Social Contacts and Occupational Choice*

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Abstract

Social contacts help find jobs, but not necessarily in the occupations where workers are most productive. Hence social contacts can generate mismatch between workers' occupational choices and their productive advantage. Accordingly social networks can lead to low labor force quality, low returns to firms' investment, and depressed aggregate productivity. We analyze surveys from the US and Europe including information on job finding through contacts. Consistent with our predictions, contacts reduce unemployment duration by 1-3 months on average, but they are associated with wage discounts of at least 2.5%. We also find some evidence of negative externalities on aggregate productivity.

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1 Introduction

Friends and relatives are often recognized as a useful source of information on jobs, and much research has emphasized the positive role of friends and relatives in helping people to find jobs. In a number of studies for the US surveyed by Montgomery (1991), the share of workers reporting to have found their jobs through social contacts ranges from 24% to 74%, depending on the type of job and the locality of reference.

However, social contacts are usually acquired and maintained for other purposes than providing information on jobs and they typically help a worker to find a job only in specific occupations or segments of the labor market.[1] Thus the availability of social contacts and the opportunity of finding a job more easily may convince a worker to undertake a career in professions, sectors, or locations where his abilities are not fully exploited. In other words, social contacts may produce a mismatch between workers' comparative productive advantage and their occupational choices.

To analyze the aggregate implications of this mismatch, we consider a labor market characterized by search frictions. Each worker has productive advantage in a given occupation while he may have contacts which are useful for finding a job in either the same or other occupations. Then some workers face a trade-off between choosing the occupation in which they would be most productive and using their contacts to find a job. The larger the worker's endowment of social contacts, the larger is the productive advantage needed for him to choose an occupation where his productive advantage is

[1] Workers do not have contacts in every relevant occupation. Indeed, in one of the databases that we use in this paper, which includes workers in three large US cities (Atlanta, Boston, and Los Angeles) in the early 1990s, 53% of the respondents state that they did not talk to relatives as a method of job search and 26% that they did not talk to friends.

fully exploited but his contacts are not.

In the equilibrium of this economy, workers do not internalize the effects of the average productivity of the labor force on vacancy creation and on firms' incentives to invest capital in those vacancies, and so they tend to exploit too little of their productivity potential. In fact, social contacts can be so distortionary that aggregate net income may fall in response to an increase in the aggregate endowment of social contacts. In this situation, subsidies to workers' occupational or spatial mobility or other policies that weaken the influence of family ties and neighbor networks on occupational choices are welfare improving. Also, since workers find social contacts relatively more valuable when the labor market is sluggish and jobs are harder to find, the economy may get stuck in a trap in which some workers do not fully exploit their productivity potential because the economy is depressed, which in turn is due to the poor average productivity of the labor force induced by workers' occupational choices.

An occupation in our model refers to a segment of the labor market where workers search for jobs, firms search for workers, and where search frictions are present. One can think of it as an island in the standard Lucas and Prescott (1974) economy or as a submarket in the more recent papers by Shimer (2007), Alvarez and Shimer (2007), and Mortensen (2007). In the data this may correspond to the set of jobs identified by a given educational level, a given industry, and specific skill requirements (say the market for high school graduates searching for a managerial position in manufacturing). Our theory predicts that, in each submarket, on average, jobs found through contacts are obtained more quickly but also pay lower wages, since at least some of them are filled by workers who sacrificed their productive advantage in order to get a job more easily. We test the two sides of this prediction with both US and European data. The US data come from the "Multi-City Study of Urban Inequality, 1992-1994" (see Bobo *et al.*, 2000), a survey particularly well-suited for testing the model's key trade-off, since it contains detailed information about workers' job search methods. The data for the European Union (EU) come from the European Community Household Panel over 1995-2001. In both datasets we find evidence that the typical unemployment duration for workers who found a job through contacts is significantly lower than for workers who found a job through other methods. We also find a statistically significant wage discount for jobs found through family and friends, of the order of 2.5 to 3.5%. The discount is robust to the use of alternative implicit definitions of an occupation and also of extensive controls for cognitive ability, for the economic and family background of the workers, and for several other personal characteristics. While falling short of a direct test of the theory, this evidence is consistent with the model's prediction that individuals may trade off an easier access to jobs for higher productivity and wages.

Previous theoretical research on the role of contacts in the labor market emphasizes that they may be beneficial because they inform the employer about the worker (Saloner, 1985, and Montgomery, 1991), because they allow workers to more effectively sample a given wage distribution (Mortensen and Vishwanath, 1994, and Calvo-Armengol and Jackson, 2004, 2007), because they are a source of peer monitoring (Kugler, 2003), or because they provide a cheaper search channel (Holzer, 1988, and Santamaria-Garcia, 2003). In all these models an increase in the worker's endowment of contacts implies a net improvement in his wage possibilities, and so they predict that jobs found through contacts should pay higher wages. We emphasize instead that workers may sacrifice their productive advantage so as to find a job more easily, which can explain why jobs found through social contacts exhibit a wage discount rather than a premium.

By assuming that contacts in an occupation improve search efficiency, our modelling of social contacts borrows from the literature on search frictions and endogenous search intensity (see Pissarides, 2000, for a review of the literature). Fontaine (2007a,b) has also analyzed the effects of social contacts in the context of a standard equilibrium search model à la Pissarides. He shows that an increase in the endowment of contacts can lead to an increase in equilibrium unemployment, because firms may reduce their investment in recruitment practices other than using social contacts. In these papers, however, social contacts do not lead to a mismatch between workers' productive advantage and their occupational choices, and social welfare always increases in response to an increase in the endowment of contacts.

Prior empirical work on the effects of contacts on job finding, unemployment duration, and wages generally confirms that contacts are individually beneficial to workers in the first two dimensions—see, for example, Holzer (1988), Blau and Robins (1990) and Kramarz and Nordstrom Skans (2007). The effect of contacts on wages is empirically less clear. Most of the literature has focused on the effects of employee referrals, obtaining mixed results. Granovetter (1974), Corcoran *et al.* (1980), Simon and Warner (1992) and Kugler (2003) document positive (albeit sometimes non-significant) wage premia for jobs found through referrals from employees of the worker's current employer. Through our focus on the effects of the contacts provided by family and friends, our evidence is more comparable to that in Simon and Warner (1992), who consider a sample of US scientists and engineers, and also find a wage discount associated with jobs found through this type of contacts. Moreover, we focus our analysis on relatively young workers (and, in the case of the EU, on workers at their first permanent job) in order to better capture the effects of non-professional contacts and to detect mismatch effects on the worker's productivity (which are likely to get diluted over time, by force of experience and further specialization).[2]

This paper is also related to three other strands of the literature. First, it contributes to the growing literature on *social capital*; see, for example, Knack and Keefer (1997) and Guiso *et al.* (2004). Social contacts enhance the spreading of information and, thus, are a form of social capital in the sense of Coleman (1988).[3] Opposite to the common wisdom about the virtues of social capital, our results point out the potential inefficiencies that some forms of social capital may cause. Second, our paper hints at a novel interpretation for high inter-generational persistence of *segregation* by skill and occupation, as documented for instance by Borjas (1995). The literature has generally attributed this phenomenon to the (positive) role of peer effects in the transmission of human capital. Our theory suggests instead that inter-generational occupational and spatial mobility

[2] Friends acquired later in workers' careers might include former co-workers or other colleagues who provide referrals and, potentially, improve the matching between worker and job characteristics. Indeed, Marmaros and Sacerdote (2002), who consider a sample of Dartmouth College seniors, and Munshi (2003), who deals with a sample of Mexican migrants to the US, find that an increase in the endowment of contacts of these specific groups of workers leads to better-paying jobs. These findings do not necessarily contradict our theory since, within their corresponding worker samples, occupational choices are likely to have been determined prior to search for the workers' observed jobs.

[3] Indeed he states that "An important form of social capital is the potential for information that inheres in social relations (...). In this case relationships are valuable for the information they provide."

may remain low because workers seek to use their inherited social connections to find jobs more easily—jobs in which they are not necessarily more productive. Finally, our findings are relevant for the growth literature that, following Hall and Jones (1999), stresses how *social infrastructure* affects capital-labor ratios and aggregate productivity. We identify social contacts as one possible reason why the average quality of the labor force and the return to firms' investments may remain low.

The rest of the paper is organized as follows. Section 2 describes the model. Section 3 analyzes the equilibrium of the model, its comparative statics, and its efficiency. Section 4 describes the data and the empirical results. Section 5 concludes.

2 The model

As in Acemoglu (1996, 1999), we consider a static version of the standard search model à la Pissarides (2000). In the economy there are two occupations, $i = 1, 2$, for each of which there is a separate labor market.[4] There is a continuum of measure two of risk-neutral workers who, for simplicity, derive no utility from leisure. Workers' long-term occupational choice consists of deciding where to search for a job. Creating a job requires that a firm open an occupation-specific vacancy, which has a cost $k > 0$, and that the vacancy is filled by a suitable worker. Firms are expected profit maximizers and there is free entry.

