With a Little Help from my … Parents?

Family Networks and Youth Labor Market Entry[*]

by

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Abstract

The paper studies the importance of family networks and the way these networks affect the transition from school to work. We use a Swedish population-wide linked employer-employee data set that also includes detailed information on family ties, schools, and class composition. Because we are able to follow all students graduating from a given school in the same year within the same class and the same field of study (thus the same expected occupation) into their employing plant, where one of their parent can also be employed, we can identify the direct effect of family relationships controlling for confounding factors, in particular those related to location, education, or occupation. Results show that family ties are indeed important for the transition from school to work, in particular for low-educated males who are more likely to follow their fathers. In addition, a firm hires more graduating workers when children of its employees just graduated. Analyzing parental characteristics (wage and seniority, in particular), job characteristics (type of occupation, wage, stability), the economic environment (unemployment, job structure in the municipality) at hiring, as well as employing firm characteristics, we find that family networks reduce the uncertainty inherent in the transition between school and work and that children in "weak" positions tend to get their first jobs with a little help from their "strong" parents.

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1 Introduction

There is a large and growing literature documenting the importance of networks in facilitating the process of matching workers and firms on the labor market. The literature is essentially divided into two parts, one focusing on the supply side where social networks help workers find jobs, and one focusing on the demand side where firms use networks to find better suited workers to fill vacancies. Our contribution is empirical and looks at both sides simultaneously. We restrict attention to the role of family networks and examine if and how parents assist their children in the transition from school to their first stable job and if and how firms use and benefit from parental networks. Using detailed data from both sides of the market – children, firms, with the parents in-between – we analyze how demand and supply side factors interact to determine when networks are used, and how the use of networks affect subsequent outcomes for the agents involved.

Studying the role played by parents for youths entering the labor market has three main empirical advantages. First, we can precisely identify which person is involved in the network. This stands in contrast with most previous empirical work where networks are often defined in a way (someone in the same neighborhood, school or village for example) that provides no direct evidence on the exact person who created the link or that the persons indeed interacted.[1] Second, we can define exactly on which side of the network each person is: we know that firms are on the demand side, that the entering youths are on the supply side, and that parents are agents of the match, informing their kids about job characteristics, job openings and informing their employing firm about their children' availability, quality, and characteristics. Thus, we simultaneously look at factors on the two sides of the market, trying to assess which side plays a bigger role.[2] Finally, because networks should help when "external" information available is minimal and because students exiting the education system generally have little prior contact with the world of the firm, this moment should be one when workers and firms rely most on "networking" data.

The existing literature on the extent and role of social networks in developed economies, is burgeoning both on the theoretical side (see Montgomery, 1991, or more recently Calvó-Armengol, Verdier and Zenou (2007), Ballester, Calvó-Armengol and Zenou (2006), Calvo-Armengol, 2004, Calvo-Armengol and Jackson, 2004 and 2007, Casella and Hanaki, 2008, among many authors, and Jackson, 2004 for a very thorough survey) as well as on the empirical side (see Munshi, 2003, Bayer, Ross and Topa, 2008, Bertrand, Luttmer and Mullainathan, 2000, Fredriksson and Åslund, 2009, Laschever, 2005 again among many authors, and Ioannides and Loury, 2004 for a very detailed survey) after a period of relative calm following the path-breaking articles of Rees (1966) and Granovetter (1973).

The "informal" hiring channel is the focus of growing number of empirical contributions. But the phenomenon, as happens virtually always, preceded its extensive study. As early as 1923, De Schweinitz (1932) finds that more than 40% of workers in the hosiery industry in Philadelphia obtained their job through friends and relatives. The importance of this "informal" channel as a resource for getting jobs has been documented by various surveys. It appears to be pervasive irrespective of the occupation or country. Ioannides and Loury (2004) provide a comprehensive overview of many of the literature findings. Bewley (1999, p. 368) gives a slightly older list of studies that were published between the years 1932-1990. The percent of jobs or job offers obtained through the informal channel of friends and relatives goes from 18% to 78% (from 30 to 60% in

[1] See however the recent contribution of Bandiera, Barankay, and Rasul (2009).
[2] In addition we do not face the "reflection problem" discussed in the neighboring literature on peer effects (Manski, 1993).

most cases). In the following paragraphs, we focus on some recent articles that try to get at the *exact channel* of entry into jobs.

Because of their diversity, we focus in our discussion on those contributions that address questions that we also address in this paper. We distinguish between three types of issues in this paper.

First, we will look at parents (networks) **as a source of information about jobs**. Indeed, a very active line of study investigates whether the neighborhood constitutes such a source of information and therefore tries to give a more precise content to “friends”. This informational aspect of location networks was used by Topa (2001) to explain the clustering of unemployment within Chicago neighborhoods. He adopted a probabilistic approach for the likelihood of a contact (which allows for “spillover” of information across census tracts). The recent work of Bayer, Ross, and Topa (2008) goes a step further and contributes to a better understanding of the referral aspect of networks again at this neighborhood level. Using micro-level census data for Boston, they find that those who live on the same block are more than 50% more likely to work together, than those living in nearby blocks. Munshi (2003) examines the role of the city of origin for Mexican immigrants but his data does not allow him to investigate the workplace. Laschever (2005) relies on the random assignment of American WWI veterans to military units. Using a small data set (n=1,295), he is able to show that an increase in peers’ unemployment decreases a veteran’s likelihood of employment. Laschever’s focus is identification of various peer effects. To perform his identification of peer effects, he contrasts two reference groups for each veteran: those who served with him at WWI and his closest neighbors (in terms of physical distance) at the 1930 Census. A new set of papers (Cingano and Rosolia, 2008 and Åslund, Hensvik and Skans, 2009, Dustmann, Glitz, and Schönberg, 2010are good examples) looks at matched longitudinal employer-employee data to follow workers who have worked in the same firm at some point in time and check if the characteristics (say, the geographic origin) of their network has an impact on job search or other outcomes. Corak and Piraino (2010) use data somewhat similar to ours but focus on intergenerational earnings mobility for men who have the same main employer as their fathers (but were not necessarily simultaneously employed at the same firm). By contrast with our analysis, their focus is clearly not on referral or networks at the moment of entry on the labor market.

In contrast with most of these papers, we use extremely precise and error-free measures of family links between the informed (the child) and the informant (the father or the mother). For both of them, we have virtually all information that one classically has in surveys, even though the data we use are administrative. Hence, the father and the mother are our equivalent to the neighborhood in Bayer et al.’s approach. Furthermore, again in contrast with most previous studies, we use an exhaustive sample of individuals. Our data allow us to follow all graduates who leave Swedish schools between 1988 and 1995 during a seven years period (i.e. until 2002 for the latest cohort). In addition, we can use data on any worker employed in the Swedish economy, even though we focus on those employed at plants in which the parents or the children also work. Thus, we can also look at differences in behavior between different parents working in the same firm or plant. In the spirit of Laschever, we define reference groups for each person for whom we examine entry in a first job – most often, students who graduate from the same classroom, i.e. in the same year, at the same school, in the same field of education – but our use of this reference group is different from his. A child and her parents in our strategy constitute the equivalent of Laschever’s or Bayer et al.’s reference groups (those with whom a person potentially works and gets job information from). Hence, every child in a classroom faces different information sets because of the privileged access each one has to his or her parents. Hence, we can use two types of variations to identify the effects of interest: on the child side (grades, sex, field of education); on the parent’s side (in particular, the parent’s plant characteristics and identifier, the parent’s sex, field of education, wage or tenure,…).

Second, we try to look at parents **as a source of information on the quality of the applicants**, inspired by a second line of study, mostly theoretical up to now, that insists on the role of networks in solving the **adverse selection** problem that firms face when selecting among job applications. For instance, Montgomery (1991) shows that referrals and networks help the firms to select workers when their type is not widely observable by the market. Other papers insist on this unobserved ability component of the referred individual. Empirically, very few articles have attacked this issue directly, at least to our knowledge. Again, in contrast with all previous studies, we directly measure some aspects of quality of the applicants. First, our data sources include national grades, for all compulsory school and high-school graduates (but not university graduates). Because we include all students in the same class, we are able to compute a relative measure of quality. Second, because we are able to track the exact plant at which young workers are employed, we measure outcomes (such as wages) relative to other new entrants or workers within the employing entity (using plant fixed effects). More precisely, we compare outcomes at the level of the plant where parents and children are working together, by looking at co-workers and new hires that entered this particular plant by channels other than "family hiring" (referral).

Third, and finally, we examine how jobs obtained through family links unfold within the employing firm. In particular, we look at **mid-term outcomes** for the children, as well as for the parents. Interestingly, the within-plant relation may allow the firm to solve **moral hazard problems** (if the child does not provide enough effort, breaches the contract, or if the parent lied about the quality of the child) using potential punishments. In general, because we follow workers, parents as well as children, over time we are in a position to examine the mid-term outcomes for those who were hired by referral as well as for those who acted as referral and contrast them with those hired from the same class without a parent in the plant. We believe that we are the first analysts to look at this precise question in the job search context. There is another context, though, where this type of problems emerges: credit market failures in developing economies. Ghatak and Guinanne (1999), Ghatak (2002), and Conning (2005) look at microfinance when peers can monitor members of their social network. This reciprocal monitoring can facilitate credit access. Millo and Pasini (2007) presents a theoretical framework that helps understand how repeated interactions together with social networks help alleviate moral hazard in non-market insurance situations. All these papers may help us understand **how the joint presence of a child and a parent in a plant may be useful for their employing firm**.

Our findings can be summarized as follows:

- First, family networks matter for the employer at which young workers find their first jobs.[3] Specifically, a plant is more likely to hire one of his employees' children than someone else from the same class, and the plant hires more graduates in the years when children of the employees enter the market.

- Second, the network effect is stronger for children with poor labor market prospects, in particular those with low schooling and poor grades as well as in years of high unemployment.

- Third, networks matter more if the demand side (the parent seen as an agent of the firm) is well attached to the plant. Specifically, networks matter more if the agent is a high-wage worker with relatively long tenure at the plant, even controlling for plant fixed effects. The

[3] Our analysis exclude self-employed parents.

effect is substantially weaker if the parent has left the plant, and (except for university graduates) at other plants within the parents' firm.

- Fourth, gender matters: boys follow parents more than girls and paternal links matter more than maternal links. The finding that networks matter more for males concur with results in previous research (e.g. in Bayer et al, 2008), but a novel finding is that the gender effect on the demand side is driven by the type of employing plant: within plants, the gender of the parent does not matter. On the supply (child) side, boys benefit from parental networks more than girls, even within plants.

- Fifth, similarity between parents and children reinforce the network effect, in particular fathers (mothers) matter more for sons (daughters) and the network effect is stronger when children have an education which is similar to that of their parents.[4]

- Sixth, information is a key driver of network effects. Occupations in which parental hiring is widely used are those with many outgoing students and relatively broad scope of potential receiving firms in the municipality (not bakers or masons or any other craft but skilled metal workers or energy workers) together with a large number of potential employers in the municipality. In addition, occupations where skills transmission (from parents to children, say) is potentially more intensive, through apprenticeship for instance, happen to be those where networks are less in use.

- Finally, when analyzing the consequences of parental networks for subsequent labor market outcomes we find that the initial wage paid to the child is lower than for equivalent persons entering the plant through other channels. However, this is partially compensated in the mid-term; these children spend longer spells in their first job than hires without a parent in the plant. For firms, parental hiring thus appears to be one way of reducing (young) workers subsequent turnover.

To summarize, based on the findings that initial wages are lower and information deficiencies increases the importance of networks, the parental networks do not matter because children (the supply side) are better informed of the quality and characteristics of jobs but because parents constitute a two-way transmission device of information flowing between their children and their firms (the demand side). This increased flow of information induces plants to hire more (graduating) workers than they otherwise would have done, to increase the tenure of hired workers, and (perhaps most importantly) to assist poor performing children in getting their first real jobs, with a little help from their well attached parents.

The rest of the paper is structured as follows: First, section 2 discusses some elements of theory and the empirical model. Section 3 provides a brief background of Swedish institutions and the labor market conditions at the time of study. Section 4 gives a detailed description of the used data and how it has been constructed. Section 5 provides empirical results and Section 6 concludes.

[4] This analysis accounts for the direct effect of similarity by using classmates whose parents also have the same type of education as their parents to estimate the counterfactual probability of entering the plant.

2 Theory and empirical model

2.1 Theory

In this subsection, we investigate the potential roles of networks in labor markets. In particular, we try to focus on the specific role of parents. We examine in turn various functions.

Roles of (parental) networks when searching for a job:

Informing the job searcher: in a situation where job openings are rare, dispersed, or difficult to locate, a job searcher will use informants more intensively. In addition, networks may help the job searcher to learn about the quality of the jobs in the contact firms. This effect should be even stronger for young workers examined in this paper who generally have little first-hand knowledge of the labor market.

Informing the firm about the job searcher's quality: in this context, the presence of unobserved heterogeneity with potential adverse selection problems may explain why firms use referring. A good starting point when studying the potential role of referral hiring on labor market outcomes in this context is Montgomery (1991). Workers can be of two types, either high or low ability. Firms do not observe workers' type before production (high ability workers produce one unit of output when low ability workers produce zero). On the firm side, each firm has at most one worker and the profit is productivity minus wage (set before production). Montgomery adds the following social structure (with two periods). Each period-1 worker knows at most one period-2 worker. But, period-2 workers may hold multiple ties with period-1 workers. If a period-1 worker holds a social tie, the specific period-2 worker's type is first randomly selected (by assumption, this period-2 worker has the same type as the period-1 worker with probability α strictly greater than ½). Then, the specific worker is chosen among those with the type just selected. The firm may hire through the market or through referrals. If a referral hire is offered, the period-1 worker with a social tie conveys the offer to her period-2 acquaintance. Period-2 workers compare wage offers received and, when refusing referral offers, find employment through the market.

In equilibrium, "a firm makes a referral offer if and only if it employs a high-ability worker in period 1". Therefore, most workers receiving referral offers are high-ability (because high-ability old workers are connected with high-ability young workers with probability greater than ½).

Now, given free entry of firms and the lack of information on workers' quality, firms hiring through the market make expected zero profit when firms using referrals earn expected positive profit in the second period.[5] For this line of research, quality of the applicants is what matters for the firm.

However, other papers tend to emphasize the potential productivity mismatch in referral hiring (Bentolila, Michelacci, Suarez, 2010). In this case, referral hiring should be associated with lower wages for those hired through referrals than for those hired through normal channels.

Holding a job with the help of a network:

To summarize some predictions of the Montgomery model, workers who provide contacts should be high-ability workers, with longer tenures in the firm (allowing the firm to know workers' ability). According to this model, workers who are hired through referrals should be high-ability too, should be better compensated, and should also stay longer periods of time in the hiring firm.

