When Strong Ties are Strong:

*Networks and Youth Labor Market Entry**

by

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Abstract

The conditions under which young workers find their first real post-graduation jobs are both very important for the young' future careers and insufficiently documented given their potential importance for young workers welfare. To study these conditions, and in particular the role played by social ties, we use a Swedish population-wide linked employer-employee data set of graduates from all levels of schooling which includes detailed information on family ties, neighborhoods, schools, class composition, and parents' and children' employers over a period covering years with both high and low unemployment, together with measures of firm performance. We find that strong social ties (parents) are an important determinant for where young workers find their first job. The effects are larger if the graduate's position is "weak" (low education, bad grades), during high unemployment years, and when information on potential openings are likely to be scarce. On the hiring side, by contrast, the effects are larger if the parent's position is "strong" (long tenure, high wage) and if the parent's plant is more productive. The youths appear to benefit from the use of strong social ties through faster access to jobs and by better labor market outcomes as measured a few years after entry. In particular, workers finding their entry jobs through strong social ties are considerably more likely to remain in this job, while experiencing better wage growth than other entrants in the same plant. Firms also appear to benefit from these wage costs (relative to comparable entrants) starting at a lower base. They also benefit on the parents' side; parents' wage growth drops dramatically exactly at the entry of one of their children in the plant, although this is a moment when firm profits tend to be growing. Indeed, the firm-side benefits appear large enough for (at least small) firms to increase job creation at the entry level in years when a child of one of their employees graduates.

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1 Introduction

Labor market entry is a defining moment for young workers when important decisions must be made.[1] In this process, the finding of a first "real" job is essential.[2] However, the precise strategies used by young job searchers when looking for entry jobs are not well-understood. Since previous studies have documented the importance of social ties in the process of matching workers and firms in many parts of the labor market, we conjecture that the use of social ties and networks are central ingredients in young workers transition into the labor market. This paper therefore stands at the junction of the literatures on school-to-work transitions and social job-finding networks and our aim is to provide a first thorough analysis of how social ties affect the labor market entry of young graduates from different levels of schooling. We focus on the role of *strong social ties*, we show their importance, identify their nature, and their effects using a unique population wide data set linking graduation records and family ties to a longitudinal matched employer-employee data set with information on the firms (sales, profits) and plants. We also examine the importance of weaker ties, such as classmates or neighbors.

The seminal work of Granovetter (1973, 1983) formulated much of the economic and sociological thinking regarding job-finding networks by distinguishing between strong and weak social ties (i.e. close friends or family vs. acquaintances). A central finding in Granovetter's work is that weak social ties are important components of the labor market since such ties bring new information about vacancies between agents with different information sets. The "strength of weak ties" hypothesis has also been central in much of the recent empirical work on job finding networks based on register data where networks are often defined through neighborhoods or shared immigrant background (see references below).

Other strands of the network literature have however pointed at the importance of strong social ties and such strong ties should be particularly relevant for young workers who are in the process of entering the labor market. Boorman (1975) presumed that employed agents first disseminate information about vacancies to their strong social ties, and only spread information to weaker acquaintances if closer friends already are employed. This channel, which may be thought of as a form of nepotism through privileged information, may be particularly important in cases when the social ties provide links to young workers struggling to find their first jobs. A different channel is suggested by Montgomery (1991), in a model of employee selection where firms choose to recruit

[1] See among many contributions, Keane and Wolpin (1997).
[2] See e.g. Beaudry and DiNardo (1991) and recently Kahn (2010) on the impact of unemployment on wages of new hires.

workers with social ties to productive incumbent employees in order to find suitable workers when ex ante information about worker quality is imperfect, a precondition which is particularly likely to be relevant for young, untested, workers.[3] Since this argument builds on the presumption that good workers associate with other good workers it arguably makes more sense the stronger the ties are between the workers. One argument for firms to rely on networks for recruitments is monitoring, in particular Heath (2013) develops a model where the referrer is punished if the referred worker misbehaves (see also the model in Dhillon, Iversen, and Torsvik, 2013). In order to motivate the latter to exert effort, social ties between these agents have to be strong.

Job finding networks as tools for disseminating information between (prospective) workers and firms are likely to be particularly important in cases where information about suitable jobs is scarce and ex ante information about worker quality is noisy. We argue that these conditions are likely to be relevant for young workers who lack previous work experience. Even if the education system may compensate for such information deficiencies to some degree, uncertainty about graduates' quality will vary with the type or level of education. This is likely to be especially true for the low-educated young, with a non-technical and non-specific type of training.

For these reasons, our analysis of young workers entry into the labor market is focused on the use of strong social ties, although we also provide an analysis of various weak social ties as a contrast. When separating these two types of social ties, we rely on Granovetter who defined the strength of social ties by a "*…combination of the amount of time, the emotional intensity, the intimacy (mutual confiding), the reciprocal services which characterize the tie.*" (Granovetter, 1973, p. 1361). Thus, family ties are the archetypical form of strong social ties, whereas neighbors, classmates, and the parents of classmates are good examples of weak ties.

Survey evidence indeed suggests that family ties are an empirically important source of information in the job finding process. Data from the International social survey program ISSP (2001) for 30,000 respondents spanning 30 developed countries suggest that more than 10 percent of respondents found their last job through the immediate family (parent, brother or sister),[4] 7 percent from other relatives and 13 percent from close friends. Notably, the ISSP survey cover all age groups whereas our analysis focuses on the young, where family ties could be expected to matter

[3] See also Dustmann, Glitz, and Schönberg (2011) who models social ties as a means to reduce uncertainty about match quality.
[4] The survey does not cover Sweden from where we draw our data but the average across neighboring Norway, Finland and Denmark was 6 percent, and for the US 5.4 percent.

the most.[5] If this conjecture is right, our focus on strong social ties is set to teach us something important on entry on the labor market of children just graduating from the education system, in particular how social ties precisely operate at this stage of a worker's career.

Previous literature: In the first statistical analysis we are aware of, De Schweinitz (1932) reports that, in 1923, more than 40% of workers in the hosiery industry in Philadelphia obtained their jobs through friends and relatives. Since then, the importance of "informal" channels as a job finding resource has been documented by numerous surveys. Although results vary substantially between studies (mostly ranging from 30 to 60% according to Bewley, 1999) the use of informal contacts appear to be pervasive irrespective of occupation or country.[6] Consequently, the existing literature is now burgeoning both on the theoretical[7] side and on the empirical[8] side after a period of relative calm following the path-breaking articles of Rees (1966), Granovetter (1973), and Boorman (1975).

The part of the previous empirical literature which is most closely related to ours in a methodological sense has primarily been investigating whether the neighborhood constitutes a source of information and thereby trying to give a more precise content to the general concept of a social tie. Topa (2001) explained the clustering of unemployment within Chicago neighborhoods using a probabilistic approach for the likelihood of a contact (which allows for "spillover" of information across census tracts). Bayer, Ross, and Topa (2008), uses micro-level census data for Boston, and find that those who live on the same block are more than 50% more likely to work together than those living in nearby blocks. 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.

[5] Results from the first wave of the U.S. NLSY1997 show that 23 percent of employed respondents where either recommended or recruited in a firm where their parents were working (and about 20 percent through other relatives).
[6] Although Sweden has a developed system of job matching through the Public Employment Service (PES) relying on a national data base of vacancies, the market share of the PES has always been low. Survey evidence show that the most prevalent channel for youths to find jobs is through networks; 40 percent of 19-24 year olds who were employed in 2004 claim that they found their jobs through people they know whereas less than 6 percent found the job through the PES (Ungdomsstyrelsen, 2005).
[7] See Montgomery (1991) or more recently Calvó-Armengol, Verdier and Zenou (2007), Ballester, Calvó-Armengol and Zenou, (2006), Calvó-Armengol (2004), Calvó-Armengol and Jackson (2004) and (2007), Casella and Hanaki (2008), and Galeotti and Merlino (2011) among many authors, and Jackson (2004) and (2008) for very thorough surveys.
[8] See e.g. Munshi (2003) Bandiera, Barankay and Rasul (2009) Bayer, Ross and Topa (2008), Bertrand, Luttmer and Mullainathan (2000) Fredriksson and Åslund (2009), Dustmann, Glitz, and Schönberg (2011) among many authors, and Ioannides and Loury (2004) for a very detailed survey.

A new set of papers (Cingano and Rosolia, 2012, Åslund, Hensvik and Skans, forthcoming, and Dustmann, Glitz, and Schönberg, 2011 are good examples) looks at matched longitudinal employer-employee data to follow workers who have worked in the same firm at some point in time or share ethnicity and check if the characteristics of their network (former or present co-workers) has an impact on job search or other outcomes. Cappellari and Tatsiramos (2011) use a novel source of information on individuals and their friends collected within a British panel (BHPS) to examine how friends' employment affect individuals own employment. Finally, Corak and Piraino (2011) 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 social ties at the moment of entry on the labor market.

The literature so far contains very little evidence on firm-side responses to social ties. A few notable exceptions are Kramarz and Thesmar (forthcoming) for French CEOs as well as single-firm field experiments and lab experiments (e.g. Bandiera, Barankay and Rasul, 2007 and Beaman and Magruder, 2012), all showing that firm performance tends to be negatively affected by the use of networks when managerial incentives are low. Another very recent exception is Brown, Setren, and Topa (2012) who use evidence from a single firm's employee referral system to examine various predictions of theoretical models of referrals. Among other findings, in the analyzed firm, referred workers experience an initial wage advantage. As far as we know, however, the literature so far is void of large-sample studies documenting how firms adjust hiring patterns or wages of incumbent workers in response to the use of social ties.

Our contribution: In contrast to the existing literature and (to the best of our knowledge) for the first time, we examine the role played by social ties using an empirical strategy which relies on directly observing all three components of a potential match: the worker looking for his or her first job, the employed worker on the other end of the social tie, and the firm. To accomplish this task, we focus on links between children and their parents, and contrast these with various examples of weak ties. We analyze how "supply side" (the graduate) characteristics, "demand side" (firm) characteristics, and the "agent of the match" (the parent) characteristics interact to determine when and by whom the social ties are used to form a match, and how the use of these ties affects subsequent outcomes for all agents involved. In particular, we try to assess the benefits and limits of the resulting exchange for all three participants.

We use comprehensive data on all Swedish students graduating from compulsory school, high school, or universities/colleges during an eight year (1988-95) period and follow each cohort for seven years. These data are linked to graduation records and a population-wide set of very accurate intergenerational indicators as well as longitudinal workplace, neighborhood identifiers and measures of firm performance.

We use these data to estimate how the presence of a parent within a plant affects the probability that a young graduate finds employment within the same plant. Since social ties may correlate with other complementarities between workers and firms, identification of the effects of social ties requires careful treatment of the data. Our baseline specification uses classmates to estimate the counterfactual probability of entering each plant, but we also show that the estimates are robust to a wide set of variations including those comparing similar graduates with parents in different plants within the same firm as well as specifications where we use classmates whose parents used to work within the very same plants to estimate the counterfactual.

Relying on network information from parent-child links in register data we are not in the capacity to directly measure if a referral took place (in contrast to single firm studies using data from human resource departments such as e.g. Brown et al, 2012). We are also not able to directly measure the type of information flows that are associated with the social ties. Our data, however, allow us to compare situations when the parent is present in the actual plant versus when the parent has recently left the plant, as well as to perform comparisons across plants within the same firm. These exercises allow us to uncover the role of the social ties, net of factors related to potential intergenerational correlations in firm-specific skills. In addition, our data allow us to perform a varied set of additional, and unique, empirical exercises, e.g. by identifying differences in effects between graduates with or without parental links to the same plant; by exploring the consequences of social ties for the parent (after showing that he/she is the agent of the match) and for the firm (on hiring), as well as tentatively exploring the links between the use of social ties and productivity/ profits.

To preview our results, we find that strong social ties are important, *but only if the parent is present within the plant,* i.e. not in other plants of the same firm, and not if the parent has left the plant. Strong ties are particularly important for low educated youths, whereas the impact of weak ties tend to be largely independent of the level of education.[9] Strong ties matter more for low educated

[9] Our analysis excludes self-employed parents.

graduates with low grades, even after accounting for firm-specific heterogeneity and many other aspects of parental heterogeneity. For a given level of education, strong ties also appear to be more important the less specific (i.e. targeted to a specific type of occupation or firm) the training is.

Even though the aggregate consequences of job finding networks can be negative, for instance because of a reduced match quality (see Bentolila, Michelacci and Suarez, 2010, Pellizzari, 2010, and Horvath, 2012), the use of social ties in the matching process should benefit all agents that are directly involved in the recruitment for the match to take place. Indeed, our analysis shows that the youths who find employment in plants in which they have a strong social tie seem to benefit in terms of much shorter school-to-work transitions, and better labor market outcomes at a few years horizon (employment, wages) than comparable youths from the same classes or entering the same plants; benefits that should be particularly useful in the high unemployment years when the social ties are used more frequently. However, they seem to pay a price in terms of lower entry wages and their training also provide a less frequent match to the receiving industry relative to comparable youths. Even though the parent's wage growth is shown to be negatively affected by the entry of the child in the parent's plant, the parent is likely to benefit from the increased and accelerated financial independence of their child.

