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ABSTRACT

Information Spillover and Factor Adjustment*

We investigate the role of information spillovers (IS) in determining firms' labour adjustments. We test the proposition that information on relevant state variables spills over through one firm's decision to affect those of other firms. Our test is based on the assumption that spillovers matter only among firms that are both similar and geographically close. Using a large panel of manufacturing firms, we identify those that are located in a given industrial district and produce the same goods as satisfying both criteria. We propose a solution to the identification problem typical of the empirical analysis of social effects. Our results show that firms' decisions are indeed affected by those of similar, neighbouring firms, while the actions of firms not satisfying either of the criteria have no impact. We test other implications of the theory and find further supporting evidence of the relevance of IS. First, measures of extreme adjustments exert a stronger influence than mean adjustments; second, smaller firms seem to rely more on external sources of information; third, the effects depend on a number of the reference group's characteristics, such as its size and the presence of large firms. Finally, given that firms exposed to IS tend to adjust simultaneously, we find that spillovers amplify the effect of aggregate shocks and constitute a powerful mechanism of amplification of the business cycle.

JEL Classification: D21, D83, E32
Keywords: information spillovers, factor adjustment, business cycle, social learning

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NON-TECHNICAL SUMMARY

Information is central to any agent's decision-making process. One way in which agents can acquire useful information is through social learning, i.e. by observing the behaviour of other agents that face the same problem. Lack of information about some underlying state variable of public interest can be made up for, at least partly, by looking at what other similar agents do. If the information that is privately available to agent A to form his decisions has some value for agent B (a neighbour of A) the observation of A's actions can help B to make a better decision since A's actions carry implications about his information. In other words, private information spills over through individual actions. This process of social learning can be seen at work in a variety of situations: for instance, a decision to enter a new market with uncertain demand is likely to be affected by the observation of other firms entering and the performance of previous entrants. Similarly, the decision to undertake an investment project or to hire or dismiss workers may draw on the observation of what neighbouring firms do. On the consumer's side, the decision to try a new product is likely to be influenced by the observed popularity of the product with other consumers; the same holds for the choice of a book or a movie. Information spillovers have been indicated as an important factor in the timing and extent of the recent crisis in Asia, as investors were learning about the structural problems of those economies at the same time as the crisis was erupting.

While the theory is relatively well developed, there are essentially no studies testing its empirical validity. Our intention is to fill this gap. We rely on a large panel of Italian industrial firms with about 30,000 firms per year for 15 years, which allows us to classify them into two groups, those that are more likely to be exposed to information spillovers and a control group for which information is unlikely to be passed on through their actions. The idea is that social learning will only take place if a) a firm's actions convey useful information because its problem is analogous to that faced by other firms and b) these actions are readily observable. Thus, to identify exposure to information spillovers we rely on firms' similarity, identified with their product branch and proximity, i.e. geographical distance. Similarity assures that other firms' actions potentially contain valuable information, proximity implies that they are readily observable. In order to classify firms according to the degree of exposure to information spillovers, we use location within an industrial district. One interesting feature of the Italian economy is that firms, particularly small and medium-sized firms specializing in a particular good, (ties, chairs, shoes and leather goods, textiles, corks, etc.), tend to group together in the same area, which becomes an 'industrial district'. Presumably firms in a district

should be more exposed to spillovers than firms in the same industry that are not part of a district.

Our test for information spillovers turns on the adjustment of productive factors. We relate the labour adjustment of a given firm in a given sector and located in a given district to the labour adjustment of the other firms in that sector and district and to that of firms in the same sector but outside the district or located in the district but producing unrelated goods. If information spillovers are present, we expect that, controlling for shocks, one firm's adjustment is affected by the adjustments of firms in the same district and sector but not by that of firms outside the district or the sector. Furthermore, for firms not located in a district, what other similar firms do should be irrelevant. We regress each firm's labour adjustment on a set of controls and on various measures of the adjustment of other firms. After carefully controlling for shocks that are common to all firms in the same district and sector and for firm-specific shocks, each firm's factor adjustment is positively and significantly affected by the average adjustment of the other firms in the same district and sector (i.e. the reference group): an increase in employment of 1% in the firms in the reference group leads to a response of approximately a third of a point by each other firm in the group. This is a remarkable effect and is clearly consistent with the idea that firms rely heavily on the information contained in the actions of other, similar firms. The actions of non-reference group firms have no impact. This result is robust to alternative specifications, to disaggregation by sector and to different measures of neighbours' adjustments. We further rule out alternative interpretations of our findings such as imperfect control for common shocks and different degrees of sectoral heterogeneity among reference and non-reference group firms. We further find that firms react more to indicators of large adjustments, such as the 10th and 90th percentiles of the adjustment of others, which is consistent with the idea that larger adjustments carry more information. In fact, while small changes in employment may reflect 'business as usual', a dramatic change in a firm's labour force could influence the information set of its competitors more powerfully and thus prompt emulative action. In addition, after controlling for large adjustment, the average adjustment of the reference group is no longer significant. We interpret this as ruling out 'real spillovers', such as those due to technological externalities that increase all firms' productivity: if the latter were driving our results, then one should expect that what matters is the average adjustment. Finally, dividing the sample with quartiles by size, we find that smaller firms are more reactive than larger ones to their neighbours' adjustment, which is consistent with the idea that large firms have alternative sources of information and a better capacity to process it and thus rely less on local sources.

One strand of the theoretical literature on information spillovers deals with the business cycle implications of social learning. The fact that agents can extract useful information from the actions of others constitutes an incentive to postpone action by comparison with a situation of strictly individual learning. Once some agents do act, however, the information could induce further actions, triggering a snowball effect. We should therefore expect a positive correlation between the individual and the aggregate level of activity, even after controlling for exogenous causal factors. As a consequence, there may be periods of low activity, in which the incentive to delay dominates, followed by surges in activity, perhaps without large changes in the underlying state variables. We test this proposition by exogenously identifying ‘adjustment years’, i.e. years in which adjustment intensity is particularly strong, and by noticing that the theory implies that firms exposed to information spillovers (in our case district firms) should be less sensitive than non-district firms to aggregate shocks in non-adjustment years and more sensitive in adjustment years. Non-exposed firms should show no substantial differences between adjustment and non-adjustment years, given that for them all that matters is presumably the observation of the shocks. We find evidence consistent with these implications: district firms’ reaction to aggregate shocks in adjustment years is stronger than in non-adjustment years and the former is stronger than the reaction of non-district firms. Furthermore, we cannot statistically reject the hypothesis that non-district firms’ sensitivity to aggregate shocks is the same in adjustment and non-adjustment years. Further, the extra sensitivity to aggregate shocks in adjustment years is weakened when we control for the adjustment of others, which is consistent with information spillovers being the source of the extra sensitivity.

Overall, we view our findings as strong evidence of the relevance of strategic learning to firms’ decisions. Future work should go deeper into the determinants of the amount of learning, its consequences for performance and the environments that are most conducive to social learning. We have taken a preliminary step in this direction by extending our analysis to the structure of the reference group and its effect on performance. One result in the literature is that the diffusion of information works better and therefore the performance of firms should be superior, if the reference group is large and if there are no informational-dominant firms, i.e. firms whose actions are observed by all others. We test these implications, selecting only district firms. Measuring performance as gross ROA and indicating information leadership by the share of the top three firms in the reference group’s sales, we find that the stronger information leadership is, the worse firms’ performance is; this implies that performance in districts with dominant players is systematically worse than in districts with none, which accords with the theory. Finally, we find that performance improves with number of firms in the district, which is consistent with the idea that environments where information spillovers are more intense fare better.

1 Introduction

Recent years have seen the emergence of a new literature that emphasizes the interaction between information acquisition and agents' decisions. Although there are various strands, the common feature of this literature is that agents can acquire useful information through social learning, i.e. by observing the behavior of other agents facing the same problem. The central idea is that the lack of information about some underlying state variable that is of public interest can be made up for, at least partly, by looking at what other, similar agents do. If the information that is privately available to agent A to form his decisions has some value for agent B - a neighbor of A - the observation of A's actions can helpB to make a better decision since A's actions will partly reveal his information.

More generally, consider situations where a pool of agents are uncertain about some relevant common variable and can learn about it through time by direct accumulation of information. Suppose each agent has some private piece of information which, if pooled with the others' would increase the information available to each. If pooling is ruled out, each agent's private information will be embedded in his decisions; thus, the other agents' choices become an alternative source of information. As a consequence, individual agents' decisions will be affected both by their private information and by other agents' decisions. In other words, private information spills over through individual actions.

This process of social learning can be seen at work in a variety of situations; for instance, a decision to enter a new market with uncertain demand is likely to be affected by the observation of other firms entering and the performance of previous entrants. Similarly, the decision to undertake an investment project or to hire or lay-off workers may benefit from the observation of what neighbor firms do. On the consumer's side, the decision to try a new product is likely to be influenced by the observed popularity of the product with other consumers; the same holds for the choice of a book or a movie. During a bank run as well, the single depositor's decision to withdraw his money will depend not only on his own assessment but also on what the other depositors do, as this may reveal valuable information on the fragility of the bank. Information spillovers have been indicated as one of the important factors in determining the timing and extent of the recent crisis in Asia, as investors were learning about the structural problems of those economies at the same time as the crisis was erupting.

The theory is relatively well developed, then, but there are essentially no studies testing its empirical validity. Our intention is to fill this gap. To this end we rely on a panel of Italian industrial …rms that allows us to classify them into two groups, a study group of …rms that are more likely to be exposed to information spillovers and a control group for which information is unlikely to be passed on through their actions. The idea is that social learning will only take place if a) …rms' actions convey useful information because their problem is analogous to that faced by other …rms and b) these actions are readily observable. Thus, to identify exposure to information spillovers we rely on …rms' similarity, identi…ed with their product brand, and proximity, de…ned in terms of geographical distance. Similarity assures that other …rms' actions potentially contain valuable information, proximity implies that they are easily observable.

To classify …rms according to the degree of exposure to information spillovers we use location within an industrial district. One interesting feature of the Italian economy is that often …rms, particularly small and medium sized …rms specialized in the production of a particular good, such as ties, chairs, shoes and leather goods, textiles, corks, etc., tend to group together in the same area, which becomes an industrial district. Presumably …rms in a district should be more exposed to spillovers than …rms in the same industry that are not part of a district.

Our test for the existence of information spillovers turns on the adjustment of productive factors. We relate the factor adjustment of a given …rm in a given sector and located in a given district to the adjustment of the other …rms in the same sector and district and to that of …rms that are in the same sector but outside the district or are located in the district but produce unrelated goods. If information spillovers are present, we expect that - controlling for shocks - one …rm's adjustment is a¤ected by the adjustments of …rms in the same district and sector but is una¤ected by that of …rms outside the district or the sector. Furthermore, for …rms not located in a district what other, similar …rms do should be irrelevant. Our …ndings are consistent with the idea that learning takes place not only through the direct signals that a …rm receives on its market environment but also by exploiting the information contained in other …rms' actions. Indeed, if each …rm has just one small, independent piece of information and there are many participants, the information contained in other …rms' action may be much more valuable than that directly available to the …rm.