[5] Many extensions are clearly possible and some are mentioned in Montgomery (1991) or in Casella and Hanaki (2008). Interesting for us is the possibility of the existence of two types of technologies in firms, one more ability sensitive than the other. Hence, the high-ability type is more productive in the former whereas the low-ability type is more productive with the latter. Then, referred workers will be assigned preferentially to the ability sensitive technology.

The analysis of outcomes beyond the moment of hiring for those workers, both informant and informed, within firms that use referrals is absent from this strand of the literature. However, the references mentioned above, from the development literature, show that moral hazard is a central issue resolved through delegated or peer monitoring. In our context, a similar solution can be found for three actions that are subject to moral hazard. First, the referral may have lied on the referred type (using Montgomery's perspective). Second, more classically, the referred may not select the appropriate level of effort. In this last case, the referral provides a natural peer when implementing some form of monitoring.[6] Third, if turnover is costly, the referral might be expected to induce the referred worker to stay longer periods of time.[7] In all cases, punishment of the deviating individual remains an issue that we will try to address.

2.2 Empirical models and identification strategies

Our empirical models should help us understand how networks, as measured by parents' employment in a given plant, affect the search for the first stable job of their children. In addition, we want to apportion the role of the respective characteristics of the student, of the parents, of the plant of the parents, of the children and parents fields of study and occupations, and of labor market conditions. Because we try to capture *causal* effects of parental presence at a plant, we need an empirical model that accounts for the fact that there is a (counterfactual) probability that the graduate would have ended up in her parent's plant, **even if the parent had not worked there**. We use *classmates* to construct such a counterfactual. Our empirical models should also help us understand the direct effect of "parental hiring". First, we must measure the children outcomes and compare them with those that entered a given plant with no parent around. Second, we want to capture the impact on parents if "parental hiring" was not successful. Below we present the details of our empirical models, starting with the basic model that will help us assess the existence and the magnitude of parental networks in hiring.

2.2.1 The set-up

Whether a high-school or university graduate finds her first stable job in a particular firm depends on how well her skills and social networks overlap with those needed by the firm. In order to estimate the effects of a particular network (in our case provided by the parents-children relations), we need a model which accounts for all potential sources of overlap between skills of the graduate and characteristics and needs of the firm.

Consider a set of graduates, indexed by i, each graduating from a particular class, $c(i)$. The class defines a specific location (school), a time (year of graduation) and an occupation (the specifics of the education, the field of study). Each graduate may start working in any of the plants (indexed by j) present in the economy. Using a formulation similar to Kramarz and Thesmar (2006), we analyze the following linear model for the probability that graduate i starts working in plant j:

$$(1) \qquad E_{i,c(i),j} = \beta_{c(i),j} + \gamma A_{i,j} + \varepsilon_{i,j},$$

[6] For instance, Millo and Pasini (2007) mix Arnott and Stiglitz (1991), who study the effect of the presence of non-market insurance on market insurance when moral hazard is a concern, with Vega-Redondo (2006), who looks at stability in social networks. They show that more cohesive networks allow for a better control of moral hazard.

[7] Discouraging turnover could also be modeled using Montgomery's model if the types are not low versus high quality but low versus high mobility.

where $E_{i,c(i),j}$ is an indicator variable taking the value one if individual *i* from class *c(i)*, starts working in plant *j*. $A_{i,j}$ is an indicator variable capturing whether a parent of the graduating student *i* works in plant *j*, $\beta_{c(i),j}$ is a match effect that captures the propensity that graduates from a given class may end up working in a particular plant (skills, size,…). In this model, because we control for the match specific effect just described, our parameter of interest measuring the network effect is captured by γ. For now, we assume that γ is a constant, but in the results section, we present useful extensions. Finally, the error term ε captures all other factors within a class that affects the probability that graduate *i* starts working in plant *j*. We assume that $E(\varepsilon_{i,j} | A_{i,j}, c(i) \times j) = 0$ where the product between *c(i)* and *j* captures the controls for the interaction between the class and the plant effects.

If ε and *A* are orthogonal given the class-plant fixed effects β as assumed just above, we are, in theory, able to obtain a consistent estimate of γ. The practical problem of estimating equation (1) is however non-trivial. Estimation of (1) as such would require a data set with one observation for each combination of individual and plant. As our data set contain over 600,000 graduates and over 300,000 plants per year, estimation of such a model would therefore require construction of a data set with nearly 200 billion observations.

In practice we estimate two transformations of equation (1), based on two identification strategies. The first transformation results in a *within-class model* where we compare the average hiring probabilities of linked (A=1) and unlinked (A=0) graduates by class-plant combination. The second transformation assumes that the class-plant effect (β) is constant over time which allows us to estimate a plant level *timing model*. The model compares the plant's probability of hiring workers from the (time-constant) "class type" (a school-field of study combination) before and after the graduation of a linked child (i.e. with a parent in the plant).

2.2.2 Identification using within-class comparisons

In order to transform equation (1) into an estimable model, we use a methodology invented by Kramarz and Thesmar (2006). First, we restrict the sample under study to cases where there is within plant-class variation in *A*. Hence, we exclude plant-class combinations in which no parent of the class's graduates are employed as well as classes where all parents work in the same plant. However, this is not sufficient to make the model estimable. We thus aggregate the model by computing, for each plant-class combination, the ratio of the fraction of graduates with parents in the plant who were hired

$$(2) \qquad R_{cj}^{A} \equiv \frac{\sum_{i}^{c(i),j} E_{i,c(i),j} A_{i,j}}{\sum_{i}^{c(i),j} A_{i,j}} = \beta_{c,j} + \gamma + \tilde{u}_{c,j}^{A},$$

In words, equation (2) relates the fraction of graduates from class *c* with parents in plant *j* who were hired by this particular plant to parameters of equation (1). However, because the match specific effect $\beta_{c(i),j}$ is still present in the equation, the model is still not estimable. Therefore, we now calculate the corresponding ratio for graduates from each class hired by a plant in which *none* of their parents is working. Note that because of our sample restriction, it implies that at least one student from the same class has a parent working in that same plant.

$$R_{cj}^{-A} \equiv \frac{\sum_{i}^{c(i),j} E_{i,c(i),j}(1-A_{i,j})}{\sum_{i}^{c(i),j}(1-A_{i,j})} = \beta_{c,j} + \tilde{u}_{c,j}^{-A}$$

Thus, taking the difference between the two ratios eliminates the plant-class fixed effects $\beta_{c(i),j}$:

$$(3) \qquad G_{cj} \equiv \frac{n_{cj}^{E,A}}{n_{cj}^{A}} - \frac{n_{cj}^{E,-A}}{n_{cj}^{-A}} = \gamma + u_{c,j}^{G} .$$

Note that $u_{cj}^{G} = \sum_{i}^{c(i),j} \varepsilon_{i,j} A_{i,j} \Big/ \sum_{i}^{c(i),j} A_{i,j} - \sum_{i}^{c(i),j} \varepsilon_{i,j}(1-A_{i,j}) \Big/ \sum_{i}^{c(i),j}(1-A_{i,j})$ so that $E(u_{cj}^{G}) = 0$ if the original error term is uncorrelated with *A*.

The variable *G* is computed for each plant-class combination as the fraction of those hired in the plant from the class *among those with a parent in a plant* **minus** the fraction of those hired in the plant from the same class *among those without a parent in the plant*.[8] It is worth stressing that *G* is computed as the difference between two probabilities: working in a specific plant for those with a parent in the plant and working in the same plant for those without a parent there. Conceptually this computation is very close to taking the difference in hiring probabilities between pairs in the same class where one has a parent in the plant and the other not.[9]

Estimating γ from *G* allows us to answer the question "*how much more likely is the average plant to hire a child of one of its employees than someone else from the child's class?*" Equivalently it answers the question "*how much more likely is it for a graduate of a given class to start working in a plant where her parents are employed than it is for her classmates?*". Note that both of these questions refer to the importance of existing links, i.e. the estimates are defined for graduates with employed parents and, equivalently, for plants with (parental) links to graduates.

(At least) two main objections can be raised to the above identification strategy. First, classmates may not be a valid control group. Our estimates will be biased if a worker with a parent in a plant would have had a higher probability than his classmates of working in the plant *even if the parent had not worked there*. Second, there may be "crowding-out" of classmates in their hiring probabilities. If there is competition over vacancies, when someone in a class has a parent in a specific plant, the probability of working there for classmates *without* a parent in the plant may well be reduced. Both of these possible concerns will lead us to overestimate the importance of family networks. We will return to these questions in the empirical section, discuss them extensively, and present various robustness checks within this framework to assess their importance.

[8] When estimating (3) we weight all regressions by the number of parents (from the class) in each plant in order to get representative estimates, but this weighting is not essential since it is rare that several graduates from the same class have parents in the same plant.
[9] Melissa Tartari, in her discussion of our paper, rightly suggested that looking at pairs of classmates with one parent working in a plant, when the other parent does not, suffices for estimating γ. Other transformations of the data that allows identification and estimation of γ must exist; we do not investigate them here.

2.2.3 Identification using the timing of graduation

An alternative identification strategy relies on the following idea. When a student graduates in a given year, for the plant that employs his/her parent this event constitutes a potential supply shock directed to this specific plant in this specific year. We rely on this variation to estimate a model that relates the plant's recruitments of *any* worker (resembling the child of an employee) to the timing of the child's graduation. In this case, we define the type of worker by the combination of school and field (but, obviously not the year of graduation). We then calculate for each year (going from 5 years before to 5 years after graduation) the fraction of graduates (of the type) who enter the linked plant.

Essentially, we think of the graduation year of the child as creating an idiosyncratic link between the plant where the parent works and the type of worker defined by the child's characteristics. In this alternative strategy, we ask whether this new link affects actual recruitments or not. More precisely, it measures whether firms hire a larger fraction of the available workers with a given set of characteristics at the moment of graduation of an employee's child (endowed with these characteristics), rather than before.

3 Institutional background

3.1 The Swedish educational system

The Swedish educational system is tuition-free at all levels. Children are, with few exceptions, required to start school in August during their 7th year and attend 9 years of compulsory schooling. After finishing 9th grade (during their 16th year) most students choose to start high-school. As an example, 85 % of those born 1973 graduated from high-school before the age of 20.

High school students are enrolled in one of several possible "programs". Admissions to the programs are based on the compulsory school grade point average (GPA) whenever there are more applicants than can be admitted. Programs are either "Academic" or "Vocational". Academic programs provide general education with some (broad) specialization such as "Science" or "Social Sciences" whereas Vocational programs provided specific training into occupations through programs such as the Construction worker program or the Office assistant program. Up to 1994, Academic programs could either be 2 or 3 years long (with a 4-years version for engineers) whereas vocational programs were 2-years long. All students from the academic programs but, in general not those from the short vocational programs, were eligible for university admission. Due to a reform of the vocational programs in 1994, all Swedish high school students graduating after 1994 receive a 3 years long education that qualifies for university studies. However, the transition rates from vocational programs to higher education remain very low.

3.2 The business cycle

Our period under study goes from 1988 to 2002. This includes the most turbulent period ever faced by the Swedish labor market since the 1930s. The unemployment rate which had been below 5 % since the 1960s (and was below 2 % in the late 1980s) suddenly increased to 9.5 % in the early 1990s (see Figure 1).[10] The unemployment rate remained high until the late 1990s when it started to

[10] The recession started with the adverse effects of high inflation combined with a fixed exchange rate. It was accompanied by high interest rates, a rapid fall in private spending due to a tax reform, and a collapsing real estate market. Starting in 1993 there was also a large reduction in public sector employment (see e.g. Holmlund, 2006).

decline and by the year 2001 the unemployment rate had reached 5 % again. Youth unemployment showed a similar time pattern. The 1990s also saw a rapid expansion of the proportion of the working-age population enrolled in some form of education. Upper secondary education was prolonged for students on vocational programs and the number of students in tertiary education was dramatically increased. As a result, the employment to population ratio did not recover as much as the unemployment rate after the recession, the difference being especially strong for younger workers.

4 Data and description

The paper makes use of a wide range of Swedish population-wide data sources combined in the "IFAU database". Part of the data comes from a linked employer-employee data set covering the entire Swedish economy between 1985 and 2002. In addition, the paper uses links between children and their biological parents. Furthermore, we use detailed information from graduation records stemming from different levels of schooling. These records contain information, not only on the exact type of education, but also give details on the exact school at which graduation took place. Combining these various data sources into a working data set is a complex procedure. Appendix A provides a fairly detailed overview of the procedure we used in creating our final data set.

4.1 Establishment and parental link data

4.1.1 Establishment data

The linked employer-employee part of the data set is originally based on tax records filed by firms and collected by Statistics Sweden.[11] The data contain annual information on all 16–65 year-old employees receiving remuneration from Swedish employers (both private and public) between 1985 and 2002. These annual data sets contain information on each individual's earnings received from each single employer as well as the first and last remunerated month during the year. We use these data to find each workers primary job in February each year. The job is defined by a wage and a plant.[12]

Throughout the analysis we exclude workers in the agricultural-forestry sector and children of self employed parents. These restrictions are however not essential for any of the results.

We link basic demographic characteristics to the data set. These include gender, age, level of completed education, and country of birth as well as an indicator of whether a person is self-employed or not. We calculate plant size as the number employees and construct variables capturing average wage and the fraction of employees having various characteristics within each plant. Wages are deflated by the average wage within the sample for that year to account for both inflation and real wage growth. Tenure is calculated as the number of consecutive years (since 1985 at most) that the person has worked in the same plant. We further add some generic plant characteristics such as county of the plant (there are 24 counties in Sweden), industry (38 two-digit codes and 9 one-digit codes)[13] and sector (private or public). For each two-digit industry we

[11] Statistics Sweden refers to this data base as RAMS.
[12] We refer to all establishments as "plants".
[13] Due to a change in the industry classification system in 1992 this "reduced" two-digit level is the finest level at which we can have consistent industry codes over the period.

calculate an employment based Herfindahl-index (H)[14] measuring the lack of dispersion as a distance between zero and one, where one corresponds to a situation with one dominant plant and zero corresponds to a situation with an infinite number of plants, each with an infinitesimal market share.

4.1.2 Parent-child links

The overall data set contains links between all parents and children present in the data set. The information is based on registers of legal parents, thus the links are between children and their biological parents *or* if applicable, their adoptive parents. Missing values are rare (less than 3 percent in the various samples, see table A1) mainly occur either if the parent was older than 65 already in 1985 or did not reside in Sweden at all during 1985-2002. There are also a (very) small number of “father-unknown” cases.