We also document notable benefits on the firm side. Hiring of employees' children is more frequent when the parents are high-wage workers and have long tenure, suggesting that firms use parental quality as a signal of youth reliability. As mentioned above, firms also benefit from reduced wage growth for the parent in comparison with other co-workers and lower starting wages for the children than for other comparable entrants. The wage impact of entrants is however short-lived, within a three year horizon they earn wages at par with the average entrant competing entrant, but on the other hand, linked graduates tend to remain longer within the plant. Overall, the firm-side benefits appear large enough to induce (at least small) firms to create jobs that otherwise would not have been created, or at least not filled by other youths.

Our identification strategy does not allow us to fully explore the causal impact on firm performance but a tentative exploration of various performance measures does not indicate that strong social tie recruitments are associated with firm-level inefficiencies. In contrast, the use of strong social ties appear to be more frequent among well-performing firms and, in comparison with years when other graduates were hired by the same plant, in years when performance measures are about to improve.

These results are broadly consistent with the notion that it is more profitable for productive firms to use social networks for recruitment as suggested by Montgomery (1991).[10]

The paper is structured as follows: Section 2 discusses the empirical model. Section 3 provides a brief background of Swedish institutions and labor market conditions. Section 4 describes the data we use. Section 5 provides empirical results and Section 6 concludes.

2 The empirical model

Our empirical models are designed to identify how social ties affect the search for the first "real" job of new graduates. Here, we describe the set-up for parental links (strong ties), but our analysis of classmates' and neighbors' networks (weak ties) are based on the same principle.

We want to measure the contribution of parental presence in a plant to the probability that a child (of this) employee starts to work in that particular plant. We thus need an empirical model that accounts for the counterfactual probability that the graduate would have ended up in her parent's plant, even if the parent had not worked there. Our main strategy is to use *classmates* to measure the counterfactual. Our analysis also studies the respective role of the graduate, the parents, and the parental plant for the magnitude of the social tie impact. In addition, we study how social ties affect the outcomes for all three agents of the match. However, this section focuses on the details of the main identification strategy, leaving the details of auxiliary models to the empirical section.

2.1 The set-up

Whether or not a fresh graduate finds her first stable job in a particular firm is likely to depend on how well her skills and social networks overlap with those needed by the firm. In order to estimate the effects of a social ties (in our case provided by the parent-child relations), we therefore need a model which accounts for other potential sources of overlap between skills of the graduate and characteristics and needs of the firm. To this end, 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).

[10] Alternatively, it could be argued that well-performing firms may be the ones which can afford to engage in inefficient recruitments of family members. We are not able to separate between these competing interpretations.

Each graduate may start working in any of the plants (indexed by *j*) present in the economy. Using a formulation based on Kramarz and Thesmar (forthcoming), we analyze the following linear model for the probability that graduate *i* starts working in plant *j*:

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

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 specific effect that captures the propensity that graduates from a given class end up working in a particular plant. Such a match specific effect could come from different professors teaching to that class with tighter contacts to some establishments.[11] Its introduction therefore controls for this type of links of the class to the plant. Controlling for the match specific effect, our parameter of interest which measures the effect of social ties is γ. The estimate of γ answers the following question: *How much more likely is the average plant to hire a child of one of its employees than someone else from the same class*". For now, 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.

One aspect which may confound the estimate of interest is differences in skills within classes. To the extent that these differences are revealed to the employing plants through the social ties, we can think of them as being part of the overall effect of the social tie. But specific skills that *i*) vary within a class, *ii*) would be readily observed by the firms even without the social tie, and *iii*) are particularly relevant for the firms where the parents work, will confound the interpretation of the estimates. A second possible confounder is competition over rationed vacancies which may make a graduate less likely to start in a particular firm when classmates have social ties there. In order to minimize the impact of these two possible confounders, we will present a large set of robustness checks in the empirical section by either manipulating the composition of the control group (the class), or the set of receiving firms, in order to corroborate our interpretation of the main results.

[11] We thank a referee for suggesting this interpretation.

We also present results from a very different, alternative, identification strategy derived from the firm side which 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 Within-class estimation in practice

If ε and A are orthogonal given the class-plant fixed effects β, we are, in theory, able to obtain a consistent estimate of γ. Direct estimation of (1) would however require a data set with one observation for each combination of individual and plant. As our data set contains nearly 600,000 graduates and over 300,000 plants per year, estimation of such a model would therefore require construction of a data set with almost 200 billion observations. In order to transform equation (1) into an estimable model, we use a version of the Kramarz and Thesmar (forthcoming) fixed effects transformation. First, we restrict the sample under study to cases where there is within plant-class variation in *A*. This restriction is without loss of identifying variation since the discarded observations are uninformative conditional on the fixed effects. We then aggregate the model by computing, for each plant-class combination, the fraction of graduates with parents in the plant who were hired by that particular plant:[12]

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

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). We then calculate the corresponding fraction for graduates from each class hired by a plant in which none of their parents are working.

$$(3) \qquad R_{cj}^{-A} \equiv \frac{\sum_{i}^{c(i),j} E_{i,c(i),j} (1 - A_{i,j})}{\sum_{i}^{c(i),j} (1 - A_{i,j})} = \beta_{c,j} + \tilde{u}_{c,j}^{-A}$$

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

$$(4) \qquad G_{cj} \equiv R_{cj}^{A} - R_{cj}^{-A} = \gamma + u_{c,j}^{G}.$$

[12] Note that the restrictions we use imply that we drop plants where no parents work. Given our very broad data set (described below) we do however keep observations describing a very large part of the labor market. In total we have observations from parents in 160,000 establishments which is about half of all establishments in the economy. In addition, since the sample is drawn on the employee side, lost establishments are typically very small.

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 that plant* minus the fraction of those hired in the plant from the same class *among those without a parent in that same plant*.[13] 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 this plant, and working in the same plant for those without a parent there. Conceptually, this difference between two groups of students is close to a matching estimator taking the difference in hiring probabilities between pairs (instead of, as we do, the mean probabilities) of children within the same class where one has a parent in the plant and the other not.[14]

2.3 Firm-side identification using the timing of graduation

Our alternative identification strategy relies on the following idea: the event when a student graduates constitutes an exogenous supply shock directed to the plant that employs his/her parent 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.

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 the analysis, we ask whether this 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 (with these characteristics) than before.

The effect is therefore insensitive to differences in skills that are unobserved to us (but observed to the firms), as long as these skills are equally distributed across cohorts. Similarly, as long as firms

[13] When estimating (4) 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.

[14] Melissa Tartari, in a 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.

do not shift vacancies between years, competition between graduates within a class will not affect the estimates since the estimates are based on comparisons between classes (of the same type).

3 Institutional background

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) the majority of students choose to start high-school (about 85 percent of a cohort graduates from high school).

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-year long (with a 4-year version for engineers) whereas vocational programs were 2-year 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-year long education that qualifies for university studies. However, the transition rates from vocational programs to higher education remain very low.

Sweden has for a longtime had a well-developed system of job matching through the Public Employment Service (PES) relying on a national data base of vacancies. However, the market share of the PES has always been low. Survey evidence shows that the most prevalent channel for youths to find jobs is through networks; 40 percent of 19-24 year olds who were employed in 2004 claim that they found their jobs through people they know whereas less than 6 percent found the job through the PES (Ungdomsstyrelsen, 2005).[15]

Our period under study goes from 1988 to 2002. This includes the most turbulent period faced by the Swedish labor market since the 1930s. Most notably for our purposes, the youth unemployment rate, which was below five percent in the late 1980s, rapidly increased to over 20 percent in the

[15] The other main channel was through direct contact with an employer (30 percent).

early 1990s (see Figure 1 below).[16] The unemployment rate remained high until the late 1990s when it started to decline and by the year 2001 the youth unemployment was just above 10 percent. Overall unemployment showed a similar pattern with approximately half the rate.

4 Data and description

The paper makes use of a large 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 parents. Furthermore, we use detailed information from graduation records stemming from different levels of schooling. These records contain information on the field of study and the exact school of graduation. Combining these various data sources into a working data set is a complex procedure and details are presented in Appendix A.

4.1 Establishments, demographics and neighborhoods

The linked employer-employee part of the data set draws on the tax records filed by firms. The data contain annual information on all 16–65 year-old employees receiving remuneration from an employer between 1985 and 2002. Data contain information on each individual's earnings received from each single employer as well as the first and last remunerated month during each year. The data gives us each worker's primary job, defined by a wage and a plant, in February each year.[17] 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.

Our data also include measures of firm performance for a subset of plants and firms. For a sample of firms we measure productivity as sales per worker and the profit rate defined as declared profits divided by total assets. At the plant level, we are able to measure productivity (sales per worker) for a sample of manufacturing plants.[18] Both of these data sets heavily under-sample small units.

Data include basic demographic characteristics such as gender, age, level of completed education, and country of birth. 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

[16] 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).
[17] We refer to all establishments as "plants". See Table A1 in the Data Appendix for details about parental employment.
[18] Due to the immense dispersions of these performance measures we drop the extreme one percent of each year's distribution.

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)[19] and sector (private or public).

Our data on neighborhoods are based on Statistics Sweden's definition of SAMS (Small Area Market Statistics) referring to homogeneous neighborhoods in terms of building structures. The median resident has 450 working aged neighbors within his or her neighborhood.

4.2 Parent-child links

The overall data set contains links between all parents and children in the data set. Missing parent identities 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.

4.3 Graduation data

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

We study four different populations defined by their educational attainment:

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

We refer to graduates from the same school, graduating at the same time, and within the same field of education as graduating from the same *class*. Even though this concept does not necessarily correspond to an exact class as such, the definition serves our purposes well since the main purpose is to control for factors that are time, region and occupation specific and we do not mainly use the

[19] 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.

concept to capture social interactions between classmates.[20] In Appendix A, we explain in detail how the class concepts are defined for each of the four different groups of graduates (see also Table A1, in the Data Appendix).

Graduation data include information on grade point average (GPA) for compulsory and high school graduates. Each grade is set on a scale of 1 to 5 (ascending) with a national average of 3 and a standard deviation of 1. We further construct an indicator for when the graduate and the parent share the same 1-digit (ISCED) field of education (irrespective of level).

4.4 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 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 corresponding to at least 3 month work as a full-time janitor (a minimum wage proxy). As shown in the Data Appendix (Table A2), 53 percent of graduates satisfy these criteria the year after graduation. Naturally, it takes substantially longer for Compulsory school graduates to find their first stable jobs than for University graduates who, in general, find the jobs very shortly after graduation (see Table 1, below).

4.5 Descriptive statistics

In Table 1 we present descriptive statistics. The first row shows the fraction of graduates who find their first job at a plant where their parent is employed. Among compulsory school graduates, 14 percent find their first job at a plant where one of the parents is employed. The fraction is somewhat lower, but still substantial (11.5 percent) for high school graduates (pooling academic and vocational tracks). For university graduates on the other hand, the fraction starting the career at a parent's plant is only 3.2 percent. For the full sample pooling all levels of schooling, the fraction is 10 percent. We also computed the probability for a young graduate, who does not work with her father or mother, to start working in the plant where one of her siblings is employed. We did a similar computation for brothers and sisters of her parents (hence, aunts and uncles). Our computations show that 2 percent of those not working with their parents find their first job in their siblings' plant (2.9 percent for the least educated, 0.6 percent for university). We also find that on average 1.7 percent of those not working with their parents or siblings find their first job in their uncles' or aunts' plant, with similar patterns across education levels (2.2 percent among compulsory

[20] Although we do discuss robustness checks where we try to account for the possibility of such effects, through friendship networks.

graduates, and 0.6 percent in the university sample). These are sizable numbers considering that surveys suggest that social contacts in total account for about 40 percent of all jobs found by youths (Ungdomsstyrelsen 2005), confirming that recruitments through close family members in general, and parents in particular, make up a substantial chunk of young workers' job finding through social contacts.

The following rows of Table 1 show child and parent characteristics separately for those that find a job at a parent's plant and those that do not. The first part shows that it is more common to find employment at a father's plant than at a mother's plant.[21] The second part reiterates the educational differences and also shows that it is more frequent among vocational than academic high school graduates to find the first job a parent's plant.

Turning to individual characteristics, those who find jobs at a parent's plant are more often male. The impact of immigration status varies across levels of schooling; more educated immigrant youths are overrepresented among those finding the first job at a parent's first plant, whereas the pattern is the opposite for those with low schooling. The impact of GPA is consistent across the columns; graduates who find employment with their parents have, on average, lower grades. Young workers finding jobs at a parent's plant find these jobs faster than other graduates, and their initial employment are more often in the private sector. Looking at the patterns from the demand side, it is evident that parents with longer tenure, a higher wage, and those working in the private sector are more likely to see their child finding his or her first job within their plant.

The final row shows the county unemployment rate, indicating that graduates more often find jobs where the parents work when the unemployment rate is relatively high. In order to make this last point clearer, we have also calculated the fraction working with parents in the total stock of employees across all the years in our data and related this fraction to a time series of aggregate unemployment in Figure 1. It is evident that there is a clear co-movement between aggregate youth unemployment and the fraction working with parents.

Next, we turn to our attempts to identify the causal component of these associations.

5 Results

[21] The fraction do not sum to one since about 10 percent of those that do have a parent at their first job have both parents there.

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 (4). The parental hiring effect (γ) captures the excess probability of a graduate to find her first stable job at the parent's plant over the probability that someone from the same class without a parent in the plant works exactly there. To do so, we apply the transformation described in Section 2 to remove the fixed effect capturing the interaction of the exact education, location, time of graduation (i.e. the class) and the specific plant. An observation in the regressions is a combination between a class and a plant after the fixed effect has been removed.