We start in Section 2 by reviewing the theoretical literature on information spillovers and …rms' decisions. In Section 3 we lay out a simple analytical framework to organize our empirical strategy and derive the main implications to be tested. Section 4 addresses the identi…cation problem that emerges in estimating models with social interactions. In Section 5 we describe the data and discuss how we measure exposure to information spillovers. Section 6 presents the results of the estimates for the adjustment of labor in our basic specification, and Section 7 extends the estimates in various directions and checks their robustness to changes in specification and sample selection. Section 8 tests some implications of information spillovers for firms' response to aggregate shocks, showing that they can be a powerful mechanism of amplification of business fluctuations . Section 9 considers the effect of different reference group structures on the learning process and, through that, on the performance of firms. Section 10 concludes.

2 Literature review

The theoretical literature on information spillovers studies how social learning influences the decision-making of an agent who faces an optimization problem in an uncertain environment. The focus is on how the private information of the agents is transmitted through actions, and how information spillovers influence the timing and outcomes of the decision-making process. A useful classification is based on timing. A first group of models assumes that actions are taken sequentially and at a pre-set time, and that before taking her decisions each agent can observe the actions of the previous agents. This literature is mainly concerned with the possibility of information cascades, which occur when agents disregard their own private information and base their action only on the history of previous actions.[1] A second class of models, which is the direct reference of our empirical work, endogenizes the timing of actions, so that in each period all agents can decide their course of action, unless they have already made an irreversible decision. Chamley and Gale (1994) consider the case of a group of agents that get the option to make an investment of uncertain value (but perfectly correlated across agents); the value, in turn, is positively correlated with the unknown fraction of agents in the population that get the option. They show that the equilibrium involves inefficient delay, because each agent has an incentive to wait to see how many others exercise the option, to better asses the optimality of doing so. Caplin and Leahy (1994) study a model of

[1] See the seminal contributions of Banerjee (1992) and Bikhchandani et al. (1992), or Bikhchandani et al. (1998) for a recent survey. Anderson and Holt (1997) find that information cascades tend to occur frequently in controlled experiments. In a recent paper, Avery and Zemsky (1998) show that they cannot arise in financial markets if there is a market maker that observes the previous pattern of transactions, because the latter will price according to such information, thus prompting the agent to resort to private information.

a multi-stage investment project with a continuum of …rms, in which at each stage each agent receives a private signal about the common value of the project, and decides whether to continue the investment or to pull out. In their equilibrium, actions have an extremely discontinuous character, with a phase of no action followed by a period in which the actions of a fraction of agents totally resolve uncertainty, thus prompting a large mass of agents to act simultaneously in the subsequent period. Rob (1991) and Horvath et al. (1997) study the e¤ect of the information revealed by previous entrants on subsequent entry into a market with unknown demand. Rob obtains an entry pattern that is monotonically decreasing over time, whereas Horvath et al. get di¤erent entry patterns according to the structure of uncertainty, including paths with a discontinuous character, in which most of the …rms enter the market in a short period of time. Backing away from full rationality, Ellison and Fudenberg (1993, 1995) propose a model in which boundedly rational agents choose between two alternative technologies according to a rule that weights new information and the behavior of other agents. Their models o¤er an alternative way to rationalize the correlation between individual and aggregate decisions and show that even naive rules can lead to socially e¢ cient outcomes. In an extension of the model, they introduce di¤erent locations and assume that each agent looks only at the decisions of people in the neighborhood; this idea constitutes the basis of our own empirical approach. In a similar framework, Bala and Goyal (1998) study the role of the structure of the reference group on the learning process. They show that if the group has a small subset of players with particularly high visibility, such as a few large …rms in an environment of small …rms, then information cascades can occur even where the timing of actions is endogenous and choices are repeated over time, with negative e¤ects on …rms' pro…tability. At the same time, the probability of a cascade occurring decreases with the size of the reference group. We will test the validity of these predictions in the context of our empirical speci…cation.

One of the main implications of this strand of the literature is that, under certain informational conditions, the pattern of agents' actions will follow a distinctive path. If decisions are costly to revert, the fact that each agent can extract useful information from the actions of others constitutes an incentive to delay actions by comparison with the case where learning is strictly individual. Once some agents act, however, the information revealed could induce further actions, triggering a self-reinforcing process that will lead a large number of agents to acting within a brief period. We should therefore observe a positive correlation between the individual and the aggregate level of activity, even after controlling for exogenous causal factors.

In addition, one should observe periods of low activity, in which the incentive to delay dominates, followed by periods of sharp increase in the level of activity, without large changes in the underlying state variables.[2] Schivardi (1998) applies this idea to explain the large increase in job destruction in cyclical troughs (Davis et al., 1996), showing how relatively small aggregate shocks can induce a burst of reallocation if they touch o¤ information revelation.

We are not aware of any empirical study directly addressing information spillovers. Some parallel lines of research should be kept in mind, however, and may be usefully reviewed to better identify the speci…city of information spillovers. One such is the literature on location choices in relation to knowledge spillovers. This literature has been inspired by the recent surge of interest in economic geography, which stresses that production tends to be concentrated in regions that specialize in some particular product.[3] The idea is that there might be substantial increasing returns from concentrating …rms in a given location, due to knowledge spillovers, which occur when the expertise and the R&D of one …rm bene…ts the neighbors. While the results are far from conclusive, a consensus has emerged that knowledge spillovers are an important factor in …rms' location. For example, Ellison and Glaser (1997) construct a series of indexes to study concentration and show that, for the US economy, production is more concentrated than a random distribution of …rms would predict, even controlling for the natural characteristics of the regions. Moreover, the narrower the de…nition of the sectors, the greater the degree of concentration. We see these results as complementary to our own. This literature focuses on knowledge spillovers, i.e. spillovers that directly a¤ect productivity. We consider, instead, the e¤ects of actions through the changes they induce in the information set, without necessarily inducing a change in any real variable. Moreover, our analysis is at high frequencies, considering the changes in factors in response to business cycle shocks, while economic geography takes a longer-run perspective, stressing the knowledge spillovers as factors in the long-term development of regions and in growth.

Our work is also related to the macroeconomic literature on production spillovers at business cycle frequencies,[4] initiated by Caballero and Lyon

[2] Models that formalize this idea are presented, among others, by Caplin and Leahy (1994, 1996), Chamley and Gale (1994), and Horvath {nem et al.} (1997).

[3] See for example Audtretsch (1998), Feldman and Audretsch (1998), Harrison et al. (1996), Ja¤e et at (1993), Wallsten (1998).

[4] See for example Basu and Kimball (1997), Caballero and Lyon (1992), Hall (1988), Jimenez and Marchetti (1998) and Sbordone (1997).

(1992). This literature is predicated on the observation that labor productivity is procyclical. This could be interpreted as a sign that the productivity of the single firm is positively affected by the aggregate level of activity, due to some form of thick-market externality (Diamond, 1982). External economies could then induce a positive correlation across firms in factors demand, independently from information spillovers. In recent years, a body of literature has challenged the assertion that the Solow residual is procyclical, claiming that the empirical finding disappears once one considers variations in effort, intermediate goods, cyclical variations in capital utilization and aggregation effects.[5] Moreover, even assuming that the empirical finding is correct, there are other explanations for pro-cyclical Solow residuals, such as labor hoarding (Basu and Kimball, 1997), internal increasing returns (Hall, 1988) or changes in the distribution of firms' productivity due to entry and exit (Horvath, 1999). Due also to Sbordone (1997), who considers the different dynamic implications of the alternative explanations, a consensus has emerged that external effects cannot be the main reason for the procyclical nature of productivity. Although we also offer direct evidence on the importance of information flows in inducing positive correlation in labor adjustments, we consider that the consensus view's underplaying the role of external economies contributes to ruling out an alternative explanation for our empirical findings.

3 A simple analytical framework

To illustrate our empirical specification, we construct a simple reduced-form model that abstracts from the strategic aspects underlying information spillovers, which are discussed in the papers reviewed in Section 2. Assume that the prospects of a firm at time t are summarized by a state variable $X(t)$, which is a sufficient statistic for determining the optimal level of the firm's factors of production $N_i(t)$ (employment or the stock of capital). For firm i, the evolution of the state variable is governed by the following equation:

$$X_i(t) = A_i X_i(t-1) E_i(t)^{\beta_0} E(t)^{\beta_1} \left(\frac{N_{-i}(t)}{N_{-i}(t-1)} \right)^{\beta_2} \quad (1)$$

where $E_i(t)$ and $E(t)$ are log-normally distributed, independent random variables. Equation (1) - which should be interpreted as reduced-form - shows

[5] See for example Basu and Fernal (1997) and Burnside, Eichenbaum and Rebelo (1995).

that the evolution of firm i's prospects depends on a firm-specific characteristic, A_i, which may be thought of as long-run efficiency; an idiosyncratic shock, $E_i(t)$ and a common shock $E(t)$. The assumption that the adjustment of other firms influences firm i's evaluation is modeled by assuming that firm's i prospects improves if other firms are increasing their factor of production and conversely. For example, an entrepreneur might become more pessimistic upon observing other firms in the same sector going out of business, assigning a higher weight to any negative signal.[6] The adjustment of other firms is denoted by $N_{-i}(t)$, with β_2 parametrizing the strength of the channel. If what other firms do has no effect on firm's i evaluation, then $\beta_2 = 0$. For any variable $Y(t)$, define $y(t) \equiv \log Y(t) - \log Y(t-1)$. Then, taking logs in equation (1), redefining $\alpha = \log A$; $\varepsilon = \log E$ and rearranging, we get:

$$x_i(t) = \alpha_i + \beta_0 \varepsilon_i(t) + \beta_1 \varepsilon(t) + \beta_2 n_{-i}(t) \tag{2}$$

Finally, we assume that the percentage change in factor N depends on that in X:

$$n_i(t) = f(x_i(t)) + u_i(t) \tag{3}$$

where $u_i(t)$ is an error term uncorrelated with $\varepsilon_i(t)$ and $\varepsilon(t)$. Assuming that $f(\cdot)$ is an affine transformation, so that $f(x) = a + bx$, substituting equation (2) into (3) and assuming without loss of generality $b = 1$ we obtain our basic empirical specification:

$$n_i(t) = a + \alpha_i + \beta_0 \varepsilon_i(t) + \beta_1 \varepsilon(t) + \beta_2 n_{-i}(t) + u_i(t) \tag{4}$$

The absence of information spillovers implies $\beta_2 = 0$, and this hypotheses can be directly tested once we specify how to measure $n_{-i}(t)$. In our basic specification we will measure the adjustment of others as the mean adjustment of firms in i's reference group, excluding i's adjustment. Notice that information spillovers tend to induce co-movement among the firms that are subject to them since they add a common factor. Thus, one should find a higher degree of co-movement among firms with a high degree of exposure to information spillovers, an implication that will be discussed in future work.

