Spillovers in Industrial Districts*

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Abstract

We study the role of social interaction (SI) in determining firms' employment adjustments in industrial districts. We assume that SI matters only among firms that are both similar -i.e. produce the same good - and geographically close -i.e. located in the same district. Our first test is based on the correlation between individual and aggregate measures of employment adjustments. Consistent with theory, firms' decisions are affected by those of similar, neighboring firms, while the actions of those not satisfying either of the criteria have no impact. Exploiting the richness of our data, we directly address the problems of self selection and unobserved common shocks, that plague most of the empirical literature. Our second test shows that, as theory predicts, spillovers give rise to amplified responses to shocks and bunching of adjustments. This gives further support to the role of SI in industrial clusters and suggests that information spillovers, according to which firms' actions convey useful information about a common problem, are one of the channels through which they take place.

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1 Introduction

The 1990s saw the rise of a large literature on social interaction (SI), defined as the set of relations that individuals develop outside the market place as social agents. Social interactions can account for a variety of important phenomena, ranging from market crashes (Shiller 2000), to technology adoption by firms and households' choice of financial instruments (Ellison and Fudenberg 1993, Guiso, Sapienza and Zingales 2004), to search (Diamond 1982). Among these, the area that has probably the oldest tradition in economics is SI in industrial clusters. The role of spillovers for local development was first pointed out by Marshall (1890), who argued that the concentration of activity gives rise to beneficial externalities. Since then, a large body of empirical work has studied both the role of SI in determining the success of industrial clusters and the specific channels through which interaction takes place (see Rosenthal and Strange (2003) for a recent survey). This literature has considered how local conditions impact on measures of economic performance, such as productivity levels (Ciccone and Hall 1996), TFP growth (Cingano and Schivardi 2004) and the circulation of knowledge (Jaffe, Trajtenberg and Henderson 1993).

In this paper, we take a different approach. Rather than looking at performance, we consider how SI directly affects how agents behave. We show that a set of robust empirical predictions can be derived for a very general class of models of SI: in particular, they generate correlated actions among interacting agents and induce amplified responses to shocks (the"social multiplier"). These insights, used by the ample literature on the effects of SI on individual decision-making (see Glaeser and Scheinkman (2000) for a survey), form the backbone of our empirical approach.

We consider a panel that allows us to classify Italian industrial firms into two groups, a *study* group of firms that are more likely to be exposed to SI and a *control* group that is unlikely to interact in the social space. We assume that SI will only take place if firms are sufficiently similar industrially and geographically. Thus, to identify exposure to SI we rely on firms' *similarity*, identified with their product brand, and *proximity*, defined in terms of geographical distance. Districts - a well known feature of the Italian economy - are small geographical areas characterized by a high concentration of similar, supposedly connected firms (Becattini 1998); they therefore constitute an ideal lab in which to study SI.

We consider what is arguably firms' most relevant decision, i.e. employment, and assess how employment changes are influenced by SI. Our first test is based on the correlation between individual and aggregate measures of employment adjustments. As we know, identification of spillovers along these lines is a hard task, fraught with difficulty. First, if firms that share some similar characteristic, such as a high propensity to grow, tend to locate close to one another, then correlation across actions may just reflect the omission of this decision-relevant but unobserved characteristic. This is known in the literature as the self-selection problem. Second, these tests are subject to the reflection problem noticed by Manski (1994). Finally, SI gives rise to correlated actions among closely related units in *addition* to the correlation induced by shocks that are common to these units. In empirical testing one has to make sure that common shocks have been appropriately taken into account before concluding that correlated actions reflect spillovers, arguably the most problematic aspect of this approach. Our work addresses these problems jointly.

We deal with self-selection by exploiting the panel nature of our data. We control for unobserved heterogeneity by using firm fixed effects estimates, which remove any firm-level fixed characteristics, thus controlling for the possibility that firms with certain attributes relevant to employment adjustments might tend to locate in a given area. Following a suggestion of Brock and Durlauf (1999), we address the reflection problem by identifying firm-level variables that arguably affect decisions only directly and not as group means across firms in the references group .

To test for the relative importance of spillovers versus unobserved common shocks, we relate the factor adjustment of a given firm in a given sector and district to the adjustment of the other firms in the same sector and district - the *reference group* - and to that of firms that are in the same sector but outside the district or in the district but in a different sector. We show that, even after controlling for shocks with firm level sales, one firm's adjustment is affected by the adjustments of the other firms in the same district and sector - the reference group - but not by that of firms outside the district or the sector or both. This allows us to exclude the possibility that our results may be due to unobserved shocks to the sector or the locality. We estimate an elasticity of individual adjustment to average adjustment in the reference group of around 0.3 in the baseline specification, a substantial effect, consistent with the view that social interaction is at play in industrial clusters.

This test, however, still leaves the possibility that the correlation may be driven by shocks that are specific to a sector *and* to a location at the same time and are not captured by our sales-based proxies. As a final check, for a subset of firms we construct the reference group using information reported by the entrepreneurs in an ad-hoc phone survey to directly identify the links that each firm establishes with its neighbors. We then construct firm-specific reference groups and calculate a measure of the reference group adjustment that varies by firm. We can thus reliably separate the effect of spillovers from that of unobserved common shocks by inserting a full set of year-district-industry effects. We find that reference group adjustment remains significant, albeit reduced in magnitude, arguably because of the imprecision of our measure of individual reference groups.

Exploiting this dataset, we also investigate more subtle aspects of *how* SI takes place. First, we consider the correlation between individual actions and characteristics of distribution of adjustments other than the mean. The empirical literature on SI tends to consider only the correlation between individual and average actions. This represents more a working assumption than an implication derived from formal modeling. We find that firms react to extreme rather then to mean adjustments, possibly because the former are more easily observable. Second, we study the role of firm size in determining the sensitivity to the actions of the other firms in the reference group. We hypothesize that SI should be less relevant for larger firms, an this is clearly supported by the data.

