errors allow for clustering at the municipality$\times$year level.

Table A.4. *Robustness, parents with upper secondary education*

Outcome	Same 5-digit sector		Stable job	
	(1)	(2)	(3)	(4)
	Estimates, by years	Mean dep. variable	Estimates, by years	Mean dep. variable
by years	variable	by years	variable	
Year 3	-0.003	0.019	-0.145***	0.527
	(0.008)		(0.038)	
Year 4-9	0.004	0.021	-0.021	0.598
	(0.004)		(0.017)	
Year 10-14	-0.005	0.023	0.007	0.737
	(0.004)		(0.012)	
Year 15-20	-0.009***	0.020	-0.001	0.788
	(0.003)		(0.009)	
Obs	1158948		1158948	

Notes: */**/*** denotes significance at the 10/5/1 percent level respectively. Sample includes all vocational students enrolled in municipalities that participated in the trial between 1987-1990. "Compulsory" refers to students whose parents have at most finished compulsory education, "Upper secondary" to students whose parents have obtained at least some upper secondary education (but no tertiary education) and "Tertiary" to students have at least one parent with some tertiary education. Estimates and standard errors are calculated with lincom as average effects of the IV estimates β_τ of model 3.2 for the indicated years. Mean of the outcome variables is displayed for the indicated years. Robust standard errors allow for clustering at the municipality$\times$year level.

Table A.5. *Effect on log earnings and studying, by parents' level of education*

Parental ed.	Log earnings				Studying			
Outcome	Compulsory		Tertiary		Compulsory		Tertiary	
	(1) Estimates, by years	(2) Mean dep. variable	(3) Estimates, by years	(4) Mean dep. variable	(5) Estimates, by years	(6) Mean dep. variable	(7) Estimates, by years	(8) Mean dep. variable
Year 3	-0.255**	10.73	-0.218	10.49	0.582***	0.311	0.449***	0.476
	(0.125)		(0.165)		(0.042)		(0.046)	
Year 4-9	-0.065	11.10	0.009	10.96	0.079***	0.122	0.115***	0.253
	(0.060)		(0.075)		(0.016)		(0.021)	
Year 10-14	0.082*	11.71	0.017	11.70	0.006	0.062	0.014	0.130
	(0.043)		(0.054)		(0.010)		(0.015)	
Year 15-20	0.038	12.05	-0.032	12.11	-0.003	0.043	-0.027***	0.059
	(0.034)		(0.038)		(0.007)		(0.009)	
Obs.	581742		337842		581742		337842	

Notes: */**/*** denotes significance at the 10/5/1 percent level respectively. Sample includes vocational students enrolled in municipalities that participated in the trial between 1987-1990. "Compulsory" refers to students whose parents have at most finished compulsory education, "Upper secondary" to students whose parents have obtained at least some upper secondary education (but no tertiary education) and "Tertiary" to students have at least one parent with some tertiary education. Estimates and standard errors are calculated with lincom as average effects of the IV estimates β_τ of model 3.2 for the indicated years. Mean of the outcome variables is displayed for the indicated years. Robust standard errors allow for clustering at the municipality$\times$year level.

Table A.6. *Robustness, by gender*

Outcome	Same 5-digit sector				Stable job			
Gender	Women		Men		Women		Men	
	(1) Estimates, by years	(2) Mean dep. variable	(3) Estimates, by years	(4) Mean dep. variable	(5) Estimates, by years	(6) Mean dep. variable	(7) Estimates, by years	(8) Mean dep. variable
Year 3	-0.003	0.023	-0.002	0.016	-0.056	0.545	-0.143***	0.522
	(0.011)		(0.006)		(0.052)		(0.032)	
Year 4-9	0.005	0.026	0.005	0.018	-0.024	0.556	-0.030**	0.623
	(0.007)		(0.003)		(0.023)		(0.014)	
Year 10-14	0.014**	0.026	-0.001	0.020	0.044**	0.626	0.025***	0.807
	(0.009)		(0.003)		(0.018)		(0.017)	
Year 15-20	0.017**	0.023	-0.005**	0.018	0.008	0.695	0.000	0.846
	(0.004)		(0.002)		(0.014)		(0.007)	
Obs	835830		1242702		835830		1242702	