[6] Indeed, the adjustment of others could be thought as amplifying a given realization of the aggregate shock $E(t)$; a point on which we will return later.

The specification in equation (4) has two features: first, it implies a linear response of $n_i(t)$ to the adjustment of others. Yet it may be that the firm's adjustment is triggered by the adjustment of others only when the latter is substantial. This would occur, for instance, if there were costs of adjusting factors as in Caplin and Leahy (1994) so that agents tend to adjust infrequently but substantially. We will account for the presence of non-linearities by replacing the mean adjustment in equation (4) with various quintiles of the distribution of the adjustment of others. Second, what matters is assumed to be the current adjustment of others; thus, one could ask who adjusts first. With high frequency data, the lagged adjustment would be more appropriate; with annual data such as ours, simultaneous adjustment is probably not too restrictive; and as Section 6 shows, this intuition is supported by our empirical evidence.

4 Identification and the "reflection" problem

A potentially serious problem in estimating equation (4) is that it could be impossible to identify β_2, owing to what Manski (1994) calls the "reflection problem". This arises because the actions of the individual agents in a group are related to the average action of the members of the group through an adding-up condition. Thus, without some prior restriction, the parameter characterizing the presence of information spillovers (and in general the other parameters as well) is not identified. To illustrate the identification problem, consider a simplified version of equation (4):

$$n_i(t) = b_0 x_i + b_1 z + b_2 n_{-i} \tag{5}$$

where x_i is an individual characteristics and z is a characteristic common to all elements of the reference group, and where for simplicity we have dropped the time index t and the random component u_i.

Notice that, for all t, $\frac{1}{K}\sum_i n_{-i} = \frac{1}{K}\left(\frac{\sum_i n_i - n_1}{K-1} + \dots + \frac{\sum_i n_i - n_K}{K-1}\right) = \frac{1}{K}\sum_i n_i = \bar{n}$, where K is the (known) number of firms in the reference group. Using (5), averaging over i and solving for $\bar{n}$ we have:

$$\bar{n} = \frac{b_0}{1-b_2}\bar{x} + \frac{b_1}{1-b_2} z \tag{6}$$

where $\bar{x} = \sum_i x_i / K$ is the mean of the individual variable. Noticing that $n_{-i} = \frac{K}{K-1}\bar{n} - \frac{1}{K-1} n_i$, substituting into (5) and using (6), we obtain the reduced form equation for firm i's adjustment decision:

$$n_i = \frac{(K-1)b_0}{K-1+b_2}x_i + \frac{b_2}{1-b_2}\frac{K b_0}{K-1+b_2}\bar{x} + \frac{b_1}{1-b_2}z =$$

$$= Ax_i + B\bar{x} + Cz \tag{7}$$

Suppose now that $\bar{x} = z$; that is the variable that enters the equation individually is the same that enters as average; then, we can factor equation (7) as

$$n_i = Ax_i + (B + C)z \tag{8}$$

This is the situation analyzed in Manski (1994), and identification cannot be achieved unless imposing some additional restrictions. This is clear from (7) where only the composite parameters A and $(B+C)$ are identified. However, as noted by Brock and Durlauf (1999), if $z \neq \bar{x}$; so that x_i only enters the equation individually, than the system is identified and we can retrieve the social interaction parameter.[7] Notice that, to make the illustration as simple as possible, we have assumed that x_i and z are scalars. The argument generalizes to the case in which x and z are vectors, in which case the condition for non-identification is that all the variables that enter individually also enter as averages, that is $\bar{x} \subseteq z$: Notice also that the identification problem only arises if the social interaction variable enters equation (5) in a linear fashion: otherwise, $\bar{x}$ would also enter equation (7) in a nonlinear fashion, and the factorization of equation (8) would not be possible even if $\bar{x} = z$.[8] We will exploit this property in a set of regressions later.

To achieve identification in our empirical specification, we rely on proxies for liquidity constraints. It is our contention that liquidity constraints are an impediment to adjustment, especially when it involves pecuniary costs. This is obvious in the case of upward adjustments in the quantity of factors of production, as they directly involve pecuniary outlays. But even downward adjustments, particularly in labor, might imply pecuniary costs, as stressed by the literature on firing costs. Firing workers in Italy involves, among other things, a severance payment dependent on the worker's tenure, which can be as large as 2-3 times a worker's annual wage.[9] In these circumstances,

[7] Dividing A by B in equation (7) and taking K as known one recovers parameter b_2; given b_2 the other parameters are obtained.

[8] We refear the interested reader to Brock and Durlauf (1999), which gives an excellent treatment of the issues of identification reviewed here.

[9] At the time of separation each worker is entitled to receive an amount of cash equivalent to its (gross) monthly wage times the number of years he has been with the firm.

adjustment may be limited by the presence of liquidity constraints arising from limits to the access to the credit market.[10] To achieve identi…cation we will insert in our empirical speci…cation …rm-level proxies for liquidity constraints and assume that while they a¤ect …rms directly, their group average does not directly a¤ect …rms adjustment decisions. Our justi…cation for this is that …rms creditworthiness - which determines access to credit - depends on …rm speci…c variables but not on group averages once the former are controlled for [11]

5 Data description

We estimate several variants of the model illustrated in Section 3, using a panel of Italian manufacturing …rms drawn from the Company Accounts Data Service (CADS) which collects annual balance-sheet data on a sample of about 30,000 …rms, over a period of 15 years (from 1982 to 1996). Besides reporting balance-sheet information the Service also reports employment and a detailed description of demographic characteristics.[12] To identify …rms with high exposure to information spillovers, we merge this database with the Industrial Districts Database (IDD) constructed by the National Statistical Institute (Istat). To this purpose the national territory is divided into local labor systems (LLS), i.e. territorial groupings of municipalities characterized by a certain degree of working-day commuting by the resident population. If a LLS is characterized by a high concentration

[10] An alternative way to achieve identi…cation would be to use …rm-level measures of adjustment costs, if available. Our approach can, in a sense, be regarded as equivalent. Although very little has been done on the interaction between …nancial constraints and adjustment costs, a few papers point out that they are observationally equivalent. Within the context of a business-cycle general equilibrium model, Carlstrom and Fuerst (1997) show that …nancial market imperfections can be regarded as endogenous costs of adjusting the capital stock. More related to our approach is the paper by Campbell and Fisher (1998) who claim that di¤erences in the observed job creation and destruction rates of U.S. manufacturing plants are better explained by di¤erences in employment adjustment costs across plants rather than in …nancial constraints. Implicitly, they are assuming that …nancial constraints a¤ect …rms' production factors adjustment in the same way as adjustment costs.

[11] This is not to say that …rms in one district cannot all get more credit than …rms in another district. For instance, if all …rms in a certain district use less speci…c capital than …rms in another district, compared to the latter they will all have a higher debt capacity, since they can o¤er better collateral, and have a greater creditworthiness. However, district average creditworthiness - as measured by the district average capital speci…city - will play no role once …rms capital speci…city is controlled for.

[12] For a more detailed description of the CADS database, see the Appendix.

of small to medium-sized …rms in the same two-digit sector classi…cation, it is classi…ed as a district. Districts are allocated to a 9-sector classi…cation according to their product specialization. We then identify …rms that are in the same district and sector and thereby divide the sample into a study group (…rms in the same district and sector, i.e. those with high exposure to information spillovers) and a control group of …rms with low exposure to information spillovers (…rms in the same sector but not located in districts). The geographical classi…cation ensures that the …rms that we include in the study group satisfy the observability criterion. Since they belong to the same sector, the similarity requirement is also ful…lled. In fact, this is an ideal context to test the relevance of information spillovers in shaping …rms' decisions. Table I reports summary information sector-by-sector for the sample, using Istat's 9-sector classi…cation. Panel A compares the sample with the population; the …rst two columns show the incidence of employment in specialized district …rms (i.e. …rms located in a district and belonging to the speci…ed sector) on total employment in the sector for the sample and for the population, respectively. It is clear that the sample tracks the population very well. "Textile and clothing", "leather and footwear", "wood, furniture, construction materials and glass", "machinery, computers and production tools" stand as sectors where a large portion of total output is accounted for by districts. These are also the sectors where districts are most widespread and they account for 167 out of the total of 199 (Column 6). For the remaining sectors the share of employment accounted for by specialized district …rms is minor. Columns 3 and 4 show employment in specialized district …rms as a share of total employment in the district for the sample and for the entire population. Again, the structure of employment in the sample is close to that in the population, particularly in those sectors where production typically takes place in districts. Panel B reports summary statistics for the total sample by sector.

The overall sample has two problems: …rst, for some districts, there are only a few …rms. For instance, the average number of specialized district …rms in the "food, beverages and tobacco" industry is 9.8, and in 1991 only 1 district out of 16 had more than 30 …rms. The …gures for "paper, printing products and publishing" are 4.3 and 0 respectively and for "metallurgy and metal products" 3 and 0 respectively. If not all …rms in the true reference group are included, then relying on a small sample may lead to noisy measures of the adjustment of others. We tackle this by excluding all the districts with fewer than 30 …rms in any sample year.[13] Second, some sectors

[13] This excludes …rms producing "rubber, plastic and chemical products" and …rms

are characterized by a high degree of heterogeneity when a two-digit classi…cation is used, making it hard to ful…ll the similarity criterion. The last column of the table classi…es the 9 sectors according to product heterogeneity. The classi…cation was made by informally comparing the list of products in the 4-digit classi…cation for each of the 9 sectors. Some sectors show a high degree of product heterogeneity. When relevant, we have dealt with this problem by reclassifying districts according to their specialization at the three-digit level. Sometimes, however, even at the three-digit level there remains considerable heterogeneity - as in some mechanical industries. In these cases - given that a four-digit classi…cation was never feasible in terms of observations - we have dropped the districts. After these exclusions, we are left with 14 districts in 5 sectors for a total of 20,334 observations and 1,485 …rms; non-district …rms in the …ve sectors are 3,146 for a total of 42,022 observations.[14]

Table II reports summary statistics for each sector and district and for non-district …rms, taking 1991 as the reference year. It is worth noticing that the sectors selected are those that, on the basis of panel A of Table I, have the highest incidence of employment in district …rms, and all of Italy's well-known industrial districts are included in the sample. Most districts are in "textiles" (6 out of 14) and are located in the North (10 out of 14); only 4 are in the Center and none in the South. This is consistent with the general under-industrialization of the South. The size of the districts measured by the number of specialized …rms (observations) ranges from a minimum of 38 …rms (552 observations) in the production tools district of Padua, to 329 (4,250) in the wool district of Prato. Though district …rms are typically small, their average size varies from a minimum of 26 employees (in the Prato district) to a maximum of 113 (Cossato). Concentration of production - measured by the ratio of the 95th percentile of employment to the median (Column 4) - is generally small, as one would expect in a network of similar …rms. Yet it varies across districts, as does …rm performance (return on assets, Column 6). In Section 9 we investigate the relation between …rm

classi…ed as "other manufacturing".