The market for each occupation is subject to search frictions: the total number of jobs created in occupation $i = 0, 1$ is determined by a matching function $M(V_i, U_i)$, where V_i denotes the number of vacancies opened for occupation i and U_i denotes the total

[4] The analysis can be trivially extended to cases with more than two occupations.

efficiency units of search used by the workers who choose occupation i. As usual, this function is assumed to be homogeneous of degree one, increasing in both arguments, concave, continuously differentiable, and upper-bounded by $\min\{V_i, U_i\}$. Thus, one efficiency unit of search of any given worker yields a match with a firm with probability

$$p(\theta_i) \equiv m(\theta_i, 1) = \frac{m(V_i, U_i)}{U_i},$$

where $\theta_i \equiv V_i/U_i$ is the so-called level of tightness in the market for occupation i. Clearly, $p(\theta_i)$ is increasing in θ_i.

When a match is formed, the match surplus is split by using a Generalized Nash Bargaining Solution where the worker's and the firm's bargaining powers are β and $1-\beta$, respectively. Since the outside options of both the firm (leaving the vacancy unfilled) and the worker (remaining unemployed) are worth zero at that point, each employed worker's wage is equal to a fraction β of his output in the job.

Workers are heterogeneously endowed with occupation-specific productive advantage and social contacts. A worker produces $(1+a)y$ when he can exploit his productive advantage and $y > k$ when he cannot. Social contacts are useful for finding a job in a specific occupation. In particular, let us normalize to one the efficiency units of search associated with the exclusive use of formal channels (newspaper ads, private or public employment agencies, internet search, etc.). Then we assume that the efficiency units of search usable for finding a job in an occupation where the worker has contacts are $1+\tilde{s}$, where $\tilde{s}$ measures the worker's endowment of contacts specific to that occupation.

For simplicity, we assume that every worker has his productive advantage in exactly one occupation and his contacts in exactly one occupation as well. These occupations need not coincide. Specifically, we assume that the allocation of productive advantage across occupations is purely random—half of the workers have their productive advantage in each occupation—and that each worker has a probability $\rho \in [0,1]$ of having his productive advantage and his contacts in the same occupation. Thus, ρ measures the extent to which productive advantage and contacts are aligned. Social contacts and productive advantages are uncorrelated if $\rho = 0.5$.

Finally, we assume that a fraction μ of workers has a large endowment of contacts, $\tilde{s} = S$, while the remaining fraction $1-\mu$ has a small one, $\tilde{s} = s$. In order to induce each of these groups to resolve differently their trade-off between exploiting productive advantage and using contacts, we additionally assume that $s < a < S$. The formal justification for this assumption will become obvious after presenting equation (1) below.

3 Equilibrium analysis

Since the fundamentals of the market for each occupation are identical, we focus on symmetric equilibria and drop the occupation subscript i in the remainder. In equilibrium, these markets attract one unit mass of workers each and are characterized by identical levels of tightness θ and identical compositions of the labor force in terms of per capita productivity and efficiency units of search used. We start by solving for these equilibrium variables and then derive the equilibrium values and the comparative statics of some empirically relevant variables, such as average wages and unemployment duration conditional on the channel whereby the workers find their job. The section concludes by discussing the efficiency of the equilibrium allocation.

3.1 Solving for equilibrium

Clearly, a worker whose contacts and productive advantage are in the same occupation always chooses to search for a job in that occupation. Instead, a worker with productive advantage in one occupation and social contacts in another faces a trade-off, that gets resolved in favor of productive advantage if and only if searching for a job in such an occupation yields higher expected income, that is:

$$p(\theta)\beta(1+a)y \geq p(\theta)(1+\tilde{s})\beta y, \tag{1}$$

where the relevant job finding probabilities are $p(\theta)$ and $p(\theta)(1+\tilde{s})$, and the wages are $\beta(1+a)y$ and βy, respectively. Clearly, under the assumption $s < a < S$, the worker follows his productive advantage if his endowment of contacts is small, $\tilde{s} = s$, but not if it is large, $\tilde{s} = S$.

Given these choices, the total efficiency units of search in the market for each occupation can be computed as

$$\begin{aligned} U &= \mu\rho\,(1+S) + (1-\mu)\,\rho\,(1+s) + \mu(1-\rho)\,(1+S) + (1-\mu)\,(1-\rho) \\ &= \mu\,(1+S) + (1-\mu)\,(1+\rho s)\,, \end{aligned} \tag{2}$$

which results from adding up the efficiency units of search used by the various types of workers in the market. Specifically, the first two terms in the first line correspond to the measures $\mu\rho$ and $(1-\mu)\rho$ of workers with contact endowments of $\tilde{s} = S$ and $\tilde{s} = s$, respectively, who happen to have contacts and productive advantage in the same occupation. The third term corresponds to the measure $\mu\,(1-\rho)$ of workers with high contacts, $\tilde{s} = S$, who resolve their conflict between contacts and productive advantage by sacrificing the latter. Lastly, the fourth term corresponds to the measure $(1-\mu)(1-\rho)$ of workers with low contacts, $\tilde{s}=s$, who resolve the conflict in favor of their productive advantage.[5]

The fraction of efficiency units of search in the market accounted for by workers with productive advantage (or *high productivity* workers) is given by

$$\gamma = 1 - \frac{\mu(1-\rho)(1+S)}{\mu\,(1+S)+(1-\mu)\,(1+\rho s)}, \tag{3}$$

which we express as one minus the fraction of efficiency units of search accounted for by workers who fail to exploit their productive advantage. These are the mass $\mu\,(1-\rho)$ of workers with $\tilde{s}=S$ who face a conflict between contacts and productive advantage. Clearly, $1-\gamma$ is a measure of the *mismatch* between workers' occupational choices and productive advantage. By (3), the degree of mismatch is increasing in μ and S, and decreasing in ρ and s (up to s equal to a). Intuitively, mismatch falls with any parameter that increases the presence of workers who follow their productive advantage.

Since firms appropriate a fraction $1-\beta$ of the worker's output in the job, the expected profit from opening a vacancy in any occupation is equal to

$$\Pi = q(\theta)\,(1-\beta)\,(1+\gamma a)y - k, \tag{4}$$

where $q\,(\theta) \equiv m(V,U)/V = p(\theta)/\theta$ is the probability that the firm fills the vacancy, which is decreasing in the level of labor market tightness, $\theta = V/U$.

The creation of vacancies till the exhaustion of rents implies the free-entry condition

[5] Due to the static nature of the model, the different exit rates out of unemployment of the various groups of workers do not affect the stock of search efficiency units. This effect would instead be present in a dynamic version of the model.

$\Pi = 0$, that is:

$$q(\theta)\,(1-\beta)\,(1+\gamma a)y = k. \tag{5}$$

Intuitively, given a mass of workers searching for jobs, U, firms will open vacancies up to the point at which V implies a level of tightness θ such that the expected net profit per vacancy is zero. Assuming hereafter that $(1-\beta)y \geq k$, $\lim_{x\to 0} q(x) = 1$, and $\lim_{x\to\infty} q(x) = 0$, it follows that (5) has a (unique) interior solution. Clearly, (5) implies a positive relationship between the expected value of a worker to the firm, $(1-\beta)(1+\gamma a)y$, and the equilibrium level of labor market tightness, θ.

3.2 Some derived statistics

Let the binary variable c indicate whether a worker has found a job using his contacts ($c = 1$) or not ($c = 0$). Correspondingly, let φ_1 and φ_0 denote the proportions of high productivity workers among those employed through formal channels and through contacts, respectively. To compute φ_1 note that the total number of jobs filled through contacts is given by the product of $p(\theta)$ and $\mu S + (1-\mu)\rho s$, which is the sum of two terms. The first corresponds to workers with $\tilde{s} = S$, whose measure is μ, who always use their contacts to search for a job and actually find one through contacts with probability $p(\theta)S$. The second corresponds to workers with $\tilde{s} = s$, whose measure is $1-\mu$, who use their contacts only if this does not come into conflict with exploiting their productive advantage, that is, with probability ρ. For them contacts prove to be useful to find a job with probability $p(\theta)s$. Finally, remember that the only workers who fail to exploit their productive advantage are those with a conflict between using their large endowment of contacts and exploiting their productive advantage. This reasoning yields the proportion:

$$\varphi_1 = 1 - \frac{\mu(1-\rho)S}{\mu S + (1-\mu)\rho s} = \frac{\mu\rho S + (1-\mu)\rho s}{\mu S + (1-\mu)\rho s} \in [0,1].$$

Now, since wages are equal to a fraction β of each worker's output, the average wage paid in a job which is filled through social contacts is equal to

$$E(w|\ c=1) = \beta(1+\varphi_1 a)y. \tag{6}$$

By a similar reasoning, we can obtain that

$$\varphi_0 = 1 - \mu(1-\rho) < 1,$$

which uses the fact that all the jobs filled through formal channels correspond to high productivity workers except when the matches involve workers with a high endowment of contacts in an occupation where they do not have their productive advantage. The average wage paid in a job which is filled through formal channels is then equal to

$$E(w|\ c=0) = \beta(1+\varphi_0 a)y. \tag{7}$$

From (6) and (7), it follows that

$$E(w|\ c=1) - E(w|\ c=0) = -\beta(\varphi_0 - \varphi_1)ay,$$

which is strictly negative since we have:

$$\varphi_0 - \varphi_1 = \mu(1-\rho)\frac{(1-\mu)\left(S-\rho s\right)}{\mu S + (1-\mu)\rho s} > 0.$$

Thus:

Proposition 1 *Jobs found through social contacts pay on average a lower wage than jobs found through formal channels.*

Intuitively, since contacts induce some workers to sacrifice their productive advantage, the pool of workers who found jobs through social contacts have lower average productivity. Note that the wage discount would disappear if all workers had few contacts ($\mu = 0$) or if contacts and productive advantage were never in conflict ($\rho = 1$), since in these cases no worker would give up his productive advantage.