Notes: */**/*** denotes significance at the 10/5/1 percent level respectively. Sample includes all vocational students enrolled in municipalities that participated in the trial between 1987-1990. "Compulsory" refers to students whose parents have at most finished compulsory education, "Upper secondary" to students whose parents have obtained at least some upper secondary education (but no tertiary education) and "Tertiary" to students have at least one parent with some tertiary education. Estimates and standard errors are calculated with lincom as average effects of the IV estimates β_τ of model 3.2 for the indicated years. Mean of the outcome variables is displayed for the indicated years. Robust standard errors allow for clustering at the municipality$\times$year level.

References

Altonji, J. G. and Pierret, C. R. (2001). Employer learning and statistical discrimination. *The Quarterly Journal of Economics*, 116(1):313–350.

Bayer, P., Ross, S., and Topa, G. (2008). Place of work and place of residence: Informal hiring networks and labor market outcomes. *Journal of Political Economy*, 116(6):1150–1196.

Casella, A. and Hanaki, N. (2008). Information channels in labor markets: On the resilience of referral hiring. *Journal of Economic Behavior Organization*, 66(3):492 – 513.

Corak, M. and Piraino, P. (2011). The intergenerational transmission of employers. *Journal of Labor Economics*, 29(1):37–68.

Corcoran, M., Datcher, L., and Duncan, G. (1980). Information and influence networks in labor markets. In Duncan, G. and Morgan, J., editors, *Five Thousand American Families: Patterns of Economic Progress.*, pages 1–37. Institute For Social Research, Ann Arbor.

Datcher, L. (1983). The impact of informal networks on quit behavior. *The Review of Economics and Statistics*, 65(3):491–495.

Dustmann, C., Glitz, A., Schönberg, U., and Brücker, H. (2016). Referral-based job search networks. *Review of Economic Studies*, 83(2):514–546.

Elliot, J. R. (1999). Social isolation and labor market insulation: Network and neighborhood effects on less-educated urban workers. *The Sociological Quarterly*, 40(2):199–216.

Fredriksson, P., Hensvik, L., and Skans, O. N. (2018). Mismatch of talent: Evidence on match quality, entry wages, and job mobility. *American Economic Review*, 108(11):3303–38.

Glitz, A. (2017). Coworker networks in the labour market. *Labour Economics*, 44:218 – 230.

Grönqvist, H. and Hall, C. (2011). Education policy and early fertility: Lessons from an expansion of upper secondary schooling. IFAU Working Paper 2011:24.

Grönqvist, H., Hall, C., Vlachos, J., and Åslund, O. (2015). Education and criminal behavior: Insights from an expansion of upper secondary school. IFAU Working Paper 2015:15.

Hall, C. (2012). The effects of reducing tracking in upper secondary school: Evidence from a large-scale pilot scheme. *Journal of Human Resources*, 47(1):237–269.

Hall, C. (2013). Does more general education reduce the risk of future unemployment? Evidence from labor market experiences during the Great Recession. IFAU Working Paper 2013:17.

Hensvik, L., Müller, D., and Skans, O. N. (2017). Connecting the young: High school graduates' matching to first jobs in booms and great recessions. IFAU Working Paper 2017:2.

Hensvik, L. and Skans, O. N. (2016). Social networks, employee selection and labor

market outcomes. *Journal of Labor Economics*, 34(4):825–867.

Holmlund, B. (2003). The rise and fall of swedish unemployment. CESifo Working Paper No. 918.

Holmlund, H., Häggblom, J., Lindahl, E., Martinson, S., Sjögren, A., Vikman, U., and Öckert, B. (2014). Decentralisering, skolval och fristående skolor: resultat och likvärdighet i svensk skola. IFAU Rapport 2014:25.