[14] To reduce product heterogeneity we have split the "textile & clothing" sector into its two components "textiles" on the one hand and "clothing" on the other. Since none of the "clothing" districts in the sample had the minimum number of …rms, they were all dropped. We have also reclassi…ed the mechanical sector using a three-digit classi…cation; the only sector with a low degree of product heterogeneity that had the minimum number of …rms was "production tools", which has three districts. Finally, we have separated "wood & furniture" from "construction materials and glass" which in the 9-sector classi…cation are lumped together. This way, we retain three districts in "wood & furniture" and one in "construction materials and glass".

performance and district concentration. Column 8 reports the share of …rms in the modal four digit sector both within each district and for …rms out of districts. As expected, the concentration is generally higher within each district, indicating a tendency to specialize in some particular production. This is stronger for the leather and the furniture sectors (which are more concentrated also out of districts), while "textile" and "production tools" are characterized by a majority of districts where the modal four digit sector accounts for less than …fty percent of specialized …rms. The high degree of similarity among district …rms could give rise to a correlation in factor adjustments not necessarily due to information spillovers, a possibility that we will explicitly take into account in our empirical analysis. Finally, the last column of Table II reports the number of non-specialized …rms - i.e. …rms located in the district but producing di¤erent goods. Comparing the average number of specialized and non-specialized …rms gives a clue of the production focus of the various districts and reveals that districts di¤er along this dimension as well.

6 Results

We start estimating equation (4) for the whole sample of district …rms. One can base the tests on any factor of production; we choose to rely on labor adjustments and disregard the adjustment of the stock of capital. The fact is that we have information on employment year by year, but no reliable data on capital. Balance-sheet data are reported at historical costs, and the time span covered by the data is too short to use permanent inventory methodology to estimate the capital stock.[15] To account for unobserved variables that may be relevant to factor adjustment, we estimate a …xed-e¤ects model. In order to implement speci…cation (4) we still need measures of the aggregate and speci…c shocks that …rms face. It is now well established that …rms adjustments are characterized by a considerable degree of heterogeneity (Davis and Haltiwanger (1992), Caballero, Engel and Haltiwanger (1997), Boeri (1996)). To control for such di¤erences, we run an auxiliary regression of the rate of growth of real sales in deviation from its mean and standardized with its standard deviation, on a full set of year

[15] The number of workers employed is a piece of information not required for the balance sheet, but supplied in addition to it. As a consequence, the records may not always be accurate and outliers may be present. To take care of outliers we have excluded the observations with a tenfold increase in employment or with a decrease in real sales accompanied by a twofold increase in employment. This led to exclude 372 observation on the total of district and nondistrict …rms.

dummies interacted with location and sector dummies to allow for aggregate shocks di¤ering across area and sector. To better account for local shocks, for district …rms we allow for one location dummy for each district, while for non-district …rms we use provinces.[16] We then use the …tted values from this regression (common within a group of …rms in the same region and sector) as a measure of the aggregate shocks; the residuals are taken as proxies of the idiosyncratic shocks.[17]

As argued in Section 4, to achieve identi…cation we follow the idea that adjustment involves pecuniary costs which are more easily faced if no credit impediments are present, and rely on …rm-level proxies for …nancial constraints. As a measure of the latter we use the ratio of …rms cash ‡ow to total sales.[18] Since both positive and negative adjustment should be dampened by …nancial constraints, we expect more positive and more negative adjustments by less credit constrained …rms. To capture this e¤ect we interact the proxy for …nancial constraints with two dummies, one for non-positive and one for non-negative adjustments. If indeed these variables are picking up easiness in adjustment we should …nd a positive e¤ect on the …rst interaction and a negative on the second. Indeed, in all regressions the pattern of signs is as expected. [19]

For each …rm and for each year in the sample, we measure the adjustment by other …rms in the same district and sector (the reference group) as the (unweighted) average percentage change in employment by the …rms in the group, excluding the adjustment of the …rm in question. If the signals received by each …rm in a given district and sector are all equally informative, than the unweighted average adjustment is adequate to summarize the information contained in the decisions of others; if the information content

[16] The italian territory is divided into 103 provinces, each broadly equivalent to a US county. This is the …ner classi…catin allowed by our dataset for non-district …rms. A district is a much smaller territory than a province, often coinciding with a few suburbs of a city or town.

[17] Given that the regressions include a measure of …rm-speci…c shocks to sales one could argue that identi…cation of social e¤ects could be reached this way; however, since they average out to zero within districts they cannot help achieving identi…cation.

[18] We have also experimented with alternative measures of credit constraints, such as the share of intangible assets on total assets - a measure of …rm's ability to pledge collateral - or the share of liquid assets on total …rm's assets, an indicator of …rms ability to face liquidity needs. Our results are essentially invariant to the measure used, and thus we only report those based on …rms' cash ‡ow.

[19] To save on space we do not report the coe¢ cients of the proxies for liquidity constraints; in all regressions they turn out to be statistically signi…cant and to have the expected signs. In general, the positive adjustments interaction carries a larger coe¢ cient, suggesting that liquidity constraints matter most when factors are adjusted upwards.

of the signals di¤ers across …rms (increasing with size, say), then weighted averages may be preferable. Given that one of the de…ning characteristics of industrial districts is the predominance of small …rms, and that the choice of weights contains a degree of arbitrariness, for the time being we use unweighted averages.

Table III, Column 1 shows the results of parameter estimates for the simplest speci…cation, which only includes controls for aggregate and idiosyncratic shocks and the adjustment of similar and observable …rms, i.e. those located in the same district and sector.[20] Both aggregate and …rm-speci…c shocks have a positive and highly signi…cant impact on factor adjustment, though idiosyncratic shocks are economically twice as important as aggregate shocks (the estimated coe¢cients are 0.056 and 0.026 respectively). The estimates show that each …rm's factor adjustment is positively and signi…cantly a¤ected by the adjustment of the other …rms in the same district and sector (coe¢cient = 0.308; t statistic = 5.61): an average increase in employment of one percent by the …rms in the reference group leads to a response of approximately a third of a point by each other …rm in the group. This is a remarkable e¤ect and is clearly consistent with the idea that …rms rely heavily on the information contained in the actions of other, similar …rms.

Other interpretations are possible, however. In particular, it may be that our proxy for common shocks is imperfect and that the average adjustment is picking up unexplained sector-district shocks rather than true information spillovers. To address this problem we add to the regression two explanatory variables: …rst, for each …rm j and year t in the sample we compute the average (unweighted) adjustment of …rms located in other districts but in the same sector as …rm j.[21] Second, for the same …rm j and all years, we compute the average adjustment of …rms located in the same district as …rm j but belonging to sectors other than that of j. If our measure of adjustment by …rms in reference group is picking up unaccounted sector shocks or district-speci…c shocks, these two variables should absorb part of the e¤ect and the estimate of the reference group adjustment

[20] Since we use the change in employment as our left-hand side variable, we lose some observations with respect to those reported in Table II; adding those lost due to missing values, we are left with the sample of 17,456 observation, for district …rms and 34,795 for non-district …rms.

[21] To calculate the adjustment of …rms in other districts, for sectors with multiple districts we con…ne ourselves to the districts already included in the sample; for the two sectors with only one district, we must resort to the districts that are not in the sample, given that the "same sector, other districts" set within sample is empty.

should diminish in both magnitude and signi…cance. On the other hand, if our controls are correctly picking up aggregate sector-district shocks and the reference group adjustment re‡ects information spillovers, the two additional regressors should have no explanatory power. In the case of the …rst indicator this is so because, since it refers to …rms located in other districts, it does not ful…ll the observability requirement; for the second, non-sector …rms, because it does not ful…ll the similarity requirement. Finally, we include as an additional regressor the average adjustment of non-district …rms in the same sector as …rm j: if actions by others only a¤ect one's decision through information spillovers, this variable should not be statistically signi…cant. The results of the estimates are shown in Column 2 of Table III. The parameters of the aggregate and speci…c shocks are essentially unaffected, as is that of the adjustment by …rms in the reference group, which is only slightly smaller (0.287 compared to 0.308) and equally signi…cant. None of the other measures of adjustment included in the regression (by …rms in other districts, those in other sectors, or non-district …rms in the same sector) has explanatory value. They all have small and statistically insigni…cant coe¢ cients whether taken alone or as a group (the group test for the hypothesis that they are jointly equal to zero has a group-value of 0.578).

There is yet another explanation for these results. As shown in Table II, district …rms tend to have a relatively high degree of sectoral concentration when measured at four digit levels. If a shock hits the particular class of goods in which the district is specialized, then one should expect that the adjustment of …rms out of district has little explanatory power, because such …rms are not as specialized in the same goods. To account for this possibility, we further restrict the de…nition of sector when selecting the control group. For each district, we retain the …rms in the modal four-digit sector and, if this has less than …fty percent of the …rms, all …rms in any other four-digit sector with at least twenty-…ve percent of …rms. For …rms in these sectors, we then construct the adjustment of non-reference …rms (in other districts or out of districts) within the narrower sector de…nition. For reference group …rms, we maintain the same measure of adjustment as before, based on the coarser sector de…nition. From a sectoral classi…cation viewpoint, there is now more heterogeneity in the reference group …rms than in the non-reference group ones, which implies that, if our previous results are driven by a shock to a particular class of goods, than the adjustment of non-reference group should be at least as important as that of the reference group. The results are reported in Column 3 of Table III. The coe¢ cient of the adjustment of the reference group drops slightly, arguably for the higher heterogeneity; however, the adjustment of non-reference group …rms still fail to have any impact, suggesting that our results are not driven by the higher similarity among district …rms, and that proximity is indeed a necessary condition for the e¤ects that we …nd.

So far we have assumed that what matters for …rm j decisions is current actions of the …rms in the reference group. Some papers assume an information (or observation) lag. It may thus be argued that the relevant actions are those of the past actions. This is obviously an empirical problem, and we address it in Column (4) where we include the one-year lagged adjustment by reference group …rms as well as current adjustment. The estimates show that lagged adjustment has no explanatory value when current adjustment is included, perhaps because we are using low-frequency data.