Our second test is based on the notion that SI should give rise to amplified responses to shocks, because the initial impulse is magnified by the response of the other members of the reference group. Additional implications can be drawn from models of a specific form of SI, i.e. information spillovers. In these models, agents are facing a common problem in an uncertain environment and each agent has a piece of private information, which can be inferred from other agents' actions (Banerjee 1992, Bikhchandani, Hirshleifer and Welch 1992). Information spillovers models predict a distinctive temporal pattern of adjustments.

The possibility of observing the behavior of others induces an incentive to delay adjustments in order to gather more information; once someone does act, however, the information revealed could trigger further actions, and start a self-reinforcing process that prompts many agents to undertake the adjustment within a short time span. We should therefore expect periods of low adjustment activity followed by upsurges, even if the underlying forcing variables have changed little (Schivardi 2003). Our empirical results conform to these predictions, giving further support for the role of SI in industrial clusters and suggesting that information spillovers are one of the mechanisms.

Our work is related to a large and growing literature that shows that SI is at play in a variety of situations (see Brock and Durlauf (1999) for a survey). In terms of the specific forms in which SI takes place, recent work in the development literature has found convincing evidence that information spillovers are an important channel for the diffusion of new agricultural techniques in developing countries (Bandiera and Rasul 2004, Conley and Udry 2003, Foster and Rosenzweig 1995, Munshi 2003). Most of the work is based on case studies or experiments; this approach has some advantages, in that one can more easily control for confounding factors and address the mechanism giving rise to SI more precisely, by inquiring directly how it takes place. On the other hand, its interest is limited by the nature of the experiment and by the span of applicability of the case study. Our work instead tests SI in the context of a general dataset referred to a representative sample of firms.

The rest of the paper is organized as follows. In Section 2 we lay out a simple framework to show how SI translates into correlated actions for the different types of spillovers that have been emphasized in the theoretical literature. In Section 3 we describe the data and discuss how we measure exposure to SI. In Section 4 we construct the first empirical test, based on the interaction between individual and aggregate measures of adjustments, present the results on the basic sample and discuss the findings from the ad-hoc phone survey. Section 5 extends the framework to test whether extreme moments matter more than the mean adjustment and whether the effect of aggregate actions differs across firms of different size. Section 6 tests the implications of SI for firms' response to aggregate shocks while Section

7 concludes.

2 Theoretical background

Our empirical analysis studies whether labor adjustments at the firm level are affected by SI, with the reference group defined in terms of geographical distance and product similarity. While we could test SI focusing on some other firms action, such as technology adoption or innovation, in practice such data are much more difficult to obtain. As we will discuss later, by focusing on labor we can consider a large number of firms over a long period of time, which allows us to address a number of criticisms that plague the empirical literature on the subject. In addition, employment is ultimately the most relevant choice of a firm from a welfare perspective, so that studying how SI affect it is policy relevant. To understand what kind of mechanism could give rise to an effect of the reference group labor adjustment on the single firm adjustment, we briefly review the literature on SI, focusing on the issues that are relevant for our case. While a comprehensive taxonomy is well beyond the scope of this paper, it is useful to present a simple scheme that will guide the empirical analysis.[1]

In models of SI the expected utility that an agent draws from an action depends on those chosen by other agents in the same reference group, defined as the set of agents with whom she directly or indirectly interacts. This effect occurs above and beyond that conveyed by some market indicator, such as price. These models can therefore be thought of as externalities models. For the purposes of this paper, we are interested in models of *strategic complementarities,* in which the expected utility that an agent draws from an action increases with an aggregate measure of the action of the other agents in her reference group. This definition is very general and encompasses a large class of models, that in turn describe many different economic situations. In terms of our empirical study, it is useful to consider a firm's profit maximization problem, which depends on the choices of other firms

[1] Glaeser and Scheinkman (2000) offer an in-dept analysis of the first class of models reviewed below, and Duranton and Puga (2003) review the literature on knowledge and information spillovers.

in the reference group. Formally:

$$\text{Max}_{l_i}\Pi(l_i, l_{-i}) = E\{F(l_i, l_{-i}) - wl_i\} \quad (1)$$

where l_i is firm's i employment choice, l_{-i} is an indicator of the choices of the other firms in the reference group, w is the wage rate and E the expectation operator.

A first class of models is predicated on the notion that the action of the agents in one's reference group directly affect her utility. In term of the above expression, these models assume that $\frac{\partial^2 F}{\partial l_i \partial l_{-i}} > 0$: the marginal product of labor (and therefore labor demand) is increasing in the employment level of other firms. We name this class as relating to *real spillovers (*RS*)*. Among the most common examples are network externalities, where the utility from adopting an innovation increases in the number of other agents doing so (Katz and Shapiro 1986); search, where the utility of searching increases with the number of other searchers (Diamond 1982); intermediate input variety, where the number of intermediate inputs that can be used in production increases productivity (Romer 1990).

A second class of models is based on *knowledge spillovers* (KS), where the knowledge of each agent spills over to the other agents in the reference group. Typical examples of activities that give rise to KS are R&D investments, technological innovation and human capital accumulation. In these models, agents benefit from the proximity with other agents because they can learn from them and so increase their own productivity. In terms of equation (1), these models can be formalized by assuming that the production function is shifted by a disembodied productivity term which in turns depend on the employment of other firms: $F(l_i, l_{-i}) = A(l_{-i})f(l_i)$, with $\frac{\partial A(l_{-i})}{\partial l_{-i}} > 0$. This effect arises if the activities mentioned above are correlated with employment. Note that this class of models can be regarded as a special case of the previous one. We consider it separately because of the recent surge of interest in economic geography, which stresses that production tends to be concentrated in regions that specialize in some particular product, arguably due to knowledge spillovers.[2] Indeed, this is traditionally seen as the main reason of the success of

[2]See for example Audtresch (1998), Audretsch and Feldman (2003), Harrison, Kelley and Gant (1996), Jaffe et al. (1993).

industrial clusters (Saxenian 1994), such as the industrial districts we study.