Kramarz, F. and Skans, O. N. (2014). When strong ties are strong: Networks and youth labour market entry. *Review of Economic Studies*, 81(3):1164–1200.

Magruder, J. R. (2010). Intergenerational networks, unemployment, and persistent inequality in South Africa. *American Economic Journal: Applied Economics*, 2(1):62–85.

Montgomery, J. D. (1991). Social networks and labor-market outcomes: Toward an economic analysis. *The American Economic Review*, 81(5):1408–1418.

Müller, D. (2020). Lost opportunities: Market work, establishment closures and the impact on career prospects. mimeo.

Munshi, K. (2003). Networks in the Modern Economy: Mexican Migrants in the U.S. Labor Market. *The Quarterly Journal of Economics*, 118(2):549–599.

National Board of Education (1990). Rapport om försöks- och utvecklingsarbetet i gymnasieskolan 1988/89. Report no 90:3.

Oyer, P. and Schaefer, S. (2011). Personnel economics: Hiring and incentives. In Ashenfelter, O. and Card, D., editors, *Handbook of Labor Economics*, volume 4b, chapter 20. Elsevier.

Pellizzari, M. (2010). Do friends and relatives help in getting a good job? *Industrial and Labor Relations Review*, 63(3):494–510.

SOU (1989a). Sextusen platser och tiotusen platser för försök i gymnasieskolan - hur, var och varför. Report 1989:106, UGY.

SOU (1989b). Utvärdering av försöksverksamheten med 3-årig yrkesinriktad utbildning i gymnasieskolan första året. Report 1, 1989:90, UGY.

SOU (1990). Utvärdering av försöksverksamheten med 3-årig yrkesinriktad utbildning i gymnasieskolan. Report 2, 1990:75, UGY.

SOU (1992). Utvärdering av försöksverksamheten med 3-årig yrkesinriktad utbildning i gymnasieskolan. Final report 1992:25.

Topa, G. (2011). Labor markets and referrals. In Benhabib, J., Bisin, A., and Jackson, M. O., editors, *Handbook of Social Economics, Volume 1B*, pages 1193–1221. Elsevier Science, Amsterdam.

Economic Studies

1987:1 Haraldson, Marty. To Care and To Cure. A linear programming approach to national health planning in developing countries. 98 pp.

1989:1 Chryssanthou, Nikos. The Portfolio Demand for the ECU. A Transaction Cost Approach. 42 pp.

1989:2 Hansson, Bengt. Construction of Swedish Capital Stocks, 1963-87. An Application of the Hulten-Wykoff Studies. 37 pp.

1989:3 Choe, Byung-Tae. Some Notes on Utility Functions Demand and Aggregation. 39 pp.

1989:4 Skedinger, Per. Studies of Wage and Employment Determination in the Swedish Wood Industry. 89 pp.

1990:1 Gustafson, Claes-Håkan. Inventory Investment in Manufacturing Firms. Theory and Evidence. 98 pp.

1990:2 Bantekas, Apostolos. The Demand for Male and Female Workers in Swedish Manufacturing. 56 pp.

1991:1 Lundholm, Michael. Compulsory Social Insurance. A Critical Review. 109 pp.

1992:1 Sundberg, Gun. The Demand for Health and Medical Care in Sweden. 58 pp.

1992:2 Gustavsson, Thomas. No Arbitrage Pricing and the Term Structure of Interest Rates. 47 pp.

1992:3 Elvander, Nils. Labour Market Relations in Sweden and Great Britain. A Comparative Study of Local Wage Formation in the Private Sector during the 1980s. 43 pp.