4.1.3 Description: Parent-establishment links in the overall data

Here we describe the pattern of parent-child joint employment that can be found in the overall establishment data. We use the information on employment that was described above and add links between parents and children as well as basic demographic characteristics. We restrict the description to parent-child pairs in which both parties are employed. Furthermore, we only include cases where the children are aged 40 or below. The first column of Table 1 shows descriptive regressions on the probability of at least one parent employed at the plant if at least one of them is employed using data for 2002. The second and third columns show regressions for the probability of having the mother and father respectively employed at the plant if the relevant parent also is employed. The last column shows regressions for having both parents in the plant if both are employed.

The results show that being male, young, low educated and living in a rural area makes it more likely that a person is working with his parents. Differences between immigrants and Swedish born are only minor although the estimate is imprecise due to the fact that too few foreign born employees have parents that are employed (in Sweden). Figure 2 shows the time pattern from 1985 onwards using the 1985 distribution of age, gender, education, immigration status and type of region as weights in order to purge the time pattern of changes in individual characteristics. We find little evidence of trends, but a clear cyclical pattern with a much higher frequency of working together during the high unemployment years (i.e. 1993-1998, see Figure 1 above).

Although this description is suggestive, it has its limitations in terms of understanding network effects. These limitations highlight why our identification strategy is useful: from these statistics we cannot know if parents work with children because they are similar (e.g. in terms of education and where they live) or because of the existence of networks. Even if we could, we would not be able to differentiate between the supply and demand sides of the market since we do not know who hired whom. Our solutions to these problems are to focus our main analysis on graduates’ first jobs and to make the within-class and timing comparisons outlined in Section 2.

[14] Calculated as the sum of squared employment shares in each plant (j) which captures the level of competition by industry (I) and year (t): $H_{I,t} = \sum_j \left(Size_j \Big/ \sum_j Size_j \right)^2$

4.2 Graduation data and first stable jobs

4.2.1 The population of interest

Our population of interest is constructed from graduation records from all three major levels of schooling in the Swedish system (see Section 3 for details on the schooling system): We use data on all individuals graduating from Compulsory schools (9 years of schooling), High Schools (11, 12 or 13 years) or Universities (15 years or more) during 1988 to 1995.

We study four different populations defined by their educational attainment:

1. *Compulsory schooling* includes individuals who completed compulsory schooling but did not complete high school.
2. *Vocational high school* includes individuals who completed a two or three year vocational high school education before age 21 without proceeding to university before finding a first stable job.
3. *Academic high school* includes individuals that completed a two, three or four year long academic high school program before age 21 and who do not proceed to university before finding a first stable job.
4. *University* includes graduates from a university (college) education that is at least 3 year long. Only those graduating before age 30 are included. This sample also includes graduates from various post high school educations within health care (if they are at least three years long) such as nursing school graduates.

4.2.2 Defining classes and classmates

Our identification strategy essentially builds on comparisons between graduates coming from the same school, graduating at the same time, and within the same field of education. We refer to the combination of school, graduation time, and field as a “class”. Even though this measure does not necessarily correspond to an exact class as such, the definition serves our purposes well since we mainly use the concept of a class to control for factors that are time, region and occupation specific (how this is done was explained in section 3 above) and we do not use the concept to capture social interactions between classmates.[15] In Appendix A we explain in detail how the class concepts are defined for each of the four different groups of graduates.

4.2.3 Other educational variables

Apart from basic demographic characteristics, data contain information on grade point average (GPA) for compulsory school graduates and the two sets of high school graduates. Each grade is set on a scale of 1 to 5 by the teacher (in some cases with the help of nation-wide tests) so that grades should have a national average of 3 and a standard deviation of 1.

We further construct two key variables describing the similarity between the education of a graduate and the education of his or her parent and the industry of the parent: First, we construct an indicator equal to 1 when the graduate and the parent share the same 1-digit field of education (irrespective of level). Second, for each type of education (field and level), but over all schools and years, we measure the fraction of graduates finding a job in each of the 38 different industries. This measure of average education-industry flows is used to capture how relevant an industry is for a

[15] Although we do discuss robustness checks where we try to account for the possibility of such effects.

graduate with a specific education. This measure is then used to quantify how expected or unexpected is a graduate's choice of industry, given his or her education.

4.2.4 Definition of the first stable job

In order to study parental networks and their role for children's labor market insertion, we need to define what "real" or stable jobs are, in particular in contrast to those jobs held when at school (for which parents are likely to help even more). For this reason, we define a "stable job" as a job which lasts for at least 4 months during a calendar year and which produces total (annual) earnings of (at least) 3 times an average janitor's wage which we use as a proxy for a "minimum wage" (Sweden have no legislated minimum wage). As shown in the data appendix (Table A3) 53 percent of graduates satisfy these criteria the year after graduation.

Figure 3 shows the time elapsed in order to find a first stable job for the different types of educational attainment. The figure clearly highlights that there are large differences between the different samples. It is clear that it takes a substantial amount of time before Compulsory school graduates finds their first stable job, whereas University graduates in general find jobs very shortly after graduation. When analyzing the time pattern we found, unsurprisingly, that the negative labor market shocks in the early to mid 1990s coincides with an increased duration between graduation and work, in particular for the low educated (results are available on demand).

Appendix A provides descriptive statistics. Table A1 describes children and parents in the four education groups where parents' characteristics are computed conditional on employment. Table A2 describes the construction on data on jobs held by parents. Table A3 presents statistics for the creation of the graduates' first stable jobs. When estimating equation (6) we transform the data according to our empirical model and Table A4 shows descriptive statistics for these transformed data for all the variables used in the main empirical analysis.

5 Results

5.1 Using within-class variations – how important are parents?

In this section we estimate the probability that the first stable job is found at the plant of the parent using equation (3). The parental hiring effect (γ) we estimate captures the excess probability a graduate has to find her first stable job at the parent's plant after removing a specific effect capturing the interaction of the exact education, location, time of graduation (the class, to summarize), year of the first stable job, and the hiring plant since comparisons are made within the combination of class, year of first job, and plant. Self employed parents and parents in the agricultural-forestry sector are excluded throughout.

Table 2 presents the estimation results. We present estimates of γ for mothers and fathers separately, respectively in the first panel and in the second panel. Each column presents separate estimates for the four education groups. Finally, for each panel, we present estimation results for children of both sexes jointly, as well as for male and female children separately.

All estimates are strongly positive and significant. Hence, graduating students are much more likely than their classmates to go in the plant where one of their parents is employed. The effect is particularly strong for the low-education group. As an example, the estimates for graduates who enter the labor market without any post-compulsory school education suggest that the probability of working in a specific plant is increased by 8 percentage points by the mere fact that the mother works there. The corresponding estimate for the plant where the father works is 10 percentage points. The effect is also quite large for students graduating from Vocational or Academic high-schools. It is much lower though for students graduating from the university (at the undergraduate or at the graduate level). And, strikingly, fathers tend to hire their sons when mothers tend to hire their daughters, even though the latter happens with a lower intensity.

Table B1 presents similar results for each year after graduation. Hence, the first column shows results in the graduation year. Then, results for one year, two years, or more, after graduation are given in the next columns. Again results are presented for mothers and fathers separately as well as by education group. It is important to remember that each child is present only once in the analysis. Hence, for example, estimates shown in column "*t=1*" are obtained for those children who find a job one year after graduation. The comparison group is made up of classmates who find a job after the same number of years. Results show that the effect is stronger just after graduation for most groups (see in particular those graduating from compulsory schools). It is slowly decaying afterwards, never disappearing even after seven years. However, clear exceptions are children graduating from vocational high-school, who have roughly the same likelihood of finding their first job in a plant where their father works just after graduation or three years after graduation. In addition, and not surprisingly, the number of children who find a job in more than 2 or 3 years after graduation is small for all groups but the low-education (with only compulsory schooling).

5.2 Robustness checks

We have performed a variety of robustness checks, in particular in order to examine how results are affected by some of our initial modeling choices. All the detailed results described in this subsection are either presented in Table 3, in Appendix B, or are available upon request.

First, we performed sensitivity tests in order to assess the quality of our main identifying assumption that classmates are a valid control group. The consistency of the estimates relies on the assumption that there is no unobserved factor which makes a child more likely to work in the same plant as her father or mother (in comparison with other students in her class) other than the parent working there. Such a factor could be an unobserved taste for that particular plant. This is indeed difficult to test. As an attempt to falsify the assumption we did three different robustness checks: We partitioned each class by the industry in which their parents worked so that we only compared one graduate to other graduates with *parents employed in similar (same industry), but not identical, plants*. The results are essentially similar to, albeit a little smaller than, the results presented above (Table 3). We then performed the same analysis by partitioning the class according to the industry the graduate ends up in and again the results (Table 3) are very similar to the ones presented in Table 2. This shows that graduates end up in their parents' plants more often than other graduates from the same class *who start working in similar (same industry), but not identical, plants*. Third, the taste for a particular plant may reveal a common taste or skill shared by the parent and the child, denoted α_i. Its presence would bias our above estimates since it would be correlated with $A_{i,j}$ and included in the residual $\varepsilon_{i,j}$ without any way of controlling for it. In particular, our within-class-plant transformation which leads to equation (3) does not eliminate it as soon as some students in the class share such a taste with their parent when others do not. Hence, one strategy is to restrict attention to those students that are most likely to share this taste. Therefore, we re-estimated equation (3) but we only included graduates who had the *exact same education as their parent*. [16]

[16] We thank Raquel Fernandez and Daron Acemoglu for suggesting this procedure.

We perform this test for three levels of aggregation of education categories, 1-digit, 2-digits, and 3-digits. Results are presented in the Appendix Table B2. Again, we find extremely similar results, with slightly larger estimated effects, and a little less precise when we use 3-digits education categories.[17] Because we only have children who share some α_i component with their parents, the quasi-differencing procedure embedded in equation (3) should eliminate the bias. Overall, these results suggest that the estimated referral effect is not strongly sensitive to diverging preferences over types of firms within a class. The idea of restricting attention to children with the exact same education as their parent tries to capture the idea that, within occupation or education groups, referral effects are present. Doing this shuts down the supply side explanation that children are better informed of the characteristics of the job and the nature of the occupation thanks to their parents: here, all children in a class with the exact same education as their parents are on *equal* footing. However, the data sources do not contain the exact occupation of the parents but their exact education. Therefore, we did the following robustness check where we tried to measure the effects within groups of parents with similar positions/occupations. We divided each classroom by the 2-digit industry and the *within-firm* wage quartile of the parents' firm and re-estimated our equation. The results, presented in Table 3 (first panel, third column), show absolutely no change in the estimated effects. Hence, when we compare the role of parents with similar positions in similar firms, referral effects are again present. And demand factors seem to be driving the effect.

Second, our identification rests on the assumption that classmates provide a valid control group for each graduate. But, if vacancies are rationed, it is possible that a worker who gets hired by a parent "takes" a vacancy away from the classmates. If this happens our estimates will be upward biased. To see this, let us rewrite equation (1) with the possibility that classmates are potential competitors for the same job:

$$E_{i,c(i),j} = \beta_{c(i),j} + \gamma A_{i,j} + \tilde{\gamma} A_{-i,j} + \varepsilon_{i,j}$$

where $A_{-i,j}$ denotes parental employment for all other children in the class in this plant *j*. Then, taking first difference, between two classmates, *i* and *i'*, yields

$$E_{i,c(i),j} - E_{i',c(i'),j} = \gamma(A_{i,j} - A_{i',j}) + \tilde{\gamma}(A_{-i,j} - A_{-i',j}) + \varepsilon_{i,j} - \varepsilon_{i',j}$$

Assuming that only two pupils belong to the class, with *i* having a parent in *j*, whereas *i'* does not, then the two variables, $(A_{i,j} - A_{i',j})$ and $(A_{-i,j} - A_{-i',j})$ are negatively correlated, with correlation -1. Hence, our estimation would yield $\gamma - \tilde{\gamma}$ rather than γ. Given that $\tilde{\gamma}$ is likely to be negative because of this crowding-out effect due to limited vacancies, then our estimate would be biased upwards.

However, this effect is likely to be small. Indeed, as seen above, the parental hiring effect is sizeable but not huge, and the "crowding out" of classmates employment probabilities should be shared by all the classmates. Hence, the effect per classmate should therefore be very small. We have nevertheless performed three sets of robustness checks to see if this conjecture holds. First, we have estimated a separate effect in the (few) cases when there is more than one parent from a particular class in a given plant. The effect (unreported, but available upon request) is very similar to our main estimates suggesting that different graduates with parents in the same plant do not decrease each others' probabilities of being hired. Second, we have estimated the model separately by total numbers of hires (1, 2-5, 6-10, 11 or more) made by the plant in the relevant year. Results are shown in Table 3, second panel. Here, if there is "crowding out", the effect should be strong for plants that only hire a unique person – whereas crowding out should be less of a problem if many new employees are hired. The estimates for plants that hire a unique worker are slightly larger than for those hiring 2 to 5 or 6 to 10 workers, but are essentially similar to the estimates for those plants

[17] Using a 3-digits match reduces the sample considerably so in this specification we did not condition on the school, but instead used graduates from the same education, the same *municipality* (except university sample), and the same year.

hiring more than 11 workers. In addition, we re-estimated the model of equation (3) relaxing the definition of the comparison group, using either graduates from other years, or graduates from other schools in the same municipality (except for university) but in the same year. The (unreported, again available) results were essentially unchanged.

Third, the estimates we present are based on plant-level data. It is however possible that some parents not only help their children to enter their own plant but also other plants within the same firm. This could be particularly true for the highly educated (for instance, someone trained in law might not find an appropriate job in the local plant where her parents work but in the main office). In order to study the effects at the firm level, we need to restrict the analysis to the private sector. Looking at private plants increases the estimates quite a lot because the use of referrals is much more limited in the public sector (as will be shown in the following section). The correction is especially important for mothers who more often than fathers work in the public sector.[18] Now, changing our unit of analysis (from plant to firm), given that we only examine the private sector, leaves the estimates essentially unchanged (Table 3, last panel). The difference in estimates between the plant and the firm specifications is less than 1 percent (less than 0.5 percent for the university sample), suggesting that parental hiring is mostly performed at the plant, rather than at the firm. This is true for all educational levels.

Fourth, we changed the definition of the *timing* to the first job.[19] Our baseline specification compares all those within a class who find a job within the same year, in order to be sure that our results are not driven by time effects or differences in overall hiring probabilities. Changing the definition and extending the comparison group to involve all graduates who find a first job at some point during the 7 years following graduation, rather than using only those finding a job in the same year, does not alter our results (unreported, available upon request).