The last class of models of SI we consider is that with *information spillovers* (IS). In these models, the agents in the reference group are facing a common problem in an uncertain environment. Each agent has a piece of private information, which can be inferred by other agents from the action taken. Actions, therefore, do not directly influence the utility (as in RS models) or the knowledge (as in KS models), but rather change the information set based on which an agent takes a decision. In terms of the expression above, we can model IS by assuming that productivity is uncertain, and that the actions of others shift the probability distribution over outcomes:

$$E\{F(l_i, l_{-i})\} = \int A d\Gamma(A|l_{-i}) * f(l_i) = E(A|l_{-i}) * f(l_i)$$

where the cumulative density function $\Gamma(A|l_{-i})$ is decreasing in l_{-i} in a first order stochastic dominance sense: higher employment of other firms shifts beliefs toward the good productivity states, so that $\frac{\partial E\{A|l_{-i}\}}{\partial l_{-i}} > 0$. The focus in the literature is on how the private information of the agents is transmitted through actions, and how IS influence the timing and outcomes of the decision-making process.[3]

The common feature of social interactions models is that they predict that individual actions are positively correlated with aggregate measures of activity, even after controlling for common factors, because the expected returns of an action increases with the aggregate level of activity. In terms of the scheme above, $\frac{\partial^2 \Pi}{\partial l_i \partial l_{-i}} > 0$ for all models. This is the prediction that forms the basis for our empirical test on employment changes. While the prediction is common, its interpretation depends on the particular mechanism behind it. In the case of RS and KS, the correlation would signal that changes in the level of activity of other firms, as measured by employment, impact individual productivity and therefore labor demand, due for example to thick market externalities (Diamond 1982) or because KS increase with the level of activity. In this case, employment changes would be the observed outcome of SI. In the case of IS, employment adjustments would be the channel through

[3]See, among others, Banerjee (1992), Bikhchandani et al. (1992), Caplin and Leahy (1994), Chamley and Gale (1994), Ellison and Fudenberg (1993), Horvath, Schivardi and Woywode (2001), Rob (1991) .

which individual information on a common problem is conveyed. For example, in a declining industry firms might be uncertain about the persistence of negative demand shocks, and they could interpret other firms' employment reductions and exits as an indication that they assess such shocks to be permanent, thus updating their beliefs consequently.

In addition to a positive correlation between individual and aggregate actions, models of SI also imply an increase in the variance of aggregate outcomes (the "social multiplier"), because SI provide a natural amplification mechanism of shocks. In terms of the formalization above, this can be seen by assuming that the productivity term A has an exogenous component and an endogenous one, dependent on firms actions. As usual in models with strategic complementarities (Milgrom and Shannon 1994), a change in the exogenous component will be amplified because it will prompt an additional endogenous response induced by firms' actions. This implication offers an alternative way of testing the effects of SI that we will exploit later.

Our first aim is to provide evidence on the relevance of SI for firms in industrial districts, so that all mechanisms above fall within the boundaries of our analysis. It is more difficult to tell the different mechanisms apart, due to their similar empirical implications. In what follows, therefore, we take a rather agnostic view on the source of SI; however, we will try to discern if some features of the data are more suggestive of one or another of the mechanisms reviewed in this section.

3 Data description

Our dataset consists of a panel of Italian manufacturing firms drawn from the Company Accounts Data Service (CADS) which collects annual balance-sheet data on a sample of about 30,000 firms, 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.[4] To identify firms with high exposure to SI, we merge this database with the Industrial Districts Database (IDD) constructed by the National

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

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 of small to medium-sized firms in the same two-digit sector classification, it is classified as a district. Districts are allocated to a 9-sector classification according to their product specialization. We then identify firms that are in the same district and sector and thereby divide the sample into a study group (firms in the same district and sector, i.e. those with high exposure to SI) and a control group of firms with low exposure to SI (firms in the same sector but not located in districts). The geographical classification ensures that the firms that we include in the study group satisfy the observability criterion. Since they belong to the same sector, the similarity requirement is also fulfilled. In fact, this is an ideal context to test the relevance of SI in shaping firms' decisions. Table I reports summary information sector-by-sector for the sample, using Istat's 9-sector classification. Panel A compares the sample with the population; the first two columns show the incidence of employment in specialized district firms (i.e. firms located in a district and belonging to the specified 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 firms is minor. Columns 3 and 4 show employment in specialized district firms 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: first, for some districts, there are only a few firms. For instance, the average number of specialized district firms in the "food, beverages and tobacco" industry is 9.8, and in 1991 only 1 district out of 16 had more than 30 firms. The figures 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 firms 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 firms in any sample year.[5] Second, some sectors are characterized by a high degree of heterogeneity when a two-digit classification is used, making it hard to fulfill the similarity criterion. The last column of the table classifies the 9 sectors according to product heterogeneity. The classification was made by informally comparing the list of products in the 4-digit classification 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 classification 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 firms; non-district firms in the five sectors are 3,146 for a total of 42,022 observations.[6]

Table II reports summary statistics for each sector and district and for non-district firms, 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 firms, 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

[5]This excludes firms producing ”rubber, plastic and chemical products” and firms classified as ”other manufacturing”.

[6]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 firms, they were all dropped. We have also reclassified the mechanical sector using a three-digit classification; the only sector with a low degree of product heterogeneity that had the minimum number of firms was “production tools”, which has three districts. Finally, we have separated “wood & furniture” from “construction materials and glass” which in the 9-sector classification are lumped together. This way, we retain three districts in “wood & furniture” and one in “construction materials and glass”.

specialized firms (observations) ranges from a minimum of 38 firms (552 observations) in the production tools district of Padua, to 329 (4,250) in the wool district of Prato. Though district firms 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 firms. Yet it varies across districts, as does firm performance (return on assets, Column 6). In Section 9 we investigate the relation between firm performance and district concentration. Column 8 reports the share of firms in the modal four digit sector both within each district and for firms 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 fifty percent of specialized firms. The high degree of similarity among district firms could give rise to a correlation in factor adjustments not necessarily due to SI, 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 firms - i.e. firms located in the district but producing different goods. Comparing the average number of specialized and non-specialized firms gives a clue of the production focus of the various districts and reveals that districts differ along this dimension as well.