Finally, Column 4 of Table III reports the basic regression for non-district …rms. We take as the reference group for these …rms all other non-district …rms in the same sector. Since no restriction is put on location, …rm j and the …rms in its reference group will on average be located far apart and the observability requirement will not be ful…lled. Consequently, if information spillovers are the reason why other …rms' actions a¤ect …rm j's decisions, the adjustment of others should have no e¤ect when equation (4) is estimated on the sample of non-district …rms. And this is what we …nd: while the measures of aggregate and idiosyncratic shocks are both signi…cant and with coe¢ cients comparable to those found for district …rms, the adjustment by other non-district …rms in the same sector as …rm j has a small coe¢ cient, with the wrong sign and not statistically di¤erent from zero. Taken together, these results are remarkably consistent with the idea that …rms' actions reveal valuable information to other …rms in their district and industry.

7 Robustness and extensions

7.1 Reaction to large adjustments

If information revelation is what drives the results in Table III, then one should expect that the whole distribution of adjustments by others, not only its mean, should matter. Moreover, as is argued in Section 3, in the presence of adjustment costs extreme adjustments are likely to carry more information. While small changes in the labor force may re‡ect "business as usual", observing a …rm undergoing a dramatic change in employment could have a stronger in‡uence on the information set of the competitors and thus prompt emulative action. To allow for this possibility, we calculate the 10^{th} and 90^{th} percentiles of the distribution of the adjustments by …rms in the reference group and in other control groups and estimate equation (4) using such variables as proxies for other firms' adjustment. Table IV shows the results.[22] Column 1 gives the estimates for the simplest specification: both the 10^{th} and the 90^{th} percentiles have a positive impact on firms' decisions. Although the parameters are likely to be imprecisely estimated given the high collinearity of the regressors (expected when the distribution of adjustments moves symmetrically), an F test rejects the hypothesis that the two variables are jointly equal to zero even at the 1 percent level of confidence. Notice also that the upper tail carries a larger coefficient and is more significant than the lower tail. This could be a consequence of the fact that our dataset does not record exits, potentially a fundamental source of information, while start-ups with a strong increase in employment are in the sample.

The use of quintiles also allows us to perform an indirect comparative test of the information and the "real" effects of the significance of the adjustment of others. If the adjustment of other firms is reflecting "real" spillovers, due for example to technological externalities that increase all firms' productivity, then one should expect that what matters is the average adjustment; if on the contrary it is mainly due to informational spillovers, and if the most extreme adjustments convey the most information, then one should expect that the extreme quintiles are more important in determining the size of the adjustment. We therefore run a regression that includes both the mean and the top and bottom quintiles of the distribution of the adjustment of others. The results, shown in Column 2 of Table IV, are clear-cut: adding a measure of central tendency, such as the average adjustment, has no explanatory value once the two extreme quintiles are present. This strongly suggests that the phenomenon we are analyzing cannot be explained by real factors.

Column 3 shows the estimates including adjustment by non-reference groups, measured by the 10^{th} and 90^{th} percentiles. The inclusion of the corresponding measures of adjustment in these other groups, while making the estimate of the effect of the lower tail in the reference group smaller and less precise, does not affect that of the upper tail. Three out of six coefficients of the added regressors have the wrong (negative) sign and only the 10^{th} percentile of firms out of districts is significantly different from zero at 10 percent (but not at five). In addition the hypothesis that they are

[22] As argued in Section 4, with nonlinear measures of adjustments the identification problem does not arise. For comparability, and given that they are significant, we include the proxies for liquidity constraints also in this set of regressions.

jointly equal to zero cannot be rejected by an F test (p-value = 0.246).

Finally, Column 4 runs the regression for non-district …rms with the 10th and 90th percentiles in the adjustment of other non-district …rms. In this case, the 10th percentile has a large and signi…cant coe¢cient but the 90th percentile is not signi…cant and has the wrong sign.

The results using the adjustment in the tails of the distribution con…rm those using average adjustment; however, they also strengthen the interpretation of the results in terms of information spillovers.

7.2 Fraction of …rms adjusting

To further assess the robustness of our results we estimate our basic regressions using a third measure of other …rms' actions: the share of …rms that change employment by more than a given threshold amount. As is shown by Chamley and Gale (1996), in certain circumstances the share of …rms that adjust can be taken as a su¢cient statistic of other …rms' actions: the higher the share that raises or lowers the factor of production above or below a certain threshold, the stronger the signal. To test this implication we replace the adjustment of others by the share of …rms that increase or decrease sta¤ by at least 25 percent. When these shares refer to the reference group, we expect the former variable to exert a positive e¤ect on the adjustment of the …rm, the latter a negative e¤ect. When the shares refer to non-reference groups, there should be no statistically signi…cant e¤ect. The results, shown in Table V, are fully consistent with these predictions: the share of reference group …rms that lower employment by more than 25 percent a¤ects the adjustment of a given …rm negatively and signi…cantly: the e¤ect of the share of …rms that raise employment by more than 25 percent is positive (and more pronounced, Column 1). Adding the mean adjustment in the group adds no extra explanatory power (Column 2). However, when the adjustments of all the other non-reference groups are inserted, we fail to formally reject the assumption that, taken together, their coe¢cients are equal to zero (p-value for the test = 0.033). But notice that some coe¢cients have the wrong sign and that four out of the six coe¢cients do not statistically di¤er from zero. Finally, running the regression among non-district …rms, we obtain results very similar to those of the previous table, with the lower measure of adjustment signi…cantly di¤erent from zero and the higher with the wrong sign (Column 4). Thus, overall, these results are not qualitatively di¤erent from those reported in Table III and Table IV.

7.3 Evidence from single sectors

The estimates reported so far restrict the effect of the adjustment of firms in the reference group to be the same across the five sectors in the sample. It could be, however, that information spillovers are only relevant in some sectors, such as those producing very similar goods or those where goods, even if not similar, are highly complementary in demand implying that firms in these sectors are subject to the same aggregate shocks. If this were so, firms could learn even by observing the decisions of other firms producing different but related goods. Though we have been careful to select sectors that group similar or related goods, our procedure is judgmental and potentially arbitrary. It could thus be that the results for the whole sample are driven by particularly strong informational interactions among the firms of only one of the sectors. We check this possibility in Table VI where we report the estimate of the basic specification for each of the five sectors. In each case we report the specification with only the adjustment of the firms in the reference group and also that with the other groups, using the mean to capture the adjustment of others. When only the reference group is included, its coefficient is always positive; the point estimate in the "textile", "leather & footwear" and "production tools" sectors is comparable to that of the entire sample (0.406, 0.461, and 0.260 respectively compared to 0.308) and always statistically significant, it is smaller (0.087) but significant for "construction material and glass" and not statistically significant for the firms producing "wood & furniture". Adding the adjustment of other firms not in the reference group adds no explanatory value to the regression except for "wood & furniture" where we cannot reject the hypothesis that the adjustment of non-reference group firms matters (p-value for the test = 0.0033). If we take these results at face value, spillovers seem to be stronger in the "light industry" sectors, probably because they produce a more homogeneous set of goods.

8 Firm size and sensitivity to social learning

Presumably, not all firms react in the same way to the information contained in the actions of others. Some firms may not rely, or need rely less, on the observation of others' actions to extract information because they already receive enough signals; thus, they may attach little weight to information stemming from the decisions of others. These are presumably the larger firms, which are likely to have both more private information and a better capacity to process it. Furthermore, if there are fixed costs of gathering and processing signals, larger …rms have more incentive to incur them, because any advantage coming from new information would apply to a larger output (this is the same reason for which large …rms are more willing to undertake investment in process-enhancing technological innovation). Finally, larger …rms have presumably access to a larger network than smaller …rms to gather information, which makes them less sensitive to local information spillovers. It is thus conceivable that the degree of reliance on neighboring …rms' actions as a source of valuable information decreases with …rm size. To test this hypothesis we split the sample of district …rms by size and run our basic speci…cation for each quartile. The results, reported in Table VII, are supportive of the above idea: the e¤ect of reference group adjustment, while positive and signi…cant for all size groups, declines monotonically with the size of the …rm. Taking the …rst and the last quartile, the di¤erence in impact is substantial: among …rms in the …rst quartile the impact of the adjustment of others is more than three times as great as among …rms in the fourth quartile (0.679 compared to 0.177). For the middle two quartiles the coe¢cient is in between these two extremes, around 0.3, close to that for the sample as a whole.

9 Ampli…cation of aggregate shocks

We have argued in Section 2 that IS o¤er a natural mechanism of ampli…cation of aggregate shocks. The endogenous pace of information revelation can in fact be speeded-up in a nonlinear fashion by shocks that break the inertial behavior induced by social learning. Schivardi (1998) applies this idea to explain the large increase in job destruction that we observe at the troughs (Davis et alt. 1996), showing how relatively small aggregate shocks can induce a burst of reallocation activities if they set in motion the process of information-revealing actions. The implication in terms of the two groups of …rms in our dataset - i.e. district and non-district …rms - is that …rms that are subject to information spillovers should tend to concentrate adjustments in certain periods while the control group should follow a smoother pattern of labor adjustment. To test the validity of this implication we identify a series of periods, which we call "adjustment years", in which adjustment intensity is particularly strong. If the predictions of the model are correct, we should …nd that district …rms have a lower sensitivity to aggregate shocks in non-adjustment years and a higher one in adjustment years,because those should be the years in which the response to shocks is ampli…ed by information ‡ows. Non-district …rms should show no substantial di¤erences between adjustment and non-adjustment years, given that for them the observation of the shocks is presumably all that matters.

We identify adjustment years relying on out-of-sample information. We use the data from ISDB, a database constructed by the OECD that contains information on factors of production and output value at the sector level for a set of OECD countries. We select payroll employment for Italy from 1970 to 1996 for four sectors:[23] "textiles, apparel and leather"; "wood"; "production tools and metal products excluding machinery"; "non-metallic mineral products". For each, we calculate the average annual percentage changes in employment and classify as "adjustment years" those in which the sector recorded an employment increase or decrease larger than the mean over the period plus one standard deviation. With this definition, the adjustment years for the period covered by our sample are 1983-84, 1988, 1992-93 for "textiles and leather"; 1983-1985 and 1993 for "wood"; 1984-87 and 1992-93 for "metal products" and 1983-89 and 1991 for "non-metallic mineral products",[24] with all the adjustments except "wood" in 1985, textile in 1988 and "non-metallic mineral products" in 1986-89 being on the downside, in line with the downward trend of employment in manufacturing over the period.[25] We then construct a dummy that, for each firm-year observation, is equal to one if the observation falls in an adjustment year for the relevant sector and zero otherwise. Finally, we interact this dummy with the aggregate shock, distinguishing between district and non-district firms and estimate the following equation:

$$n_{it} = \beta_1 \varepsilon(t) d_{na;d} + \beta_2 \varepsilon(t) d_{a;d} + \beta_3 \varepsilon(t) d_{na;nd} + \beta_4 \varepsilon(t) d_{a;nd} + \beta_5 \varepsilon_i(t) + u_i(t) \tag{9}$$

where $d_{x;y}$ is a dummy taking value 1 if the observation is in year x (x= [a (adjustment); na (non-adjustment)]) and location y (y = [d(district); nd (non-district)]) and zero otherwise.