12 Dillén, Mats. Studies in Optimal Taxation, Stabilization, and Imperfect Competition. 1993. 143 pp.

13 Banks, Ferdinand E.. A Modern Introduction to International Money, Banking and Finance. 1993. 303 pp.

14 Mellander, Erik. Measuring Productivity and Inefficiency Without Quantitative Output Data. 1993. 140 pp.

15 Ackum Agell. Susanne. Essays on Work and Pay. 1993. 116 pp.

16 Eriksson, Claes. Essays on Growth and Distribution. 1994. 129 pp.

17 Banks, Ferdinand E.. A Modern Introduction to International Money, Banking and Finance. 2nd version, 1994. 313 pp.

18 Apel, Mikael. Essays on Taxation and Economic Behavior. 1994. 144 pp.

19 Dillén, Hans. Asset Prices in Open Monetary Economies. A Contingent Claims Approach. 1994. 100 pp.

20 Jansson, Per. Essays on Empirical Macroeconomics. 1994. 146 pp.

21 Banks, Ferdinand E.. A Modern Introduction to International Money, Banking, and Finance. 3rd version, 1995. 313 pp.

22 Dufwenberg, Martin. On Rationality and Belief Formation in Games. 1995. 93 pp.

23 Lindén, Johan. Job Search and Wage Bargaining. 1995. 127 pp.

24 Shahnazarian, Hovick. Three Essays on Corporate Taxation. 1996. 112 pp.

25 Svensson, Roger. Foreign Activities of Swedish Multinational Corporations. 1996. 166 pp.

26 Sundberg, Gun. Essays on Health Economics. 1996. 174 pp.

27 Sacklén, Hans. Essays on Empirical Models of Labor Supply. 1996. 168 pp.

28 Fredriksson, Peter. Education, Migration and Active Labor Market Policy. 1997. 106 pp.

29 Ekman, Erik. Household and Corporate Behaviour under Uncertainty. 1997. 160 pp.

30 Stoltz, Bo. Essays on Portfolio Behavior and Asset Pricing. 1997. 122 pp.

31 Dahlberg, Matz. Essays on Estimation Methods and Local Public Economics. 1997. 179 pp.

32 Kolm, Ann-Sofie. Taxation, Wage Formation, Unemployment and Welfare. 1997. 162 pp.

33 Boije, Robert. Capitalisation, Efficiency and the Demand for Local Public Services. 1997. 148 pp.

34 Hort, Katinka. On Price Formation and Quantity Adjustment in Swedish Housing Markets. 1997. 185 pp.

35 Lindström, Thomas. Studies in Empirical Macroeconomics. 1998. 113 pp.

36 Hemström, Maria. Salary Determination in Professional Labour Markets. 1998. 127 pp.

37 Forsling, Gunnar. Utilization of Tax Allowances and Corporate Borrowing. 1998. 96 pp.

38 Nydahl, Stefan. Essays on Stock Prices and Exchange Rates. 1998. 133 pp.

39 Bergström, Pål. Essays on Labour Economics and Econometrics. 1998. 163 pp.

40 Heiborn, Marie. Essays on Demographic Factors and Housing Markets. 1998. 138 pp.

41 Åsberg, Per. Four Essays in Housing Economics. 1998. 166 pp.

42 Hokkanen, Jyry. Interpreting Budget Deficits and Productivity Fluctuations. 1998. 146 pp.

43 Lunander, Anders. Bids and Values. 1999. 127 pp.

44 Eklöf, Matias. Studies in Empirical Microeconomics. 1999. 213 pp.

45 Johansson, Eva. Essays on Local Public Finance and Intergovernmental Grants. 1999. 156 pp.

46 Lundin, Douglas. Studies in Empirical Public Economics. 1999. 97 pp.

47 Hansen, Sten. Essays on Finance, Taxation and Corporate Investment. 1999. 140 pp.

48 Widmalm, Frida. Studies in Growth and Household Allocation. 2000. 100 pp.

49 Arslanogullari, Sebastian. Household Adjustment to Unemployment. 2000. 153 pp.

50 Lindberg, Sara. Studies in Credit Constraints and Economic Behavior. 2000. 135 pp.

51 Nordblom, Katarina. Essays on Fiscal Policy, Growth, and the Importance of Family Altruism. 2000. 105 pp.

52 Andersson, Björn. Growth, Saving, and Demography. 2000. 99 pp.

53 Åslund, Olof. Health, Immigration, and Settlement Policies. 2000. 224 pp.