Table 2 presents estimates of γ. Each column shows a separate estimate for each of the four education groups, with an aggregate estimate in the final column. All estimates are strongly positive and significant. This implies that graduating students are much more likely than their classmates to find jobs in the plant where one of their parents is employed. The estimated magnitude is 6 percentage points for the average parent and graduate. For the set of graduates who have two employed parents working in different plants, this suggests that the social ties to parents predict 12 percent of their destination plants compared to the classmates.[22] Indeed, predicting where a graduate finds her first job is extremely difficult using the observables classically available in most data sets and the (counterfactual) probability of working in the plant of a classmate's parent is very low (estimated to be around half a percent). The estimates of the model are therefore, in general, very close to the raw mean probabilities to start working in the parent's plant.[23]

The results of Table 2 show that the estimated effects of parent-child ties are particularly large for the low-educated. For graduates who enter the labor market without any post-compulsory school education we find that the probability of working in a specific plant is increased by 9 percentage points if a parent works there (again, from a baseline of half a percent). The effects are also fairly large for students graduating from Vocational (6.6 percent) or Academic (7.4 percent) high-schools but much lower for University graduates (1.8 percent).

[22] Note that this number is higher than the average probability of working with a parent (10 percent) for two reasons: The first, and most important, is that not all parents are employed. The second reason is that some parents are employed at the same plant. For statistics on both aspects, see Appendix A.

[23] Because it is essentially impossible to predict the employing plant of any graduate, the baseline probabilities captured by the fixed effects in equation (1) are very small, at least compared to the estimates of interest: The average plant has about 50 employees, thus, starting to work in any particular plant is very rare. Fortunately, our empirical model gives us the tools to measure both the realized outcomes ("association") and the counterfactual outcomes.

In the following subsections, we present a wide set of robustness checks which we believe validate that these sorting patterns are the result of a causal effect stemming from the parent's presence in the plant. Before turning to these exercises we will however make a detailed description of the variation in the patterns according to various characteristics of the involved agents.

In Table 3, we simultaneously analyze how the role of strong social ties varies with characteristics of the graduate, the parent, and the market. To this end, we expand our original framework (equation 1) so as to incorporate effects that may vary with characteristics of the graduate (*i*), the parent (*p*), the labor market (*l*) or the plant (*j*). This yields the following model:

$$(5) \qquad E_{i,j} = \beta_{c(i),j} + [\gamma^i X_i + \gamma^p X_{p(i)} + \gamma^l X_{l(j,t)} + \gamma^j] A_{i,j} + \varepsilon_{i,j},$$

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-specific network effect γ^j. We proceed as in Section 2 to get an expanded regression framework corresponding to equation (4):

$$(6) \qquad G_{cj} = \gamma_0 + \gamma^i \overline{X}_i^A + \gamma^p \overline{X}_{p(i)}^A + \gamma^l \overline{X}_{l(j,t)}^A + \gamma^j + u_{c,j}^G$$

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. Our *X*-variables correspond to the variables described in Table 1 above. The results of the regressions should be interpreted as conditional associations between the variables and the importance of family ties.

We estimate the models with and without plant-specific network effects (see Appendix B for details about estimation). The model with plant-specific network effects compares graduates from different graduating classes with parents in the same plant (possibly in different years), while accounting for plant-specific propensities to hire children of employees. Thus, when plant effects are included, identification comes from plants where more than one parent worked during the analysis period. The more than 700,000 contacts in our data are distributed over 160,000 plants which imply that each plant, on average, employ 4 to 5 parents of graduates over the 8 years we study. Less than 10 percent of the contacts are employed in plants with single links during the sample period within the full sample.

Results displayed in the Table 3 confirm that parental ties matter more for the less educated, even accounting for characteristics of the parent, of the plant, and of the labor market. This result even holds when comparing parents who work within the same plant. We also find that poor grades (low GPA) increase the magnitude of the effect. Moving from the top to the bottom of the grade distribution increases the effect of parents by nearly four percentage points holding the characteristics of the parent and the identity of the parent's plant constant. Similarly, we see that contacts are more relevant for youths with an immigrant background, except for the university graduates, although the impact is relatively weak (about one percentage point).

The results further show that links between fathers and sons matter the most, whereas links between fathers and daughters matter the least (to enter the father's plant). Links involving two females have a smaller effect than those involving two males (i.e. comparing mothers/daughters to fathers/sons), a result in line with Bayer, Ross and Topa (2008) who shows that residential networks are less important for females. We also present the baseline estimates separately for fathers and mothers, daughters and sons, in Online Appendix Table 1.

Table 3 also displays the impact of parental characteristics.[24] Let us stress again the nature of our analysis: we study if 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. Overall, the results yield support to the Montgomery (1991) model in the sense that well-attached (demand-side) workers appear to be more important: the effects are significantly larger for graduates with links to high-wage and long-tenured parents, even controlling for plant fixed effects.

We also find that social ties to low-educated parents increase the impact, which to some extent contrasts the picture emerging from the analysis of tenure and wages. One possible explanation for this, in particular since the effect is much less pronounced for university graduates, is that social ties may matter more for graduates who have the same education as their parents. Further results indeed show that strong ties are more likely to result in recruitments when parents share a (broad) field of study with their children. Thus, family links appear more relevant when formal skills also overlap.

[24] Previous versions estimated these effects separately for mothers and fathers, but the estimates were nearly identical.

Table 3 also shows that a high (county level) unemployment rate is positively related to the size of the effects of strong social ties. This result, in line with the descriptive patterns shown in Figure 1 for aggregate unemployment, reinforces the notion that strong ties are more important for children with poor labor market prospects, i.e. that strong ties are more powerful for workers in weak positions (see also Boorman, 1975 and Galeotti and Merlino, 2011). Interestingly though, this is one of the few heterogeneity-results where education matters; the estimated impact of unemployment is insignificant (and negative) for university graduates. The overall result also helps alleviate the concern that working with parents reflects an unobserved taste for a given plant. Indeed, it is not clear why such a taste should be time-varying. Furthermore, a taste-based explanation would be easier to align with a negative correlation between link-usage and unemployment, assuming that preferences for particular jobs are harder to satisfy in times when job access is rationed.

The estimated effects are clearly larger for parents working in the private sector. This result is perhaps expected because the private sector should be less constrained in its hiring practices, even though recruitments within the Swedish public sector tend to be informal and decentralized, leaving some scope for this type of practices (see e.g. Åslund and Skans, 2012).[25] This also suggests that using social ties are *profitable*, in some way, to these private firms.

Finally, we show how the effect varies with the "specificity" (defined below) of the type of occupation/education. More precisely, we examine whether effects for types of education that cater to a limited set of potential employers differ from the effects estimated for types of education where the receiving labor market is broader. Here, we restrict attention to vocational high-schools since the fields within this sample are very well-defined and closely related to associated occupations (e.g. masons, restaurant staff, telecom electricians, secretaries…). To measure how diverse the receiving market for each type of education is, we computed the number of plants that employ at least one worker with such an occupation-education in each municipality (using the full stock of employees in the 1995 data) and measure how the effects of strong social ties co-vary with this measure (controlling for the number of workers). The results displayed in the final row of Table 2 imply that strong social ties matter more for occupations that are dispersed across a larger number

[25] This result is important since it implies that changes in the identifying sample that alters the share of parents employed in the public sector change the results of the baseline model; this will be evident in the following subsection.

of plants.[26] Put differently, the less *specific* the destinations of a field is in terms of which plants may hire the workers, the more prevalent is the use of strong social ties. An interpretation in terms of information seems in order. Students may have more use for parents' help when the set of possible receiving firms is large which is also when plants have more use for matchmakers; i.e. when filling jobs that are less specific to them. Hiring the child by the parents' employer appear as an obvious solution, a natural focal point, to this coordination problem.

5.2 Robustness: unobserved match-specific skills and crowding-out

An interpretation of the results of Table 2 and 3 as causal effects of strong social ties requires that the probability that the graduate would have found employment within the parent's plant, in a counterfactual world where the parent does not work in this particular plant, is captured by the classmates' employment opportunities in the same plants. The estimates could potentially be affected by differences in skills or tastes within a given class which would contaminate our estimates if they are correlated with the needs of parents' plants *and* if their relevance for the employment decision is independent of the social tie. The fact that graduates may have hidden skills (either general skills or skills that are specifically relevant to the parent's firm) that are revealed through the social tie, should not be thought of as a confounder since it would not have affected the recruitment without the social tie. The idea that social ties may affect hiring decisions by revealing information about hidden skills is in fact the foundation of many standard models of employee referrals (e.g. Montgomery 1991 and Dustmann et al 2011). In order to address concerns that our indicators of social ties partly capture a confounding overlap in skills and firm-specific demand (i.e. skills that would have been relevant for the plant's hiring decision even if the parent had not worked there), we have performed a variety of robustness checks to see how the estimates change when we reduce the scope for such confounding processes.

The results of 12 numbered specifications are displayed in Table 4. Specification (1) replicates the baseline whereas Specifications (2) to (8) refer to sensitivity tests based on variations of the comparison group. For each specification we report the "Comparison Group", we provide the estimate, standard error, number of observations used, the raw probability that a graduate within the identifying sample works with his parent ("association"), the probability that the graduates in the comparison group (with no parent in the plant) start working in the same plants ("counterfactual"), and the estimate from the baseline within-class model using the same sample. Specifications (9) to

[26] We also control for the number of workers in each municipality having each type of education (again, in the year 1995). We have performed a wide set of robustness checks, using alternative ways to measure the specificity of education and the results all point in the same direction. Results are given in Online Appendix Table 3.

(12) instead address the potential for crowding-out of classmates' probability of entering plants where other graduates' parents are employed; these are discussed at the end of the subsection.

First, we address concerns about unobserved tastes by partitioning each class by the industry in which their parents work so that we only compare each graduate to other graduates with *parents employed in similar (same industry), but not identical, plants*.[27] The results shown in Table 4, Specification (2) are essentially similar to, albeit a little smaller than, the baseline results for the full sample. Importantly, the reduction in estimates is purely driven by a change in the identifying sample as can be seen from the baseline estimate for this sample.[28] We then performed a similar analysis by partitioning the class according to *the industry where the graduate finds employment*, and the results are again (Table 4, Specification 3) very similar to those of the baseline model for the same sample.

Another strategy is to restrict attention to those students that are most likely to share tastes or skills with the parent. Therefore, we re-estimated equation (4) restricting to graduates who received the *same education as their parent*.[29] We perform this test for three levels of aggregation of education categories, 1-digit, 2-digits, and 3-digits. Results are presented in Appendix B Table B1. Again, we find stable results, with slightly larger estimated effects, and less precision when we use 3-digits education categories. Restricting attention to children with the exact same education as their parent tries to capture the idea that children may be better informed of certain job characteristics thanks to the occupations of their parents. This analysis shuts down such "supply side" explanations since all children in a class with the exact same education as their parents are on equal footing. We would also like to analyze situations where parents have the same positions within the firms, but our data lack information on occupations. Therefore, we instead measured the effects within groups of parents with similar positions by dividing each class by the 2-digit industry and the *within-firm* wage quartile of the parents' firm and re-estimated our equation. The estimates (Table 4, Specification 4) are again very similar to the results for the full sample.

[27] When estimating this model we also need to allow for separate fixed effects depending on whether the father, the mother or both work in each industry. When estimating the baseline models for the same samples we also (for computational reasons) allow for separate fixed effects depending on the gender of the parent, but do not separate these according to the industry of the parent. We use a corresponding strategy for all other models where the classes are divided according to parental characteristics.
[28] The main difference stems from the fact that the identifying sample now contains a higher fraction of mothers within the public sector, where the effects tend to be smaller.
[29] We thank Raquel Fernandez and Daron Acemoglu for suggesting this procedure.

As a next step, we compare graduates with the same type of education and with parents in the same firm, but in different plants. The idea here is that this comparison group should capture any aspect related to the overlap between graduates' abilities and the firm specific production process. In this specification it is clear that the counterfactual is biased downwards if graduates can use the networks of their parents to enter other plants within the parent's firm but not the one in which the parent works (we return to this issue below). Thus, this specification should deliver a lower bound of the causal effect of the parental presence within a plant. In order to have a sample of sufficient size, we compare graduates within the same *type of class*, defined exactly as the class, but without conditioning on a specific graduation year.[30] Table 4, Specification (5), shows that the effects are only marginally smaller than those of the baseline estimates for the same sample, suggesting that within-firm sorting of family-specific abilities is of minor importance for our main results.[31] More important, it suggests that the presence of the parent within the actual plant is necessary.

Finally, and in the same spirit as just above, Specification (6) of Table 4 presents estimates when the control group comprises classmates whose parents used to work in the same plant. Hence, our sample is based on the combination of classes and plants where some classmates have current links to the plant, and others have lagged links (from previous employment spells). Again, the resulting estimate is likely to be downward biased if lagged links also matter for the employment patterns of youths (we return to this issue below). The estimated effect is, as expected, somewhat smaller than for the baseline model, the difference being less than a third (0.22 over 0.77) of the baseline estimate for the same sample. As we show below, the main reason for the relative difference comes from the university sample where lagged links are more important in relative terms.