4 Spillovers or unobserved common shocks?

In this section we test the most basic implication of SI models: individual and aggregate measures of action should be positively correlated *even after controlling for shocks.* This is the natural starting point, because finding no correlation would rule out any spillover. At the same time, given that we do find a correlation, we are far from having shown that SI matter, because such correlation is also consistent with both unobserved common shocks and self-selection (i.e. the endogenous choice of firms with a similar adjustment propensity to locate close to each other). We carefully address both points below.

4.1 Empirical specification

The basic empirical equation that we will estimate is:

$$n_i(t) = a + \alpha_i + \beta_0\epsilon_i(t) + \beta_1\epsilon(t) + \beta_2 n_{-i}(t) + u_i(t) \tag{2}$$

where $n_i(t)$ is the percentage change in firm's employment, α_i a measure of the firm long-run efficiency, $n_{-i}(t)$ is the percentage adjustment of other firms in firm i reference group, $\epsilon_i(t)$ and $\epsilon(t)$ are an idiosyncratic and a common shock and $u_i(t)$ is an error term uncorrelated with $\epsilon_i(t)$ and $\epsilon(t)$. Equation (2) formalizes the first implications of SI, i.e. the positive correlation between individual and aggregate adjustments. The absence of 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.

The identification of SI faces important empirical challenges. Given that any common shock would influence aggregate adjustment, if we were not correctly controlling for them we would find a spurious correlation between aggregate and individual adjustments. The aggregate adjustment would in fact be a proxy for the unobserved shocks, and the interpretation of $\beta_2 \neq 0$ as an indication of spillovers would not hold. Stated differently, $\beta_2 \neq 0$ is a necessary but not a sufficient condition for spillovers.

The interpretation of β_2 as evidence of SI is further complicated by the self-selection problem. In fact, it could be that, due to for example input-output relations, firms with similar, but unobserved characteristics (say with a particularly high propensity to grow) tend to locate close to each other.[7] This would give rise to a positive correlation in adjustments independently of SI. Fortunately, the longitudinal structure of our data allows us to address this problem by estimating a fixed effects regression: in this way, we control for any firm-level characteristic, such as a higher than average propensity to grow, which leads

[7] There is evidence that is contrary to this. In a survey on a sample of Italian firms located in industrial districts run by the Bank of Italy in 1997, it is found that in 95 per cent of the cases the firm owner, often the founder and manager of the firm, was born in the district where the firm is located, suggesting that entrepreneurs' location choice is dictated mostly by birth.

geographically close firms to behave similarly.

An additional problem relates 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. Following Brock and Durlauf (1999), we solve the reflection problem by identifying some variables that enter equation (2) at the firm level but not as averages for the group. 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.[8] We will insert in our empirical specification firm-level proxies for liquidity constraints and assume that while they affect firms directly, their group average does not directly affect firms adjustment decisions. Our justification for this is that firms credit-worthiness - which determines access to credit - depends on firm specific variables but not on group averages once the former are controlled for.[9]

In order to estimate equation (2), we still need measures of the aggregate and specific shocks that firms face. We construct a measure of such shocks using firm-level sales, that should capture any interaction that takes place directly through market mechanisms. 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 dummies interacted with location and sector dummies to allow for aggregate shocks differing across area and sector. To better account for local shocks, for district firms we allow for one location dummy for each district, while for non-district firms we use provinces.[10] We then use the (time-varying)

[8] 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.

[9] As a measure of financial constraints we use the ratio of firms cash flow to total sales. To save on space we do not report the coefficients of the proxies for liquidity constraints; in all regressions they turn out to be statistically significant and to have the expected signs. We have also experimented with alternative measures of credit constraints, such as the share of intangible assets on total assets (a measure of firm's ability to pledge collateral) and the share of liquid assets on total firm's assets (an indicator of firms ability to face liquidity needs). Our results are invariant to the measure used.

[10] The Italian territory is divided into 103 provinces, each broadly equivalent to a US county.

fitted values from this regression (common within a group of firms in the same region and sector and, therefore, common to all specialized firms within a district) as a measure of the aggregate shocks; the residuals, firm specific, are taken as proxies of the idiosyncratic shocks. Needless to say, this variables might not be enough to capture all common shocks, a point that we address at length in the following analysis.

4.2 Results

For each firm and for each year in the sample, we measure the adjustment by other firms in the same district and sector - the reference group - as the average percentage change in employment by the firms in the group, excluding the adjustment of the firm in question. To account for unobserved variables that may be relevant to factor adjustment, and particularly for self-selection issues, we estimate a fixed-effects model. Obviously, the fixed effects will also pick up any unobservable variable that is common to all firms in a given district-sector and does not vary over time. Table III, Column 1 shows the results of parameter estimates for the simplest specification, which only includes controls for aggregate and idiosyncratic shocks and the adjustment of similar and observable firms, i.e. those located in the same district and sector.[11] Both aggregate and firm-specific shocks have a positive and highly significant impact on factor adjustment, though idiosyncratic shocks are economically twice as important as aggregate shocks (the estimated coefficients are 0.056 and 0.026 respectively). The estimates show that each firm's factor adjustment is positively and significantly affected by the adjustment of the other firms in the same district and sector (coefficient = 0.308; t statistic = 5.61): an average increase in employment of one percent by 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

[11] 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 us to exclude 372 observations on the total of district and non-district firms. 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 firms and 34,795 for non-district firms.

the idea that firms are heavily influenced by the actions of other, similar firms.[12]