Next, let us refer to the inverse of a worker's probability of finding a job as his unemployment *duration*, denoted by d.[6] The average unemployment duration across workers who find a job through formal channels is

$$E(d|\ c = 0) = \frac{1}{p(\theta)} \left[\frac{\mu}{1+S} + \frac{(1-\mu)\rho}{1+s} + (1-\mu)(1-\rho) \right]. \tag{8}$$

To understand this expression, note that it averages three unemployment durations proportional to $1/p(\theta)$. For the fraction μ of jobs found through formal channels by workers with a large endowment of contacts, the proportionality factor is $1/(1+S)$. For the fraction $(1-\mu)\rho$ of jobs found through formal channels by workers with no conflict between productive advantage and their small endowment of contacts, the factor is $1/(1+s)$. Finally, for the fraction $(1-\mu)(1-\rho)$ of workers who exploit their productive advantage instead of using their small endowment of contacts, the factor is just 1.

By an analogous reasoning we can compute the average unemployment duration of workers who find a job through contacts. This is equal to

$$E(d|\ c = 1) = \frac{1}{p(\theta)} \left[\frac{1}{1+S} \frac{\mu S}{\mu S + (1-\mu)\rho s} + \frac{1}{1+s} \frac{(1-\mu)\rho s}{\mu S + (1-\mu)\rho s} \right]. \tag{9}$$

In Appendix 1 we show that $E(d|\ c = 1) - E(d|\ c = 0)$ is negative, implying that:

[6] Despite the static nature of the model, we use this terminology to stress the connection with the relevant empirical counterpart.

Proposition 2 *Workers who find their jobs through contacts exhibit lower unemployment duration than workers who find their jobs through formal channels.*

Since contacts help workers to find jobs, extending the result to a dynamic setup would imply that the unemployment spells of workers who find jobs through contacts would be shorter on average than those of workers who find their jobs through formal channels.[7] Propositions 1 and 2 together reflect in empirically measurable terms the key trade-off faced by some of the workers in their occupational choices: jobs found through contacts tend to be found more quickly but they tend to pay lower wages.

3.3 Comparative statics

The following result illustrates the impact on labor market tightness of mismatch due to the effect of social contacts on workers' occupational choices. According to (5), labor market tightness, θ, is increasing in the probability that a firm matches with a high productivity worker, γ, which in turn is decreasing in the proportion of workers with a large endowment of contacts, μ. Thus:

Proposition 3 *An increase in the proportion of workers with a large endowment of contacts reduces workers' expected productivity, causing labor market tightness to fall. The effects on aggregate unemployment are generally ambiguous.*

Intuitively, the reduction in the number of workers who exploit their productive advantage decreases the expected value of a worker to the firms. But then, firms react by creating less vacancies per efficiency unit of search, making labor market tightness fall

[7] See subsection 3.5 for a discussion of the dynamic version of the model.

to the point where the free-entry condition (5) is restored. The fall in labor market tightness implies that the probabilities of finding a job of both the workers who use contacts, $(1+\tilde{s})p(\theta)$, and those who do not, $p(\theta)$, also fall. The effects on aggregate unemployment are however ambiguous because increasing μ means increasing the fraction of workers who use a large endowment of contacts in searching for a job.

3.4 Efficiency

Since workers and firms are risk-neutral, we define social welfare W as aggregate output net of vacancy creation costs. Formally,

$$W = 2\left[p(\theta)(1+\gamma a)y - k\theta\right]U \tag{10}$$

where the factor 2 accounts for the existence of two occupations and U denotes the total efficiency units of search used by workers in each occupation, as given by (2). Note that W is a function of the proportion of employed workers who exploit their productive advantage, γ, given by (3), and the level of labor market tightness, θ, given by (5).

By deriving with respect to θ and after using (5) to replace $q(\theta)(1+\gamma a)y$ by $\frac{1}{1-\beta}k\theta$, we get

$$\frac{\partial W}{\partial \theta} = \frac{2k\left[\beta - \eta(\theta)\right]U}{1-\beta}, \tag{11}$$

where $\eta(\theta) \equiv \frac{q(\theta)+\theta q'(\theta)}{q(\theta)}$ is the elasticity of the matching function with respect to U. As the value of β and $\eta(\theta)$ need not coincide, the above derivative is generally different from zero, which means that the equilibrium level of tightness is generally inefficient. This inefficiency is of the type first pointed out by Hosios (1990) and it is due to the combination of search frictions and bargaining in the labor market.[8]

[8] The anticipated division of the surplus, that results from the bargaining between firms and workers,

We have observed that as the proportion of workers with a large endowment of contacts, μ, increases, workers' expected productivity falls, since a greater number of workers sacrifice their productive advantage. Despite the fact that contacts may help workers find jobs more easily, the effect on productivity can be strong enough for aggregate net income to fall when μ increases. In this sense, social contacts can be inefficient. To prove this result, consider the total derivative of W with respect to μ, which after using (10) reads as

$$\frac{dW}{d\mu} = \frac{\partial W}{\partial \mu} + \frac{\partial W}{\partial \theta}\frac{d\theta}{d\mu}, \tag{12}$$

where

$$\frac{\partial W}{\partial \mu} = 2\frac{1}{1-\beta}k\theta\left[\beta(S-\rho s) - \frac{a}{1+\gamma a}\frac{(1-\rho)(1+\rho s)(1+S)}{\mu(1+S)+(1-\mu)(1+\rho s)}\right], \tag{13}$$

in which we have used (5) to replace $p(\theta)(1+\gamma a)y - k\theta$ by $\frac{\beta}{1-\beta}k\theta$. We want to identify sufficient conditions under which the expression in (12) is negative. Notice that $\partial W/\partial\theta \geq 0$ if and only if $\beta \geq \eta(\theta)$, while Proposition 3 implies that $d\theta/d\mu < 0$. So all we need is to identify cases with $\beta \geq \eta(\theta)$ in which the derivative in (13) is strictly negative, which boils down to requiring that the expression in brackets is strictly negative. The following result, proven in the Appendix, provides a sufficient condition based on the difference between S and s not being too large:

Proposition 4 *An increase in the fraction of workers with a large endowment of contacts, μ, does not necessarily increase aggregate net income. Actually, if $\eta(\theta) \leq \beta < (1-\rho)/[(1-\rho)+(S/s-1)]$, aggregate net income is decreasing in μ.*

determines firms' incentive to create new vacancies. But opposite to Walrasian prices, the bargaining powers β and $1-\beta$ do not adjust to reflect the marginal social value of a vacancy. When, for instance, workers are "too strong" ($\beta > \eta(\theta)$), firms appropriate too little surplus and so they create too few vacancies.

In words, we can ensure that an increase in the fraction of workers with a large endowment of contacts leads to a reduction in social welfare when: (i) the economy is not characterized by excess vacancy creation (that is, $\beta \geq \eta(\theta)$ so that $\partial W/\partial\theta \geq 0$) and (ii) the private gains from using contacts and sacrificing productive advantage are not too large (that is, S is not too far from s and, hence, none of them is too far from the productive advantage parameter a, which lies in between).[9]

This inefficiency is due to an externality similar to that emphasized by Acemoglu (1996). In equilibrium workers tend to sacrifice their productive advantage beyond what is socially optimal because they do not internalize the adverse effect that a reduction in aggregate labor productivity has on vacancy creation (and on firm's investment had capital been endogenous).[10] In a first-best allocation, an increase in the aggregate endowment of social contacts would necessarily lead to an increase in aggregate net income, since contacts would only facilitate job finding. But in the equilibrium of this economy social contacts can cause an excessive mismatch between workers' occupational choices and their productive advantage—a level of mismatch that does not pay in terms of aggregate net income.

3.5 Some extensions

The simple search model discussed so far is static. Extending it to a dynamic setup—where, for example, workers are infinitely lived, jobs are destroyed with some probability,

[9]Notice that the condition stated in the proposition is compatible with non-negligible differences between S and s. For instance, when social contacts and productive advantages are uncorrelated and production is equally shared between workers and firms, i.e., when $\rho = \beta = 0.5$, S could be up to 1.5 times s.