54 Bali Swain, Ranjula. Demand, Segmentation and Rationing in the Rural Credit Markets of Puri. 2001. 160 pp.

55 Löfqvist, Richard. Tax Avoidance, Dividend Signaling and Shareholder Taxation in an Open Economy. 2001. 145 pp.

56 Vejsiu, Altin. Essays on Labor Market Dynamics. 2001. 209 pp.

57 Zetterström, Erik. Residential Mobility and Tenure Choice in the Swedish Housing Market. 2001. 125 pp.

58 Grahn, Sofia. Topics in Cooperative Game Theory. 2001. 106 pp.

59 Laséen, Stefan. Macroeconomic Fluctuations and Microeconomic Adjustments. Wages, Capital, and Labor Market Policy. 2001. 142 pp.

60 Arnek, Magnus. Empirical Essays on Procurement and Regulation. 2002. 155 pp.

61 Jordahl, Henrik. Essays on Voting Behavior, Labor Market Policy, and Taxation. 2002. 172 pp.

62 Lindhe, Tobias. Corporate Tax Integration and the Cost of Capital. 2002. 102 pp.

63 Hallberg, Daniel. Essays on Household Behavior and Time-Use. 2002. 170 pp.

64 Larsson, Laura. Evaluating Social Programs: Active Labor Market Policies and Social Insurance. 2002. 126 pp.

65 Bergvall, Anders. Essays on Exchange Rates and Macroeconomic Stability. 2002. 122 pp.

66 Nordström Skans, Oskar. Labour Market Effects of Working Time Reductions and Demographic Changes. 2002. 118 pp.

67 Jansson, Joakim. Empirical Studies in Corporate Finance, Taxation and Investment. 2002. 132 pp.

68 Carlsson, Mikael. Macroeconomic Fluctuations and Firm Dynamics: Technology, Production and Capital Formation. 2002. 149 pp.

69 Eriksson, Stefan. The Persistence of Unemployment: Does Competition between Employed and Unemployed Job Applicants Matter? 2002. 154 pp.

70 Huitfeldt, Henrik. Labour Market Behaviour in a Transition Economy: The Czech Experience. 2003. 110 pp.

71 Johnsson, Richard. Transport Tax Policy Simulations and Satellite Accounting within a CGE Framework. 2003. 84 pp.

72 Öberg, Ann. Essays on Capital Income Taxation in the Corporate and Housing Sectors. 2003. 183 pp.

73 Andersson, Fredrik. Causes and Labor Market Consequences of Producer Heterogeneity. 2003. 197 pp.

74 Engström, Per. Optimal Taxation in Search Equilibrium. 2003. 127 pp.

75 Lundin, Magnus. The Dynamic Behavior of Prices and Investment: Financial Constraints and Customer Markets. 2003. 125 pp.

76 Ekström, Erika. Essays on Inequality and Education. 2003. 166 pp.

77 Barot, Bharat. Empirical Studies in Consumption, House Prices and the Accuracy of European Growth and Inflation Forecasts. 2003. 137 pp.

78 Österholm, Pär. Time Series and Macroeconomics: Studies in Demography and Monetary Policy. 2004. 116 pp.

79 Bruér, Mattias. Empirical Studies in Demography and Macroeconomics. 2004. 113 pp.

80 Gustavsson, Magnus. Empirical Essays on Earnings Inequality. 2004. 154 pp.

81 Toll, Stefan. Studies in Mortgage Pricing and Finance Theory. 2004. 100 pp.

82 Hesselius, Patrik. Sickness Absence and Labour Market Outcomes. 2004. 109 pp.

83 Häkkinen, Iida. Essays on School Resources, Academic Achievement and Student Employment. 2004. 123 pp.

84 Armelius, Hanna. Distributional Side Effects of Tax Policies: An Analysis of Tax Avoidance and Congestion Tolls. 2004. 96 pp.