The theory implies that $\beta_1 < \beta_2$ (firms exposed to information spillovers respond more to aggregate shocks in adjustment years), $\beta_1 < \beta_3$ (firms exposed to information spillovers are more responsive to aggregate shocks in

[23] The dataset does not distinguish between textiles and leather, so we have to aggregate these two sectors in determining adjustment years.

[24] The more volatile and less correlated behavior of "non-metallic mineral products" is in line with the greater cyclical sensitivity and the cyclical misalignment of the construction sector, to which this sector is closely linked.

[25] We have experimented with stricter definitions of adjustment years, increasing the band outside which the change in employment must lay (and threfore reducing the number of adjustment years) up to the mean plus or minus 1.5 times the standard deviation. Our results are robut to such changes.

non-adjustment years), $b_2 > b_4$ (exposed …rms respond more than non-exposed …rms in adjustment years), and $b_3 = b_4$ (no di¤erence in responsiveness to aggregate shocks among non-exposed …rms). The estimation results are reported in Table VIII. The point estimates (Column 1) support the predictions. The response of district …rms to aggregate shocks is three times as large in adjustment years when compared to non-adjustment years (0.072 vs 0.023), implying that in such years the e¤ects of the shocks are greatly ampli…ed. The coe¢cient for district …rms in non-adjustment years (0.023) is smaller than that of non-district …rms (0.059). The latter, in turn, is smaller than that of district …rms in adjustment years (0.072). Tests of equality of the coe¢cients reported at the bottom of the table con…rm at least in part this conclusion, with only the test of the null hypothesis that district …rms have a higher response than non-district in adjustment years being rejected. Furthermore, we cannot reject the hypothesis that the response to aggregate shocks for non-district …rms is the same in adjustment and non-adjustment years (i.e. that $b_3 = b_4$).

Since the de…nition of adjustment years is somewhat arbitrary both in sample period and in threshold, we have checked our results de…ning only 1993 as an adjustment year. In 1993 the Italian economy recorded the sharpest rate of job destruction since the Second World War and a record contraction in manufacturing employment, common to all manufacturing sectors; as we have seen, the previous procedure indicates 1993 as an adjustment year for all sectors except "non-metallic mineral products". The estimates, reported in Column 4 of Table VIII, are very similar to those obtained when all adjustment years are used; however, given probably to the fact that in this case the parameter of the adjustment year is estimated with less observations (and therefore less precisely), we fail to reject the hypothesis that the coe¢cient is the same in 1993 ($b_1 = b_2$).

We can further sharpen our test of the implications of information spillovers for the sensitivity of factor adjustment to aggregate shocks. If the extra response to shocks that we observe in adjustment years for district …rms is indeed due to social learning, then this e¤ect should decrease or disappear (implying that $b_1 = b_2$) when we control for the adjustment of others. To test this, we estimate equation (8) on the subgroup of district …rms; we then run the same regressions including the average adjustment of other …rms in the same district and sector. The results are reported in Columns 2 and 3 using all adjustment years and in Columns 5 and 6 using only 1993. Whatever the adjustment year, we …nd a sizable decline in the di¤erence between the coe¢cients when the adjustment of others is included in the regression. Formal tests of equality of the coe¢cients, however, do not give qualitatively different results, although the test statistics do change in the expected direction. This lends support to the idea that the adjustment of others might be responsible for the extra response of firms to aggregate shocks in adjustment years, and that information spillovers might constitute a relevant channel of amplification of shocks.

10 Reference group structure and learning

In this section we extend the analysis to inquire into the structure of the reference group and its effect on learning. It is intuitive that if a group of firms tends to behave similarly because they learn from each other, then they should end up performing similarly. It is also plausible that the structure and amount of learning that takes place may depend on the structure of the group. Bala and Goyal (1998) formalize these intuitions using a framework where firms (agents) learn from their neighbors' actions and outcomes as well as from the past records of their choices. They study how the social structure affects the long-run performance of a group of connected[26] firms and the nature of the learning process. They show that similar, informationally connected firms end up undertaking the same actions and, in the limit, performing similarly. Obviously, this does not imply that the action chosen by all members of the connected group is the optimal action; it only implies that it is chosen by all. Yet, depending on the structure of the group, firms may end up choosing the profit-maximizing action. Bala and Goyal (1998) show that this is more likely to happen if the reference group is large and if there are no informationally dominant firms, i.e. firms whose actions are observed by all other firms in the group. On one side, more firms in a group simply means that more information can be gathered by observing the behavior of others; on the other, if the group contains one or more dominant players, firms may end up being trapped into a sub-optimal action. The intuition behind this result is that, since a dominant player is observed by all other members of the group, each member will tend to emulate him, disregarding his own private information, in the spirit of an information cascade. As a consequence, the process of information revelation and diffusion is impaired, and the private information of agents is not revealed efficiently.

We can use our data to test this hypothesis. It implies that each firm's

[26] According to Bala and Goyal (1998), firms in a group are connected if for every pair of firms i and j, either i directly observes j or there exist firms $i_1; \ldots; i_m$ such that i directly observes i_1, which directly observes $i_2; \ldots; i_m$, which directly observes j.

performance should be positively related with the number of other …rms in the district and negatively with the weight of informationally dominant actors - for instance large …rms - other things being equal. We use the sample of district …rms. For each district we compute the number of specialized …rms in each year and construct an indicator of information leadership as the share of the largest three …rms' sales in the reference group's sales (i.e. the total sales of the …rms in that district that are present in our sample). As an alternative measure, we use the 95th percentile of sales divided by median sales in the district. We measure performance as the …rm's gross pro…ts over total assets. We then regress this measure of pro…tability on the number of specialized …rms, the proxy for information leadership and on a full set of year dummies, sector dummies and regional dummies as controls for performance shocks. If the theoretical prediction is correct, a higher concentration of sales should have a negative e¤ect on pro…tability, while a higher concentration of …rms should have a positive one. Since pro…tability can change systematically with the size of the …rm, we also insert a set of size dummies, one for each quartile of …rms' sales. Results are shown in Table IX. The …rst two columns report estimates using a …xed-e¤ects estimator to account for …rms' heterogeneity in performance. The …rst column shows the estimates when information leadership is proxied by the sales of the largest three …rms; the second column when the ratio between the 95th percentile and median is used. In both cases the results are as expected: the coe¢cient of the number of …rms in the district is positive and signi…cant and that of the proxy for information leadership is negative and signi…cant, implying that the performance of …rms in districts with dominant players is systematically worse than that of …rms in districts without dominant players, in line with Bala and Goyal (1998). Furthermore, the e¤ect of dominant players is economically meaningful: increasing the ratio between the 95th percentile of sales and the median by 10 percent starting from its mean lowers pro…tability by 1.2 percentage points, almost 10 percent of its mean value[27]. Since our measure of performance is characterized by the presence of several extreme observations on both tails of the distribution, we have also run our estimates using a least absolute deviations estimator omitting …xed e¤ects. The results, shown in the third and fourth columns of Table IX, are very similar to those obtained in the …rst two columns, reassuring us that the previous conclusions are robust to the presence of outliers.

[27] For this computation we are using the estimates in column 2 of Table IX.

11 Conclusions

We have exploited a rich dataset on a sample of Italian manufacturing …rms to assess whether information spillovers are an important factor in determining …rms' labor adjustment decisions. Using the concepts of product similarity and geographical proximity to identify a set of …rms that are more likely to be exposed to information spillovers, we have shown that, after controlling for aggregate and individual shocks, individual adjustments in labor are strongly in‡uenced by various measures of aggregate adjustment within the reference group. In addition, we …nd that large adjustments tend to induce a proportionally stronger response, arguably because they are more visible. We have also shown that the adjustments of …rms that fail to satisfy either of the criteria have no impact on individual adjustments.

In accordance with the predictions of the theory on strategic learning, we have found that information spillovers tend to induce concentration of adjustments in some periods, which we have de…ned as adjustment years, suggesting that they actually constitute a powerful mechanism of ampli…cation of aggregate shocks. Finally, we have investigated the role of the structure of the reference group on the learning process, showing that an increase in the number of …rms in the reference group has a positive impact on pro…tability, while the presence of large …rms might be a barrier to the e¢cient dissemination of information and therefore reduce average pro…tability.

The analysis can be extended in many di¤erent directions. We plan to study more directly the implication of IS in terms of comovements of factor demands, by considering how the individual hazard functions for factor adjustments are in‡uenced by social learning. A second extension we plan to pursue relates to the estimation of the rate at which such e¤ects die out with distance, to asses how "local" spillovers are. This would imply relating the adjustment of …rms in a district to that of …rms in other districts, controlling for the distance between them. Finally, it would be important to further investigate the e¤ects of social learning on …rms' performance. This would help to better asses the implications of IS for industrial policy, particularly for phenomenon, such as the di¤usion of technological innovation, the entry in a new market or the early phase of development of a new industry in a region, in which information plays an important role.

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A APPENDIX

THE COMPANY ACCOUNTS DATA SERVICE (CADS)

Our data are drawn from the Italian Company Accounts Database, a large data set collecting balance sheet information and other items on a sample of over 30,000 Italian …rms. The data, available since 1982 and up to 1996, are collected by Centrale dei Bilanci, an organization established in the early 1980s jointly by the Bank of Italy, the Association of Italian Banks (ABI) and a pool of leading banks with the intent of building up and sharing information on borrowers. Besides reporting balance sheet items the database contains detailed information on …rms demographics (year of foundation, location, type of organization, ownership status, structure of control, group membership etc.), on employment, and their ‡ow of funds. Balance sheets are reclassi…ed in order to reduce the dependence of the data on the accounting conventions used by each …rm to record income …gures and asset values. Balance sheets for the banks' major clients (de…ned according to the level of their borrowing) are collected by the banks. The focus on the level of borrowing skews the sample towards larger …rms. Furthermore, because most of the leading banks are in the Northern part of the country, the sample has more …rms headquartered in the North than in the South. Finally, since banks are most interested in …rms that are creditworthy, …rms in default are not in the data set, so that the sample is also tilted towards higher than average quality borrowers. Despite these potential biases the comparison between sample and population moments in Table I appear to suggest that the CADS is not too far from being representative of the whole population. This is con…rmed by the data reported in Table A1 which compares the marginal frequency distribution by size and geographical location in the sample and in the population in 1990. While the gegraphical distribution of …rms in the sample is not too far from that in the population, it is is biased towards larger …rms expecially those above 999 employees.