Second, in an attempt to make the comparison and the control groups most similar, we changed the control group to only include classmates living in the same neighborhood, or with parents working in the same neighborhood and industry. (Naturally this reduced the sample dramatically for the university sample.) The estimates presented in the second panel of Table 4 specification (7) and (8) are, on average, reduced by a very small amount, and the pattern is stable across educational levels (not in the table). Clearly though, this attempt is likely to introduce concerns about residential sorting. We come back to this point later when analyzing weak ties directly.

[30] We only compare private sector plants within the same combination of firm/organization, (plant level) industry, and municipality.
[31] Here it should be noted that the effect from the baseline model is substantially smaller in the case where we allow for multi-plant organizations in the public sector, a pattern which is entirely driven by the fact that the restriction oversamples the public sector, where the effect is substantially smaller. Importantly though, the effects changes only marginally when changing the model in order to account for the organization of the parent.

Third, we changed the definition of the *timing* to the first job.[32] Our baseline specification compares all those within a class who find a job within a seven year window since graduation. As an alternative specification we only compare classmates who find jobs within a given 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 using only those finding a job in the same year instead of using the full set of classmates does not alter our results.[33] Results by year of first job are presented in Online Appendix Table 2.

Fourth, any threat to the validity of our control group, classmates, is a threat to our identification strategy. Indeed, if vacancies are rationed, it is possible that a graduate who gets hired by a parent "takes" a vacancy away from the classmates. If this happens our estimates will be upward biased. However, our conjecture is that this effect is likely to be small since, 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. We have nevertheless estimated the model separately by total numbers of hires (1, 2-5, 6-10, 11 or more) made by the plant in the year the graduate finds his or her job. Results are shown in Table 4, bottom panel.[34] 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 hiring more than 11 workers.

Overall, the results presented in this subsection suggest that the estimates are essentially identical to the base estimates if we restrict the control group to classmates who are similar in terms of parental or own industry preferences, or if we account for differences in educational similarity with parents, or for differences in parental hierarchical positions within the linked firms, or for geographical sorting. Furthermore, we have shown that it is, in general, *necessary for the parent to be present at the linked plant*; the effects are only marginally affected when using similar graduates whose parents work in other plants within the same firm as the comparison group, or using classmates whose parents have a previous employment history within the same plant. Although it is difficult to completely rule out the possibility that unobserved heterogeneity has an impact on the results, we find it reassuring that the results are robust to our treatment of the various dimensions of observed

[32] In fact, we performed an even more basic robustness check. We randomly allocated parents and children within a class and re-estimated our model. The coefficients of the baseline model were equal to zero for all education groups.
[33] An earlier version of the paper used this definition throughout, and all estimates remain virtually unchanged from the revision.
[34] Here we need to rely on the specification (discussed above) where we partition each class by the year of the first job.

heterogeneity and that the estimates suggest that the parent has to be present for the effect to be potent. Altogether we thus take the evidence as being consistent with the notion that strong social ties are the main driver of the baseline results presented in Table 2.

5.3 Weak social ties and the level of schooling

In order to contrast our main results which focus on the role of strong social ties as defined by parent-child links, we investigate the role of other links which could proxy weaker ties. In this analysis we follow in a wide set of recent studies relying on indicators of weak tie networks where the existence of a social tie cannot be known with certainty.[35]

In the first panel of Table 5[36] we show how classmates' parents affect the probability of working in a specific plant. In this specification we analyze how the probability of a first job is affected by class proximity (hence, working in the plant of a classmate's parent). The empirical strategy is similar to that of the main analysis, but the network indicator (*A* in equation (1)) in this case is equal to 1 when a classmate's parent works in the plant and the fixed effect which defines the comparison group comprises all combinations of school, field, and plant of a parent (rather than class and plants of a parent). The effects are therefore identified through differences in probability of working with parents of classmates relative to working with parents of students who graduated in a previous or a later cohort from the *same* field and the *same* school. The estimates are all very close to zero. Hence, classmates' parents do not, in general, play an important role in the job-finding process.

The second panel analyzes the role of proximity by splitting each class by neighborhood and analyzing whether it is more likely to start working with a neighboring classmate's parent than in the plant of other classmates' parents. Here we exclude parents and children who start working together and parents who work in another municipality, and (for computational convenience) restrict the sample to plants where only one parent within the class works. The results indicate small, but mostly significant, network effects arising from being neighbors. Most estimates are between a half and one percent, which is in the vicinity of the baseline probability of working with any given parent of another graduate from the same class.[37] Interestingly, in this case we do not see the clear pattern of diminishing network effects for graduates of higher levels of education. A child's own parent is 10-20 times as important as a neighboring classmate's parent for the least

[35] Examples include Bayer et al (2008) who use neighbors, and Dustmann et al (2011) who rely on workers sharing an ethnic origin.
[36] Note that we change the units of analysis to the level of the field-school, or class-year-neighborhood, which explains the much smaller sample sizes.
[37] See the row labeled *counterfactual* in Table 2

educated (comparing results in Tables 2 and 4), but less than 4 times as important for the university graduates. This is consistent with the presumption that the strength of a social tie matters more for weak graduates.

Third, we analyze the direct role of classmates (leaving parents' role aside). Here we estimate the difference in probability of finding the first job at the same plant as someone in the same class relative to those in other cohorts, but same field and school. It is important to note that this analysis suffers from some potential shortcomings. Most notably, we cannot isolate which agent is responsible for the match, in contrast with the rest of our analysis, and therefore we miss a clear identification strategy. Anyway, we find considerably smaller effect than for parents, and, again, we do not find the clear pattern of smaller effects for more educated youths.

Another strategy is to take *other plants within the firm of the parent* as a measure of plants to which the graduates are weakly, rather than strongly, linked. The key advantage of using this particular measure of weak ties in our case is that we can contrast the impact of strong social ties and weak social ties respectively within the same set of firms in a condensed way. To this end, we use graduates with parents in firms that have multiple plants operating in the same municipality and plant-level industry. The results are presented in the bottom panel of Table 5. The results show that the impact of parental presence in a firm on the probability of working in another plant within that firm (industry and municipality) is significant, but small. To make a clear comparison of the importance of parental presence, the final row of Table 5 shows the ratio of the baseline estimates (the effect on working in the parent's plant) and the weak tie estimates (the effect on other plants within the same firm) by level of schooling for the same set of firms. A clear message from this exercise is that tie strength is relatively more important for the low educated, the strong tie effect is more than six times as important as the "weak tie" (i.e. other plants) effect although, the latter, naturally, include a much wider set of potential plants for the compulsory schooling sample, whereas the strong tie impact is only twice as large as the weak tie effect for the university sample.

We have also used the plants where the parent *used to* work as an alternative indicator of weak ties. To this end, we first take the last observed previous plant in which the parent was employed. In a second step we check if the plant still existed when the graduate found his or her first job.[38] We

[38] To avoid a potential downward bias due to downsizing plants we also require that the plant recruited at least *someone* (not necessarily a graduate) during the relevant year. For computational convenience we drop cases where there are multiple links of different lags between the same class and plant.

then analyzed the probability of entering these plants relative to the classmates. The results are shown in Figure 2. The Figure shows point estimates corresponding from models corresponding to the baseline model, but where the actual plant is replaced by earlier plants of the parent, separately for the number of years since the parent worked there. On average, the probability of entering a parent's previous plant is 1 percentage point higher than for the classmates. For the same sample of youths, we get an estimate of 5 percent from the actual plant of the parent. The impact of previous plants dies out with time; and the impact of "fresh" links from the year before labor market entry is about twice as large as the impact of 7 year old links. We also find that the differences between current and past plants of parents are much smaller for the university educated graduates where the lagged effect of about one percent is fairly close to the effect of current plants (1.8 percent), whereas the differences for the compulsory school graduates are quite substantial (the 1–2 percent of the figure should be related to a baseline of 9 percent for current plants). The overall impression from all of these exercises is that our results consistently show that the use of weaker ties are independent of the level of schooling whereas stronger ties are substantially more important for the less educated.

5.4 Firm side responses; hiring and parents' wages

The empirical literature on labor market networks is largely silent on how the use of social ties or networks affects employers' recruitment frequencies or the wage setting of workers employed in such firms (an exception is provided by the very recent paper on a single firm by Brown, Setren, and Topa, 2012). Since our data provide information about well-defined links between workers and employers who can both be followed over time, we are able to produce a first set of results regarding these firm-side responses.

We start by estimating how plants' recruitments are affected by the graduation of a worker (graduate) who is linked to an existing employee (parent). Identification relies on within-plant variation over time in hires of graduates from a given school and field (by contrast with the time varying-definition of a class used in the previous model).[39] Figure 3 shows the probability of hiring a graduate of the same type as the graduating child over time, before and after the graduation year. Table B2 in Appendix B gives precise numbers and standard errors.[40] 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

[39] For computational convenience we exclude the few cases where there are multiple years during our sample period where there are links between a plant and the certain type of graduate.
[40] Note though that the constant in Table B2 correspond to the average of the entire before-period displayed in the figure.

expect since not all graduates find their first job immediately. Importantly, the models include the full effect of the tie, which includes any spill-over or crowding-out effects. The fact that there is no visible (or statistically significant) decline over time before graduation suggests that, in fact, employers do not postpone recruitments until the linked child graduates. As shown in Figure B1 in Appendix B, the rate of decline is rapid for university graduates (who find jobs fast, as indicated by Table 1 above) and slow for compulsory school graduates (who find jobs slowly).

An interesting follow-up question is whether links induce plants to hire more workers overall, or if they mainly redirect their hiring intentions. To analyze this question, we focus on small plants (at most 15 employees per year during the sample period) where the shocks to total networks are likely to be most pronounced. 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 graduation timing 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 presented in Figure 4 (see Table B2 in Appendix B for details) 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 that stronger networks to graduating students induce (at least small) plants to hire more graduates.[41] 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. This 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 notion 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 estimated effects of strong social ties do 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).

[41] 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).

Next, we turn to the cases where the social tie really results in the recruitment of a linked graduate and analyze the consequence for parental outcomes, more specifically wage growth. We use a similar identification strategy as above and relate parents' wage growth before and after the recruitment relative to other stable employees within the same plant. Stable employees are defined as those aged 35 and over with at least 5 years of tenure in order to give an appropriate baseline and in order to exclude the recruited children from the calculation. For the sample of stable workers (*i*) we estimate the following model:

$$\Delta lnW_{ijt} = \sum_{\tau-3}^{3} D_{ijt}^{\tau} + \delta X_{it} + \mu_{jt} + \varepsilon_{ijt.}$$

The model explains wage growth (for staying employees) by a set of indicators equal to one for parents in the three years pre and post- recruitment of the child in the establishment (*j*). The model also includes individual covariates (*X*) capturing gender, age (with square), education (7 dummies), tenure and a year-specific establishment fixed effects (μ). Results are presented in Figure 5; they are quite striking. Wage growth among the parents is above the mean wage growth within their establishment before the graduate is recruited, which is consistent with the picture emerging from the heterogeneity analysis, presented earlier. This extra wage growth does however stop exactly at the moment of recruitment of the graduate (child).[42] The pattern shown in the Figure suggests that parents either over-perform before the (possible) recruitment in order to provide a positive signal, or that child's recruitment is a substitute for their own wage increases. In any case, this result (as well as the results from the timing model) indicates very strong real firm side responses to the supply shocks induced by the link of employees to their graduating children.

5.5 Graduates' short and medium term outcomes

In this subsection we provide evidence on how the labor market entry process differs between graduates that find jobs at plants where the parents work and other graduates. We estimate three sets of regression models:

$$y_i = \varphi(E_{ij}A_{ij}) + \delta X_i + \alpha_{c(i)} + \varepsilon_i$$

$$y_i = \varphi(E_{ij}A_{ij}) + \delta X_i + \theta Z_i^c + \alpha_j + \varepsilon_i$$

[42] We have also analyzed the wage growth pattern of the other parent as well, and we do find some indications of falling wage growth around the time of the child's first job (suggesting a labor supply reaction), but without the sharp pattern around the time of hiring shown in Figure 5.

$$y_i = \varphi\big(E_{ij}A_{ij}\big) + \delta X_i + \alpha_{c(i)j} + \varepsilon_i$$

The variable of interest is the impact of being employed by a firm where a parent works i.e. E times A in the notation of equation (1). The first model includes class fixed effects and controls for the individual characteristics (X), grades, immigration status and gender. The second model uses entry-plant fixed effects and captures the characteristics of the class (Z) by educational level (26 categories), fields (17 categories) and years. The third model includes a fixed effect for each combination of plant, field, and school (not graduation year), thus accounting for match-quality up to aspects related to the combination of the plant, the field and the school.

As outcomes (y) we focus on the time to the first job, entry wages, "match quality" at entry (measured by an index of the fraction of graduates entering the 2-digit industry with that exact education), the probability of remaining in the plant after three years, the overall employment probability after three years, and wage growth during the first three years. Outcomes measured after three years are estimated on those that find a job within four years and we can only measure wage growth for those who are employed (in some firm) three years after entering their first job.