[10]Notice that, for sufficiently small S, the inefficiency grows with μ even if the Hosios rule holds (i.e., $\beta = \eta(\theta)$), implying that its operation does not require conditional inefficiency in the process of vacancy creation.

and wages are determined by output sharing—would allow us to take into account the effects of unemployment inflows and outflows on aggregate quantities. Yet the qualitative results would change little: in particular, jobs found through contacts would still pay lower wages on average, and a higher endowment of contacts could still lead to a fall in aggregate social welfare, due to higher mismatch. In the dynamic model, however, certain parameter configurations give rise to multiple equilibria. This is so because the terms of the conflict between easier job finding and productive advantage depends on labor market tightness—social contacts are relatively less valuable when the labor market is thriving and tightness is high—which in turn is affected by how workers resolve such a conflict—due to free entry, labor market tightness is higher when more workers exploit their productive advantage.[11]

Multiplicity of equilibria can also arise if working in an occupation where the worker has social contacts gives him/her some non-pecuniary benefits—say, because individuals feel pressed to choose a particular profession to comply with social conventions or family traditions. The existence of non-pecuniary benefits attached to contact use also implies that the degree of mismatch in the labor market can be large even if the effect of contacts on job-finding probabilities appears to be small.

4 Empirical evidence

The model predicts that workers may choose occupations where their productive advantage is not fully exploited because they want to use their contacts to find a job more easily. The direct testing of this prediction (as well as the structural estimation of the

[11] Coexisting equilibria could be Pareto rankable. In particular, if vacancy creation is inefficiently low ($\beta > \eta(\dot{\theta})$), equilibria where more workers exploit their productive advantage are superior.

model) is difficult with the available data, since we do not observe a worker's productivity or wages in occupations different from the one actually chosen. Yet our theory has implications for how groups of workers who differ in their observable use of contacts differ in terms of average wages (Proposition 1) and average unemployment duration (Proposition 2). As explained in Section 3, this is because of composition effects derived from workers' self-selecting occupational choices. Checking these implications of the theory in the data only requires standard regression analysis, since they are established in terms of pure conditional means, without any underlying causality problems that need to be fixed or controlled for. Of course, since the microeconomic details of the mechanism are unobservable, the differences in conditional means that we find (and which turn out to be consistent with the model) might also be consistent with other explanations. In a series of robustness checks described below, we specify some of these alternative interpretations and try to introduce controls for them. Adding controls for confounding factors should reduce the likelihood that the partial correlation of the use of contacts with unemployment duration and wages is due to such factors.

To calculate conditional means, one has to take a stand on what is the empirical counterpart of an occupation in the model. There it refers to a segment of the labor market where workers search for jobs, firms search for workers, and where search frictions are present. In the data this may correspond to the set of jobs identified by a given educational level, a given industry, and specific job requirements (e.g., the market for high school graduates searching for a managerial position in manufacturing). Our theory predicts that, in each segment, workers who find jobs through contacts are on average less productive but they experience shorter unemployment durations. Since the definition of occupation is somewhat arbitrary (see also Alvarez and Shimer, 2007, for further discussion on this point), we experiment with several alternatives: one where the relevant segments of the labor market are simply defined by education, one where they are defined by education, industry, and firm size, and a final one that also includes job dummies. Moreover, in each segment of the labor market, the contacts variable just identifies the presence of some mismatched workers, and so our theory predicts that it should have a negative effect on wages and unemployment duration, independently of the degree of aggregation and the exact definition of occupation used.

4.1 Data

We use two data sets, one for the US and the other for the European Union (EU). Our model is intended to capture the effects of social contacts that are not determined by the occupational choice of the worker and, in this sense, differ from the professional contacts which workers acquire after their choice has been made. For this reason we focus on workers younger than 35 years old and, thus, at a relatively early stage in their professional careers (though later on we check the results on a wider sample).

Our first dataset is the "Multi-City Study of Urban Inequality, 1992-1994," a survey carried out by the Inter-University Consortium for Political and Social Research at some different points in time over those three years in four US cities—Atlanta, Boston, Detroit, and Los Angeles (see Bobo *et al.*, 2000). Except for Detroit, the survey includes the question "Did you find your (last/present) job through friends or relatives, other people, newspaper ads, or some other way?" that allows the following answers: 1) friends or relatives, 2) other persons, 3) newspaper ads, 4) other, and 5) refused/don't know/missing. To identify jobs founds through social contacts we construct the variable *Contacts* as a dummy taking the value 1 for reply 1 and 0 for replies 2-4.[12] We restrict *Contacts* to jobs found through friends and relatives, excluding "other persons," because the latter are more likely to include contacts originated in the context of the worker's professional activity.

Starting from the initial samples for Atlanta, Boston, and Los Angeles, we identify all individuals younger than 35 years old, who respond to the questions about job-finding methods, who are currently employed (employees, on temporary layoff or on sickness/maternity leave) and for whom there is reliable data about personal characteristics and wages. Appendix 2 gives details about the construction of the final sample, which includes 927 observations.[13] About one half of workers included in this sample found their job through contacts.

The European data come from the European Community Household Panel (ECHP). We use the waves from 1995 to 2001, corresponding to 13 countries of the European Union (EU): Austria, Belgium, Denmark, Finland, France, Greece, the Netherlands, Luxembourg, Ireland, Italy, Portugal, Spain, and the United Kingdom. The survey provides information on personal characteristics of household members and, for employed workers, on job-finding methods. It includes the question: "By what means were you first informed about your present job?" that allows the following answers: 1) through

[12] To increase the accuracy of the variable, we actually refine it by making sure that the worker also gave answers 1 or 2 to the following question: "Which of the following best describes your relationship to the one person who most directly helped you get your (last/current) job?: 1) Relative, 2) friend, 3) acquaintance, 4) someone else, and 5) refused/don't know/missing".

[13] The number of observations varies when we include industry and job dummies and other controls.

family, friends or other contacts, 2) applying to the employer directly, 3) inserting or answering adverts in newspapers, TV, or radio, 4) through an employment or vocational guidance agency, 5) started own business or joined family business, 6) other, and 7) missing. In this case we construct *Contacts* as a dummy variable taking the value 1 for reply 1 and 0 for replies 2-4 and 6.

This sample includes all employees who are younger than 35 years old, who responded to the question about job-finding methods, and for whom personal characteristics and wages are available. The large size of the initial sample allows us to focus on workers observed in their first job and who have a permanent contract, as they correspond more closely to the target population of workers who made their occupational choice under contacts not associated with prior employment history.[14] The final baseline sample includes 17,262 observations–corresponding to 7,021 different individuals–of which 31% are for jobs found through contacts. Appendix 2 contains further details.

The US survey is more accurate than the EU survey in identifying contacts unrelated to a worker's employment history. First because of the wording of the relevant questions (which refers to finding rather than to being informed about the job), and secondly because the first possible answer in the EU questionnaire includes "other contacts", together with contacts from family and friends, increasing the likelihood that an affirmative answer refers to contacts of a professional origin. This provides a strong argument for restricting the EU sample to workers in their first job. The two datasets are, however, complementary. In addition to allowing us to check the robustness of our

[14] In many cases temporary jobs do not reflect a career choice, while contacts used for finding second or later jobs are more likely to include professional referrals.

results in the face of variation in the institutional framework, the EU survey provides a larger sample, it includes some control variables that are unavailable for the US (see below), and it allows us to test for the presence of aggregate externalities—by aggregating the data at the level of 51 different regions.

Table 1 reports some descriptive statistics for the variables included in all regressions. We describe separately the samples of workers who did and did not find their jobs through contacts. For the US, the variables are age, gender (*Male*), being *Born in the US*, race (*White*, *Black*, *Asian*, and *Other race*, which includes Hispanics and Native Americans), years of formal education (*Schooling*), potential *Experience* (current age minus the age at which the person first left full-time education), and working for a *Small firm* (i.e. with less than 100 employees).[15] Those who found jobs through contacts appear to be slightly younger, more likely to be male, less likely to have been born in the US and to be White, Black or Asian rather than Hispanic or Native American, less educated, more experienced, and more likely to work in a small firm. These differences in average characteristics across contact and non-contact workers are consistent with standard findings in the literature (for instance, see Holzer, 1988).

The EU descriptive statistics throw a similar picture except for average experience, which is the same in both groups. Nevertheless, the two data sets are not entirely comparable. For instance, in the EU race is unavailable and schooling is measured by the highest completed level of education rather than by the number of years of education.[16]

[15] To better control for firm heterogeneity, in the regressions below we include six firm size dummies instead.

[16] This has the advantage of enhancing comparability across countries with different education systems.