TABLE I

SUMMARY STATISTICS FOR THE WHOLE SAMPLE

PANEL A — SAMPLE-POPULATION COMPARISONS:1991

Sector	Employment in specialized district firms/ Employment in the sector		Employment in specialized district firms/ Employment in district firms		Number of districts	
	Sample	Population	Sample	Population	Sample	Population
1) Food, beverage & tobacco	7.2	5.5	27.8	24.9	16	17
2) Textile & clothing	36.3	38.1	36.7	40.3	65	69
3) Leather & footwear	41.9	39.4	40.7	41.3	26	27
4) Timber, construction materials and glass	24.6	20.8	37.6	35.2	39	39
5)Metallurgy and metal products except machines	0.4	0.3	62.5	17.6	1	1
6)Machinery, computers & tools	13.3	14.4	47.7	49.9	30	32
7) Rubber, plastic & chemical products	2.1	3.1	26.8	19.2	4	4
8) Paper, printing & publishing	1.4	1.6	43.5	23.4	6	6
9) Other manufacturing	34.7	52.2	13.5	20.8	4	4
Total.	14.3	17.6	38.5	41.3	191	199

PANEL B — SAMPLE INFORMATION

Sector	Average N. of specialized district firms: 1991	N. of districts with at least 30 specialized firms: 1991	Total n. of observations, 82-96		Product Heterogeneity
			District	Non-district	
1) Food, beverage & tobacco	9.8	1	2,211	26,076	High
2) Textile & clothing	23.0	9	19,102	21,911	Medium
3) Leather & footwear	21.6	4	6,605	6,974	Low
4) Wood, construction materials and glass	12.7	5	5,751	13,330	Medium
5)Metallurgy and metal products except machines	3	0	50	8,664	High
6)Machinery, computers & tools	45.3	13	19,977	76,646	High
7) Rubber, plastic & chemical products	18.2	1	1,125	34,235	Medium
8) Paper, printing & publishing	4.3	0	343	16,134	High
9) Other manufacturing	21.5	1	1,154	1,908	High
Total	22.7	34	56,318	205,878	

Specialized district firms are those located in the district and belonging to the sector.

TABLE II

SUMMARY STATISTICS FOR THE SELECTED SAMPLE: 1991

DISTRICTS FIRMS

District (product)	Area of location	N. of Specialized firms	Aver. empl.	Ratio of 95th to 50th percentile of employment	Total Number of obs. (1982-96)	Median ROA	SD of ROA	Share of firms in the modal 4 digit sector and sectort n.	N. of district firms in other sectors
	1	2	3	4	5	6	7	8	9
Textile									
Biella (wool)	North	76	79.00	7.73	1,198	0.089	0.11	60.5 (1710)	28
Cossato (wool)	North	59	112.86	8.74	951	0.094	0.18	55.9 (1710)	13
B.Arsizio	North	97	87.49	5.79	1,498	0.090	0.13	28.9 (1730)	226
Gallarate	North	60	73.38	6.76	836	0.094	0.09	31.7 (1770)	99
Como (silk)	North	187	61.95	3.73	2,657	0.108	0.06	32.6 (1724)	218
Prato (wool)	Center	329	25.78	4.81	4,250	0.119	0.08	54.4 (1710)	38
Total		808	56.46	6.29	11,390	0.107	0.11	35.9 (1710)	622
Leather & footwear									
S. Croce Arno (tannery)	Center	220	20.76	3.36	2,550	0.111	0.38	77.3 (1910)	37
Wood and furniture									
Desio	North	99	59.24	4.25	1,225	0.102	0.09	79.6 (3610)	262
Udine (chairs)	North	53	72.75	6.75	889	0.096	0.07	73.6 (3610)	132
Pesaro (furniture)	Center	41	55.27	3.1	577	0.087	0.16	95.1 (3610)	36
Total		193	62.11	4.38	2,691	0.096	0.11	80.8 (3610)	430
Construcion materials									
Sassuolo (tiles)	Center	96	142.77	10.17	1,388	0.094	0.08	53.1 (2620)	190
Tools									
Lecco	North	82	61.41	5.31	1,162	0.137	0.08	40.2 (2870)	157
Bergamo	North	48	55.17	3.36	651	0.156	0.11	37.5 (2850)	226
Padova	North	38	55.76	3.19	552	0.104	0.11	34.2 (2870)	154
Total		168	58.35	3.64	2,365	0.132	0.10	31.6 (2870)	537

NON-DISTRICT FIRMS

Textile		538	99.34	8.70	7,592	0.095	0.09	21.2 (1710)	
Leather & foot.		234	71.99	6.31	3,371	0.0.91	0.11	52.6 (1930)	
Timber & furn.		533	54.16	5.84	7,180	0.094	0.09	51.2 (3610)	
Constr. Materials		836	80.97	9.84	11,048	0.110	0.11	32.3 (2660)	
Tools		1,005	61.95	5.80	12,831	0.100	0.13	30.8 (2810)	
Total		3,146			42,022				

TABLE III
EMPLOYMENT ADJUSTMENT AND INFORMATION SPILLOVERS: FULL SAMPLE ESTIMATES

Explanatory variables	District firms				Non-district firms
	(1)	(2)	(3)	(4)	(5)
Aggregate shocks	0.026 (0.007)	0.025 (0.007)	0.024 (0.008)	0.022 (0.007)	0.058 (0.004)
Specific shocks	0.056 (0.002)	0.056 (0.002)	0.059 (0.003)	0.053 (0.002)	0.068 (0.002)
<u>Average adjustment by other firms in:</u>					
- the same distr. and sect.	0.308 (0.055)	0.287 (0.057)	0.249 (0.068)	0.300 (0.060)	
- the same distr. and sect. (t-1)				-0.019 (0.057)	
- other distr. but same sect.		-0.005 (0.031)	-0.049 (0.072)	-0.002 (0.111)	
- the same distr. but other sect.		-0.080 (0.100)	0.006 (0.040)	0.001 (0.031)	
Average adjustment by other non-distr. firms in the same sect.		0.171 (0.124)	-0.013 (0.072)	0.102 (0.132)	0.061 (0.083)
Number of observations	17,456	17,456	10,914	16,407	34,795
Number of firms	2,308	2,308	1477	2,295	4,896
F test for fixed effects (*P*-value in parent.)	1.84 (0.000)	1.84 (0.000)	1.65 (0.000)	1.85 (0.000)	1.57 (0.000)
p-value for the *F* test for adjustment by non-reference group firms = 0		0.578	0.912	0.852	

The left-hand side variable is the firm's percentage change in employment. Standard errors are reported in parentheses. All regressions include firm fixed effects. Aggregate shocks are the coefficients of the year-dummies in a regressions of the standardized rate of growth of real sales among district firms (respectively non-district for the estimates reported in column (5)) belonging to the same sector on a set of year-region dummies; specific shocks are the residuals from this regression. The adjustment by other firms is the unweighted average of the percentage change in employment among the firms in the reference group; when the reference group is the same as the firm in the left-hand side, the adjustment of the latter is excluded when computing the average adjustment. Column (3) reports the results for a subgroup of firms for which the adjustment for the non-reference group has been calculated using a finer (4-digit) definition of "same sector", while maintaining the same measure of adjustment as before for the reference group. All regressions include two controls for liquidity constraints measured by the firm's cash flow as a share of total sales and interacting this measure with two dummies, one for non-positive and the other for non-negative adjustments.

TABLE IV

EMPLOYMENT ADJUSTMENT AND INFORMATION SPILLOVERS: ESTIMATES WITH PERCENTILES OF ADJUSTMENT. WHOLE SAMPLE OF DISTRICT FIRMS

Explanatory variables	District firms			Non-district firms
	(1)	(2)	(3)	(4)
Aggregate shocks	0.023	0.022	0.026	0.059
	(0.007)	(0.007)	(0.007)	(0.004)
Specific shocks	0.056	0.056	0.056	0.068
	(0.002)	(0.002)	(0.002)	(0.002)
Adjustment measures by other firms :				
A: Firms in the same district and sector				
- 10th percentile	0.042	0.028	0.007	
	(0.039)	(0.045)	(0.042)	
- 90th percentile	0.172	0.155	0.168	
	(0.025)	(0.039)	(0.026)	
- Mean adjustment: same district and sector		0.053		
		(0.092)		
B: Firms in other districts, same sector				
- 10th percentile			-0.040	
			(0.070)	
- 90th percentile			-0.061	
			(0.056)	
C: Firms in same districts, other sector				
- 10th percentile			0.050	
			(0.026)	
- 90th percentile			-0.007	
			(0.017)	
D: Non-districts firms, same sector				
- 10th percentile			0.126	0.145
			(0.072)	(0.053)
- 90th percentile			0.018	-0.027
			(0.059)	(0.044)
Number of observations	17,456	17,456	17,456	34,795
Number of firms	2,308	2,308	2,308	4,896
F test for fixed effects (*p*-value)	1.84	1.83	1.83	1.57
	(0.000)	(0.000)	(0.000)	(0.000)
p-value for the *F* test for adjustment by non-reference group firms = 0			0.246	

The left-hand side variable is the firm's percentage change in employment. Standard errors are reported in parentheses. All regressions include firm fixed effects. Aggregate shocks are the coefficients of the year-dummies in a regressions of the standardized rate of growth of real sales among district firms belonging to the same sector on a set of year-region dummies; specific shocks are the residuals from this regression. The adjustment by other firms is measured by various moments of the distribution of the percentage change in employment in each sample year among the firms in the reference group; when the reference group is the same as the firm in the left-hand side, the adjustment of the latter is excluded when computing the adjustment by other firms. All regressions include two controls for liquidity constraints measured by the firm's cash flow as a share of total sales and interacting this measure with two dummies, one for positive and the other for negative adjustments.

TABLE V

EMPLOYMENT ADJUSTMENT AND INFORMATION SPILLOVERS: ESTIMATES WITH FRACTION OF FIRMS ADJUSTING. WHOLE SAMPLE OF DISTRICT FIRMS

Explanatory variables	District firms			Non-district firms
	(1)	(2)	(3)	(4)
Aggregate shocks	0.024 (0.007)	0.023 (0.007)	0.027 (0.008)	0.060 (0.004)
Specific shocks	0.056 (0.002)	0.056 (0.002)	0.056 (0.002)	0.068 (0.002)
Adjustment by other firms :				
-25%, same district and sector	-0.137 (0.078)	-0.065 (0.093)	-0.082 (0.084)	
+25%, same district and sector	0.404 (0.069)	0.291 (0.105)	0.388 (0.071)	
Mean adjust: same district and sector		0.129 (0.091)		
-25%, other district and same sector			0.170 (0.137)	
+25%, other district and same sector			-0.172 (0.120)	
-25%, same district and other sector			-0.064 (0.058)	
+25%, same district and other sector			0.082 (0.046)	
-25%, non district and same sector			-0.390 (0.156)	-0.373 (0.123)
+25%, non district and same sector			0.029 (0.163)	-0.149 (0.118)
Number of observations	17,456	17,456	17,456	34,795
Number of firms	2,308	2,308	2,308	4,896
F test for fixed effects (*p*-value in parent.)	1.84 (0.000)	1.83 (0.000)	1.84 (0.000)	1.58 (0.000)
p-value for the *F* test for adjustment by non-reference group firms = 0			0.033	

The left-hand side variable is the firm's percentage change in employment. Standard errors are reported in parentheses. All regressions include firm fixed effects. Aggregate shocks are the coefficients of the year-dummies in a regressions of the standardized rate of growth of real sales among district firms belonging to the same sector on a set of year-region dummies; specific shocks are the residuals from this regression. The adjustment by other firms is measured by the share of firms in the reference group that in each sample year adjust employment by more than 25 percent and by less than 25 percent respectively; when the reference group is the same as the firm in the left-hand side, the adjustment of the latter is excluded when computing the share of firms that adjust in excess of |25| percent. All regressions include two controls for liquidity constraints measured by the firm's cash flow as a share of total sales and interacting this measure with two dummies, one for positive and the other for negative adjustments.