As explained before, this result could also be due to the presence of unobserved, time-varying common shocks not picked up by our measure of aggregate shocks. A first possibility is that these are shocks either to the sector or to the location (natural disasters, public investments etc.) of the firms in a given reference group. To address this problem we add to the regression two explanatory variables: first, for each firm i and year t in the sample we compute the average adjustment of firms located in *other districts* but in the *same sector* as firm i.[13] Second, for the same firm i and all years, we compute the average adjustment of firms located in the *same district* as firm i but belonging to sectors *other* than that of i. If our measure of adjustment by firms in reference group is picking up unaccounted sector-specific or location-specific shocks, these two variables should absorb part of the effect and the estimate of the reference group adjustment should diminish in both magnitude and significance. The results of the estimates are shown in Column 2 of Table III. The parameters of the aggregate and specific shocks are essentially unaffected, as is that of the adjustment by firms in the reference group, which is only slightly smaller (0.287 compared to 0.308) and equally significant. None of the other measures of adjustment included in the regression (by firms in other districts, those in other sectors, or non-district firms in the same sector) has explanatory value. They all have small and statistically insignificant coefficients whether taken alone or as a group (the group test for the hypothesis that they are jointly equal to zero has a p-value of 0.578). This allows us to exclude the possibility that unobserved common shocks are either sector-specific or location specific.[14]

[12] We have also experimented adding as additional controls to our basic specification the lagged dependent variable, the lagged value of the adjustment of others and the growth in unit labor costs at the firm level, obtained dividing the total wage bill by the number of employees. The results (not shown for brevity) are invariant to the inclusion of these additional controls.

[13] To calculate the adjustment of firms in other districts, for sectors with multiple districts we confine 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.

[14] There is yet another explanation for these results. As shown in Table II, district firms tend to have a relatively high degree of sectorial 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 firms out of district has little explanatory power, because such firms are not as specialized in the same goods, and therefore are not good proxies for sectoral shocks. To account for this possibility, we further restrict the definition of sector when selecting the control group. For each district, we retain the firms in the modal four-digit sector and, if this has less than fifty percent of the firms, all firms in any other four-digit sector

Finally, Column 3 of Table III reports the basic regression for non-district firms. We take as the reference group for these firms all other non-district firms in the same sector. Since no restriction is put on location, firm j and the firms in its reference group will on average be located far apart and the observability requirement will not be fulfilled. Consequently, if SI are the reason why other firms' actions affect firm j's decisions, the adjustment of others should have no effect when equation (4) is estimated on the sample of non-district firms. And this is what we find: while the measures of aggregate and idiosyncratic shocks are both significant and with coefficients comparable to those found for district firms, the adjustment by other non-district firms in the same sector as firm j has a small coefficient, with the wrong sign and not statistically different from zero.

Taken together, this evidence strongly suggest that the correlation between individual and aggregate adjustments is not due to unobserved sector or location specific shocks. Still, one could argue that we do not account for shocks that are simultaneously specific to a given sector *and* to a given location. We now tackle this last possibility.

4.3 Self-reported reference groups

The relevance of shocks that hit only a specific sector in a given location is more questionable than that of sector-wide or area-wide shocks. However, given that this is a theoretical possibility, we also tackle this issue. A radical way to solve the problem is to insert a full set of district-sector-year dummies to pick up any common shock to the firms in a given district and sector. The difficulty with this approach is that most of the variability in the adjustment of the reference group would be absorbed by the time dummies, inhibiting identification of SI effects. In fact, reference group adjustment is constructed as the average

with at least twenty-five percent of firms. For firms in these sectors, we then construct the adjustment of non-reference firms (in other districts or out of districts) within the narrower sector definition. For reference group firms, we maintain the same measure of adjustment as before, based on the coarser sector definition. From a sectorial classification viewpoint, there is now more heterogeneity in the reference group firms 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, not reported but available from the authors, reveal that the coefficient of the adjustment of the reference group drops slightly, arguably for the higher heterogeneity; however, the adjustment of non-reference group firms still fails to have any impact, suggesting that our results are not driven by the higher similarity among district firms, and that proximity is indeed a necessary condition for the effects that we find.

adjustment of the other firms in the same district and sector, excluding the firm itself; as such, it has a very low degree of cross-firm variability in any given year-district. Therefore, we cannot run our basic specification including district-sector specific time-varying controls, which would wipe out all variability in the reference group adjustment.

However, such controls could be used if the reference group were firm-specific and showed enough cross-firm variability. Indeed, each entrepreneur in a district might have closer links with a sub-group of the firms. We have followed this idea to construct a test of the spillovers hypothesis that is robust to the presence of unobserved common shocks. We selected three medium-sized districts, i.e. Santa Croce (leather and footwear), Biella and Cossato (wool) and conducted a short phone questionnaire with all the firms that we were able to track down. The main question we asked is:

> *Think of the firms located in your district that are related to you, particularly firms that operate in the same market and with similar lines of production. Could you please name the firms whose decisions in terms of investment, employment, innovation and other strategic choices you are best aware of?*

This question allows us to obtain the names of the components of one firm's reference group. Firms were asked to indicate up to 12 names and the interviewer insisted to extract as many names as possible. We then matched the names reported with those in our dataset.

Overall, we were able to construct the reference group for 97 firms, slightly less than half of our original sample (231 firms in 1991 in the three districts, see Table II). The average number of firms reported is 3.4. Since our dataset does not contain the population of firms, we cannot match all the firms referenced, but we succeeded in matching approximately two-thirds: the average number of matches is 2.2.[15] Table IV shows summary statistics for the interviewed firms with at least one match. Some interesting patterns can be noted. First, in these districts there seem to be no tendency for some firms to emerge as focal points of

[15] While the number of referenced firms may seem small, it is consistent with theoretical predictions of models of (costly) network formation. For instance, Bala and Goyal (2000) show that in some equilibrium configurations firms in a network will interact directly with only one other firm in the network. Indeed, in the contest of pineapple farming in Ghana, Conley and Udry (2003) find that, even at the level of the village, networks are sparse, so that each farmer only communicates directly with a small number of other farmers.

the network: no single firm is reported by more than 10% of the respondents. Moreover, links tend to be asymmetric: few firms reference each other. Obviously, since we do not match all cited firms, the self-reported reference group adjustment will be measured with error; thus, if we find any effect of SI, this should be regarded as a lower bound of the true effect.