4.2 Social contacts and unemployment duration

We first test whether, in line with the prediction of Proposition 2, workers who find jobs through contacts exhibit lower unemployment duration than workers who find jobs through other channels. The fact that social contacts are useful for job finding has been documented before for the US by, for example, Holzer (1988). However, it is useful to document this effect of contacts on job-finding probabilities using the same data with which we later provide evidence on the other side of the trade-off—the wage effect. For each of our samples, we thus estimate the following regression on the duration of the unemployment spell preceding each worker's current job (d_{it}), expressed in months:

$$d_{it} = \psi + \alpha Contacts_{it} + \delta X_{it} + u_{it}. \tag{14}$$

The subindex *it* refers to worker i in year t. The regression is run on observations corresponding to employed workers with a previous unemployment spell; in the EU sample we only include the observation of the first year in which the worker appears as employed.[17] X_{it} is a vector of control variables that, in the baseline specification, includes the variables listed in Table 1, except *Age*, plus *Experience squared*. As already noted, in a second specification, X_{it} also includes industry dummies and firm-size categories, and in a third one it further includes job dummies.[18] To capture regional differences and time effects, X_{it} includes city dummies in the US regression and year and country

[17] Restricting the sample to workers with a previous unemployment spell may bias the estimates of the effects of contacts towards zero. The absence of an unemployment spell may correspond to a direct move from out of the labor force into employment, for which we expect contacts to have a positive effect. The absence of an unemployment spell may also be the result of a very long unemployment spell (whose beginning the worker may not remember), a situation which is less likely to occur in the presence of contacts.

[18] To economize on degrees of freedom, the industry and job classifications used in the second and third specifications are more aggregated than those used in the wage regressions discussed below (see Appendix 2 for details).

dummies in the EU regression. Appendix 2 contains detailed definitions of all variables.

For the US only one fourth of the main sample described in Table 1 includes information on the length of the preceding unemployment spell. In this sample, unemployment spells for individuals who found their jobs through contacts are on average one month shorter. Panel A of Table 2 shows that, after introducing the variables that control for worker and job characteristics, contact workers exhibit a statistically significant shortening of their unemployment spell of about one and a half months.

The findings for the EU sample are qualitatively similar. According to Table 1, the average unemployment duration for EU workers who found their job through contacts is almost two months lower than for the remaining EU workers. Panel B of Table 2 shows that, once worker and/or job characteristics are taken into account, *Contacts* are associated with a reduction of about one month in the spell length.

A problem with the results included in panels A and B of Table 2 is that they both refer to samples of employed workers who were asked about the length of their prior unemployment spell. By construction, such samples oversample positive outcomes from the job-finding process (and, in the EU, they also oversample workers with long tenure in the first job), they undersample workers who go back into unemployment after a short employment spell, and they introduce recall bias in the unemployment spell data. Ideally, the sample should be representative of the workers who start the search for jobs at a given point in time. In order to come closer to such a representative sample, we use the panel structure of the EU data to construct a sample of workers whom we observe first in full-time education and then entering unemployment. We consider all workers entering unemployment up to 1999, we follow them until they find a job, and then run the regression using the duration of their unemployment spells.[19] The results appear in panel C of Table 2. Now the reduction in unemployment duration associated with jobs found through contacts increases to about three months.

4.3 The average wage discount on contact jobs

To test whether, in line with the prediction of Proposition 1, jobs found through social contacts pay on average a lower wage than jobs found through formal channels, we estimate the same regression as in equation (14), but with the log hourly wage as the dependent variable (which is gross of taxes in the US data, and net in the EU data). The US sample still contains one observation per individual but the EU sample may contain up to seven yearly observations per worker. Thus in the EU results, we adjust t-ratios for repeated observations on the same individual. To capture regional differences in wage levels and, in the EU data, the effects of inflation, X_{it} includes city dummies for the US and year and country dummies for the EU. The variables included in X_{it} in the three alternative specifications are as for equation (14).

In the US sample, the hourly wages of workers who found their job through contacts are on average 19.4% lower than the wages of other workers, and the average difference rises to 20.6% when controlling for the city. Table 3 shows that, once we add the usual Mincer regressors, the (log) discount drops to 7.4%. This discount remains unaltered when firm-size and industry dummies are added, and it drops to 5.6% when job dummies are included as further controls. In the EU sample, the raw average wage discount on

[19] We stopped in 1999 because for such a cohort of *entrants* we observe complete unemployment spells in 88% of the cases. For the 2000 cohort, we have complete spells for only 70% of the cases.

contact jobs is 20% and it falls to 11.6% when we control for year and country. Adding the standard regressors brings the discount down to 7%, and including firm-size, industry, and job dummies leads to estimates of around 2.7%.

As with unemployment duration, one can argue that the wage data of the sample of employed workers does not provide a random sample of the wages of job searchers. If contacts affect job-finding rates (as our results actually suggest) or the probability of remaining employed, then results based on employed workers are likely to suffer some sample selection bias. As already done for duration, we take advantage of the panel structure of the EU data and consider the above-mentioned sample of workers who were first involved in full-time education, then experienced an unemployment spell, and finally got employed. This sample of *entrants* should provide us with a less biased sample of job searchers than the full sample of employed workers. The coefficients shown in Panel C of Table 3 suggest that the full sample, if anything, underestimates the effects of contacts, since the wage discount is now larger, and still significant, despite the smaller sample size.

In both areas and all specifications, the Mincer regressors yield standard results: a wage premium for males, for natives (not significant in the EU), for Whites and Asians (in the US), for the more educated and experienced, and for workers at larger firms. Since this is true in all specifications, to avoid cluttering the tables, for further regressions we only report the coefficient on *Contacts*, omitting the estimated coefficients on the remaining variables.

In sum, the wage discount is both economically and statistically significant in the US and the EU, which is consistent with our theory. The magnitude of the wage discount, relative to the more modest effects of *Contacts* on unemployment duration, suggests that, in addition to speeding up the process of finding a job, as our model explicitly captures, the choice of occupations related to social contacts might also be providing workers with non-pecuniary benefits such as those related to conforming with social expectations and customs, or to the existence of informal safety nets.

4.4 Individual ability and family background

A central prediction of our model is that the OLS coefficient on *Contacts* in the wage regression should be negative since the group of workers in jobs found through contacts includes a larger proportion of workers who sacrificed their comparative productive advantage in order to exploit their contacts. Importantly, the coefficient is predicted to be negative because of self-selection and not because of the existence of a technological or any other type of causal relationship between contact use and productivity in the job. Thus, this form of endogeneity in the choice of faster job finding over higher productivity is not a concern, but rather part of the explanation for why our theory predicts a negative coefficient on the *Contacts* variable. Of course, other factors affecting the correlation between *Contacts* and wages should be treated as confounding factors. The measured partial correlation between the two variables might be spurious if some omitted determinants of workers' productivity (e.g. ability) happen to be correlated with the use of contacts as a job-finding method. For example one could speculate that, leaving occupational choice apart, less able individuals happen to find a larger proportion of their jobs through contacts. Alternatively it could be argued that workers living in more disadvantaged areas and/or subject to a less favorable economic and family background, rely more on social contacts for finding a job, which would also explain the negative sign of the *Contacts* variable if economic and family background are not properly controlled for. Fortunately, the two surveys allow us to investigate these alternative interpretations in some detail.

The US survey contains a variety of additional controls for individual characteristics and ability. We augment our basic specifications by adding the worker's ability to speak clearly in English,[20] dummy variables for whether the person has ever been to jail, suffers from health limitations for work, and lived with both parents until he/she was 16 years old, as well as the years of schooling of the father and the mother (see descriptions in Appendix 2). The results are reported in Panel A of Table 4, which shows that, if anything, the estimated wage discount goes up.[21]

In light of the debate on the effect of measures of cognitive ability on wages (see, for example, Cawley *et al.*, 1997), we also consider a specification that includes the workers' average high school grade. This has also the advantage of limiting the sample to an arguably more homogenous group of workers: those with at least a high school diploma. Panel B presents the wage discount when this variable is added to our basic specifications. Panel C presents the results from combining the controls in Panels A and B with a further dummy variable indicating whether the person used computers in high school, which, as implied by Krueger (1993) and DiNardo and Pischke (1997), may

[20] We also tried the worker's ability to understand English and the results were very similar.

[21] These results do not arise from sample differences. For each specification presented we have reestimated the baseline wage regressions on the sample of the augmented regression and verified that the wage discounts were similar (both in magnitude and significance) to those in Table 3.

further proxy for individual ability. Again, with these controls, the discounts are very similar to—and, in some cases, larger than—those in Table 3.

With the EU data we can further control for the family and economic background of individuals by analyzing the wage effect of social contacts within pairs of siblings who belong to the same family and who share both parents (we exclude step brothers and sisters to better control for genetic traits). In particular, we estimate the equation

$$w_{ijt} - w_{kjt} = \alpha(Contacts_{ijt} - Contacts_{kjt}) + \delta(X_{ijt} - X_{kjt}) + v_{ikjt}, \tag{15}$$

where i and k denote individuals and j denotes a family. Coefficient α now captures the wage differential between an individual who found the job through social contacts and a sibling who did not. Since the time period and the country are common to both siblings, we drop the corresponding dummies from equation (15).

The sample of sibling pairs consists of 3,470 observations (corresponding to 1,739 different pairs). The raw contacts discount in this exercise is 3.1%. Table 5 shows that, once the standard controls are included, it is slightly smaller than in the broader sample, ranging from 3.4% in the baseline to 2.3% with further controls, and still significant. Overall, this evidence suggests that unobserved heterogeneity in family and economic background has some effect on wages but it is unlikely to explain the observed wage discount on contact jobs.