85 Ahlin, Åsa. Compulsory Schooling in a Decentralized Setting: Studies of the Swedish Case. 2004. 148 pp.

86 Heldt, Tobias. Sustainable Nature Tourism and the Nature of Tourists' Cooperative Behavior: Recreation Conflicts, Conditional Cooperation and the Public Good Problem. 2005. 148 pp.

87 Holmberg, Pär. Modelling Bidding Behaviour in Electricity Auctions: Supply Function Equilibria with Uncertain Demand and Capacity Constraints. 2005. 43 pp.

88 Welz, Peter. Quantitative new Keynesian macroeconomics and monetary policy 2005. 128 pp.

89 Ågren, Hanna. Essays on Political Representation, Electoral Accountability and Strategic Interactions. 2005. 147 pp.

90 Budh, Erika. Essays on environmental economics. 2005. 115 pp.

91 Chen, Jie. Empirical Essays on Housing Allowances, Housing Wealth and Aggregate Consumption. 2005. 192 pp.

92 Angelov, Nikolay. Essays on Unit-Root Testing and on Discrete-Response Modelling of Firm Mergers. 2006. 127 pp.

93 Savvidou, Eleni. Technology, Human Capital and Labor Demand. 2006. 151 pp.

94 Lindvall, Lars. Public Expenditures and Youth Crime. 2006. 112 pp.

95 Söderström, Martin. Evaluating Institutional Changes in Education and Wage Policy. 2006. 131 pp.

96 Lagerström, Jonas. Discrimination, Sickness Absence, and Labor Market Policy. 2006. 105 pp.

97 Johansson, Kerstin. Empirical essays on labor-force participation, matching, and trade. 2006. 168 pp.

98 Ågren, Martin. Essays on Prospect Theory and the Statistical Modeling of Financial Returns. 2006. 105 pp.

99 Nahum, Ruth-Aïda. Studies on the Determinants and Effects of Health, Inequality and Labour Supply: Micro and Macro Evidence. 2006. 153 pp.

100 Žamac, Jovan. Education, Pensions, and Demography. 2007. 105 pp.

101 Post, Erik. Macroeconomic Uncertainty and Exchange Rate Policy. 2007. 129 pp.

102 Nordberg, Mikael. Allies Yet Rivals: Input Joint Ventures and Their Competitive Effects. 2007. 122 pp.

103 Johansson, Fredrik. Essays on Measurement Error and Nonresponse. 2007. 130 pp.

104 Haraldsson, Mattias. Essays on Transport Economics. 2007. 104 pp.

105 Edmark, Karin. Strategic Interactions among Swedish Local Governments. 2007. 141 pp.

106 Oreland, Carl. Family Control in Swedish Public Companies. Implications for Firm Performance, Dividends and CEO Cash Compensation. 2007. 121 pp.

107 Andersson, Christian. Teachers and Student Outcomes: Evidence using Swedish Data. 2007. 154 pp.

108 Kjellberg, David. Expectations, Uncertainty, and Monetary Policy. 2007. 132 pp.

109 Nykvist, Jenny. Self-employment Entry and Survival - Evidence from Sweden. 2008. 94 pp.

110 Selin, Håkan. Four Empirical Essays on Responses to Income Taxation. 2008. 133 pp.

111 Lindahl, Erica. Empirical studies of public policies within the primary school and the sickness insurance. 2008. 143 pp.