Fifth, we have looked at the probability of being hired in a plant where the parent *used to* work rather than where the parent is currently working. We took all the cases where the parent was employed by a different plant three years before the year under consideration, the plant still existed, and the plant hired at least *someone* (not necessarily a graduate). Results are shown in Table 3, third panel - columns 3 and 4 - for different conditioning sets (results by education are not reported but are available upon request). We find some evidence that some parental referral effect remains after the parent has left the plant. However, the magnitudes of these effects are, in all cases but one (unreported, again available), considerably smaller than the effects when the parent is still present. The one exception is the university sample. In this case the effect is nearly as large when the parent has left the plant as when the parent was present.

Sixth, we have looked at various sub-samples, dividing the data according to various specificities of the parents' educational fields and industries. We find in particular that parents in fields (narrowly defined at a three digit level) which have become obsolete (defined as having more than twice as many parents than children) do not, on average, help children more or less than parents in fields that are still expanding (unreported but available).

Seventh, we looked at siblings (i.e. brothers and sisters) and found that the presence of a sibling at a firm hiring one of our graduates is low, around 3.3% in the first job (for parents, the proportion is

[18] We show below that the there are no gender differences in the use of referrals if one accounts for the characteristics of the plant of the parent.

[19] In fact, we performed an even more basic robustness check before this one. We randomly allocated parents and children within a class and re-estimated our model. All coefficients in the specifications presented in Table 2 were equal to zero.

6.7%, Table A4, first row). We will return to the impact of siblings in our analysis of later outcomes.

5.2.1 Do employee-graduate links affect plant-level hiring? The timing model

In order to further address concerns about crowding-out or spill-over effects between students in the same class we also analyze the "timing model" described in Section 2 which relies on a different form of sample variation and a different identification strategy. This alternative model analyzes plants' recruitments before and after graduation of a child linked to an employee (a father or a mother) at the plant. We use data on all graduates from the same school and type of education as that of an employee's child, and look at recruitments before and after the graduation year of the child.[20] Identification here relies on within-plant variation over time for graduates of a given school and field across the years (by contrast with the time varying-definition of a class used in the previous model). Figure 4 shows the probability of hiring a graduate of the same type as the graduating child over time before and after the graduation year. Results presented in Table 4 give the precise numbers and standard errors. This hiring probability is low and stable before the graduation of the linked child, but then increases dramatically at graduation and subsequently declines. Indeed, a gradual decline after the graduation year is what we should expect since not all graduates find their first job immediately. As shown in Figures B1 and B2 in Appendix B, the rate of decline is rapid for university graduates (who find jobs fast, as indicated by Figure 2 above) and slow for compulsory school graduates (who find jobs slowly).

The models we estimate here include the full effect of the link, which include any within-class spill-over or crowding-out effects. But the strategy may be affected by inter-temporal substitution if plants postpone recruitments until a linked worker's child graduates. However, the fact that there is no visible (or statistically significant) decline over time before graduation suggests that inter-temporal crowding out is not an important phenomenon unless plants are willing to postpone recruitments a full five years until the linked child graduates.

In order to find the full effect on plant level recruitments, we use the pre-graduation years as a baseline and sum the effects found in the post-graduation years. The summation of the estimates suggests that the firm hire 4.6 percent of graduates as a result of the father-links and 3 percent of graduates as a result of the mother-links (Table 4). These estimates do not change if we allow for pre-graduation trends.

An interesting follow-up question is whether links induce plants to hire more workers overall, or if they mainly redirect their hiring intentions. In order to analyze this question, we focus on small plants (defined as having on average less than 16 employees during the sample period) where the shocks to total networks are likely to be most pronounced. Here we aggregate the data to the plant level and look at the number of links to graduates the plant has and relate this to the total number of recruitments of graduates (in their first jobs, but from any level of schooling). As above we rely on timing at graduation for identification. We separately estimate the number of recruitments of the linked type and the number of recruitments of graduates overall. Consistent with the overall finding, the evidence suggests a positive post-graduation effect on the propensity to hire workers of the linked type. Moreover, we also find an effect on overall recruitments of graduates, which suggests

[20] For reasons of computational convenience we exclude the few cases where there are multiple years during our sample period where links are created between the same type of class and plant.

that stronger networks to graduating students induce (at least small) plants to hire more graduates.[21] Indeed, the estimated constants suggest that the average plant hires about 10 times as many graduates from other types of schooling in the pre-graduation years whereas the effects of graduation on the number of recruitments are of nearly identical size in both specifications which suggests that parental links increase the hiring probabilities of the linked children without reducing the hiring probabilities of other graduates.

Overall, the evidence presented in this section supports the results of a significant impact of the parent-child links on the child's probability of being hired by the plant of the parent. The results further suggest that (small) plants hire more graduates overall in the years when the children of employees graduate. Thus, the network effect does not appear to be the result of a reshuffling of vacancies between different graduates entering the labor market at the same time, or between similar graduates over time, but rather the result of new vacancies being opened (or made available to inexperienced workers at least).

5.3 Heterogeneous effects in the within-class model––when do parents matter?

Estimation of equation (3) answers the question of how important parental contacts are on average, but does not provide any insights into when and for whom these contacts matter the most. We therefore expand our original model (equation 1) so as to incorporate effects that may vary with characteristics of the graduate (i), the parent (p), the plant (j), or the labor market (l). This yields the following model:

$$(4) \qquad E_{i,j} = \beta_{c(i),j} + [\gamma^i X_i + \gamma^p X_{p(i)} + \gamma^l X_{l(j,t)} + \gamma^j]A_{i,j} + \varepsilon_{i,j} \,,$$

where we have included observed characteristics (X) of graduates and parents as well as time-varying labor market conditions. We also allow for each plant to have a unique propensity to hire graduates with parents in the plant by incorporation of a plant fixed effect γ^j .

Since all terms added to the framework of equation (1) are interacted with the presence of a parent in the plant we may proceed as above and get an expanded regression framework equivalent to equation (3) that writes:

$$(5) \qquad G_{cj} = \gamma_0 + \gamma^i \overline{X}_i^A + \gamma^p \overline{X}_{p(i)}^A + \gamma^l \overline{X}_{l(j,t)}^A + \gamma^j + u_{c,j}^G$$

where a 'bar' and superscript A denotes the average within class/plant for those with a parent in the plant. Consequently, $\overline{X}_i^A$ is the average of the individual characteristics among graduates from a given class with a parent in that plant.

All terms in equation (5) come from the interaction between a parental contact and the measured characteristics, but the underlying model is the same as previously. Thus, estimating the model answers the question: *when, where, and for whom do parent-child networks matter at entry in the children' first stable job after graduating from school*?

[21] Note that we cannot analyze the propensity to hire workers overall since the sampling of parents essentially means drawing "random" workers within plants, a strategy which over-samples plants in years when they have many employees. This generates a spurious hump shape in plant size peaking in the sampling year (i.e. the year of graduation).

We now present results of our analysis of heterogeneous parental hiring effects based on equation (5). All estimates therefore show *when and for whom* those effects are stronger. We start with the latter.

For whom do parents matter? : Table 5a presents estimates for various individual characteristics of the child. Because estimates turn out to differ very rarely between tracks, we only report the pooled estimates. More important though, we present estimates with and without plant fixed effects. The estimates from models with plant fixed effects compare cases where graduates from different classes have parents in the same plant (possibly in different years), to see which graduates are more likely to be hired *conditional* on the plant the parent works at. This accounts for the possibility that plants have different propensities to hire children of their employees. Thus, when plant effects are included, identification comes from plants where more than one parent worked at some point of the analysis period. Note that the 850,000 contacts are distributed over almost 200,000 plants in the data so that each plant has on average 4 to 5 parents of graduates over the 8 years we study. Clearly, we still have a fairly representative sample also in this case.

Results confirm that females benefit less from their parents. We see only small differences between immigrants and natives. Perhaps surprisingly, age at graduation has a negative impact, even controlling for plant fixed effects, i.e. within a class younger children benefit from their parents' employment more than older ones, when entering their first job. In addition, good grades (a high GPA) do not help entering one's parents' plant, on the contrary: parents may protect weak children; or, by reverse causation, children anticipating that their parents will help them in finding a job, do not work as hard as their classmates and therefore receive low grades. We have re-estimated the model replacing grade by the position of the child in the within-class grade distribution yielding similar results. We have also looked at *siblings*, showing that a given parent is more likely *to hire the child with the weaker grades*.[22] In accordance with the results presented earlier, we find that paternal links are more important than maternal links. Importantly however, we see that most of the differences between mothers and fathers disappear when introducing plant fixed effects. Hence, within a plant, parents' sex (on the demand side) does not play a role. Mothers simply work in plants which resort less to parental links. Apparently, part of the initial difference comes from mothers working more often in the public sector, where referral hiring is used much less intensively (see below).

Who are the parents who matter? : Table 5a also displays the impact of parental characteristics, separately for mothers and fathers. Let us stress again the specificity of this analysis. We study which characteristics of the incumbent workers affect the probability that the firm will hire one of the incumbent's children, holding the characteristics of the child constant. First, whereas it is well known that low educated workers use informal contacts more often (as is also true here) we see that incumbents with a lower education have a higher probability of using their network (for a given education of the child) *within plant*.

We also study the parent's wage, tenure at the plant, and a measure of coincidence in the field of study between the parent and the child.[23] Estimation results yield strong support to some elements of Montgomery's model (but not to all, see just below) in the sense that well-attached workers appear to be more important: children of high-wage and high-tenure workers, even controlling for

[22] All results not shown here are available from the authors.

[23] Since tenure only cannot be measured before 1985 it is not a perfect measure, especially so for the earlier cohorts. Hence the estimates may be biased downwards but since all comparisons are made within cohorts there is no reason to believe that measurement errors should be correlated with our outcomes.

plant fixed effects, are more likely to be hired. Interestingly however, we also find, in apparent contradiction with Montgomery's model, that the interaction between parents' wage and the grades of the child is negative: parents who are paid high wages are more likely to hire children with relatively poor grades (detailed results available upon request). Indeed, his model tells us that referrals help firms to hire high-quality applicants. However, if parents do know something unobserved by the firm (the real productive quality of their child as opposed to scholarly grades), this result is still interpretable using this model.

Furthermore, in agreement with results shown above, parents who share (broad) field of study with their children are more likely to be working in the same plant as their children. Thus, family links are more important when skills also overlap. Consistent with this, a given parent with multiple children is also more likely to hire the child endowed with the same field of education as his/her (detailed results available upon request). Hence, within a class of students graduating in the same field of study, those who have parents trained in the same broad field, or parents who are high-wage and high tenure, are more likely to work in the same plant as their parents. Note that all of our displayed estimates exclude self-employed parents but when including them we find that children of self-employed parents (not surprisingly) more often follow their parents than children of other employees in the same plants.

In which types of plants do parents who matter work? : Table 5b presents the ensuing results of the same regressions focusing on regional characteristics, unemployment, and market conditions. In particular a fragmentation of the market as measured by the Herfindahl index in the industry is associated with more referral hiring in the cross-section however, when plant fixed effects are introduced the result is reversed. Thus, referral hiring is used more intensely within plants when fragmentation in the industry is reduced (i.e. when the Herfindahl index in the plant industry increases over time). Furthermore, high unemployment seems to favor matching of parents and children within plants.

Networks are used more commonly when the industry of the parent is a more logical destination (hence more "relevant") for the typical graduate with the type of education that the child has (see section 4.2.3 regarding the definition of this education-to-industry variable). It therefore suggests that networks are used mostly when workers with types of education that fits the plants' typical needs are hired. Results on the interaction between the industry-field match and unemployment show that this pattern is strongly reduced when unemployment is high. Thus, when unemployment is high, networks are used more often. And, hired children have an education which is less likely to be adapted to plants' needs.

Finally, Table 5b also shows results for plant characteristics. First, referral hiring takes place mostly in large (or in very small) plants, in manufacturing industries, in the private sector (consistent with Table 3), and in firms with a large fraction of immigrants (consistent with patterns of workplace segregation found in Åslund and Skans, 2009). Employment growth also favors referrals.[24]

Parental networks, occupations, and the role of information: To get a better sense of the causal role of parents and the underlying mechanisms, in particular the role of information, we analyze the characteristics of the occupations/educations that are most (or least) frequently obtained through parents. Because the education categories (with closely related associated occupations) are very

[24] Not surprisingly, many estimates are imprecise when including the plant fixed effects since many of the associated variables barely change at the plant level. Interestingly, however, we see that the private sector indicator is significant, even in this specification, indicating that privatized plants tend to increase their use of referrals.

well defined for the vocational high school sample (examples are masons, restaurants, telecommunications, secretary…), we restrict attention in this paragraph to the vocational high-school sample. We start by estimating a network effect for each combination of municipality and occupation (more precisely, the detailed education received in vocational high school).[25] The resulting estimates are used as endogenous variables in a second stage where we try to explain the relative magnitude by educational and regional characteristics.

We use two different strategies to analyze the role of information. First, to measure how diverse the receiving market for each occupation-education is within the municipality, we compute the number of plants that employ at least one worker with such an occupation-education (using the full stock of employees in the 1995 data). We also compute the number of workers, in each municipality having each type of education (again, in the year 1995). Second, in order to calculate to what extent the type of education is dominating the employing workplaces (most masons work with other masons, whereas electricians have few electricians among their coworkers). We thus calculate (again, full stock of employees in 1995) the average (by type of education) fraction of coworkers having the exact same type of education as the employee. This index corresponds to “exposure” as a measure of segregation. We also calculate the fraction of others with the same education in the municipality overall as a baseline to be included as a covariate in the second stage models.

We contrast two sets of specifications: one with number of plants and number of workers as the main explanatory variables, a second with the exposure measure as the main explanatory variable. For each specification, we estimate models with or without municipality effects, with or without a control for the number of workers or the baseline, with or without weights (number of employed parents). Results are presented in Tables 6a and 6b, respectively. The results are robust to the various controls and the message clear. First (Table 6a), referral hiring through parents is used for occupations that are widespread in a municipality (used in a large number of plants). Put differently, the less *specific* the type of education is in terms of which plant hires the workers, the more prevalent is the use of networks. Second (Table 6b), when occupations (types of education) are more concentrated in the firms, networks are used less intensively.

The two sets of results give the same picture, when it is predictable into which firm graduates will start working, and when the type of education dominates its workplace, networks are used less frequently. An interpretation in terms of information seems reasonable: firms need referrals when many applicants arrive for jobs that are less specific to that firm (for which relations with (vocational) high schools are less likely to have been developed). Furthermore, students also need parents help when there are many firms that are susceptible of employing them. The parents’ employer is a natural focal point in this coordination problem. Conversely, graduates with a more specific education, (e.g. a carpenter) can easily identify firms that employ them put more general types of education (electricians, manufacturing workers…) might face more potential employers and therefore need networks to a greater extent.