TABLE VI EMPLOYMENT ADJUSTMENT AND INFORMATION SPILLOVERS: ESTIMATES BY SECTOR FOR DISTRICT FIRMS

Explanatory variable	Sector									
	Textile		Leather & footwear		Wood & furniture		Construction materials		Tools	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Aggregate shocks	0.028 (0.008)	0.029 (0.009)	0.002 (0.020)	0.004 (0.023)	0.023 (0.020)	0.028 (0.020)	0.018 (0.036)	0.052 (0.049)	0.019 (0.015)	0.020 (0.017)
Specific shocks	0.049 (0.003)	0.049 (0.003)	0.086 (0.008)	0.086 (0.008)	0.054 (0.006)	0.054 (0.006)	0.083 (0.011)	0.083 (0.011)	0.027 (0.006)	0.027 (0.006)
Average adjustment by other firms in:										
- the same district and sector	0.406 (0.079)	0.387 (0.081)	0.462 (0.190)	0.397 (0.226)	0.021 (0.126)	-0.218 (0.152)	0.087 (0.202)	0.071 (0.212)	0.260 (0.110)	0.231 (0.123)
- other districts but same sector		-0.199 (0.162)		0.238 (0.323)		-1.093 (0.386)		-0.441 (0.407)		0.065 (0.176)
- The same district but other sectors		-0.020 (0.036)		0.015 (0.115)		0.514 (0.157)		0.015 (0.138)		0.232 (0.234)
Average adjustment by other non-district firms in the same sector		0.176 (0.187)		0.052 (0.318)		0.756 (0.498)		0.687 (0.533)		-0.221 (0.305)
Number of observations	9,731	9,731	2,164	2,164	2,336	2,336	1,200	1,200	2,025	2,025
Number of firms	1,270	1,270	301	301	296	296	148	148	293	293
F test for fixed effects	1.98	1.97	1.32	1.31	1.50	1.46	2.71	2.72	1.49	1.48
(*p*-value in parentheses)	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
p value for the *F* test for the adjustment of other non-reference group firms = 0		0.576		0.849		0.003		0.549		0.676

The left-hand side variable is the firm's percentage change in employment. Standard errors are reported in parentheses. All regressions include firm fixed effects. Aggregate shocks are the coefficients of the year-dummies in a regression of the standardized rate of growth of real sales among district firms belonging to the same sector on a set of year-region dummies; specific shocks are the residuals from this regression. The adjustment by other firms is the unweighted average of the percentage change in employment among the firms in the reference group; when the reference group is the same as that of the firm in the left-hand side, the adjustment of the latter is excluded when computing the average adjustment. All regressions include two controls for liquidity constraints measured by the firm's cash flow as a share of total sales and interacting this measure with two dummies, one for positive and the other for negative adjustments.

TABLE VII

FIRMS SIZE AND THE INTENSITY OF SOCIAL LEARNING

Explanatory variables	Firm size quartile (sample of district firms)			
	1th	2th	3th	4th
Aggregate shocks	0.022	0.013	- 0.008	0.001
	(0.021)	(0.009)	(0.007)	(0.007)
Specific shocks	0.073	0.029	0.027	0.035
	(0.007)	(0.003)	(0.003)	(0.003)
Average adjustment by other firms in:				
- the same district and sector	0.679	0.338	0.224	0.177
	(0.182)	(0.076)	(0.059)	(0.052)
Number of observations	4,522	4,332	4,235	4,367
Number of firms	949	929	835	637
F test for fixed effects =0	3.16 (0.000)	3.48 (0.000)	2.10 (0.000)	1.77 (0.000)
(*p*-value in parentheses)	(948; 3,568)	(928; 3,398)	(834; 3,395)	(636; 3,725)
p-value for the *F* test for adjustment by non-reference group firms = 0				

The left-hand side variable is the firm's percentage change in employment. Standard errors are reported in parentheses. All regressions include firm fixed effects. Aggregate shocks are the coefficients of the year-dummies in a regressions of the standardized rate of growth of real sales among district firms (respectively non-district for the estimates reported in column (4)) belonging to the same sector on a set of year-region dummies; specific shocks are the residuals from this regression. The adjustment by other firms is the unweigthed average of the percentage change in employment among the firms in the reference group; when the reference group is the same as the firm in the left-hand side, the adjustment of the latter is excluded when computing the average adjustment.

TABLE VIII

EMPLOYMENT ADJUSTMENT AND INFORMATION SPILLOVERS: ESTIMATES OF EXTRA RESPONSE IN ADJUSTMENT YEARS, WHOLE SAMPLE

Explanatory variables	Adjustment years: all			Adjustment year:1993		
	All firms	District firms		All firms	District firms	
	(1)	(2)	(3)	(4)	(5)	(6)
Aggregate shocks, district firms in non Adjustment years - b_1	0.023 (0.009)	0.023 (0.008)	0.019 (0.008)	0.037 (0.007)	0.037 (0.007)	0.029 (0.007)
Aggregate shocks, district firms in Adjustment years - b_2	0.072 (0.013)	0.072 (0.018)	0.055 (0.012)	0.071 (0.036)	0.072 (0.033)	0.048 (0.033)
Aggregate shocks, non-district firms in Non adjustment years - b_3	0.059 (0.005)			0.061 (0.004)		
Aggregate shocks, non-district firms in Adjustment years - b_4	0.067 (0.006)			0.073 (0.013)		
Average adjustment by other firms in the same District and sector			0.291 (0.056)			0.314 (0.056)
Specific shock	0.067 (0.001	0.059 (0.002)	0.060 (0.002)	0.067 (0.002)	0.059 (0.002)	0.060 (0.002)
Number of observations	52,308	17,471	17,471	52,308	17,471	17,471
Number of firms	7,204	2,308	2,308	7,204	2,308	2,308
F –test for fixed effects=0	1,68	1,87	1,85	1,68	1,87	1,85
(*p*-value in parentheses)	(0,0000) (7,203; 45,099)	(0,0000) (2,307; 15,160)	(0,0000) (2,307; 15,159)	(0,0000) (7,203; 45,099)	(0,0000) (2,307; 15,160)	(0,0000) (2,307; 15,159)

TESTS OF HYPOTHESES(*p*-values for the *F* test of the specified null hypotheses):
Regression (1): *p*-value for H_0: $b_1= b_2$: 0.001; *p*-value for H_0: $b_1= b_3$: 0.002;
p- value for H_0: $b_2= b_3$: 0.364; *p*- value for H_0: $b_3= b_4$: 0.357
Regression (2): *p*-value for H_0: $b_1= b_2$: 0.006.
Regression (3): *p*-value for H_0: $b_1= b_2$: 0.014.

Regression (4): *p*-value for H_0: $b_1= b_2$: 0.339; *p*-value for H_0: $b_1= b_3$: 0.003;
p-value for H_0: $b_2= b_3$: 0.773; *p*-value for H_0: $b_3= b_4$: 0.376.
Regression (5): *p*-value for H_0: $b_1= b_2$: 0.296.
Regression (6): *p*-value for H_0: $b_1= b_2$: 0.571.

The left-hand side variable is the firm's percentage change in employment. Standard errors are reported in parentheses. All regressions include firm fixed effects. Estimates are obtained by constructing interaction dummies that let the coefficients of the aggregate shocks differ according to district-non district and adjustment year-non adjustment year. Adjustment years are defined as years in which the percentage variation in dependent employment at the sectoral level exceeds the average sectoral variation over the period 1971-1995 by one standard deviation. Sectoral employment data source: International Sectoral Data Base 1997, OECD. For the description of the variables see the note to Table III.

TABLE IX

SOCIAL LEARNING AND FIRMS PERFORMANCE

Explanatory variable	Fixed effects estimates		LAD estimates	
	1	2	3	4
Average size of largest three firms in the district/Average firm size in the district	-0.0042 (0.0008)	-	-0.0040 (0.0006)	-
95th percentile of firms size in the district / median firm size in the district	-	-0.1029 (0.0180)	-	-0.0850 (0.0151)
N. of firms in district	0.0000893 (0.000030)	0.0000676 (0.0000301)	0.0001499 (0.0000196)	0.0001067 (.0000187)
Number of observations	20,380	20,380	20,380	20,380
Number of firms	2,688	2,688	2,688	2,688
F test for fixed effects =0 (p-value in parentheses)	5.62 (0.000) (2,687;17,670)	5.63 (0.000) (2,687;17,670	-	-

The left hand side is firm's gross profits as a share of firm's total assets. Size is measured by firm sales. Only specialized firms are considered. Each regression includes a full set of year dummies, regional dummies, sector dummies and 4 dummies for firms size (one for each sales quartile); all regression except the LAD estimates include firm fixed effects.

TABLE A1

POPULATION AND SAMPLE MARGINAL FREQUENCY DISTRIBUTION BY FIRMS' SIZE, SECTOR OF ACTIVITY AND GEOGRAPHICAL LOCATION IN 1990

	Marginal frequency distribution	
	Population (1990 Census)	Sample
Firms size (number of employees)		
50 – 99	22.7	15.0
100-199	20.2	16.9
200-499	21.3	19.7
500-999	17.5	12.0
>999	18.3	36.4
Geographical location (regions)		
Piemonte and Valle d'Aosta	12.7	14.9
Lombardia	33.8	36.6
Liguria	2.5	3.9
Trentino Alto Adige	1.1	1.1
Veneto	8.9	9.3
Friuli Venezia Giulia	2.4	3.5
Emilia Romagna	10.1	9.1
Toscana	6.3	4.5
Umbria	1.6	1.1
Marche	2.4	2.1
Lazio	3.4	4.8
Abruzzi	2.1	1.4
Molise	0.6	0.1
Campania	3.9	3.7
Puglia	2.0	1.3
Basilicata	0.4	0.3
Calabria	0.6	0.2
Sicilia	1.9	1.3
Sardegna	3.2	0.7

Population and sample refer to firms with more than 50 employees.