With the firm-specific measure of reference group adjustment we can run our basic specification while taking common local and sectorial shocks into account in the most careful way.[16] To this purpose we include a full set of district-year interactions and four-digit sector-year interactions. We regress individual firm labor adjustment on a measure of specific shocks computed as in the previous section (aggregate shocks are now perfectly collinear with the time-varying dummies), the average adjustment of the firms in the individual reference group and the interaction dummies. Overall, we have a sample of 613 observations and (in addition to the firm fixed-effects) a total of 117 dummies to account for unobserved common shocks. Results for this regression are reported in the first column of Table V. The point estimate of the coefficient of the average adjustment by the firms in the self-reported reference group is 0.116, positive and significant at the 10% level (p-value 0.087), a result that speaks in support of SI. In the second column we replace the district-year and the sector-year dummies with the common component of sales - the measure of common shocks used in the previous sections. If the estimates of the adjustment by other firms in the same district and sector reported in previous subsection were reflecting omitted common shocks rather than SI, we should find here a significant *increase* in the estimated parameter of the adjustment of the self-reported reference group. Yet the estimate is 0.122, only slightly larger than that in column (1). Based on this last piece of evidence, we conclude that the adjustment of others is not proxying for unobserved common shocks but has an effect on its own, so that we are actually detecting spillovers.

[16]The use of firm-specific reference groups also helps in tackling the reflection problem. The reflection problem arises from a summing-up condition on the average adjustment. In the presence of heterogeneity of the reference groups, however, the summing-up condition does not hold.

5 Extensions

Previous research has mainly considered the correlation between individual and *average* actions. The richness of our dataset allows for the investigation of more subtle aspects of *how* SI takes place. In this section, we introduce two extensions. First, we consider the correlation between individual actions and other moments of the adjustment distribution. Then, we study if firm size has an impact on firm sensitivity to the actions of the other firms in the reference group.

5.1 Reaction to Extreme Adjustments

Most of the empirical literature on SI considers the correlation between individual and average actions. This represents more a working assumption than an implication derived from formal modeling. Indeed, nothing prevents other moments of the distribution of actions to impact on individual behavior. In this section we analyze the effects of extreme adjustments, i.e. large changes in employment. This analysis is motivated by the fact that, in terms of observability, extreme labor changes, such as large increases in the scale of operations or closures, might be more easily observed than more modest changes, so that they should be more important, particularly in terms of information spillovers.[17] Moreover, this is an interesting question in itself on the channels of diffusion of spillovers, particularly in terms of policy implications.[18]

We calculate the 10^{th} and 90^{th} percentiles of the distribution of the adjustments by firms in the reference group and in other control groups and estimate equation (2) using such variables as proxies for other firms' adjustment. Table VI shows the results.[19] Column 1

[17]For example, Conley and Udry (2003) study the diffusion of a new fertilizer in pineapple farming in Ghana, finding that agents only observe, and imperfectly, broad facts, such as the fact that another farmer had a particularly good harvest, rather than specific details.

[18]For example, a finding that extreme adjustments exert a proportionally larger effects would imply that policies of employment subsidies that aim at taking advantage of spillover effects should concentrate intervention on a few firms rather than subsidizing a little all of them. This type of analysis would be relevant also in other contests: for example, it would be interesting to study if individual students' outcomes are influenced by the quality of the top students in the classroom or by the average quality of classmates, because different findings would have very different implications for students sorting.

[19]With nonlinear measures of adjustments the "reflection " identification problem does not arise (Brock and Durlauf 1999). For comparability, and given that they are significant, we include the proxies for liquidity constraints also in this set of regressions.

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.[20]

To further sharpen our test, we 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 VI, 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 indicates that the whole distribution of adjustment matters for the effects of SI, and that extreme measures of adjustments are more important than average ones.

Finally, Column 4 runs the regression for non-district firms with the 10^{th} and 90^{th} percentiles in the adjustment of other non-district firms. In this case, the 10^{th} percentile has a large and significant coefficient but the 90^{th} percentile is not significant and has the wrong sign.

We have also experimented with other measures of "extreme adjustments", such as the share of firms that change employment by more than a given threshold amount, which, as is shown by Chamley and Gale (1994), in certain circumstances can be taken as a sufficient statistic of other firms' actions. The results, not reported for brevity, are similar to those obtained with the percentiles of the distribution of adjustments, lending further support to

[20] To replicate the analysis of the previous section, 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 5). In addition the hypothesis that they are jointly equal to zero cannot be rejected by an F test (p-value $= 0.246$). This is in line with the findings of the previous section.

the SI hypothesis. These results find a natural interpretation in terms of observability in an IS framework, lending indirect support to this particular channel of SI.

5.2 Firm size and sensitivity to social learning

Presumably, not all firms react in the same way to the actions of others. Some firms may not rely, or need rely less, on SI. Indeed, all forms of SI would suggest that these are presumably larger firms. For example, in terms of real spillovers, large, more integrated firms might have more internal economies of scale that would make them less dependent on network externalities.[21] In terms of knowledge spillovers, Eeckhout and Jovanovic (2002) argue that spillovers only occur when there are differences in the levels of knowledge among agents, with less knowledgeable firms benefiting more from them. In their model, the level of knowledge is related to firm size, so that small firms should show a higher sensitivity to spillovers. Finally, in terms of information spillovers, large firms are likely to have both more private information and a better capacity to process it, as well as access to a larger network than smaller firms. They should therefore be less sensitive to local SI.[22]

To test the hypothesis that the influence of spillovers depends on firm size we split the sample of district firms by size and run our basic specification for each quartile. The results, reported in Table VII, are supportive of the above idea: the effect of reference group adjustment, while positive and significant for all size groups, declines monotonically with the size of the firm. Taking the first and the last quartile, the difference in impact is substantial: among firms in the first quartile the impact of the adjustment of others is more than three times as great as among firms in the fourth quartile (0.679 compared to 0.177). For the middle two quartiles the coefficient is in between these two extremes, around 0.3, close to that for the sample as a whole. As theory predicts, the evidence is therefore consistent with small firms being more influenced by SI.