5.4 Do parents provide good or bad jobs?

In this subsection we provide evidence on the quality and content of the jobs provided through parental networks. We do this by studying three outcomes measured at the time of the first job: time to the first job, initial wage, relevance of the industry relative to the education of the graduate. We then proceed to more “long-run” outcomes, measured three years after the first job was found. In this case we restrict the sample to those finding a first job within four years from graduation. These

[25] Resulting estimates are available from the authors.

outcomes are the probability of being employed in the same plant three years after entry, the probability of working in the *same* plant 3 years after entry, as well as wage growth during the three years after entry. Of course, we can only measure wage growth for those who are still employed in some firm three years after entering their first job. We present results for two models, one which includes fixed effects for each combination of class and time to first job, and one which controls for educational characteristics as well as a plant fixed effects (we have estimated a model with class fixed effects and plant characteristics, giving very similar results to the one with plant fixed effects only). Interpretations of these two models are slightly different. The first model looks at the relationship between finding a first job at a parent's plant and our outcomes of interest. In this specification, the estimate may well include effects due to unobserved plant characteristics. The second model, because it includes plant fixed effects, allows estimates to be measured in difference from other graduates finding their first job in the same plant, but through channels other than parents referral hiring. The model thereby isolates the effect of getting a job through a parent, within a given plant. All models control for grades, gender, and immigration status.

Results presented in Table 7 first demonstrate that workers who find a job where their parents work, find this job faster than classmates[26] and also faster than others who start working in the same plant, after accounting for educational characteristics. Second, starting wages are lower for those who get their first job at their mothers' plant, but starting wage are not much different from classmates if children find their first job through their fathers. However, when controlling for the plant (observed or unobserved) characteristics at which the first job was found, wages are always lower than for jobs found through other channels, irrespective of the parent who helped find the job (note that we obtain similar results when we estimate the model within class and add observed plant characteristics). Children following their parents receive a low (within-plant) wage, but fathers provide access to high-wage plants. Third, graduates, getting their first jobs through their parents, find these jobs in less "relevant" industries than those their classmates find. Therefore, they enter industries in which individuals endowed with their type of education most generally do not find their first jobs. This result also holds within plant.

In the second panel of Table7, we look at outcomes three years after finding their first job. Estimates show a very strong positive effect of being referred by parents on the probability of staying in their first plant for at least three more years. This effect remains strong and significant, albeit roughly halved, when including plant fixed effects, suggesting a) that parents provide jobs in plants where the expected tenure is long, and b) provide jobs with longer tenure within each plant. Parents' referrals are also associated with slightly increased overall employment probabilities three years later. Finally, the estimated effects on wage growth display a pattern similar to the one observed for starting wages. Mothers and fathers both provide jobs with average wage growth. After controlling for plant fixed effects, wage growth for workers entering with the help of their fathers looks just slightly higher than that observed for other entry channels whereas the estimates for jobs obtained through mothers remain unchanged.[27]

[26] Obviously we do not control for time to first job in these regressions as we do in the rest of Table 9.

[27] In order to understand the potential consequences on the parent of a child's "misbehavior", we looked at the impact of a rapid child's exit from the firm on parents' employment and wage growth. Could we find evidence of some form of "punishment"? We estimated a model using all employees of those plants where at least one child was hired, comparing how outcomes of the parents of the children leaving the plant (within 1,2, or 3 years) and parents of children staying (at least 4 years) differed from those of other employees within the same plants. We found that parents of children who leave rapidly, have the same propensity to remain in the plant than other employees in the same plant. However, parents of children who stay at least three years after being hired do have a much higher tendency to stay themselves (again relative to others in the same plant). We find little evidence of wage effects for parents with children staying short periods versus longer periods (all these results are available from the authors).

Table 7 also includes an indicator for having a sibling (brother or sister) in the same plant, despite the low occurrence of such co-presence noted before (3 percent). All the estimated effects for siblings are slightly more favorable than those estimated for parents (effects of parents in models without the sibling dummies are nearly identical and available upon request). Thus, the signal on the newly hired obtained by having a sibling in the plant appears to be more positive, from the firm's standpoint, than the signal obtained through parents.

6 Conclusion

In this article, we have examined the impact of parental networks on their children labor market outcomes, as seen from the perspective of the first stable job after graduation from school. We have presented a set of empirical models that facilitate the estimation of the magnitudes, sources and effects of family based networks. For estimation, we used a unique data set constructed from various administrative data sources linking information on parents and children, giving the plant identifier of both parents and children, and identifiers of all classmates of all children graduating from any level of regular schooling in Sweden over a 7-year period.

On many aspects, results are very much in line with Montgomery (1991) type models. We show that having your first stable job in the same plant as, at least, one of your parents is quite frequent. Or, conversely, a plant is more likely to hire one of his employees' children than someone else from the same class. This effect is particularly strong for relatively low-educated children who performed poorly at school. In this process, the father is central for sons when the mother is useful for daughters and children trained in the same field of study as their father or their mother are more likely to benefit from referral hiring (or equivalently, a plant is even more likely to hire one of his employees' child than some other kid in the child's class when the child and the parent share the same field of study). The firm side is important, fathers matter more than mothers overall, but not if accounting for plant fixed effects.

Particularly telling is the fact that the occupation-education categories for which parents appear to matter the most are the ones that are least specific, and where destinations are harder to predict: not masons or cooks but education for manufacturing, for secretarial work, or for sales or administrative jobs. Hence, children in jobs that are less well-defined, used in many different industries or plants, appear to be helped by their parents in order to locate, and get, their first jobs.

However, not all parents matter. Those that do are high-wage workers and have relatively long tenures at the plant, even controlling for plant fixed effects. However, on the children side, some elements tend to show that being hired by one's parents is not necessarily an unconstrained choice. First, these children' grades at school tend to be lower than those of their classmates. Second, parental hiring tends to take place when unemployment is high. Finally, the initial wage paid to the child is lower than for equivalent persons entering the plant through other channels, again controlling for plant fixed effects.

However, this is compensated in the mid-term; these children spend longer spells in their first job than hires without a parent in the plant. Firms thus appear to benefit from parental hiring, not by selecting better applicants, as suggested in Montgomery (1991), but by keeping these young hires for longer periods of time.

The identification strategies induced by our two empirical models capture the differential supply shocks that affect the different potential employing firms leaving firms' labor demand unaffected.

Indeed, by comparing children from the same classroom with the same education potentially shared with that of their parents, we capture the differential connections between children and firms. Furthermore, by showing how the timing of graduation of children of their employees directly affect their hiring behavior; we see that firms adapt their recruitment patterns to directed supply shocks. Overall, results suggest that (some) children find their first job *with a little help from their… parents and for the benefit…of their firms*.

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Appendix A: Data

A1 The establishment data

By dividing total remuneration by the number of months between the first and the last entry, we get a measure of monthly wages received from each employer. We use this measure of wages to define employment in a procedure which closely resembles how Statistics Sweden calculates employment from these data. We define a person as being employed if an employment spell a) covers February b) generates at least 50 % of a minimum monthly wage[28] c) for individuals having several jobs satisfying these criteria during one year, we only keep the job generating the highest income.

There are two main differences with Statistics Sweden's procedure. First, we study employment in February rather than November. We select this month in order to characterize where parents work at the *beginning* of each year. Second, we use a slightly higher wage threshold in order to minimize measurement errors in wages for employees working very few hours.[29]

The procedure provides us with a data set containing one February job per worker and year. The job is defined by a wage and a plant[30] and the plant can be linked to various characteristics such as industry and location. In some cases (5-6 %) an employee's job cannot be located at a specific plant, mostly because plants are defined by physical addresses and some jobs do not take place at a specified address. Examples of such jobs include home care, some construction workers, some sales persons, security personnel and workers lacking "normal" contracts such as artists, board members, and people mostly working at home. We consider the establishment information for these individuals as missing.

Throughout the analysis we use administrative identifiers to define physical establishments. However, the administrative numbers may change over time if there is a change in ownership or industry affiliation. Since part of the analysis builds on following plants over time we correct for this by linking plants with different identifiers but (almost) the same set of employees in order to minimize the impact of such changes. A plant with code "A" in year 1 is considered to be the same as a plant with code "B" in year 2 if a) more than 50 % of employees in plant A in year 1 works in plant B in year 2 and b) more than 50 % of those at plant B in year 2 worked at plant A in year 1 and c) at least 3 people worked in both plant A in year 1 and in plant B in year 2.[31] When such correspondences are found we change all the numbers in the data set back in time in order to get consistent data series.

A2 Defining classes and classmates

In order to construct the classes we use the most detailed level of the Swedish standardized educational codes ("sun-2000").[32] The field codes are provided with a four digit "hierarchical" structure, so that fields can be described at different levels of precision.[33] Since the same field of specialization can be provided at different levels, such as two or three year-long high-school training in construction work or bachelors/master degrees in economics, we always interact the field codes with the level codes in order to get our definition of a class (so that e.g. bachelor and masters degree graduates are coded differently).

[28] Defined as the wage paid to janitors that are employed by municipalities.
[29] For papers using similar strategies see e.g. Skans, Edin, Holmlund (2009) and Åslund and Skans (2009).
[30] We refer to all establishments as "plants".
[31] We relax c) when the set of workers is identical between the two years in the two plants.
[32] We transform codes from the old system to sun 2000 by means of a matrix provided by Statistics Sweden.
[33] The fourth digit is actually a letter, in order to provide a higher level of detail when needed.

As we show below, the class concepts differ slightly between the four different groups of graduates. Since the concept of a class is the basis for our identification, it is important to understand how these are constructed. Therefore, we now discuss in some detail how the classes are defined for each type of educational attainment.

For graduates from universities, we define a class by combining information on the graduation year and semester (fall or spring) and a code for the examining university or college. There are graduates from 88 different schools in the data. The field codes are quite precise; examples of specific fields are “Economics/economic history”, “Law”, “Medical Doctor, specialized in radiology”, “Nurse, specialized in geriatrics”, “Teacher in Math/Data/Science”, “Science, Chemistry”, “Civil Engineer, Chemistry”. When we interact the field and level codes we get over 300 types of university educations within our analysis sample (see Table A1 below).

In the case of high schools we proceed similarly, and obtain 106 different vocational educations and 25 academic high school educations respectively. Because these programs are fairly standardized, we have a relatively small number of academic high school educations (as the name implies, these are mainly general courses aiming at the transition into higher education). The main academic programs are divided into “Social Sciences or Humanities”, “Science”, “Economics”, and “Engineering”. The engineering program is more job-oriented than the other programs and many different specialties are provided (e.g. construction, machinery or electronics), in which case the graduates are coded according to their specialty. The engineering program also provides the opportunity to study for 4-years (coded separately).

The level of detail in the field of study is obviously much greater for vocational programs. Here, each program is directed to a specific occupation. The graduates are coded in fields such as “Construction work”, “Auto mechanics”, “Social work, child care”, “Trade and office assistants”, “Electricians, installations”, “Electricians, data, and telecommunication” ... In this case, there are also different levels since vocational programs can be either two or three years long.

Graduates from compulsory education do not belong to specific fields. Education in the compulsory schools is quite standardized even though some courses are chosen by the individuals. Compulsory school graduates may in many cases have started high school but dropped out, but we do not know what kind of training they may have received there. We however treat members of this group as unskilled, with no field of specialization. Thus a compulsory school “class” is defined as graduates from one compulsory school in a given year that either did not proceed to high school or dropped out if they did.

A3 First stable job of graduates: Sample construction

For each graduate we look for the first stable job they have after graduation. Some of the university graduates had stable jobs before starting (or less commonly, during) university but these jobs are ignored. In order to get symmetry between the graduation cohorts we only include those that find a first stable job within 7 years after graduation (remember that the last graduating cohort is 1995 and data stop in 2002).

We then look for the plant in which each of the parents was employed in February during the year when the graduate found her first stable job. When applying our empirical model, we compare graduates from the same class finding their first stable job in a given year. Therefore, we drop observations for which all graduates from a given class found their jobs in a year and all had parents working in the same plant (since in these cases there is no variation within the fixed effect). In practice, this almost exclusively means dropping graduates who were alone in their class in finding a job in a particular year.

Our data set contains graduates, identifiers of their class (and thus their "field"), their personal characteristics, as well as the year he or she found her first stable job, as well identifiers for each student's mother and father. The identifiers are then used to check whether the plant in which the graduate finds her first stable job is a plant in which any of the parents to the classmates worked at the time.

Table A1 Descriptive statistics of graduates and parents

	Comp.	Vocational	Academic	University	All
All graduates					
Female	0.435	0.421	0.524	0.602	0.491
Nordic immigrant	0.011	0.010	0.008	0.015	0.011
Other imm.	0.067	0.031	0.032	0.029	0.035
Age	16.063	18.399	19.013	25.096	19.746
Age (sd)	0.242	0.622	0.555	2.572	3.252
GPA	2.650	3.053	3.121	3.000	3.009
GPA (sd)	0.682	0.598	0.562	0.000	0.549
Mean class size	19.8	29.5	42.4	44.6	35.2
Class size (sd)	10.4	22.4	28.7	39.3	29.1
Class size by year of first job	5.2	11.1	13.2	28.4	14.8
(sd)	3.5	11.0	11.6	30.3	18.6
Number of fields	1	106	25	321	453
Father identified	0.974	0.985	0.987	0.973	0.981
Mother identified	0.995	0.998	0.998	0.983	0.994
Both identified	0.971	0.984	0.986	0.972	0.980
Father Employed	0.673	0.762	0.804	0.691	0.747
..in known plant*	0.580	0.652	0.714	0.610	0.651
Mother Employed	0.666	0.742	0.810	0.740	0.751
..in known plant*	0.590	0.651	0.742	0.681	0.675
Both Employed	0.383	0.458	0.563	0.479	0.482
Both in same Plant	0.029	0.034	0.045	0.038	0.037
N (graduates)	82,341	238,520	178,324	141,161	640,346
Employed parents with known Plant-ID, excluding agriculture and self employed					
Mother Nordic Immigrant	0.070	0.056	0.049	0.035	0.051
Mother Other Immigrant	0.063	0.050	0.067	0.171	0.084
Mother Compulsory	0.305	0.315	0.207	0.192	0.253
Mother Tertiary	0.193	0.161	0.329	0.433	0.277
Mother in same field	0.000	0.120	0.135	0.177	0.132
Mothers log Wage	9.430	9.272	9.380	9.391	9.349
Mothers log Wage (sd)	0.386	0.354	0.381	0.398	0.381
Mothers tenure	3.921	3.569	3.755	4.179	3.801
Mothers tenure (sd)	3.834	3.088	3.114	3.118	3.203
N (mothers)	48,608	155,164	132,322	96,165	432,259
Father Nordic Immigrant	0.051	0.043	0.034	0.025	0.038
Father Other Immigrant	0.092	0.095	0.116	0.264	0.136
Father Compulsory	0.400	0.422	0.254	0.220	0.327
Father Tertiary	0.156	0.126	0.298	0.404	0.239
Father in same field	0.000	0.192	0.281	0.215	0.205
Fathers log Wage	9.747	9.639	9.808	9.841	9.745
Fathers log Wage (sd)	0.418	0.373	0.432	0.481	0.429
Fathers tenure	4.883	4.286	4.239	4.516	4.388
Fathers tenure (sd)	4.315	3.312	3.269	3.161	3.406
N (fathers)	47,786	155,542	127,357	86,129	416,814
N (parents)	96,394	310,706	259,679	182,294	849,073
N (plants)	47,580	93,885	84,963	60,551	157,573

Note: Description of all graduates and employed parents with known Plant-ID:s. See Table A4 for a description of the transformed data used in heterogeneity regressions.
* Also exclude self employed parents and parents employed in the agriculture and forrestry industry.