Results presented in the top panel of Table 6 demonstrate that graduates find their first jobs half a year faster than their classmates when these jobs are located at a parent's plant, a difference which does not appear to be driven by the characteristics of the employing plant. On the other hand, starting wages are lower for those who get their first job at a plant where a parent is employed. These results are consistent with the Bentolila et al. (2010) notion that the use of social contacts increases job finding, but that the human capital should come to less productive use and hence reduce entry wages.[43] The overall wage effects are however fairly small (four percent) in comparison with classmates but when including plant fixed effects we find that wages are in fact substantially (eight percent) lower than for those who found jobs at the same plants through other channels. Thus, within the firm, graduates who enter through their social ties receive a much lower entry wage than graduates who enter through an alternative channel. This implies that graduates who enter into their first jobs through parental contacts do so at plants that in general pay higher wages to entering graduates. The introduction of match effects does not alter our conclusions.

[43] In general, the literature contains a wide range of diverging results regarding the wage-impact of social contacts, e.g. Brown et al (2012) contain both positive and negative wage estimates depending on the type of jobs that are filled. Since Bentolila et al (2010) focus on entry wages for youths, their finding of a negative wage effect using data from various European countries and the US provide the closest comparison. An example of a positive (initial) wage effects is Dustmann et al (2010), who proxy networks by ethnic similarity among immigrants.

Finally, initial "match quality" appears to be lower for those who have a parent employed in the plant, both in comparison with classmates and in comparison with other entering graduates within the plant.[44] Here it is however important to note that we measure "match quality" by an index based on how common the match between field of education and industry is in general. A low match quality thus means that the outcome is less common, although not necessarily bad. (All these results are similar across low and high unemployment conditions).

Turning to medium-term outcomes, we find very strong effects on the probability to remain in the plant for at least three years. Given the high rates of mobility out of the entry jobs (only one third remains on average) the estimated effects of 12 percentage points (six when accounting for plant effects) are substantial. Again, the difference in estimates with and without plant fixed effects suggests that social ties are used more often in plants where entering graduates are more likely to remain for at least three years.

Since we are unable to identify quits, longer periods of employment at a firm have no clear cut interpretation in terms of quality of the match. However, we also find an increased probability of being employed three years later (around 2-3 percentage points), something which is clearly positive from the graduate's viewpoint. Our estimates also display evidence of higher wage growth over the three year period.[45] Overall, wage growth is substantial enough to overturn the lower starting wage.[46] When accounting for (entry) plant fixed effects, the data suggest that wages after three years are at par with those of graduates who enter the same plant through other means.[47]

Overall, these estimates show that young workers who enter the labor market through parental contacts do better in the short run by finding jobs faster (although at a somewhat lower starting wage and in industries less directly related to their education specialty), that they tend to enter firms that offer better conditions to entering workers overall (higher starting wages, less turnover), that they are much more likely to remain within these firms, and that the labor market outcomes after three years are more favorable both in terms of higher employment rates and higher wages.

[44] This effect is primarily driven by vocational high school graduates. Results by education are given in Online Appendix Table 5.
[45] The wage growth estimates are not conditional on staying in the plant
[46] Starting wage estimates are unchanged if we focus on the same sample as we use in the analysis of wage growth.
[47] Estimating the impact on log wages after three years gives insignificant estimates in the plant fixed effects model.

5.6 Productivity and profits

Although our analysis has so far scrutinized a number of reactions to strong tie recruitments on both the demand and supply side, we have still not addressed the relationship with firm performance measures. The question has two parts as illustrated by the Montgomery (1991) model which predicts that it is more profitable for productive firms to use referrals *and* that referrals increase firm profits. The second part (i.e. causality from networks to profits) is particularly difficult to address without data on individual productivity since any (potential) impact on firm- or plant-level profits will be heavily muted in all but the tiniest of plants or firms. However, below we do provide a tentative analysis of the association between the use of strong social ties and various firm performance measures.

We first re-estimate the "Heterogeneous Effect" model of Table 2 while including measures of firm-level performance in the regression (as well as dummies for observations with missing values on the performance measures). This model estimates if more productive firms are more likely to recruit graduating children of incumbent employees relative to their classmates. The results shown in Table 7 show that *firm-level productivity* (log of sales per worker) is only mildly related to strong-tie hiring whereas *firm-level profits* (as a share of total assets) are strongly related to strong-tie hiring, except for university graduates. This result also holds when including plant fixed effects. Hence, in years of higher profits, plants hire graduates with strong ties more intensively. Finally, the last panel of Table 7 examines the relation between the impact of strong ties and *plant-level productivity* which we only can measure within the manufacturing sector. Here, the relationship is (again) clearly positive: thus, more productive plants hire their employees' graduating children more intensively. Taken at face value the results thus suggest that strong ties are used more often when firms are more profitable and when (manufacturing) plants are more productive. This result is consistent with the result (see above) that graduates who enter the labor market through strong social ties do so at plants which, in general, pay higher wages to entering graduates.[48]

In order to take a more explicit account of the relationship between recruitments through strong social ties and firm performance, we also analyze the evolution of productivity and profits before and after the recruitment of a linked graduate, *in comparison with years when the same firm recruits graduates without parental-child links.* The analysis is done separately for firms with more or less than 50 employees since individual recruitments may affect the performance of small firms but are

[48] Derived from comparing the first two rows of Table 6.

unlikely to cause a change in larger ones (unfortunately, the smallest firms are only rarely sampled within our firm-performance data, preventing us from scrutinizing the effects in the smallest firms). The estimates are plotted in Figure 6. The top panel shows the relationship to productivity and the bottom panel focuses on profits. In all four graphs, a common pattern emerges: in years preceding entry (including the year of entry) productivity/profits increases and are maximal one year after entry of the employee's child. The "effect" is significantly different from zero for large firms and close to significance for profits in small firms. Given the presence of a productivity impact in the larger firms, a straightforward interpretation is one of "reverse" (from the network side) causality: firms on a positive profit-path appear to be more inclined to recruit graduates through strong social ties. Overall, it is clear that these results should be interpreted with caution. But even with this caveat in mind it seems clear that the overall picture is one where strong tie recruitments, if anything, appear to be positively associated with various firm-side performance measures.

6 Conclusion

In this article, we examine the role played by parental networks at the moment of entry on the labor market. We show that young workers frequently find their first stable job in the plant of a parent. This is particularly true for the low skilled, when unemployment is high and for those with an occupational training into fields which cater for a dispersed labor market where information about suitable jobs is likely to be costlier to acquire. The youths appear to benefit in terms of shorter transitions into the first jobs and better labor market outcomes at a few years horizon; benefits that should be particularly useful in the high unemployment years when the social ties also come to their most frequent use. Although we do not analyze it explicitly, it seems fair to assume that the parents benefit from the exchange in terms of the increased financial independence on behalf of the youths.[49]

A related question is why the firms accept to hire the struggling youths. Our analysis shows that firms primarily do so if the parents perform well (high-wage and tenure), suggesting that firms use parental quality as a signal of youth quality. These results are very much in line with the Montgomery (1991) notion that firms screen entering workers through their links to incumbent workers. In addition, we find that the presence of links to entering graduates induces small firms to recruit more workers and that more productive firms rely more heavily on family networks. A result

[49] We uncover evidence that finding a job is positively associated with leaving the family nest. Due to data limitations, the analysis cannot be pursued much more.

in apparent disagreement with the Montgomery (1991) model is that these entering workers have lower observed skills, even though those contacts could matter in cases where the unobserved skills revealed through family ties are relatively favorable.[50] The result which disagrees most strongly with the Montgomery model is however that the wage growth of incumbent workers decline after the recruitment of the linked graduate. Although one could hypothesize that parents strategically exert more effort in the pre-graduation years to signal productivity, it seems clear that an opposite wage pattern would have been more in line with the original formulation of the Montgomery model.[51]

However, there is no reason to believe that one model applies to all types of social ties or all types of workers. In fact, there are many reasons to believe the contrary. As we show, strong ties matter relatively more for the low skilled. Indeed, low-educated entrants are likely to be deprived of weak ties that university graduates have been able to forge along their education, social life, social, and family contacts. The price they pay is entry in jobs that do not exactly match their training, and low entry wages.[52] But, the benefits appear to be multiple: shorter first search, entry in high-wage high-productivity firms, increased stability and wage growth.

Because our data allow us to observe how firms and their workers behave when networks are available, we believe our strategy is a good starting point when opening the black box of within-firm human relations. Indeed, our analysis uncovers a number of firm-side regularities which have never been previously documented. But for the purpose of this article, we have focused exclusively on entering inexperienced workers, with a focus on the strong social ties which we show to be particularly important for this group of workers. As a consequence, it has been outside of the scope of this article to document the extent to which the firm-side regularities we document differ across experienced and inexperienced workers. An important avenue for future research should therefore be to push further towards a comprehensive documentation of how network recruitments relate to firm performance and overall recruitments for different types of workers.

[50] See Hensvik and Skans (2013) for an analysis which contrasts observable and unobservable skills.
[51] Many of the results are also in line with the Heath (2013) model of monitoring. In particular, the high wage position of the demand side agent, the low observable characteristics but strong wage growth of the entering worker, and the apparent presence of a wage penalty for the parent fits well with the logic of the model. However, both the observed relationship to the productivity of the firm and, in particular, the apparent role played market uncertainty (unemployment and educational specificity) seem to point more in the direction of models where ex ante uncertainty is a key motivation for the use of social networks in the recruitment process.
[52] Bentolila et al. (2010) argues that the use of networks can be welfare reducing by distorting the occupational choices of youths.

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Appendix A: Data

A1: Parents' employment and 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[53] 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.[54]

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, 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.[55] 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").[56] The field codes are provided with a four digit "hierarchical" structure, so that fields can be described at different levels of precision.[57] 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).

[53] Defined as the wage paid to janitors that are employed by municipalities.
[54] See also Skans, Edin, Holmlund (2009).
[55] We relax c) when the set of workers is identical between the two years in the two plants.
[56] We transform codes from the old system to sun 2000 by means of a matrix provided by Statistics Sweden.
[57] The fourth digit is actually a letter, in order to provide a higher level of detail when needed.

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 just over 400 types of university educations within our analysis sample (see Table A1).

In the case of high schools we proceed similarly, and obtain 132 different vocational educations and 29 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 have started high school but dropped out, but we do not know what kind of training they may have received there. We treat members of this group as unskilled, with no field of specialization.

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.

Table A1: Classes, Fields and Parents' Employment

	Compulsory	Vocational	Academic	University	All
Number of fields	1	132	29	404	566
Mean class size	18.5	28.1	40.6	41.9	33.2
Father missing	0.025	0.015	0.013	0.029	0.019
Mother missing	0.004	0.002	0.002	0.018	0.006
Father Employed	0.671	0.754	0.799	0.683	0.739
Mother Employed	0.637	0.703	0.777	0.712	0.716
Father emp. in known plant	0.578	0.645	0.709	0.604	0.644
Mother emp. in known plant	0.563	0.616	0.710	0.655	0.643
At least one parent employed in a known plant	0.760	0.810	0.863	0.786	0.812
N (total)	96,936	263,111	191,873	154,264	706,184
N - used sample (with at least one parent emp in known plant)	73,712	213,096	165,505	121,269	573,582

Note: Restriction on “known plants” requires that the plant has a known identifier (see the text for details) and that the parent is an employee (not self employed), and that the plant is not within the agriculture or forestry sector.

Table A2: 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.

Appendix B: Additional Figures and Tables

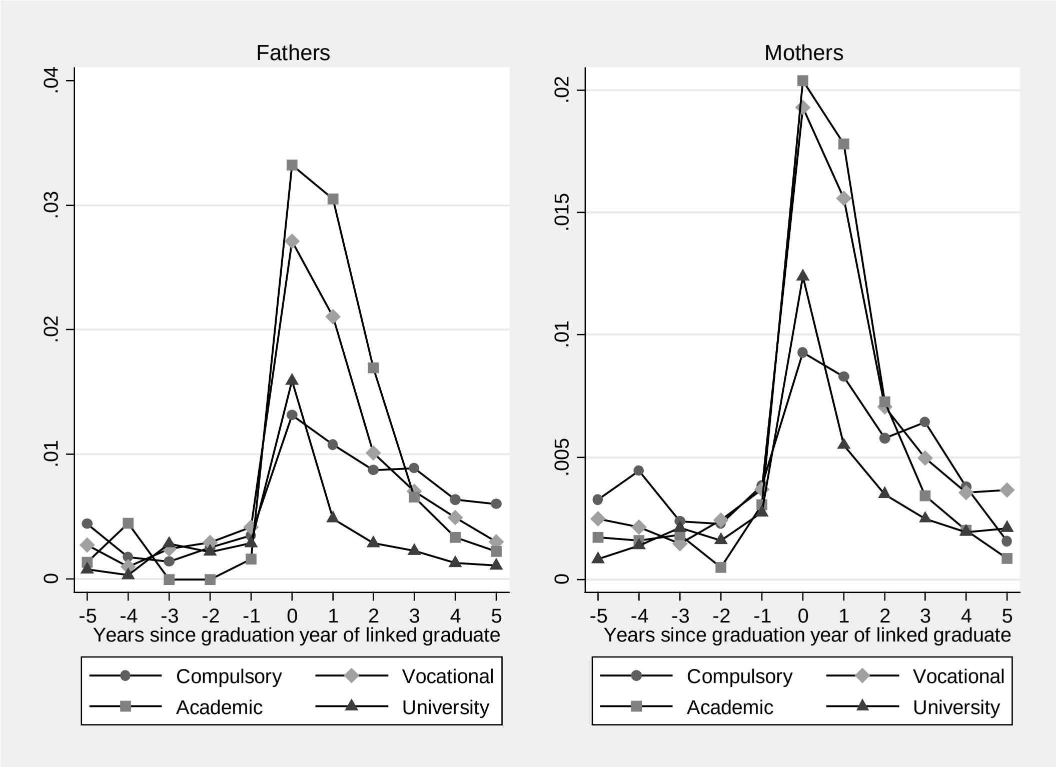

Figure B1 Fraction of graduates hired by a maternal-linked plant before and after graduation of linked graduate, by level of education

Table B1: Parental Networks Effects, by Similarity of Educational Field between Parent and Child

	Vocational high school	Academic high school	University degree
Same 1-digit field			
$\hat{\gamma}$	0.108	0.104	0.047
(s.e.)	(0.002)**	(0.001)**	(0.001)**
N	46,655	57,555	31,281
Same 2-digit field			
$\hat{\gamma}$	0.123	0.105	0.054
(s.e.)	(0.002)**	(0.001)**	(0.002)**
N	29,539	51,009	23,199
Same 3-digit field			
$\hat{\gamma}$	0.135	0.100	0.075
(s.e.)	(0.004)**	(0.002)**	(0.004)**
N	8,338	35,749	7,900
Different 1-digit field			
$\hat{\gamma}$	0.059	0.068	0.013
(s.e.)	(0.001)**	(0.001)**	(0.000)**
N	253,968	188,004	148,995
Different 2-digit field			
$\hat{\gamma}$	0.060	0.068	0.013
(s.e.)	(0.001)**	(0.001)**	(0.000)**
N	269,903	194,053	157,177

Note: An observation is a combination of class and plant. 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. Similarity of educational fields are based on the Swedish "SUN2000" adaption of the ISCED classification. Standard errors are cluster-corrected for dependencies within class. ** (*) Significant at the 1 (5) % level.