4.5 Compensating differentials

We also investigated whether the wage discount on contact jobs is compensated by other, better non-pecuniary job characteristics—say because, as suggested by Fontaine (2007c), contact jobs pay lower wages due to greater job security and/or more flexible working conditions. To check this possibility, we considered some questions contained in the EU survey on the worker's satisfaction with his/her job in terms of job security and working times (day time, night time, shifts, etc.). The answers follow a numerical scale from 1 (not satisfied) to 6 (fully satisfied), which we convert into a dummy which is equal to 0 for replies 1-3 and equal to 1 for replies 4-6.[22] We included these variables in the wage regression, finding that they are positive and very significant in all specifications. The estimated coefficients for *Contacts*, become for the three ongoing specifications (t-ratios in parentheses) -0.068 (8.1), -0.027 (3.5), and -0.026 (3.4), respectively. These are very similar to the baseline estimates in Panel B of Table 3, which suggests that compensating differentials alone do not explain the wage discount on contacts jobs.

4.6 Other checks

As noted in the Introduction, a large share of the previous literature has focused on employee referrals, stressing their information content, which should translate into a positive effect on wages. In order to purge the *Contacts* variable of referral effects, we tried adding to the wage equation the interaction of *Contacts* with a dummy variable indicating whether the contact person worked for the worker's employer, i.e. whether he or she was an *Insider*, which is observed in the US data. However, this interaction did not attract a significant coefficient, while the coefficient on *Contacts* barely increased in absolute value.[23]

Because of the increasing role of referrals along a worker's professional life, as well

[22] Very similar results were obtained by modifying the grouping criteria, for example by defining the dummy as 0 for replies 1-2 and 1 for replies 5-6.

[23] For example, in the specification including firm-size, industry, and job dummies the t-ratio on the interaction between *Contacts* and *Insider* was 0.03.

as the possibility that experience and specialization compensate the effects of any mismatch associated with earlier occupational choices, it is reasonable to expect the wage discount associated with *Contacts* to be decreasing in a worker's experience. To test this, we extend our initial samples to include workers up to 65 years old (recall that the original samples included workers up to 35 years old only), reestimate the baseline wage regressions, and then add the interaction of *Contacts* with *Experience.* The results appear in Table 6. The wage discount falls with experience, although it keeps its sign (i.e. implying a discount rather than a premium for contact jobs) until around 44 years of experience in the US and 27 years in the EU. This is consistent with the idea that *Contacts* is associated with occupational choices of a long term nature and that the effects of these choices are quite persistent (albeit not permanent).

4.7 The aggregate effects of social contacts

One equilibrium implication of the mismatch induced by social contacts is that, if firms' capital investment is endogenous, an increase in the workers' endowment of contacts will lead to a reduction in firms' investment and in aggregate productivity. This aggregate effect would imply that regions or countries with a higher fraction of jobs found through social contacts should also have lower average wages. Importantly, the magnitude of the effect associated with such a regional-level index of contact should be greater than the result of simply aggregating the effects of the *Contacts* variable in the individual-level wage regressions.

To test this implication we use the EU database, which provides data for 51 regions (see Appendix 2). We regress the average regional hourly wage on the regional averages of the variables included in the wage equations in Table 3. We include region dummies and, given the relatively small number of region-year pairs available (298), we use grouped industries and job dummies. The test amounts to checking whether the effect on the average regional wage of the fraction of workers who found their job through contacts is larger in absolute value than the coefficient of *Contacts* in the individual-level wage regressions of Table 3. Table 7 shows that the aggregate estimates are significantly different from the micro estimates. In fact, the size of the regional-level discount, ranging from 34 to 40%, is strikingly larger than the value obtained in individual-level wage regressions.

5 Conclusions

It is well known that friends and relatives are often a source of useful information for finding jobs. Previous research has emphasized the positive effects of these social contacts in the process of job finding. In this paper we highlight another effect of social contacts, namely that, as they tend to be occupation-specific, they induce some workers to undertake careers in industries, professions, or firms where their comparative productive advantage is not fully exploited. And the sacrifice in terms of productivity may be even larger if, in addition, individuals feel pressed, or prefer, to choose occupations close to those of their friends and relatives (say, in order to comply with social conventions or family traditions). Thus, in economies with dense social networks, some individuals may not fully exploit their productivity potential. The observable implication of this would be a labor market with a high degree of mismatch, depressed aggregate productivity, and low returns to firms' investment. The effects on equilibrium unemployment are instead generally ambiguous.

We have shown, with both US and European Union data, that there is indeed a wage discount, of about 2.5%-3.5% according to the most conservative estimates, for jobs found through contacts. We have also shown that this wage discount is still present after controlling for a long list of characteristics, including measures of cognitive ability and of the economic and family background of individuals. We also find that the contacts discount fades with worker experience but it is still negative for highly experienced workers. Our empirical results for both the US and Europe support the presence of a trade-off between quicker job finding and lower wages and it is consistent with the theoretical prediction that social contacts can distort workers' occupational choices and induce mismatch. With data for European regions, we have further found some evidence of negative externalities associated with the use of contacts, since the regional importance of contacts for job finding depresses average regional wages beyond what would result from the simple aggregation of the wage discounts estimated at the individual level.

Appendix 1. Proofs

Proof of $E(d|\ c=1)-E(d|\ c=0)\leq 0$ >From (8) and (9), one can obtain:

$$E(d|\ c{=}1){-}E(d|\ c{=}0)=\frac{(1{-}\mu)\left\{2\rho\mu sS{-}\mu S^2{-}\left[1{-}(1{-}\mu)\rho\right]\rho s^2{-}(1{-}\rho)\,Ss\left[\mu S+(1{-}\mu)\rho s\right]\right\}}{p(\theta)\,(1+S)\,(1+s)\left[\mu S+(1-\mu)\rho s\right]}.$$

The sign of this expression is given by that of the expression within curly brackets in the numerator. Such an expression has a derivative with respect to μ equal to

$$2\rho sS-S^2-\rho^2s^2-(1-\rho)\,Ss\,(S-\rho s)=-(S-\rho s)^2-(1-\rho)\,Ss\,(S-\rho s)\,,$$

which is negative for any $S>s$ and $\rho\in[0,1]$. But the expression in curly brackets in the numerator is clearly negative at $\mu=0$,

$$-\left[1-\rho\right]\rho s^2-(1-\rho)\,\rho s^2S,$$

so $E(d|\ c=1)$–$E(d|\ c=0)\leq 0$ for all $\mu\in[0,1]$.

Proof of Proposition 4 With $\beta\geq\eta(\theta)$, a sufficient condition for aggregate net income to be decreasing in μ is that the expression in brackets in (13) is strictly negative. This expression is decreasing in a and so it is upper-bounded by

$$A\equiv\beta(S-\rho s)-\frac{s}{1+\gamma s}\frac{(1-\rho)\,(1+\rho s)(1+S)}{\mu\,(1+S)+(1-\mu)\,(1+\rho s)},$$

since we have assumed $a>s$. Moreover, A is increasing in μ. To see this, notice that μ (only) affects the denominators of the two fractions in the second term in A. Thus A is increasing in μ if and only if

$$\frac{d\log(\mu\,(1+S)+(1-\mu)\,(1+\rho s))}{d\mu}+\frac{d\log(1+\gamma s)}{d\mu}\geq 0,$$

which, after using (3), is equivalent to having

$$\frac{S-\rho s}{\mu\,(1+S)+(1-\mu)\,(1+\rho s)}-\frac{s}{1+\gamma s}\frac{(1-\rho)(1+S)(1+\rho s)}{[\mu\,(1+S)+(1-\mu)\,(1+\rho s)]^2}\geq 0.$$

By using again (3) to substitute for γ and after some algebra, this condition can be further simplified to:

$$(S-\rho s)\{[1+\mu(S-\rho s)+\rho s](1+s)-\mu(1-\rho)(1+S)s\}\geq s(1-\rho)(1+S)(1+\rho s),\quad(16)$$

whose left hand side is increasing in μ. Thus, a sufficient condition for (16) can be found by evaluating its left hand side at $\mu=1$, in which case, the resulting condition becomes $(S-s)(1+\rho s)\geq 0$, which is always satisfied. This completes the proof that A is increasing in μ. But this means that A is upper-bounded by its particularization for $\mu=1$:

$$A'\equiv\beta(S-\rho s)-s\,(1-\rho)\,.$$

Clearly, A' is strictly negative if and only if $\beta<(1-\rho)/[(1-\rho)+(S/s-1)]$, which is the second inequality in the condition stated in the proposition.