Table A2: Creation of job data (parents)

	All 16-65 (1988)		All 16-65 (1995)		Parents (graduation year)	
	N	Fraction	N	Fraction	N	Fraction
Population (individuals)	5,334,727	1	5,607,753	1	1,265,140	1
Employment according to Statistics Sweden						
November	4,347,401	0.815	3,796,432	0.677	1,041,077	0.823
Anytime during the year	4,807,023	0.901	4,558,659	0.813	1,093,197	0.864
Data creation						
Jobs	8,149,152	1.528	6,982,150	1.245	1,817,232	1.436
Jobs with Plant-ID	6,562,635	1.230	5,880,534	1.049	1,363,349	1.078
Plants*	304,949	0.057	332,370	0.059	262,222	0.207
Individuals with jobs	4,974,115	0.932	4,696,508	0.838	1,004,948	0.794
…in February	4,588,783	0.860	4,202,953	0.749	858,312	0.678
..and earnings>cut-off	3,595,163	0.674	3,271,469	0.583	771,055	0.609
..and identified plant	3,306,485	0.620	3,058,382	0.545	718,752	0.568
..not self emp. or agriculture	3,137,681	0.588	2,900,262	0.517	681,864	0.539
Individuals with multiple jobs	53,126	0.010	42,275	0.008	158,823	0.126

Note: The "N" columns give the number of individuals, jobs or plants. The "Fraction" columns show "N" as a share of the total population (as given in first row). *Excluding self employed and agriculture/forestry.

Table A3: Creation of graduates first job data

	Time (t) after graduation			
	t=-1	t=1	t=3	t=5
Graduates with any job	0.864	0.885	0.881	0.873
Number of Jobs per graduate	1.478	1.590	1.439	1.417
Jobs at least 4 months	0.650	0.800	0.812	0.817
and 3 monthly wages	0.074	0.532	0.600	0.630
Known Plant-ID	0.067	0.479	0.551	0.590
Multiple jobs	0.002	0.029	0.032	0.040

Note: Colum for t=-1 excludes compulsory since no information is available before age 16

Table A4: Description of transformed regression data used in Table 5.

Variable	Obs	Weight	Mean	Std. Dev.	Min	Max
Hired by parent	798,356	823,516	0.067	0.245	0	1
Hired by classmates parent	798,356	823,516	0.005	0.045	0	1
Network effect	798,356	823,516	0.062	0.241	-1	1
Individual						
Female	798,356	823,516	0.492	0.495	0	1
Nordic Immigrant	798,356	823,516	0.006	0.077	0	1
Otehr Immigrant	798,356	823,516	0.022	0.144	0	1
Age at graduation	798,356	823,516	19.690	3.070	16	30
GPA	798,356	823,516	3.045	0.533	1	5
Only mother in Plant	798,356	823,516	0.495	0.496	0	1
Both parents in Plant	798,356	823,516	0.029	0.164	0	1
Compulsory	798,356	823,516	0.113	0.317	0	1
Academic HS	798,356	823,516	0.305	0.460	0	1
University	798,356	823,516	0.215	0.411	0	1
Mothers - measured relative to mean among mothers by education						
Nordic Immigrant	798,356	823,516	0.000	0.155	-0.070	0.965
Other Immigrant	798,356	823,516	0.000	0.195	-0.171	0.950
Compulsory education	798,356	823,516	0.000	0.308	-0.315	0.808
tertiary education	798,356	823,516	0.000	0.309	-0.433	0.839
Same (1d.) field as child	798,356	823,516	0.000	0.232	-0.177	0.880
Log wage	798,356	823,516	-0.002	0.282	-1.294	3.581
Tenure	798,356	823,516	-0.001	2.286	-4.179	13.431
Fathers - measured relative to mean among fathers by education						
Nordic Immigrant	798,356	823,516	0.000	0.132	-0.051	0.975
Other Immigrant	798,356	823,516	0.000	0.236	-0.264	0.908
Compulsory education	798,356	823,516	0.000	0.323	-0.422	0.780
Tertiary education	798,356	823,516	0.000	0.290	-0.404	0.874
Same (1d.) field as child	798,356	823,516	0.000	0.276	-0.281	0.808
Log wage	798,356	823,516	-0.001	0.310	-1.730	4.560
Tenure	798,356	823,516	-0.002	2.400	-4.883	12.761
Region and competition						
Metropolitan county	798,356	823,516	0.48	0.50	0	1
County Unemployment rate	798,356	823,516	0.05	0.03	0.008	0.128
Industry field match	798,356	823,516	0.08	0.16	0	1
Herfindahl	788,128	812,856	0.00	0.01	0.000	0.077
Plant						
Private	798,356	823,516	0.486	0.500	0	1
New Plant	798,356	823,516	0.031	0.174	0	1
Plant growing	798,356	823,516	0.429	0.495	0	1
Size 1-15	798,356	823,516	0.257	0.437	0	1
Size 46-125	798,356	823,516	0.207	0.405	0	1
Size 126-750	798,356	823,516	0.206	0.405	0	1
Size 750+	798,356	823,516	0.121	0.326	0	1
Plant mean age of employees	798,245	823,405	42.679	4.591	21	65
Plant share of primary ed.	798,245	823,405	0.239	0.212	0	1
Plant share of tertiary ed.	798,245	823,405	0.288	0.272	0	1
Plant share of immigrants	798,245	823,405	0.104	0.116	0	1
Plant average log wage	798,245	823,405	9.900	0.245	8.9	13.1
Manufacturing	798,356	823,516	0.215	0.411	0	1
Construction	798,356	823,516	0.056	0.229	0	1
Wholesale or retail	798,356	823,516	0.182	0.386	0	1
Financial, corporate services	798,356	823,516	0.097	0.296	0	1
Education, R&D	798,356	823,516	0.104	0.305	0	1
Health, Social work	798,356	823,516	0.217	0.413	0	1
Personal, Cultural, Sanitation	798,356	823,516	0.047	0.211	0	1
Public administration	798,356	823,516	0.072	0.258	0	1

Note: Weights are according to the number of graduates with parents in each plant.

Appendix B: Tables

Table B1: Parental Networks and the time to first job, within-class estimates

	$t=0$	$t=1$	$t=2$	$t=3$	$t=4$	$t=5$	$t=6$	$t=7$
Fathers								
Compulsory								
γ	0.291**	0.152**	0.103**	0.112**	0.099**	0.078**	0.058**	0.040**
(s.e.)	(0.013)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
N	1,299	7,603	6,414	8,166	8,296	7,005	4,913	3,178
Vocational								
γ	0.090**	0.076**	0.090**	0.082**	0.061**	0.053**	0.038**	0.032**
(s.e.)	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)	(0.004)	(0.005)	(0.007)
N	56,176	51,017	21,763	12,091	5,649	2,624	1,283	608
Academic								
γ	0.133**	0.087**	0.079**	0.071**	0.058**	0.037**	0.038**	0.023**
(s.e.)	(0.002)	(0.001)	(0.002)	(0.003)	(0.004)	(0.004)	(0.007)	(0.008)
N	36,452	45,548	23,817	10,931	4,434	1,901	810	386
University								
γ	0.034**	0.026**	0.035**	0.036**	0.031**	0.024*		
(s.e.)	(0.001)	(0.001)	(0.003)	(0.005)	(0.008)	(0.011)		
N	51,413	27,637	4,176	1,244	556	250		
Mothers								
Compulsory								
γ	0.196**	0.094**	0.089**	0.095**	0.079**	0.057**	0.050**	0.048**
(s.e.)	(0.013)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
N	1,110	7,118	6,378	8,169	8,662	7,346	5,223	3,368
Vocational								
γ	0.068**	0.056**	0.054**	0.042**	0.038**	0.033**	0.038**	0.033**
(s.e.)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.004)	(0.005)	(0.007)
N	53,507	50,445	22,345	12,645	5,947	2,797	1,382	668
Academic								
γ	0.105**	0.061**	0.048**	0.043**	0.032**	0.036**	0.028**	0.021**
(s.e.)	(0.002)	(0.001)	(0.001)	(0.002)	(0.003)	(0.004)	(0.005)	(0.007)
N	36,242	46,516	24,784	11,626	4,784	2,086	912	437
University								
γ	0.032**	0.026**	0.019**	0.022**	0.013**	0.011		
(s.e.)	(0.001)	(0.001)	(0.002)	(0.004)	(0.004)	(0.006)		
N	57,074	30,617	4,728	1,448	683	281		

Note: Estimates of parent referral effects. An observation is a combination of class, plant, and year of first job. Weighted by the number of graduates with parents in the plant. Data are for graduates 1988-1995 findning a stable job within 7 years of graduation. Only regressions with more than 100 observations are presented. Standard errors are cluster-corrected for dependencies within class. ** (*) Significant at the 1 (5) % level.

Table B2: Parental networks effect on probability of finding the first job in a specific plant, depending on whether field of study matches that of parent

	Same 1-digit field	Same 2-digit field	Same 3-digit field	Different 1-digit field	Different 2-digit field
Vocational					
Fathers					
$\hat{\gamma}$	0.122**	0.138**	0.169**	0.073**	0.074**
(s.e.)	(0.002)	(0.003)	(0.011)	(0.001)	(0.001)
N	21,852	13,235	1,426	119,735	131,159
Mothers					
$\hat{\gamma}$	0.089**	0.103**	0.130**	0.053**	0.053**
(s.e.)	(0.003)	(0.004)	(0.006)	(0.001)	(0.001)
N	13,404	8,761	4,544	131,418	137,464
Academic					
Fathers					
$\hat{\gamma}$	0.117**	0.118**	0.107**	0.087**	0.088**
(s.e.)	(0.002)	(0.002)	(0.003)	(0.001)	(0.001)
N	29,048	23,961	15,313	89,538	94,903
Mothers					
$\hat{\gamma}$	0.094**	0.095**	0.100**	0.061**	0.061**
(s.e.)	(0.002)	(0.002)	(0.003)	(0.001)	(0.001)
N	20,971	19,964	14,650	102,491	103,753
University					
Fathers					
$\hat{\gamma}$	0.037**	0.042**	--	0.030**	0.031**
(s.e.)	(0.002)	(0.003)		(0.001)	(0.001)
N	27,888	10,403		61,794	79,691
Mothers					
$\hat{\gamma}$	0.039**	0.050**	--	0.031**	0.031**
(s.e.)	(0.004)	(0.009)		(0.002)	(0.002)
N	1,975	575		97,802	99,524

Note: Estimates of parent referral effects. An observation is a combination of class, plant, and year of first job. Weighted by the number of graduates with parents in the plant. Data are for graduates 1988-1995 finding a stable job within 7 years of graduation. Only regressions with at least 100 observations are shown. Regressions only include those in the class with the same (or different) field of education as their parent. Standard errors are cluster-corrected for dependencies within class. ** (*) Significant at the 1 (5) % level.

Figures

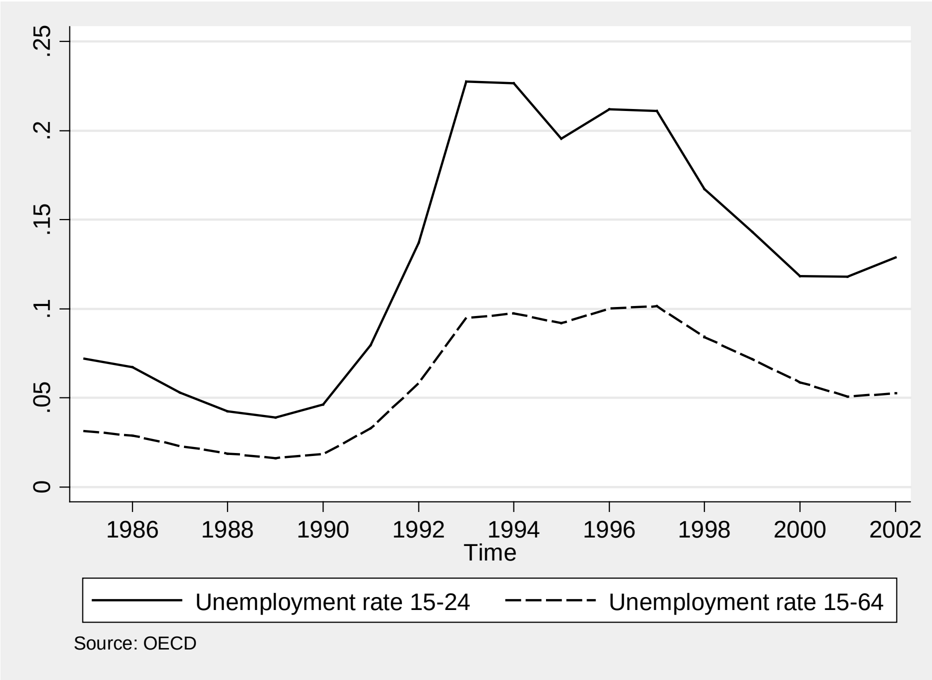

Figure 1: Unemployment 1985–2002

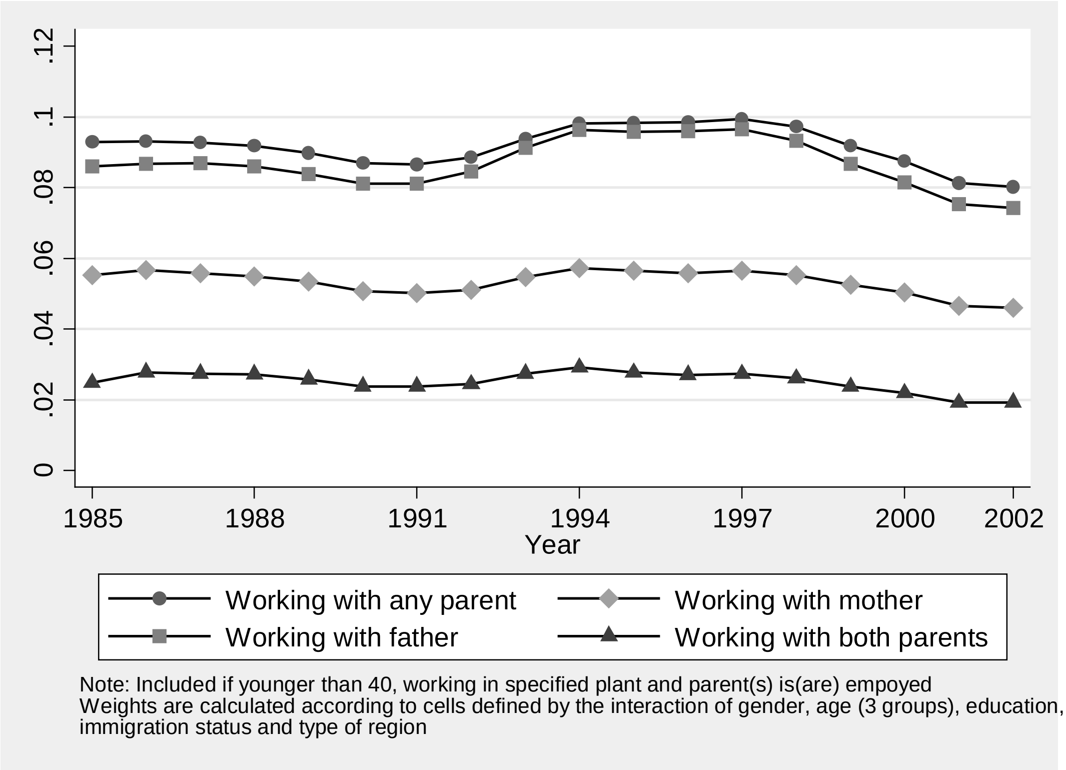

Figure 2: Time pattern of fractions working with parents, weighted by 1985 characteristics.