Table B2: 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)	0.019	0.013	0.030	0.023	0.031	0.024
	(0.001)**	(0.001)**	(0.001)**	(0.001)**	(0.001)**	(0.002)**
GY+1	0.013	0.009	0.024	0.016	0.024	0.021
	(0.001)**	(0.001)**	(0.001)**	(0.001)**	(0.001)**	(0.002)**
GY+2	0.006	0.004	0.012	0.006	0.010	0.007
	(0.001)**	(0.000)**	(0.001)**	(0.001)**	(0.001)**	(0.002)**
GY+3	0.004	0.002	0.006	0.003	0.002	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)**	(0.001)*	(0.001)	(0.001)**	(0.001)	(0.002)
GY+5	0.000	0.000	0.000	-0.001	-0.003	-0.003
	(0.000)	(0.000)	(0.001)	(0.001)*	(0.002)*	(0.002)
Constant	0.002**	0.002**	0.007	0.005	0.049	0.066
(average plant fixed effect)	(0.000)	(0.000)	(0.001)**	(0.001)**	(0.001)**	(0.002)**
Sum of effects (0-3)	0.042	0.028	0.072	0.048	0.068	0.053
	(0.002)**	(0.001)**	(0.002)**	(0.002)**	(0.004)**	(0.004)**
Sum of effects (0-5)	0.044	0.029	0.075	0.046	0.065	0.048
	(0.002)**	(0.002)**	(0.003)**	(0.002)**	(0.006)**	(0.006)**
Sample	All plants	All plants	**Small (< 16 employees) plants**			
N	2,614,984		477,670		477,670	
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 year before. 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 four last columns. Dependent variable in the third and fourth column 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. Dependent variable in the two last 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 sum of the GY to GY+5 estimates. **(*)Significant at the 1(5) % level.

Online Appendix Tables

Online Table 1: Parental Network Effect on Probability of Finding the First Job in a Specific Plant

	Compulsory school	Vocational high school	Academic high school	University degree	All
Any parent in plant					
All					
$\hat{\gamma}$	0.091	0.066	0.074	0.018	0.061
(s.e.)	(0.001)**	(0.001)**	(0.001)**	(0.000)**	(0.000)**
N	99,609	298,921	240,204	181,040	819,774
Only males					
$\hat{\gamma}$	0.103	0.076	0.085	0.021	0.073
(s.e.)	(0.001)**	(0.001)**	(0.001)**	(0.001)**	(0.000)**
N	56,544	177,063	121,136	70,264	425,007
Only females					
$\hat{\gamma}$	0.075	0.051	0.063	0.017	0.048
(s.e.)	(0.001)**	(0.001)**	(0.001)**	(0.000)**	(0.000)**
N	43,716	121,399	123,589	107,369	396,073
Father in plant					
Only males					
$\hat{\gamma}$	0.153	0.117	0.127	0.032	0.112
(s.e.)	(0.002)**	(0.001)**	(0.001)**	(0.001)**	(0.001)**
N	29,518	93,594	63,469	34,020	220,601
Only females					
$\hat{\gamma}$	0.059	0.033	0.064	0.012	0.040
(s.e.)	(0.002)**	(0.001)**	(0.001)**	(0.001)**	(0.000)**
N	22,247	64,767	64,882	53,058	204,954
Mother in plant					
Only males					
$\hat{\gamma}$	0.068	0.045	0.061	0.015	0.047
(s.e.)	(0.002)**	(0.001)**	(0.001)**	(0.001)**	(0.000)**
N	28,113	88,786	62,326	37,485	216,710
Only females					
$\hat{\gamma}$	0.104	0.075	0.074	0.023	0.064
(s.e.)	(0.002)**	(0.001)**	(0.001)**	(0.001)**	(0.001)**
N	21,679	60,730	63,831	56,953	203,193

Note: Estimates of parental network effects. An observation is a combination of class and plant. Weighted by the number of graduates with parents in each 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.

Online Table 2: 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								
g	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	8,163	8,296	7,006	4,913	3,178
Vocational								
g	0.089	0.075	0.089	0.081	0.060	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	56,173	51,018	21,762	12,091	5,649	2,624	1,283	608
Academic								
g	0.133	0.086	0.079	0.071	0.056	0.038	0.038	0.023
(s.e.)	(0.002)**	(0.001)**	(0.002)**	(0.003)**	(0.004)**	(0.004)**	(0.007)**	(0.008)**
N	36,449	45,550	23,818	10,931	4,434	1,901	810	386
University								
g	0.024	0.012	0.022	0.024	0.020	0.004		
(s.e.)	(0.001)**	(0.001)**	(0.002)**	(0.004)**	(0.006)**	(0.004)		
N	51,414	27,637	4,176	1,244	556	250		
Mothers								
Compulsory								
g	0.196	0.094	0.089	0.096	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								
g	0.067	0.056	0.053	0.042	0.038	0.032	0.035	0.036
(s.e.)	(0.001)**	(0.001)**	(0.002)**	(0.002)**	(0.003)**	(0.004)**	(0.005)**	(0.007)**
N	53,505	50,445	22,345	12,644	5,947	2,797	1,382	668
Academic								
g	0.105	0.061	0.048	0.043	0.031	0.035	0.028	0.018
(s.e.)	(0.002)**	(0.001)**	(0.001)**	(0.002)**	(0.003)**	(0.004)**	(0.005)**	(0.006)**
N	36,242	46,516	24,784	11,626	4,784	2,086	912	437
University								
g	0.023	0.016	0.011	0.011	0.006	0.000		
(s.e.)	(0.001)**	(0.001)**	(0.002)**	(0.003)**	(0.003)*	(0.000)		
N	57,074	30,618	4,728	1,448	683	281		

Note: Estimates of parental network 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 at least 100 observations are shown. Standard errors are cluster-corrected for dependencies within class. ** (*) Significant at the 1 (5) % level.

Online Table 3: Estimated Parental Network Effect by Municipality/Occupation (only Vocational High School)

	a	b	c	d
1000s of employing plants	0.026	0.027	0.068	0.040
	(0.009)**	(0.007)**	(0.013)**	(0.006)**
1000s of workers	-0.010		-0.017	-0.013
	(0.003)**		(0.004)**	(0.003)**
Constant	0.063	0.061	0.061	0.062
	(0.001)**	(0.001)**	(0.001)**	(0.001)**
N	3,228	3,228	3,228	3,228
Municipality fixed effects	No	Yes	Yes	Yes
Size weights	No	No	No	Yes

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 (unweighted average # of plants is 78, weighted average is 228, max is 3893). Network effects (i.e. the dependent variable) is estimated using all years (the same model as in table 2). ** Significant at the 1 % level.

Online Table 4: Short and Medium Term Outcomes for Graduates, Plant Fixed Effects Models, by Education

				Outcomes after three years (only if first job within 4 years)		
	Years to first job	ln(Starting wage)	Initial "match quality"	Same plant	Employed	Earnings growth (3 years)
Compulsory	-0.587	-0.064	0.000	0.094	0.044	0.030
	(0.035)**	(0.006)**	(0.000)*	(0.012)**	(0.012)**	(0.016)
Vocational HS	-0.225	-0.067	-0.035	0.059	0.024	0.065
	(0.013)**	(0.003)**	(0.002)**	(0.005)**	(0.004)**	(0.005)**
Academic HS	-0.350	-0.105	-0.004	0.044	0.010	0.088
	(0.014)**	(0.004)**	(0.000)**	(0.005)**	(0.006)	(0.007)**
University	-0.070	-0.134	-0.013	0.044	-0.003	0.100
	(0.021)**	(0.009)**	(0.005)*	(0.011)**	(0.008)	(0.012)**

Note: Estimated graduate-level effects of finding the first job at a plant where a parent works. The model includes plant fixed effects as well as graduation year dummies. All regressions control for immigration status, gender and GPA (except for university graduates). "Match quality" refers to an index of the fraction of all graduates by exact type of education that enters the 2-digit industry. Data are for graduates 1988-1995. ** (*) Significant at the 1 % (5 %) level.

Figures

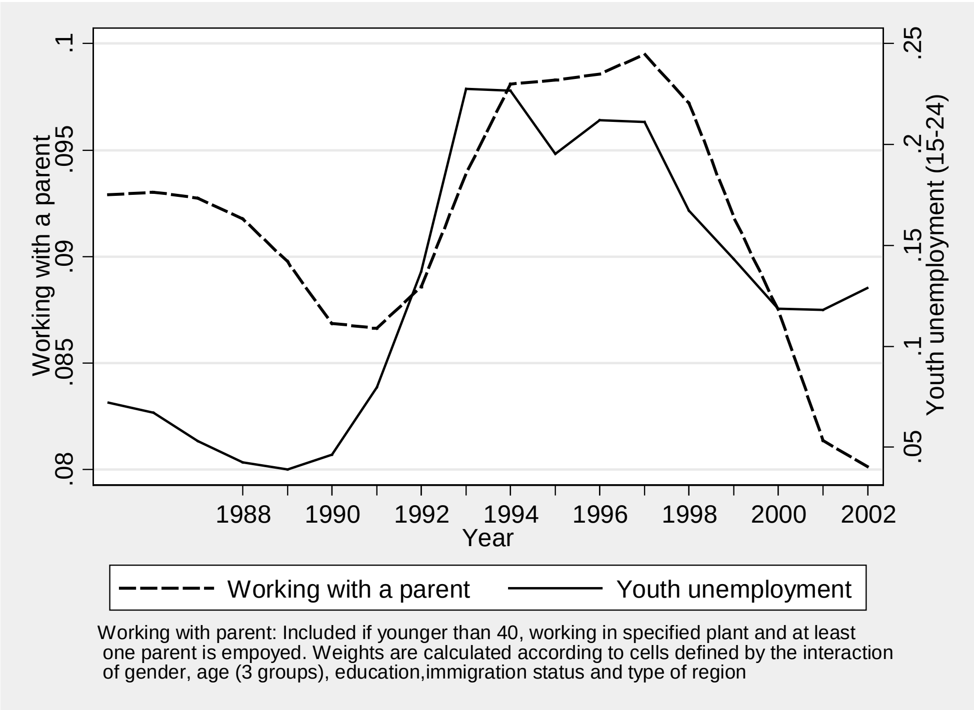

Figure 1: Youth unemployment and annual measures of fraction working with parents.

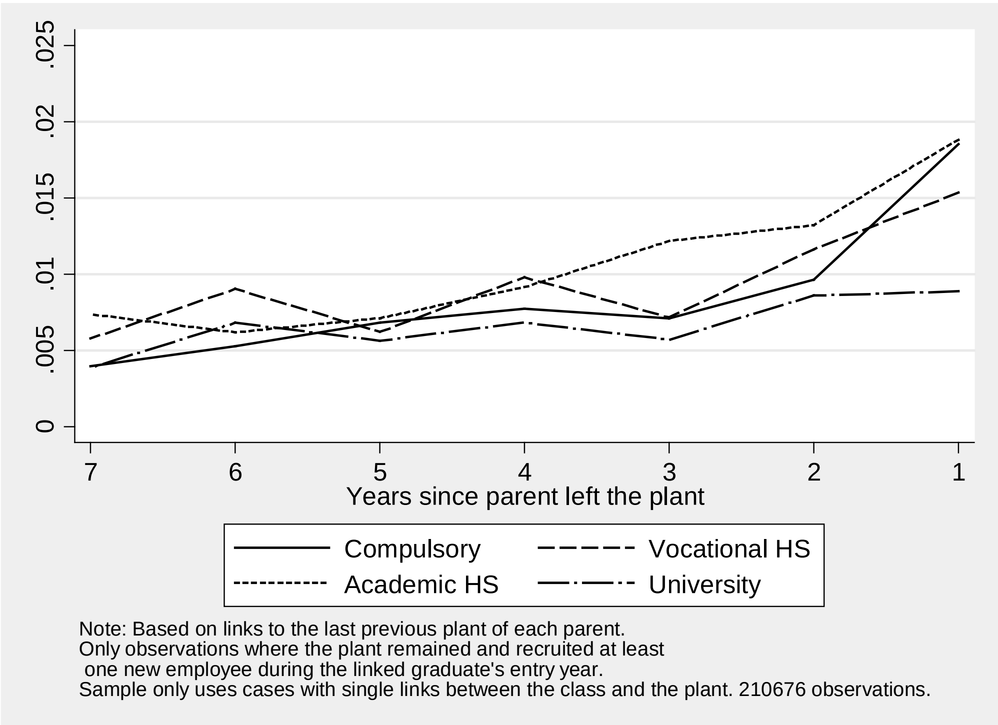

Figure 2 Estimated network effects; previous plants of the parent, by time since separation and the graduate's level of schooling.