Appendix 2. Data classifications and definitions

United States (Multi-City Study of Urban Inequality, 1992-1994)

The survey covers 8,916 individuals, but the questions about job finding methods were not asked in Detroit and were asked to only 3,357 individuals in the other three cities. The availability of personal characteristics and reliable data on wages reduces the sample to 2,640 observations, of which 1,653 correspond to currently employed workers (employees, as well as workers on temporary layoff or on sickness/maternity leave), of which 927 are younger than 35 years old. Among these, information on the length of the preceding unemployment spell is available for 242 individuals. The survey date for each city is unavailable. The description of the variables, the relevant categories in the case of dummy variables, and any further selection criteria are as follows:

Hourly wage. Pre-tax hourly wage including tips and bonuses, computed as the total amount divided by the reported number of hours. We exclude observations flagged by the survey as possible data entry errors, i.e. when the computed wage is greater than $50 per hour and not reasonable based on the respondent's occupation, or it is less than $2 per hour.

Experience. Age minus the age at which the person left full-time schooling and was not in school for 16 months or more.

Firm size. 1-4, 5-19, 20-49, 50-99, 100-499, and 500 or more employees.

Industry. 1) Agriculture, forestry, and fisheries, 2) Mining, 3) Construction, 4) Manufacturing, 5) Transportation, communications and other public utilities, 6) Wholesale trade, 7) Retail trade, 8) Finance insurance and real estate, 9) Business and repair services, 10) Personnel services, 11) Entertainment and recreation services, 12) Professional and related services, and 13) Public administration.

Grouped industry. Agriculture (Industry dummy 1), Manufacturing (Industry dummies 2 and 4), Construction (Industry dummy 3), and Services (Industry dummies 5-13).

Job. 1) Managerial, 2) Technical, 3) Services, 4) Farming, 5) Crafts, and 6) Operators.

Grouped job. White collar (Job dummies 1-3) and Blue collar (Job dummies 4-6).

Average high school grade. 1) D or lower, 2) D+/C-, 3) C, 4) C+/B-, 5) B, 6) B+/A-, and 7) A.

Ability to speak clearly in English (as perceived by the interviewer). 1) Poor, 2) Fair, 3) Good, 4) Very good, and 5) Excellent.

Been to jail. Affirmative response to "Have you ever been held in reform school, a detention center, jail, or prison?"

Health limitations. Affirmative response to “Does your health or general condition limit the kind or amount of work you can do?”

Lived with parents. Affirmative response to “Did you live with both of your parents most of the time until you were 16 years old”

Unemployment duration. Based on the answer to “How long (did you look/have you been looking) for work?” Data are reported in days, which we convert to months. We restrict the sample to durations up to 60 months (five years), to avoid confusing unemployment with inactivity.

European Union (European Community Household Panel, 1995-2001)

The initial sample contains 129,685 observations with information on all regressors, which become 129,318 after dropping unreliable wage observations. The sample of employees younger than 35 years old is composed of 71,605 observations, and keeping those observed in their first job that is permanent leaves us with a final sample of 17,262 observations, corresponding to 7,021 individuals. From these, the sample of workers whom we observe first in full-time education and then entering unemployment (up to 1999) and follow them until they find a job is limited to 252 individuals. The description of the variables, the relevant categories in the case of dummy variables, and any further selection criteria are as follows:

Hourly wage. Net monthly wage divided by the total number of hours worked per month in the worker’s main and additional jobs, expressed in a common currency (dollars). We drop monthly wages below $100 and hourly wages below $1.

Education. 1) Below secondary education (up to first stage of secondary education), 2) Secondary education (at least second stage of secondary education), 3) Tertiary education.

Experience. Age minus the age at which the person started his/her first job or business.

Firm size. As in the US data.

Industry. 1) Agriculture, hunting, and forestry, fishing, 2) Mining and quarrying, electricity, gas, and water supply, 3) Manufacturing of food products, beverages, and tobacco, 4) Manufacturing of textiles, clothing, and leather products, 5) Manufacturing of wood and paper products, publishing and printing, 6) Manufacturing of coke, refined petroleum, chemicals, rubber and plastic, 7) Manufacturing of metal products, machinery, and equipment, 8) Other manufacturing, 9) Construction, 10) Wholesale and retail trade, repair of motor vehicles, motorcycles, etc., 10) Hotels and restaurants, 11) Transport, storage, and communications, 12) Financial intermediation, 13) Real estate, renting, and business activities, 14) Public administration and defense, compulsory social security, 15) Education, 16) Health and social work, and 17) Other community, social, and personal service activities.

Grouped industry. Agriculture (Industry dummy 1), Manufacturing (Industry dummies 2-8), Construction (Industry dummy 9), and Services (Industry dummies 10-17).

Job. 1) Legislators and managers, 2) Small firm managers, 3) Science and health professionals, 4) Teaching, 5) Other professionals, 6) Science and health associated professionals, 7) Teaching and other associate professionals, 8) Office and service clerks, 9) Personal and protective services, 10) Models, salespersons, 11) Skilled agriculture and fishery workers, 12) Extraction, building, and other craft, 13) Metal and precision, printing, etc., 14) Plant operators, drivers, 15) Machine operators, assemblers, 16) Miscellaneous operators, 17) Sales and service elementary tasks, 18) Agricultural and fishery laborers, 19) Mining, construction, manufacturing, and transport laborers, and 20) Miscellaneous laborers.

Grouped job. High skill jobs (Job dummies 1-5), Medium skill jobs (Job dummies 6-16), and Low skill jobs (Job dummies 17-20).

Regions. NUTS 2 classification for Portugal, Sweden, and the United Kingdom, and NUTS 1 for the remaining countries, except for Finland, Ireland, and the Netherlands, where it is more aggregated.

Permanent job. Job with a permanent contract. The other types are: fixed-term or short-term contract, casual work with no contract, or some other working arrangement.

Sibling. A brother or sister sharing both parents (step brothers and sisters are excluded) and living together.

Unemployment duration. Number of months of continuous unemployment before current job. We restrict the sample to durations up to 27 months, since grouping of frequencies beyond this value suggests measurement error/rounding.

Satisfaction with job security. Based on the answer to "How satisfied are you with your present job in terms of job security?" Measured through a dummy equal to 0 for answers 1 (not satisfied) to 3 and equal to 1 for answers 4 to 6 (fully satisfied).

Satisfaction with working times. Based on the answer to "How satisfied are you with your present job in terms of working times (day time, night time, shifts etc.)?" Same dummy construction as for the previous variable.

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Table 1. Sample characteristics of the data

Jobs found through:	Contacts		Other channels	
	Mean	St. dev.	Mean	St. dev.
A. United States:				
Age (years)	27.3	3.9	27.9	3.9
Male	56.2	49.7	47.2	50.0
Born in the US	50.2	50.1	66.0	47.4
White	22.2	41.6	33.3	47.2
Black	26.8	44.3	30.8	46.2
Asian	6.3	24.4	12.8	33.5
Other race	44.7	49.8	23.1	42.2
Schooling (years)	12.1	3.0	13.5	2.6
Experience (years)	8.7	4.7	8.2	4.8
Works for a small firm	72.3	44.8	62.6	48.4
Hourly wage (US $, gross of taxes)	8.5	4.3	10.6	5.2
Unemployment duration (months)	2.1	6.9	3.0	3.4
No. of observations	459		468	
B. European Union:				
Age (years)	26.3	4.4	26.8	4.4
Male	55.4	49.7	51.8	50.0
Born in country of residence	95.5	20.8	96.0	19.7
Below secondary education	40.1	49.0	27.9	44.9
Secondary education	44.1	49.7	47.1	49.9
Tertiary education	15.8	36.5	24.9	43.3
Experience (years)	6.5	4.4	6.5	4.4
Works for a small firm	82.6	38.0	69.6	37.3
Hourly wage (US $, net of taxes)	4.8	2.6	6.0	3.3
Unemployment duration (months)	9.4	9.4	11.1	13.0
No. of observations	5,317		11,945	

Note: The sample corresponds to the baseline specification in Table 3, Panels A (US) and B (EU). Unless otherwise indicated, means are in percentage shares, standard deviations of fractions, multiplied by 100. Period: US, 1992-1994; EU, 1995-2001. The EU sample corresponds to 7,021 individuals. The samples for unemployment duration are those used in Table 2. Sample sizes are: 118 for contacts and 124 for other channels in the US, and 89 and 163, respectively, in the EU. Variable definitions are in Appendix 2.

Table 2. Contacts and unemployment duration

Dependent variable: unemployment duration in months

Coefficient on *Contacts*:	Coeff.	t	$\overline{R}^2$	Obs.
A. United States:				
Baseline	-1.30	(1.74)	0.00	242
Firm-size and industry dummies	-1.46	(1.90)	0.00	238
Firm-size, industry, and job dummies	-1.47	(1.90)	0.00	238
B. European Union:				
Baseline	-0.86	(1.89)	0.13	4,497
Firm-size and industry dummies	-0.88	(1.94)	0.15	4,406
Firm-size, industry, and job dummies	-0.87	(1.91)	0.15	4,306
C. European Union (entrants into unemployment):				
Baseline	-2.69	(1.77)	0.25	252
Firm-size and industry dummies	-2.99	(1.98)	0.23	249
Firm-size, industry, and job dummies	-3.07	(2.05)	0.23	248

Note: OLS regressions. Panels A and B correspond to employed workers, Panel C to employed workers for whom a complete unemployment spell is observed (after full-time education). Period: US, 1992-1994; EU, 1995-2001. The baseline specification includes: US: a constant, dummies for city, gender, race, and for being born in the US, years of schooling, experience, and experience squared; EU: a constant, dummies for year, country, gender, schooling, and for being born in the country of residence, experience, and experience squared. The second line adds firm-size and industry dummies, and the third line further adds job dummies. Industries and occupations have been grouped to save degrees of freedom. In Panels A and C the number of observations is the same as the number of individuals. In Cols. (1) to (3) of Panel B the samples correspond to 1,857, 1,834, and 1,794 individuals, respectively, with t-ratios adjusted for repeated observations on the same worker. Variable definitions are in Appendix 2.