112 Liang, Che-Yuan. Essays in Political Economics and Public Finance. 2008. 125 pp.

113 Elinder, Mikael. Essays on Economic Voting, Cognitive Dissonance, and Trust. 2008. 120 pp.

114 Grönqvist, Hans. Essays in Labor and Demographic Economics. 2009. 120 pp.

115 Bengtsson, Niklas. Essays in Development and Labor Economics. 2009. 93 pp.

116 Vikström, Johan. Incentives and Norms in Social Insurance: Applications, Identification and Inference. 2009. 205 pp.

117 Liu, Qian. Essays on Labor Economics: Education, Employment, and Gender. 2009. 133 pp.

118 Glans, Erik. Pension reforms and retirement behaviour. 2009. 126 pp.

119 Douhan, Robin. Development, Education and Entrepreneurship. 2009.

120 Nilsson, Peter. Essays on Social Interactions and the Long-term Effects of Early-life Conditions. 2009. 180 pp.

121 Johansson, Elly-Ann. Essays on schooling, gender, and parental leave. 2010. 131 pp.

122 Hall, Caroline. Empirical Essays on Education and Social Insurance Policies. 2010. 147 pp.

123 Enström-Öst, Cecilia. Housing policy and family formation. 2010. 98 pp.

124 Winstrand, Jakob. Essays on Valuation of Environmental Attributes. 2010. 96 pp.

125 Söderberg, Johan. Price Setting, Inflation Dynamics, and Monetary Policy. 2010. 102 pp.

126 Rickne, Johanna. Essays in Development, Institutions and Gender. 2011. 138 pp.

127 Hensvik, Lena. The effects of markets, managers and peers on worker outcomes. 2011. 179 pp.

128 Lundqvist, Heléne. Empirical Essays in Political and Public. 2011. 157 pp.

129 Bastani, Spencer. Essays on the Economics of Income Taxation. 2012. 257 pp.

130 Corbo, Vesna. Monetary Policy, Trade Dynamics, and Labor Markets in Open Economies. 2012. 262 pp.

131 Nordin, Mattias. Information, Voting Behavior and Electoral Accountability. 2012. 187 pp.

132 Vikman, Ulrika. Benefits or Work? Social Programs and Labor Supply. 2013. 161 pp.

133 Ek, Susanne. Essays on unemployment insurance design. 2013. 136 pp.

134 Österholm, Göran. Essays on Managerial Compensation. 2013. 143 pp.

135 Adermon, Adrian. Essays on the transmission of human capital and the impact of technological change. 2013. 138 pp.

136 Kolsrud, Jonas. Insuring Against Unemployment 2013. 140 pp.

137 Hanspers, Kajsa. Essays on Welfare Dependency and the Privatization of Welfare Services. 2013. 208 pp.

138 Persson, Anna. Activation Programs, Benefit Take-Up, and Labor Market Attachment. 2013. 164 pp.

139 Engdahl, Mattias. International Mobility and the Labor Market. 2013. 216 pp.

140 Krzysztof Karbownik. Essays in education and family economics. 2013. 182 pp.

141 Oscar Erixson. Economic Decisions and Social Norms in Life and Death Situations. 2013. 183 pp.

142 Pia Fromlet. Essays on Inflation Targeting and Export Price Dynamics. 2013. 145 pp.

143 Daniel Avdic. Microeconometric Analyses of Individual Behavior in Public Welfare Systems. Applications in Health and Education Economics. 2014. 176 pp.

144 Arizo Karimi. Impacts of Policies, Peers and Parenthood on Labor Market Outcomes. 2014. 221 pp.

145 Karolina Stadin. Employment Dynamics. 2014. 134 pp.

146 Haishan Yu. Essays on Environmental and Energy Economics. 132 pp.

147 Martin Nilsson. Essays on Health Shocks and Social Insurance. 139 pp.

148 Tove Eliasson. Empirical Essays on Wage Setting and Immigrant Labor Market Opportunities. 2014. 144 pp.

149 Erik Spector. Financial Frictions and Firm Dynamics. 2014. 129 pp.

150 Michihito Ando. Essays on the Evaluation of Public Policies. 2015. 193 pp.

151 Selva Bahar Baziki. Firms, International Competition, and the Labor Market. 2015. 183 pp.

152 Fredrik Sävje. What would have happened? Four essays investigating causality. 2015. 229 pp.

153 Ina Blind. Essays on Urban Economics. 2015. 197 pp.

154 Jonas Poulsen. Essays on Development and Politics in Sub-Saharan Africa. 2015. 240 pp.

155 Lovisa Persson. Essays on Politics, Fiscal Institutions, and Public Finance. 2015. 137 pp.

156 Gabriella Chirico Willstedt. Demand, Competition and Redistribution in Swedish Dental Care. 2015. 119 pp.

157 Yuwei Zhao de Gosson de Varennes. Benefit Design, Retirement Decisions and Welfare Within and Across Generations in Defined Contribution Pension Schemes. 2016. 148 pp.