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

[22] In a similar vein, Bandiera and Rasul (2004) study the adoption of a new crop in Mozambique, finding that more informed farmers are less sensitive to the adoption decisions of others.

6 Amplification of aggregate shocks

As argued in Section 2, SI can generate magnified responses to aggregate shocks. Glaeser and Scheinkman (2000) define this property in terms of "social multipliers", showing that it offers an alternative way to empirically investigate SI. In this section we elaborate on this idea to construct an alternative test of SI, which does not relay on the adjustment of others. Moreover, as we will argue, it will also allow to shed some light on the relative importance of the various types of spillovers.

SI offers a natural mechanism of amplification of aggregate shocks. In fact, the direct effect of the shock is magnified via the indirect effect through the adjustments of other firms in the reference group. While this implication is common to all models of SI, those based on information spillovers have some specific predictions on the time pattern of adjustments. A distinctive feature of this class of models is that they predict equilibrium strategic delays (Caplin and Leahy 1994). In fact, by waiting agents can extract useful information from the actions of others and make a better informed decision. As a consequence, the endogenous pace of information revelation can be seeded-up in a nonlinear fashion by shocks that break the inertial behavior induced by social learning, resulting in the bunching of adjustments. Schivardi (2003) applies this idea to explain the large increase in job destruction that we observe at the troughs (Davis, Haltiwanger and Schu 1996), and shows how relatively small aggregate shocks can, in theory, induce a burst of reallocation activities if they set in motion the process of information-revealing actions. The response to a given aggregate shock will be magnified only if it breaks this inertial behavior. The implication for the two groups of firms in our dataset - i.e. district and non-district firms - is that, once we identify the "adjustment years"- i.e. years in which substantial adjustment takes place (see below) - firms that are subject to IS should display 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 amplified by information flows. Non-district firms should instead show no substantial differences between adjustment and non-adjustment years, given that for them the shock is presumably all that matters.

To avoid endogeneity problems, 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 industries:[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:

$$n_{it} = b_1\epsilon(t)d_{na,d} + b_2\epsilon(t)d_{a,d} + b_3\epsilon(t)d_{na,nd} + b_4\epsilon(t)d_{a,nd} + b_5\epsilon_i(t) + u_i(t) \qquad (3)$$

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 IS model in Schivardi (2003) implies that $b_1 < b_2$ (the response of firms exposed to IS should be higher in adjustment years), $b_1 < b_3$ (firms exposed to IS are less responsive to aggregate shocks in non-adjustment years), $b_2 > b_4$ (exposed firms respond more than non-

[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 therefore reducing the number of adjustment years) up to the mean plus or minus 1.5 times the standard deviation. Our results are robust to such changes.

exposed firms in adjustment years), and $b_3 = b_4$ (no difference in responsiveness to aggregate shocks among non-exposed firms). Theories of real spillovers would instead predict that $b_1 = b_2, b_3 = b_4$ and $b_1 > b_3$ (within a group, no difference should occur between periods and, more importantly, exposed firms should be more sensitive than non exposed ones).

The estimation results are reported in Table VIII. The point estimates (Column 1) support the predictions of IS models. The response of district firms 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 effects of the shocks are greatly amplified. For non district firms, instead, the difference is much smaller (and not statistically significant). In terms of comparison across the two groups, the coefficient for district firms in non-adjustment years is smaller than that of non-district firms (0.023 versus 0.059), while the reverse holds (marginally) for adjustment years (0.072 *versus* 0.067). Tests of equality of the coefficients reported at the bottom of the table confirm these conclusion, with only the test that district firms have a higher response than non-district in adjustment years being rejected. These results suggest that district firms do concentrate the response to aggregate shocks in certain periods.

Since the definition of adjustment years is somewhat arbitrary both in sample period and in threshold, we have checked our results defining 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 VII, are very similar to those obtained when all adjustment years are used; however, in this case we fail to reject the hypothesis that, for exposed firms, the response in 1993 is the same as in the other years (i.e. that $b_1 = b_2$).[26]

We can sharpen further our test of the implications of IS for the sensitivity of factor

[26]This is probably due to the fact that in this case the parameter of the response to aggregate shocks in the adjustment year is estimated with less observations (and therefore less precisely); moreover, while restricting the definition of adjustment years to 1993 guarantees that we are selecting a true adjustment year, it also implies that the other years might include both adjustment and non-adjustment years. This will tend to bias the test towards finding no difference in sensitivity to aggregate shocks.

adjustment to aggregate shocks. If the extra response to shocks that we observe in adjustment years for district firms is indeed due to social learning, then this effect 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 firms; we then run the same regressions including the average adjustment of other firms 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 find a sizable decline in the difference between the coefficients when the adjustment of others is included in the regression. Formal tests of equality of the coefficients, 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 at least in part for the extra response of firms to aggregate shocks in adjustment years.

To sum up, results in this section offers further support to the notion that employment adjustments are influenced by SI. While not in contrast with any type of spillovers, they are remarkably consistent with the predictions on delays and bunching that are specific to IS models, suggesting that at least part of SI occur through this particular channel.

7 Conclusions

We have exploited a rich dataset on a sample of Italian manufacturing firms to assess whether SI among entrepreneurs located in industrial districts are an important factor in determining firms decisions and triggering co-movement in actions. We use the concepts of product similarity and geographical proximity to identify a set of firms that are more likely to be exposed to SI. We find that, after controlling for aggregate and individual shocks, individual adjustments in labor are strongly influenced by various measures of aggregate adjustment within the reference group. Moreover, the time pattern of adjustments lend support to the hypothesis that part of SI take the form of IS. Our results imply that geographical spillovers are at the basis of the correlation between firms actions in a connected group, suggesting that the emphasis on spillovers in the geography literature as a vehicle for enhancing local growth and productivity is well rooted.