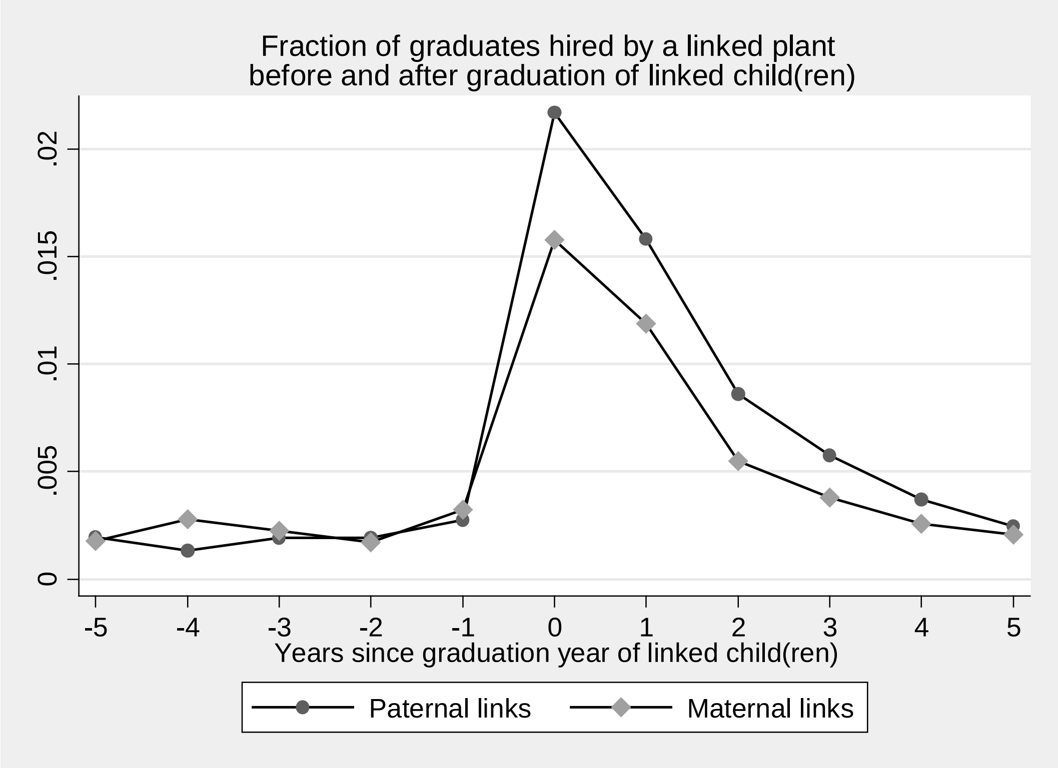

Figure 3 Fraction of graduates of a certain type hired by a linked plant before and after graduation of linked graduate of that type.

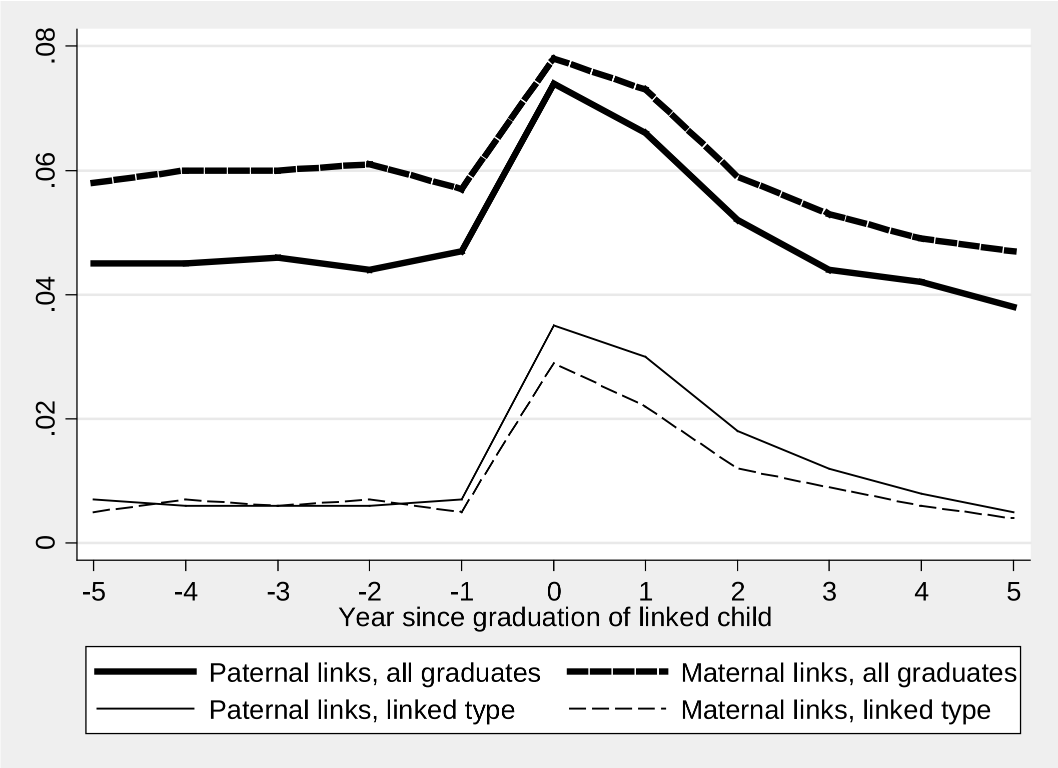

Figure 4 Number of graduates overall and of the graduate's type hired by a linked plant before and after graduation of linked graduate – small plants.

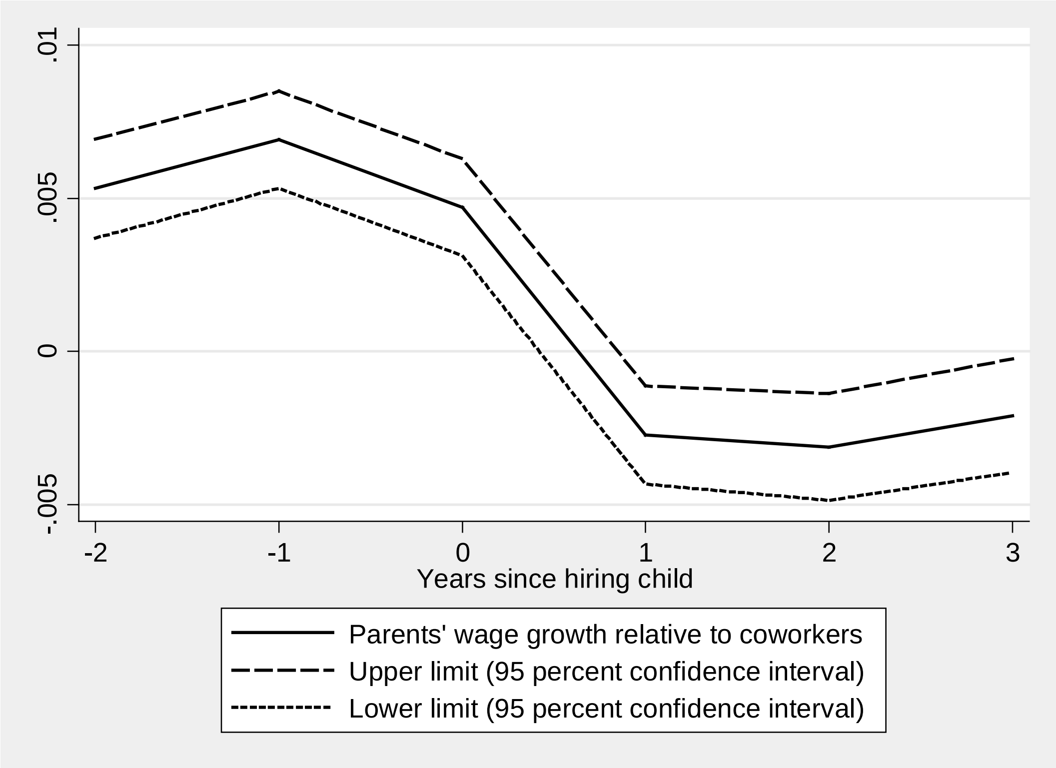

Figure 5 Wage growth (log difference) of recruiting parents relative to other stable co-workers before and after the plant hires a graduating child.

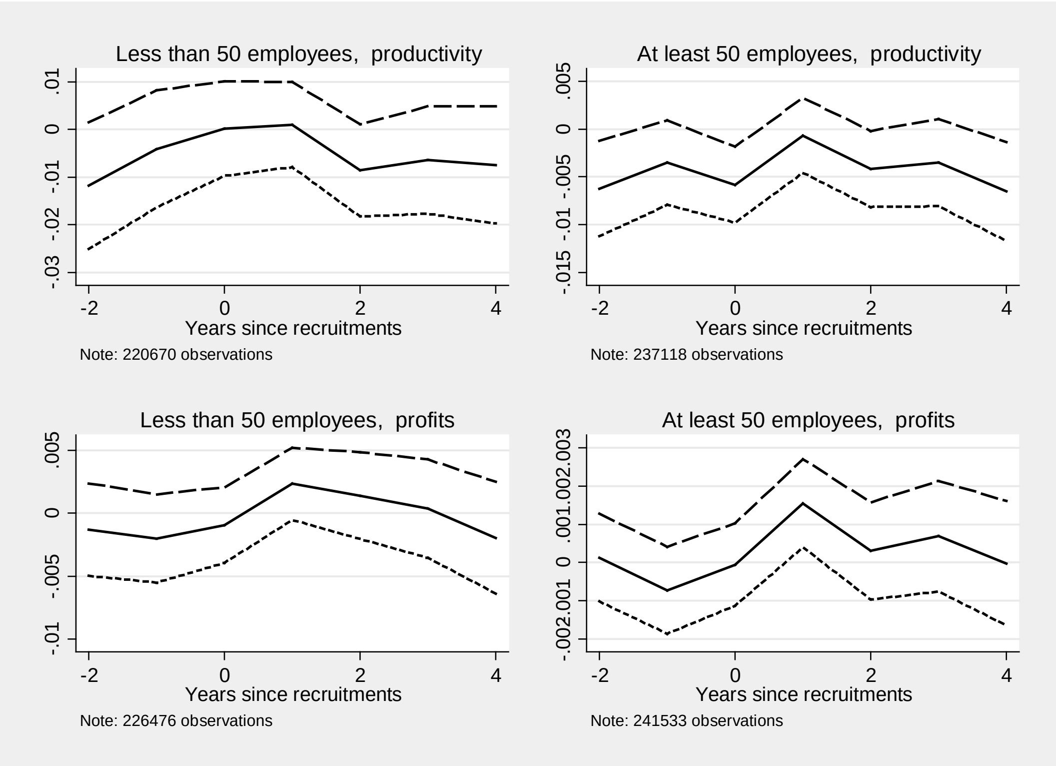

Figure 6 Productivity and profits before and after recruiting linked graduates, relative to recruitments of non—linked graduates, conditional on firm fixed effects and year dummies.

Tables

Table 1: Descriptive Statistics

Schooling of graduate:	Compulsory		High school		University		All graduates	
Parent at first job?:	Yes	No	Yes	No	Yes	No	Yes	No
Fraction, by schooling	0.140	0.860	0.115	0.885	0.032	0.968	0.101	0.899
	--------------------- *Averages by column* ---------------------							
Father at first job	0.641	0	0.655	0	1	0	0.644	0
Mother at first job	0.476	0	0.465	0	1	0	0.475	0
Schooling								
Compulsory	1	1	0	0	0	0	0.179	0.123
Vocational HS	0	0	0.535	0.567	0	0	0.403	0.368
Academic HS	0	0	0.465	0.433	0	0	0.351	0.282
University	0	0	0	0	1	1	0.067	0.228
Demographics/Grades								
Female	0.362	0.451	0.377	0.465	0.568	0.601	0.387	0.494
Immigrant	0.047	0.052	0.033	0.032	0.029	0.023	0.035	0.032
GPA (1-5)*	2.601	2.701	3.042	3.093			2.962	3.029
First job								
Years until first job	2.906	3.741	1.070	1.464	0.463	0.654	1.357	1.560
In private sector	0.806	0.786	0.744	0.716	0.463	0.380	0.736	0.645
Father's:								
Tenure**	5.180	4.861	4.729	4.281	4.989	4.572	4.824	4.414
Log Wage	10.069	10.048	10.171	10.144	10.306	10.274	10.162	10.161
Private sector	0.850	0.688	0.801	0.646	0.653	0.565	0.800	0.633
Mother's:								
Tenure**	4.010	3.934	3.915	3.701	4.604	4.248	3.981	3.859
Log Wage	9.739	9.722	9.766	9.749	9.845	9.827	9.767	9.764
Private sector	0.579	0.364	0.512	0.342	0.306	0.279	0.508	0.329
County Unemployment***	0.052	0.051	0.051	0.048	0.052	0.050	0.051	0.049
N (graduates)	10,322	63,390	43,494	335,107	3,890	117,379	57,706	515,876

Note: Descriptive statistics of the used sample of graduates between 1988 and 1995 finding a job within 7 years of graduation. The second and third columns are pooled across vocational and academic high school. The final two columns present averages for graduates from compulsory schooling, high school and universities. *GPA is not available for the university sample. **Tenure is truncated at 1985. ***Unemployment is from the Labour Force Surveys calculated according the official definition during the relevant period (roughly 2 percentage points lower than OECD measures, partly by excluding unemployed students).