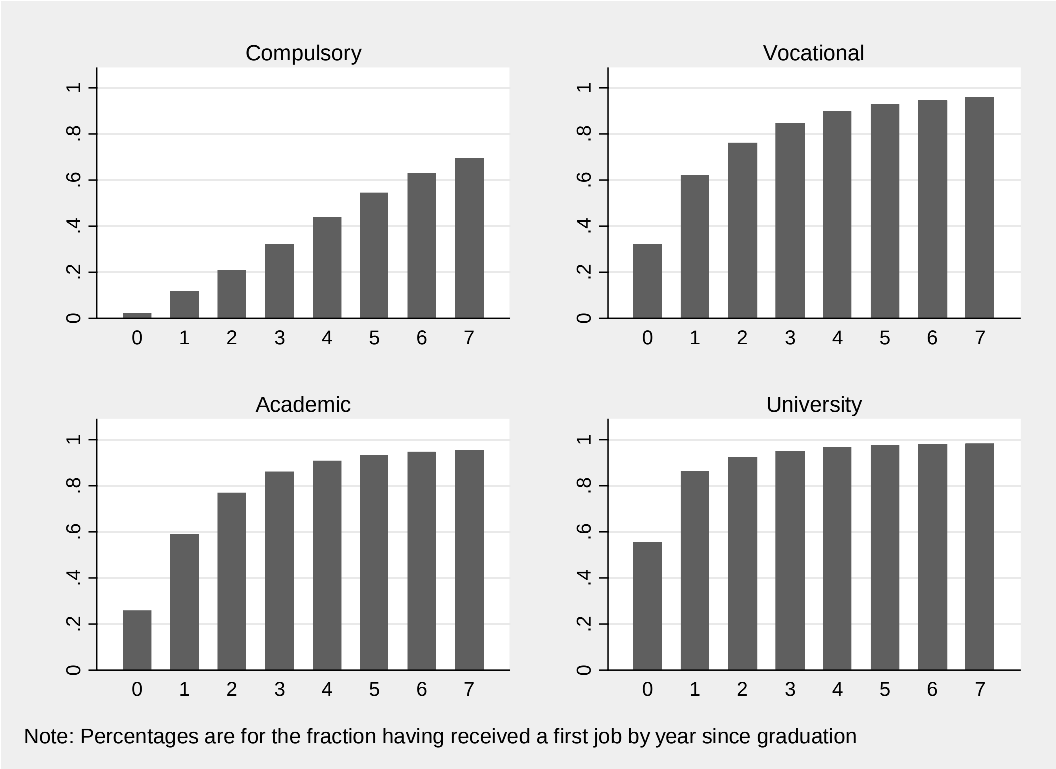

Figure 3 Time to first stable job – cumulative

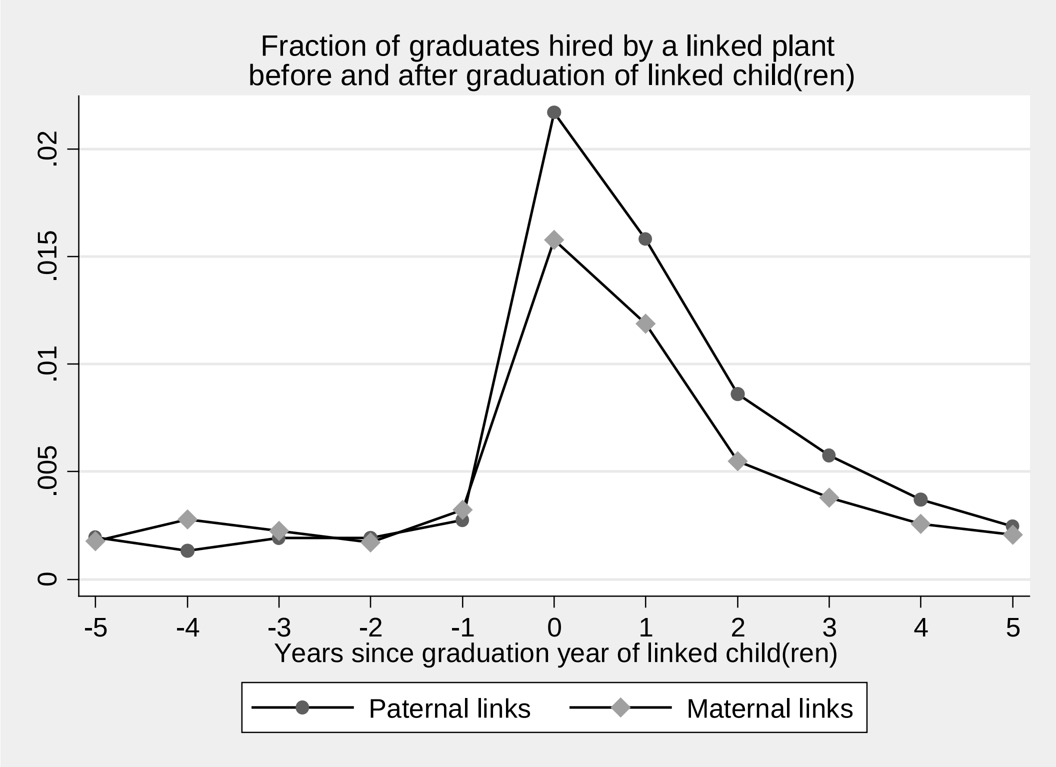

Figure 4 Fraction of graduates hired by a linked plant before and after graduation of linked child(ren)

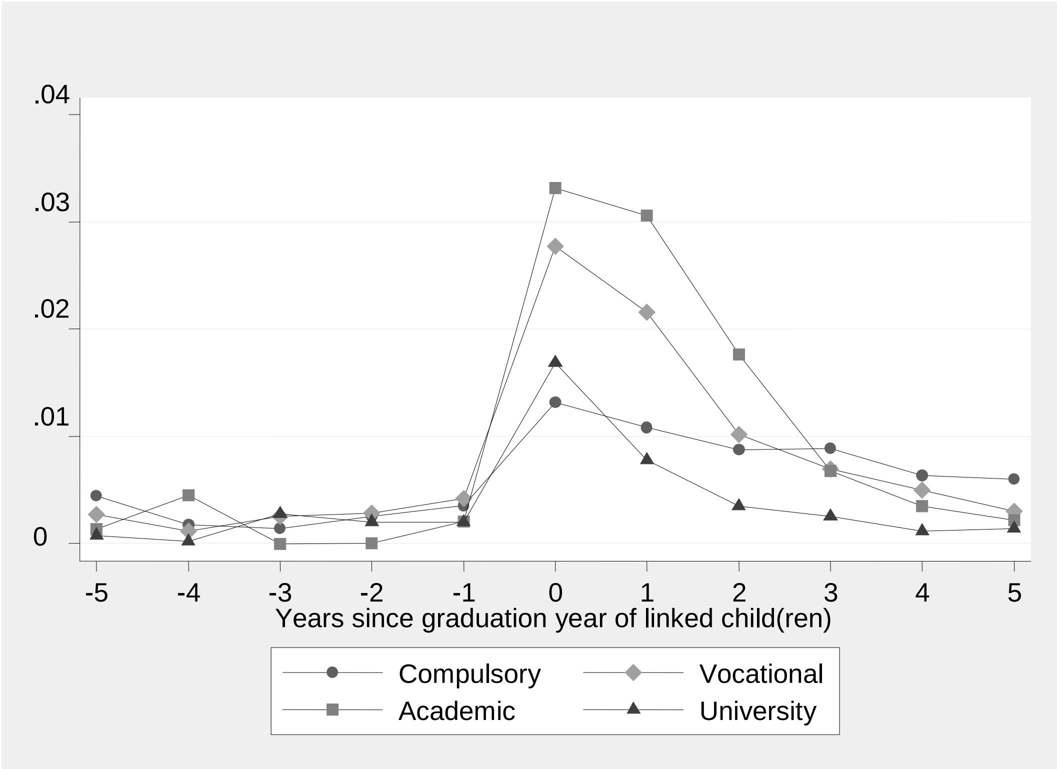

Figure B1 Fraction of graduates hired by a paternal-linked plant before and after graduation of linked child(ren), by education.

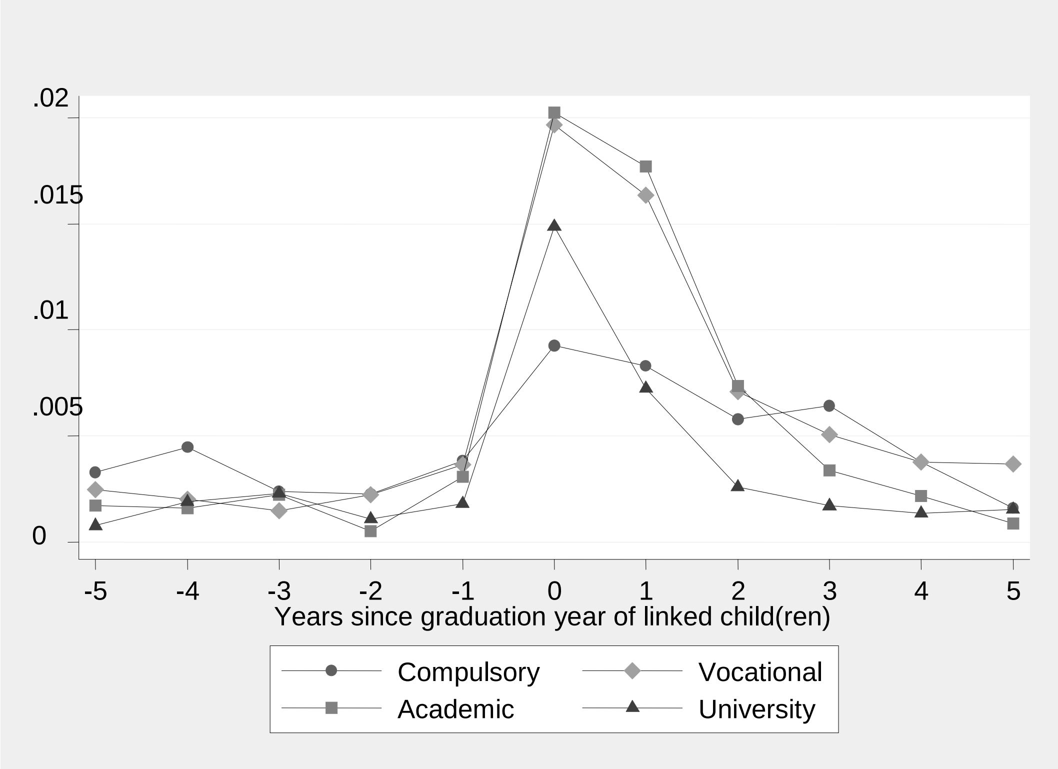

Figure B2 Fraction of graduates hired by a maternal-linked plant before and after graduation of linked child(ren), by education

Table 1: Probability of Having Parent(s) at the Child's Workplace

	Any Parent	Father	Mother	Both
Male	0.032**	0.056**	-0.015**	0.009**
	(0.001)	(0.001)	(0.001)	(0.000)
Aged 16-24	0.018**	0.005**	0.011**	-0.002**
	(0.001)	(0.001)	(0.001)	(0.001)
Aged 35-40	0.004**	0.005**	0.003**	0.004**
	(0.001)	(0.001)	(0.001)	(0.001)
Less than HS	0.063**	0.048**	0.033**	0.017**
	(0.002)	(0.002)	(0.001)	(0.001)
More than HS	-0.064**	-0.048**	-0.028**	-0.013**
	(0.001)	(0.001)	(0.001)	(0.000)
Immigrant	-0.005*	-0.005*	0.004*	0.004**
	(0.002)	(0.002)	(0.002)	(0.001)
Metropolitan	-0.028**	-0.021**	-0.011**	-0.004**
	(0.001)	(0.001)	(0.001)	(0.000)
Constant	0.104**	0.060**	0.062**	0.018**
	(0.001)	(0.001)	(0.001)	(0.000)
Observations	384,858	384,858	384,858	384,858
R-squared	0.03	0.03	0.01	0.01

Note: Linear probability model estimates of working with parent(s) in a specific plant if employed and the parent(s) is (are) employed. Data is for 2002. Population only includes (children) aged 40 or younger. * (**) Significant at the 5 (1) % level

Table 2: Parental Networks Effect on the Probability of Finding the First Job in a Specific Plant, Baseline Within-Class Estimates

	Compulsory school	Vocational high school	Academic high school	University degree	All
Fathers					
All					
$\hat{\gamma}$	0.104**	0,082**	0,095**	0,031**	0,078**
(s.e.)	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)
N	46,874	151,211	124,279	85,365	407,729
Males					
$\hat{\gamma}$	0,142**	0,118**	0,129**	0,048**	0,113**
(s.e.)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
N	23108	84798	56843	31560	196309
Females					
$\hat{\gamma}$	0,052**	0,034**	0,064**	0,021**	0,042**
(s.e.)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
N	16,094	60,904	60,842	50,251	188,091
Mothers					
All					
$\hat{\gamma}$	0,079**	0,058**	0,068**	0,029**	0,057**
(s.e.)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
N	47,374	149,736	127,387	94,939	419,436
Males					
$\hat{\gamma}$	0,063**	0,044**	0,061**	0,021**	0,047**
(s.e.)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
N	23,187	84,342	58,151	35,356	201,036
Females					
$\hat{\gamma}$	0,097**	0,075**	0,074**	0,034**	0,065**
(s.e.)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
N	16,456	60,061	62,928	55,609	195,054

Note: Estimates of parent referral effects. An observation is a combination of class, plant, and year of first job. Weighted by the number of graduates with parents in the plant. Data are for graduates 1988-1995 finding a stable job within 7 years of graduation. Standard errors are cluster-corrected for dependencies within class. **Significant at the 1 % level.