Table 3. The wage discount on contact jobs

Dependent variable: log hourly wage

	(1)		(2)		(3)	
	Coeff.	t	Coeff.	t	Coeff.	t
A. United States:						
Contacts	-0.074	(2.72)	-0.074	(2.84)	-0.056	(2.18)
Male	0.096	(3.63)	0.125	(4.63)	0.129	(4.71)
Born in the US	0.185	(5.03)	0.155	(4.36)	0.148	(4.23)
White	0.108	(2.45)	0.119	(2.83)	0.101	(2.43)
Black	-0.008	(0.20)	0.043	(1.07)	-0.036	(0.91)
Asian	0.206	(3.94)	0.233	(4.66)	0.212	(4.32)
Schooling	0.072	(12.12)	0.059	(9.96)	0.044	(7.03)
Experience	0.021	(2.19)	0.023	(2.53)	0.026	(2.90)
Experience squared	-0.003	(0.59)	-0.001	(0.93)	-0.001	(1.36)
$\overline{R}^2$	0.32		0.38		0.41	
No. of observations	927		921		921	
B. European Union:						
Contacts	-0.070	(8.30)	-0.028	(3.61)	-0.026	(3.41)
Male	0.092	(11.95)	0.078	(10.18)	0.078	(9.66)
Born in same country	-0.004	(0.21)	0.006	(0.32)	0.001	(0.08)
Secondary education	0.509	(38.93)	0.369	(29.71)	0.248	(19.91)
Tertiary education	0.208	(23.97)	0.145	(17.87)	0.112	(13.75)
Experience	0.041	(16.69)	0.034	(15.18)	0.034	(15.54)
Experience squared	-0.001	(7.82)	-0.001	(6.62)	-0.001	(6.77)
$\overline{R}^2$	0.67		0.72		0.74	
No. of observations	17,262		16,904		16,502	
C. European Union (entrants into unemployment):						
Contacts	-0.131	(2.77)	-0.093	(1.92)	-0.092	(1.95)
Male	0.069	(1.60)	0.093	(2.06)	0.101	(2.28)
Born in same country	-0.058	(0.40)	-0.008	(0.05)	0.008	(0.05)
Secondary education	0.430	(5.55)	0.519	(6.18)	0.488	(4.38)
Tertiary education	0.110	(2.14)	0.151	(2.88)	0.153	(2.92)
Experience	0.046	(3.06)	0.048	(2.96)	0.051	(3.19)
Experience squared	-0.003	(2.12)	-0.003	(2.14)	-0.003	(2.27)
$\overline{R}^2$	0.57		0.63		0.64	
No. of observations	261		239		238	

Note: OLS regressions. Panels A and B correspond to employed workers, Panel C to employed workers for whom a complete unemployment spell is observed (after full-time education). Period: US, 1992-1994; EU, 1995-2001. Other controls: US: a constant and city dummies; EU: a constant, and year and country dummies. Col. (2) adds firm-size and industry dummies, and Col. (3) further adds job dummies. In Cols. (1) to (3) of Panel B the samples correspond to 7,021, 6,903, and 6,760 individuals, respectively, with t-ratios adjusted for repeated observations on the same worker. Reference: US: female, born abroad, other race. EU: female, born abroad, less than secondary education. Variable definitions are in Appendix 2.

Table 4. The wage discount on contact jobs: Further controlling for individual ability (US sample)

Dependent variable: log hourly wage

Coefficient on *Contacts*:	Coeff.	t	$\overline{R}^2$	Obs.
A. Controlling for personal background:				
Baseline	-0.090	(2.80)	0.32	656
Firm-size and industry dummies	-0.088	(2.81)	0.37	651
Firm-size, industry, and job dummies	-0.072	(2.31)	0.39	651
B. Controlling for the average high school grade:				
Baseline	-0.070	(2.35)	0.29	741
Firm-size and industry dummies	-0.072	(2.51)	0.36	738
Firm-size, industry, and job dummies	-0.057	(2.04)	0.39	738
C. Controlling for the above and the use of computers at high school:				
Baseline	-0.078	(2.27)	0.28	580
Firm-size and industry dummies	-0.076	(2.30)	0.35	578
Firm-size, industry, and job dummies	-0.064	(1.96)	0.37	578

Note: OLS regressions based on the US sample of employed workers. The baseline specification includes the same variables as in Table 2. The second line of each of the panels in the table adds firm-size and industry dummies, and the third line further adds job dummies. Regressions in Panel A add dummies for the ability to speak clearly in English, whether the person has ever been to jail, whether he/she suffers from health limitations for work, whether he/she lived with both parents until he/she was 16 years old, and the years of schooling of the father and the mother. Alternatively, Panel B only adds the average high school grade. Panel C adds the controls of Panels A and B altogether as well as a dummy variable for whether the person used a computer at high school. Variable definitions are in Appendix 2.

Table 5. The wage discount on contact jobs:
Controlling for family background
(EU sample)

Dependent variable: log hourly wage difference between siblings

Coefficient on *Contacts*:	Coeff.	t	$\overline{R}^2$	Obs.
Baseline	-0.034	(3.06)	0.13	3,470
Firm-size and industry dummies	-0.023	(2.11)	0.16	3,430
Firm-size, industry, and job dummies	-0.023	(2.13)	0.19	3,364

Note: OLS regressions. The samples correspond to employed workers. Period: 1995-2001. The baseline specification includes the same variables as in Table 2. The second line adds firm-size and industry dummies, and the third line further adds job dummies. The first to third lines report the coefficients on *Contacts*, for samples of 1,739, 1,710, and 1,684 matched pairs of siblings, respectively. t-ratios are adjusted for repeated observations on the same pair. Variable definitions are in Appendix 2.

Table 6. The wage discount on contact jobs: The effect of experience

Dependent variable: log hourly wage

Coefficient on:	*Contacts*		*Contacts × Experience*			
	Coeff.	t	Coeff.	t	$\overline{R}^2$	Obs.
A. United States:						
Baseline	-0.143	(3.73)	0.005	(2.40)	0.33	1,653
Firm-size and industry dummies	-0.134	(2.46)	0.005	(2.46)	0.40	1,643
Firm-size, industry, and job dummies	-0.101	(3.16)	0.004	(1.93)	0.43	1,643
B. European Union:						
Baseline	-0.073	(13.80)	0.001	(4.36)	0.65	129,318
Firm-size and industry dummies	-0.038	(7.75)	0.001	(2.58)	0.69	123,151
Firm-size, industry, and job dummies	-0.030	(6.21)	0.001	(2.44)	0.71	121,081

Note: OLS regressions. The samples correspond to employed workers aged up to 65 years old. Period: US, 1992-1994; EU, 1995-2001. The baseline specification includes the same variables as in Table 2. In each panel, the second line adds firm-size and industry dummies, and the third line further adds job dummies. In the first to third lines of Panel B the samples correspond to 40,665, 39,795, and 39,276 individuals, respectively, and t-ratios are adjusted for repeated observations on the same worker. Variable definitions are in Appendix 2.

Table 7. The regional contacts discount in the EU

Dependent variable: log average regional hourly wage

Coefficient on *Contacts*:	Coeff.	t	$\overline{R}^2$	Obs.	Test for Aggregate eff.
Baseline	-0.336	(2.35)	0.95	298	0.07
Firm-size and industry dummies	-0.369	(2.21)	0.96	298	0.05
Firm-size, industry, and job dummies	-0.403	(2.51)	0.96	297	0.03

Note: OLS regressions. The samples correspond to employed workers. Period: 1995-2001. The baseline specification includes a constant, year dummies, region dummies, regional average experience and experience squared, and the regional fractions in the reference population who are male, born in the country of residence, and in each schooling group. The second line adds regional fractions working at each firm-size class and in each industry, and the third line further adds the fractions in each aggregate job category. Industries and job categories have been grouped to save degrees of freedom. The last column shows the p-value of a one-sided test for the aggregate effect, i.e. whether the estimated coefficient is significantly larger in absolute value than the point-value of the equivalent estimated coefficients in Panel B of Table 3 (individual wage regressions): i.e. -0.070, -0.028, and -0.026, respectively. There are 51 regions, and t-ratios are adjusted for repeated observations on the same region. Variable definitions are in Appendix 2.