Table 2: Estimated Parental Network Effect on Probability of Finding the First Job in a Specific Plant

	Compulsory school	Vocational high school	Academic high school	University degree	All
Average effect					
$\hat{\gamma}$	0.091	0.066	0.074	0.018	0.061
(s.e.)	(0.001)**	(0.001)**	(0.001)**	(0.000)**	(0.000)**
N	99,609	298,921	240,204	181,040	819,774
Detailed statistics:					
Association (RA)	0.0959	0.0720	0.0767	0.0207	0.0655
Counterfactual (RA-)	0.0047	0.0063	0.0030	0.0025	0.0043
N	84,103	260,589	214,976	154,521	714,189

Note: Estimates of parental network effects. An observation is a combination of class and plant. 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(5) % level. “Association” refers to the fraction employed by a parent’s plant and the “counterfactual” is the fraction of classmates hired by a parent’s plant.

Table 3: Heterogeneity - How the Estimated Parental Network Effect Varies with Characteristics of Parents, Graduates and the Market

Heterogeneous Effects	Compulsory	Vocational	Academic	University	All	All
						Plant-specific
		No plant-specific network effects				***network effect***
Father and son	0.071	0.052	0.054	0.013	0.050	0.048
(ref: Mother and son)	(0.003)**	(0.002)**	(0.002)**	(0.001)**	(0.001)**	(0.001)**
Father and daughter	-0.020	-0.021	-0.010	-0.003	-0.012	-0.012
	(0.002)**	(0.001)**	(0.001)**	(0.001)**	(0.001)**	(0.001)**
Mother and daughter	0.050	0.036	0.023	0.008	0.029	0.028
	(0.003)**	(0.001)**	(0.001)**	(0.001)**	(0.001)**	(0.001)**
GPA (1-5, ascending)	-0.012	-0.007	-0.005		-0.009	-0.007
	(0.002)**	(0.001)**	(0.001)**		(0.001)**	(0.001)**
Immigrant background	0.011	0.006	0.011	-0.001	0.006	0.002
	(0.003)**	(0.002)**	(0.002)**	(0.001)	(0.001)**	(0.001)*
Parent's tenure	0.001	0.002	0.002	0.001	0.002	0.001
	(0.000)**	(0.000)**	(0.000)**	(0.000)**	(0.000)**	(0.000)**
Parent's log wage	0.046	0.032	0.039	0.007	0.029	0.044
	(0.003)**	(0.002)**	(0.002)**	(0.001)**	(0.001)**	(0.001)**
Parent compulsory ed.	0.023	0.020	0.014	0.002	0.015	0.006
(ref. high school)	(0.002)**	(0.001)**	(0.002)**	(0.001)	(0.001)**	(0.001)**
Parent tertiary ed.	-0.031	-0.023	-0.026	-0.009	-0.019	-0.011
	(0.002)**	(0.001)**	(0.001)**	(0.001)**	(0.001)**	(0.001)**
Same educational field		0.030	0.007	0.028	0.021	0.023
(parent and graduate)		(0.002)**	(0.002)**	(0.002)**	(0.001)**	(0.001)**
Unemployment	0.126	0.270	0.221	-0.012	0.182	0.107
(county)	(0.045)**	(0.020)**	(0.023)**	(0.013)	(0.013)**	(0.017)**
Private sector	0.074	0.047	0.056	0.006	0.044	0.009
	(0.002)**	(0.001)**	(0.002)**	(0.001)**	(0.001)**	(0.004)*
#Potential receiving plants		0.010				
by field and municipality		(0.003)**				
N	84,103	260,589	214,976	154,521	714,189	714,189

Note: An observation is a combination of class and plant. 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 (5) % level. Regression coefficient from interacted network effects, dependent variable is G of equation (4). Each column is a separate regression.

Table 4: Parental Networks Effect on Probability of Finding the First Job in the Plant of the Parent, *Robustness*

Comparison Group:	Classmates (baseline, see Table 2)	Only classmates with parents in same industry	Only classmates going to the same industry	Only classmates with parents in same industry and within-firm wage quartile
Specification N°:	(1)	(2)	(3)	(4)
$\hat{\gamma}$	0.061	0.049	0.049	0.051
(s.e.)	(0.000)**	(0.000)**	(0.000)**	(0.001)**
N	819,774	444,677	537,192	261,901
Association (RA)	0.0655	0.0535	0.0540	0.0568
Counterfactual (RA-)	0.0043	0.0044	0.0111	0.0054
Baseline est., by sample	0.061 (0.000)**	0.049 (0.000)**	0.049 (0.000)**	0.052 (0.001)**
Comparison Group:	Graduates from same school/field with a parent in another plant within the same private	Graduates from same class, relative to classmates whose parents used to work in	Only classmates living in the same neighborhood	Only classmates with parents in same industry and neighborhood
Specification N°:	(5)	(6)	(7)	(8)
$\hat{\gamma}$	0.074	0.055	0.065	0.053
(s.e.)	(0.004)**	(0.003)**	(0.000)**	(0.002)**
N	7,161	17,125	345,428	33,848
Association (RA)	0.0984	0.1018	0.0723	0.0671
Counterfactual (RA-)	0.0248	0.0465	0.0078	0.0142
Baseline est., by sample	0.079 (0.004)**	0.077 (0.002)**	0.067 (0.000)**	0.054 (0.001)**
Plant hiring:	Plant hires 1 worker	Plant hires 2-5 workers	Plant hires 6-10 workers	Plant hires 11+ workers
Specification N°:	(9)	(10)	(11)	(12)
$\hat{\gamma}$	0.077	0.062	0.055	0.071
(s.e.)	(0.001)**	(0.001)**	(0.001)**	(0.001)**
N	65,188	152,482	98,498	340,693
Association (RA)	0.0771	0.0626	0.0557	0.0806
Counterfactual (RA-)	0.0001	0.0006	0.0012	0.0098

Note: Estimates of parental network effects. An observation is a combination of class (or part of a class) and plant. 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. “Association” refers to the fraction employed by a parent’s plant and the “Counterfactual” is the fraction of classmates (or division of class, depending on specification) hired by a parent’s plant. Baseline estimate is the estimate of equation (4) based on the restricted sample, induced by the restricted comparison group, used in the column. Classes are divided by the characteristics in specifications (2)-(8). A firm in (5) is a unique combination of firm identifier, municipality of plant, and industry of plant. Specifications (9)-(12) use the baseline model but classes are divided by year of first job, which are linked to the hiring frequency of the plants in that year. Standard errors are cluster-corrected for dependencies within class. **Significant at the 1 % level.

Table 5: Weak Tie Network Effects

	Compulsory school	Vocational high school	Academic high school	University degree	All
Classmates' parents (relative to parents' of other cohorts, same field and school)					
Fathers					
$\hat{\gamma}$	-0.001	0.001	0.000	0.000	0.000
(s.e.)	(0.001)*	(0.001)	(0.002)	(0.001)	(0.001)
N (classes)	6,202	13,772	7,713	5,560	33,247
Mothers					
$\hat{\gamma}$	0.002	0.002	0.001	0.001	0.001
(s.e.)	(0.001)*	(0.001)*	(0.001)	(0.001)	(0.000)**
N (classes)	6,114	13,791	7,787	5,769	33,461
Neighboring classmates' parents (relative to other classmate's parents)					
Fathers					
$\hat{\gamma}$	0.008	0.007	0.005	0.004	0.006
(s.e.)	(0.002)**	(0.002)**	(0.001)**	(0.003)	(0.001)**
N (class/neighb)	2,986	6,591	6,861	1,178	17,616
Mothers					
$\hat{\gamma}$	0.003	0.012	0.006	0.007	0.007
(s.e.)	(0.002)	(0.001)**	(0.001)**	(0.003)**	(0.001)**
N (class/neighb)	4,749	9,830	10,494	1,579	26,652
Classmates (relative to other cohorts, same field and school)					
Male classmates					
$\hat{\gamma}$	0.006	0.014	0.005	0.006	0.008
(s.e.)	(0.001)**	(0.001)**	(0.001)**	(0.001)**	(0.000)**
N (classes)	6,605	14,148	7,936	5,594	34,283
Female classmates					
$\hat{\gamma}$	0.005	0.013	0.004	0.005	0.008
(s.e.)	(0.001)**	(0.001)**	(0.001)**	(0.001)**	(0.000)**
N (classes)	6,696	14,253	8,082	5,787	34,818
Other plants within the parent's firm					
Effect relative to classmates					
$\hat{\gamma}$	0.017	0.015	0.034	0.008	0.020
(s.e.)	(0.004)**	(0.002)**	(0.002)**	(0.001)**	(0.001)**
N	3,293	11,221	12,319	8,959	35,792
Strong/weak ratio	6.53	5.53	2.94	2.38	3.90

Note: Estimates of weak tie network effects. Weighted by the number of graduates in the class or class-neighborhood. Data are for graduates 1988-1995 finding a stable job within 7 years of graduation. Excluding children who starts working with their own parents and, for neighbor analysis, parents who work in a different municipality. Standard errors are cluster-corrected at the level of the fixed effect, i.e. for dependencies within field and school (and year for the neighborhood analysis). Final panel is for plants in multi-plant firms only. “Strong/Weak ratio” refers to the ratio between the strong tie effect (defined as the estimated effect on the probability of entering the parent’s plant) and the displayed weak tie effect, where both parts are estimated on the same sample of multi-plant firms. ** (*) Significant at the 1 (5) % level.

Table 6: Short and Medium Term Outcomes for Graduates

	Years to first job	ln(Starting wage)	Initial "match quality"	Outcomes after three years: In same plant	Employed	Earnings growth (3 years)
Class fixed efffects	-0.516	-0.040	-0.026	0.120	0.030	0.056
	(0.006)**	(0.002)**	(0.001)**	(0.002)**	(0.002)**	(0.003)**
Plant fixed effects	-0.385	-0.082	-0.022	0.057	0.021	0.073
	(0.007)**	(0.002)**	(0.001)**	(0.003)**	(0.003)**	(0.003)**
Match fixed effects	-0.264	-0.053		0.070	0.030	0.054
	(0.017)**	(0.005)**		(0.007)**	(0.007)**	(0.008)**
N	573,582	573,582	573,582	526,771	526,771	380,059
Mean dep var	1.539	9.425	0.185	0.309	0.721	0.286

Note: Estimated graduate-level effects of finding the first job at a plant where a parent works. The first model includes a fixed affect for each class. The second model includes plant fixed effects and dummies for each field (17 dummies) and level (26 dummies) of education and graduation year. The third model includes a fixed effect for each combination of field and school (but not graduation year) and Plant, as well as graduation year dummies. All regressions control for immigration status, gender and GPA (except for university graduates). "Match quality" refers to an index of the fraction of all graduates by exact type of education that enters the 2-digit industry. Data are for graduates 1988-1995. ** (*) Significant at the 1 % (5 %) level.

Table 7: Firm Performance and Parental Network Effects

Heterogeneous Effects	Compulsory	Vocational	Academic	University	All	All
	No plant specific network fixed effects (FE)					***Plant FE***
Firm productivity						
	-0.004	0.004	0.009	0.003	0.004	-0.003
	(0.002)	(0.002)**	(0.002)**	(0.001)**	(0.001)**	(0.002)
# non-missing	35,151	89,550	65,296	41,690	231,687	231,687
Firm Profits						
	0.073	0.080	0.129	0.001	0.068	0.059
	(0.013)**	(0.011)**	(0.013)**	(0.008)	(0.007)**	(0.009)**
# non-missing	35,783	90,639	66,328	42,303	235,053	235,053
Plant productivity						
(manufacturing)	0.011	0.024	0.039	0.013	0.024	0.005
	(0.006)	(0.004)**	(0.005)**	(0.003)**	(0.003)**	(0.005)
# non-missing	14,691	47,561	28,496	20,781	111,529	111,529
N	84,103	260,589	214,976	154,521	714,189	714,189

Note: Each estimate is from a separate regression. The models are identical to those of the heterogeneity models shown in Table 3, except the addition of the performance measures and indicators of missing values for those that are not sampled in the performance data. Productivity is measured as log of sales per worker and profits as declared profits divided by total assets. Performance measures are lagged one year to avoid the influence of non-constant returns to scale in the productivity measure. Standard errors are cluster-corrected for dependencies within class. **Significant at the 1 % level.