Table 3: Parental Networks Effect on the Probability of Finding the First Job in a Specific Plant, Robustness of the Within-Class Model

	Baseline estimates	Only classmates with parents in same industry	Only classmates with parents in same industry and within-firm wage quartile	Only classmates going to the same industry
Fathers				
$\hat{\gamma}$	0.078**	0.068**	0,068**	0.048**
(s.e.)	(0.000)	(0.001)	(0.001)	(0.001)
N	407,729	123,469	50,267	193,381
Mothers				
$\hat{\gamma}$	0.057**	0.043**	0.043**	0.048**
(s.e.)	(0.000)	(0.001)	(0.001)	(0.001)
N	419,436	196,828	97,546	200,039
	Firm hires 1 worker	Firm hires 2-5 workers	Firm hires 6-10 workers	Firm hires 11+ workers
Fathers				
$\hat{\gamma}$	0.097**	0.089**	0.066**	0.081**
(s.e.)	(0.002)	(0.001)	(0.002)	(0.001)
N	17,006	50,202	24,660	148,217
Mothers				
$\hat{\gamma}$	0.059**	0.052**	0.043**	0.067**
(s.e.)	(0.002)	(0.001)	(0.001)	(0.001)
N	14,019	56,635	32,000	163,759
	Private plants	Firm level analysis (only private)	Old (t-3)	Old (t-3)
Fathers				
$\hat{\gamma}$	0.103**	0.108**	0.020**	0.028**
(s.e.)	(0.001)	(0.001)	(0.001)	(0.001)
N	264,920	258,389	78,470	42,975
Mothers				
$\hat{\gamma}$	0.106**	0.113**	0.018**	0.025**
(s.e.)	(0.001)	(0.001)	(0.000)	(0.001)
N	146,862	142,892	88,130	47,172

Note: Estimates of parent referral effects. An observation is a combination of class, plant, and year of first job. Weighted by the number of graduates with parents in the plant. Data are for graduates 1988-1995 finding a stable job within 7 years of graduation. Standard errors are cluster-corrected for dependencies within class. **Significant at the 1 % level.

Table 4: Parental Links and Plant-level Hiring

	Fraction of graduates "at risk" hired		*Number of hires from linked school-field*		*Total number of graduates hired*	
	Fathers	***Mothers***	***Fathers***	***Mothers***	***Fathers***	***Mothers***
Graduation year (GY)	0.020**	0.014**	0.031**	0.024**	0.032**	0.025**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
GY+1	0.014**	0.010**	0.025**	0.016**	0.025**	0.019**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
GY+2	0.007**	0.004**	0.013**	0.006**	0.011**	0.006**
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.002)
GY+3	0.004**	0.002**	0.007**	0.003**	0.003*	0.002
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.002)
GY+4	0.002**	0.001*	0.003**	0.000	0.001	-0.002
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.002)
GY+5	0.001	0.000	0.000	-0.001	-0.003	-0.004*
	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)
Constant	0.002**	0.002**	0.006**	0.005**	0.047**	0.063**
(average plant fixed effect)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.002)
Sum of effects (0-3)	0.044**	0.029**	0.076**	0.049**	0.071**	0.051**
	(0.002)	(0.001)	(0.002)	(0.002)	(0.004)	(0.004)
Sum of effects (0-5)	0.046**	0.030**	0.080**	0.048**	0.070**	0.045**
	(0.002)	(0.002)	(0.003)	(0.002)	(0.006)	(0.006)
Sample	All plants	All plants	**Small (< 16 employees) plants**			
N	2,821,886	2,615,038	650,519	477,700	650,519	477,700
Plant FE:s	Yes	Yes	Yes	Yes	Yes	Yes

Note: Sample include five years before and five years after graduation, excluding years in which the plant did not exist and plants which more than doubles or halves its labor force since the preceeding year. An observation in the first two columns is a combination of type of class (school and field), plant, and year. Dependent variable in first two columns is the hired fraction of all graduates from the same school as the child of the employee finding of their first job during the year, estimate is for the fraction of these graduates that have a father/mother in the plant. An observation is a plant in the third to to sixth columns. Dependent variable in the third and fourth columns is the number of hired workers from any school-field combination in which the plant is linked via a father/mother. Explanatory variables are the number of these children who graduate in the observation year (+ 1 to 5). Dependent variable in the fifth to sixth columns is the number of hired graduates overall (i.e. from any track) during the year. Standard errors are cluster-corrected for dependencies within plants.
"Sum of effects" is for the linear combination based of the GY to GY+5 estimates. **(*)Significant at the 1(5) % level.

Table 5a: Heterogeneous Effects of Parental Networks

	All	All - Plant FE		All	All - Plant FE
			Family link		
Graduate			*reference only father*		
	-0.025**	-0.022**		-0.010**	-0.005**
Female	(0.001)	(0.001)	Only mother in plant	(0.001)	(0.001)
	0.008*	0.010*		0.174**	0.135**
Nordic Immigrant	(0.004)	(0.005)	Both parents in plant	(0.003)	(0.003)
	-0.001	0.001			
Other Immigrant	(0.002)	(0.002)	***Education of***		
	-0.001**	-0.002**	*reference Vocational*		
Age at graduation	(0.000)	(0.000)		0.024**	0.021**
	-0.006**	-0.006**	Compulsory	(0.001)	(0.001)
GPA (1-5)	(0.001)	(0.001)		0.028**	0.027**
			Academic HS	(0.001)	(0.001)
				-0.012**	-0.012**
			University	(0.001)	(0.001)
Fathers			***Mothers***		
	0.001	0.005		0.003	0.005*
Nordic Immigrant	(0.003)	(0.003)	Nordic Immigrant	(0.002)	(0.002)
	0.006**	0.003*		0.014**	0.009**
Other Immigrant	(0.001)	(0.001)	Other Immigrant	(0.001)	(0.001)
Compulsory	0.007**	0.007**	Compulsory	0.007**	0.006**
education	(0.001)	(0.001)	education	(0.001)	(0.001)
	-0.006**	-0.011**		-0.008**	-0.011**
Tertiary education	(0.001)	(0.001)	Tertiary education	(0.001)	(0.001)
Same (1d.) field as	0.023**	0.024**	Same (1d.) field as	0.006**	0.009**
child	(0.001)	(0.001)	child	(0.001)	(0.001)
	0.033**	0.035**		0.034**	0.030**
Log wage	(0.001)	(0.001)	Log wage	(0.001)	(0.001)
	0.003**	0.002**		0.002**	0.002**
Tenure	(0.000)	(0.000)	Tenure	(0.000)	(0.000)

Note: Estimates of interacted parent referral effects. An observation is a combination of class, plant, and year of first job. Weighted by the number of graduates with parents in the plant. Data are for graduates 1988-1995 finding a stable job within 7 years of graduation. Standard errors are cluster-corrected for dependencies within class. **Significant at the 1 % level.

Table 5b: Heterogeneous Effects of Parental Networks

	All	All - Plant FE		All	All - Plant FE
Industry-region			***Plant char.***		
Metropolitan county	-0.001*	0.002	Private	0.022**	0.006
	(0.001)	(0.010)		(0.001)	(0.003)
County unemployment	0.118**	0.076**	New plant	0.014**	0.012**
	(0.017)	(0.021)		(0.002)	(0.002)
Industry-field match	0.161**	0.155**	Plant growing from last year	0.014**	0.012**
	(0.003)	(0.003)		(0.001)	(0.001)
Industry-field match* unemployment	-0.230**	-0.203**	Size <16 (ref 16-45)	0.007**	0.006**
	(0.072)	(0.077)		(0.001)	(0.002)
Market concentration	-0.284**	0.496*	Size 46-125	0.009**	-0.002
	(0.056)	(0.194)		(0.001)	(0.001)
			Size 126-750	0.018**	-0.003
				(0.001)	(0.002)
			Size 750+	0.020**	0.004
				(0.001)	(0.004)
Worker composition at plant			***Industry of plant***		
Mean age	-0.007**	-0.007**	Construction (*ref manufact.*)	0.025**	0.005
	(0.000)	(0.000)		(0.002)	(0.007)
Share primary education	0.030**	0.063**		0.000	0.007
	(0.002)	(0.005)	Wholesale, retail	(0.001)	(0.007)
Share tertiary education	-0.023**	-0.037**	Financial, corporate	-0.004**	0.001
	(0.002)	(0.004)		(0.001)	(0.007)
Immigrant share	0.026**	0.048**		0.005**	0.005
	(0.003)	(0.007)	Education R&D	(0.002)	(0.008)
Average log wage	-0.058**	-0.006		-0.040**	-0.029**
	(0.002)	(0.004)	Health, Social	(0.002)	(0.008)
			Personal &	0.003	0.006
N	788,028	729,124	Cultural	(0.002)	(0.009)
N (parents)	823,516	754,150		-0.009**	-0.002
N (plants)	157,518	88,286	Public admin.	(0.001)	(0.007)

Note: Estimates of interacted parent referral effects. An observation is a combination of class, plant, and year of first job. Weighted by the number of graduates with parents in the plant. Data are for graduates 1988-1995 finding a stable job within 7 years of graduation. Standard errors are cluster-corrected for dependencies within class. **Significant at the 1 % level.

Table 6a: Use of Parent Networks by Municipality/Occupation among Vocational HS Graduates

	a	b	c	d	e	f
Number of employing plants	0.068**	0.025**	0.028**	0.039**	0.021**	0.009**
	(0.013)	(0.009)	(0.007)	(0.007)	(0.007)	(0.002)
Number of workers	-0.017**	-0.009**		-0.013**	-0.011**	
	(0.004)	(0.003)		(0.003)	(0.003)	
Constant	0.062**	0.064**	0.062**	0.062**	0.065**	0.062**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N	3,228	3,228	3,228	3,228	3,228	3,228
R-Squared	0.10	0.00	0.10	0.17	0.01	0.16

Note: The table shows regressions where we explain the municipality/occupation specific use of parent networks by number of employing plants by education and municipality. The number of plants is calculated for 1995 by education and municipality using the full stock of employees. Network effects (i.e. the dependent variable) is estimated using all years (the same model as in table 2). ** Significant at the 1 % level.

Table 6b: Use of Parent Networks by Municipality/Occupation among Vocational HS Graduates

	a	b	c	d	e	f
Coworkers with same education	-0.064**	-0.071**	-0.025	-0.072**	-0.077**	-0.049**
(exposure)	(0.016)	(0.015)	(0.014)	(0.009)	(0.010)	(0.009)
Baseline	0.064**	0.063**	0.067**	0.067**	0.066**	0.069**
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Constant	0.438**	0.586**		0.255**	0.364**	
	(0.088)	(0.085)		(0.043)	(0.048)	
N	3,106	3,106	3,106	3,106	3,106	3,106
R-Squared	0.11	0.01	0.10	0.18	0.04	0.17

Note: The table shows regressions where we explain the municipality/occupation specific use of parent networks by the concentration of employeees with the same education in plants (exposure). Exposure is defined by calculating (for 1995) the fraction of coworkers (in the same plant) having the same education for the full stock of employees and taking averages by detailed education and municipality, The baseline is calculated as the average of others in the municipality with the same education. Network effects (i.e. the dependent variable) is estimated using all years (the same model as in table 2). ** Significant at the 1 % level.

Table 7: Effects of Finding a Job through Parental or Sibling Referral

	ln(Time to first job)		ln(Starting wage)		Relevance of industry	
Mother only	-0.170**	-0.135**	-0.051**	-0.069**	-0.018**	-0.028**
	(0.004)	(0.004)	(0.003)	(0.002)	(0.001)	(0.001)
Father only	-0.188**	-0.147**	0.012**	-0.050**	-0.047**	-0.023**
	(0.003)	(0.003)	(0.003)	(0.002)	(0.001)	(0.001)
Both	-0.253**	-0.215**	-0.035**	-0.051**	-0.041**	-0.026**
	(0.006)	(0.007)	(0.005)	(0.005)	(0.002)	(0.002)
Sibling	-0.095**	-0.075**	0.030**	-0.008**	-0.024**	-0.007**
	(0.004)	(0.004)	(0.003)	(0.003)	(0.001)	(0.001)
N	565,359	565,359	565,359	565,359	565,359	565,359
Class Fe	Yes	No	Yes	No	Yes	No
Ed. char	--	Yes	--	Yes	--	Yes
Plant FE	No	Yes	No	Yes	No	Yes

Outcomes after three years (only if first job within 4 years of graduation)

	In same plant		Employment		Wage growth (3 years)	
Mother only	0.040**	0.025**	0.007	0.011**	-0.004	-0.005
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Father only	0.095**	0.055**	0.025**	0.019**	0.026**	0.008**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Both	0.191**	0.138**	0.067**	0.060**	0.035**	0.040**
	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Sibling	0.083**	0.052**	0.032**	0.028**	0.017**	0.007*
	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)
N	514,163	514,163	514,163	514,163	374,287	374,287
Class Fe	Yes	No	Yes	No	Yes	No
Ed. char	--	Yes	--	Yes	--	Yes
Plant FE	No	Yes	No	Yes	No	Yes

Note: Estimates are for the conditional association between getting the the first job at the plant of parents and subsequent outcomes. Relevance of industry measures the fraction of all graduates with the same education who found the first job in that industry. Outcomes 3 years later are for the sample that got the first job within 4 years. The first model includes a fixed affect for each class and year of first job (only for class in the analysis of time to first job). The second model includes plant fixed effects and dummies for each field and level of education. All regressions control for immigration status, gender and GPA (except for university graduates). Data are for graduates 1988-1995. Standard errors are cluster-corrected for dependencies within class. ** (*) Significant at the 1 % (5 %) level.