158 Johannes Hagen. Essays on Pensions, Retirement and Tax Evasion. 2016. 195 pp.

159 Rachatar Nilavongse. Housing, Banking and the Macro Economy. 2016. 156 pp.

160 Linna Martén. Essays on Politics, Law, and Economics. 2016. 150 pp.

161 Olof Rosenqvist. Essays on Determinants of Individual Performance and Labor Market Outcomes. 2016. 151 pp.

162 Linuz Aggeborn. Essays on Politics and Health Economics. 2016. 203 pp.

163 Glenn Mickelsson. DSGE Model Estimation and Labor Market Dynamics. 2016. 166 pp.

164 Sebastian Axbard. Crime, Corruption and Development. 2016. 150 pp.

165 Mattias Öhman. Essays on Cognitive Development and Medical Care. 2016. 181 pp.

166 Jon Frank. Essays on Corporate Finance and Asset Pricing. 2017. 160 pp.

167 Ylva Moberg. Gender, Incentives, and the Division of Labor. 2017. 220 pp.

168 Sebastian Escobar. Essays on inheritance, small businesses and energy consumption. 2017. 194 pp.

169 Evelina Björkegren. Family, Neighborhoods, and Health. 2017. 226 pp.

170 Jenny Jans. Causes and Consequences of Early-life Conditions. Alcohol, Pollution and Parental Leave Policies. 2017. 209 pp.

171 Josefine Andersson. Insurances against job loss and disability. Private and public interventions and their effects on job search and labor supply. 2017. 175 pp.

172 Jacob Lundberg. Essays on Income Taxation and Wealth Inequality. 2017. 173 pp.

173 Anna Norén. Caring, Sharing, and Childbearing. Essays on Labor Supply, Infant Health, and Family Policies. 2017. 206 pp.

174 Irina Andone. Exchange Rates, Exports, Inflation, and International Monetary Cooperation. 2018. 174 pp.

175 Henrik Andersson. Immigration and the Neighborhood. Essays on the Causes and Consequences of International Migration. 2018. 181 pp.

176 Aino-Maija Aalto. Incentives and Inequalities in Family and Working Life. 2018. 131 pp.

177 Gunnar Brandén. Understanding Intergenerational Mobility. Inequality, Student Aid and Nature-Nurture Interactions. 2018. 125 pp.

178 Mohammad H. Sepahvand. Essays on Risk Attitudes in Sub-Saharan Africa. 2019. 215 pp.

179 Mathias von Buxhoeveden. Partial and General Equilibrium Effects of Unemployment Insurance. Identification, Estimation and Inference. 2019. 89 pp.

180 Stefano Lombardi. Essays on Event History Analysis and the Effects of Social Programs on Individuals and Firms. 2019. 150 pp.

181 Arnaldur Stefansson. Essays in Public Finance and Behavioral Economics. 2019. 191 pp.

182 Cristina Bratu. Immigration: Policies, Mobility and Integration. 2019. 173 pp.

183 Tamás Vasi. Banks, Shocks and Monetary Policy. 2020. 148 pp.

184 Jonas Cederlöf. Job Loss: Consequences and Labor Market Policy. 2020. 213 pp.

185 Dmytro Stoyko. Expectations, Financial Markets and Monetary Policy. 2020. 153 pp.

186 Paula Roth. Essays on Inequality, Insolvency and Innovation. 2020. 191 pp.

187 Fredrik Hansson. Consequences of Poor Housing, Essays on Urban and Health Economics. 2020. 143 pp.

188 Maria Olsson. Essays on Macroeconomics: Wage Rigidity and Aggregate Fluctuations. 2020. 130 pp.

189 Dagmar Müller. Social Networks and the School-to-Work Transition. 2020. 146 pp.