The analysis can be extended in many different directions. We plan to study the rate at which SI effects die out with distance, to asses how "local" spillovers are. This would imply relating the adjustment of firms in a district to that of firms in other districts, controlling for the distance between them. Secondly, we plan to extend the direct inquire to entrepreneurs, in order to study the structure of the reference groups and their formation, a topic on which empirical evidence is very scant. Finally, it would be important to investigate the effects of SI on firms' performance to better asses the implications of spillovers for industrial policy, particularly for such phenomena as the diffusion 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 essential role.

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A APPENDIX

A.1 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 firms. 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 firms demographics (year of foundation, location, type of organization, ownership status, structure of control, group membership etc.), on employment, and their flow of funds. Balance sheets are reclassified in order to reduce the dependence of the data on the accounting conventions used by each firm to record income figures and asset values. Balance sheets for the banks’ major clients (defined according to the level of their borrowing) are collected by the banks. The focus on the level of borrowing skews the sample towards larger firms. Furthermore, because most of the leading banks are in the Northern part of the country, the sample has more firms headquartered in the North than in the South. Finally, since banks are most interested in firms that are credit-worthy, firms 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 appears to suggest that the CADS is not too far from being representative of the whole population. This is confirmed 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 geographical distribution of firms in the sample is not too far from that in the population, it is biased towards larger firms especially 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

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
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)
Aggregate shocks	0.026	0.025	0.058
	(0.007)	(0.007)	(0.004)
Specific shocks	0.056	0.056	0.068
	(0.002)	(0.002)	(0.002)
<u>Average adjustment by other firms in:</u>			
- the same distr. and sect.	0.308	0.287	
	(0.055)	(0.057)	
- other distr. but same sect.		-0.005	
		(0.031)	
- the same distr. but other sect.		-0.080	
		(0.100)	
Average adjustment by other non-distr. firms in the same sect.		0.171	0.061
		(0.124)	(0.083)
Number of observations	17,456	17,456	34,795
Number of firms	2,308	2,308	4,896
F test for fixed effects	1.84	1.84	1.57
(*p*-value)	(0.000)	(0.000)	(0.000)
p-value for the *F* test for adjustment by non-reference group firms = 0		0.578	

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 (3)) 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. 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
SUMMARY STATISTICS FOR THE INTERVIEWED FIRMS WITH AT LEAST ONE MATCH

	Santa Croce	Biella	Cossato	Total
Number of firms interviewed	48	28	21	97
Average number of firms reported	3.8	2.9	3.1	3.4
Maximum number of firms reported	8	6	8	8
Average number of matches	2.5	2	2	2.2
Maximum number of matches	6	4	4	6
Total n. of referenced firms	72	31	26	129
Number of references of the most referenced firm	6	3	3	
Number of firms that reference each other	0	0	4	4

The table shows summary statistics for the results of the phone survey run on the firms in our sample located in three selected districts in order to identify the firm-specific reference group. Statistics refer to the firms with at least one match.

TABLE V

SELF-REPORTED REFERENCE GROUPS AND EMPLOYMENT ADJUSTMENT

Explanatory variables	Using time-district and time-sector dummies	Using the common component of firm's sales
	(1)	(2)
Aggregate shocks	-	0.037 (0.021)
Specific shocks	0.035 (0.013)	0.034 (0.012)
Average adjustment by other firms in:		
- self-reported reference group	0.116 (0.064)	0.122 (0.061)
- the same district and sector		
Adjusted R^2	0.225	0.101
Number of observations	613	613
Number of firms	85	85

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. In column (1) aggregate shocks are controlled for (but coefficients are not reported) with a full set of time-district and time-sector dummies; in column (2) they are controlled for using 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; 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, defined as firms referenced in the phone interview (first two columns) and all firms in the same district and sector as in Table III (third column).

TABLE VI
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	1.84	1.83	1.83	1.57
(*p*-value)	(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 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 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 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	3.48	2.10	1.77
(*p*-value)	(0.000)	(0.000)	(0.000)	(0.000)

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 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.

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	0.023	0.019	0.037	0.037	0.029
	(0.009)	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)
Aggregate shocks, district firms in Adjustment years - b_2	0.072	0.072	0.055	0.071	0.072	0.048
	(0.013)	(0.018)	(0.012)	(0.036)	(0.033)	(0.033)
Aggregate shocks, non-district firms in Non adjustment years - b_3	0.059			0.061		
	(0.005)			(0.004)		
Aggregate shocks, non-district firms in Adjustment years - b_4	0.067			0.073		
	(0.006)			(0.013)		
Average adjustment by other firms in the same District and sector			0.291			0.314
			(0.056)			(0.056)
Specific shock	0.067	0.059	0.060	0.067	0.059	0.060
	(0.001	(0.002)	(0.002)	(0.002)	(0.002)	(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)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)

TESTS OF HYPOTHESES: *p*-values for the *F* test of the specified null hypotheses

Regression (1): H_0: $b_1 = b_2$: *p*-value = 0.001; H_0: $b_1 = b_3$: *p*-value =0.002;
H_0: $b_2 = b_4$: *p*-value = 0.730; H_0: $b_3 = b_4$: *p*-value = 0.357
Regression (2): H_0: $b_1 = b_2$: *p*-value = 0.006.
Regression (3): H_0: $b_1 = b_2$: *p*-value = 0.014.

Regression (4): H_0: $b_1 = b_2$: *p*-value = 0.339; H_0: $b_1 = b_3$: *p*-value = 0.003;
H_0: $b_2 = b_4$: *p*-value = 0.965; H_0: $b_3 = b_4$: *p*-value = 0.376.
Regression (5): H_0: $b_1 = b_2$: *p*-value = 0.296.
Regression (6): H_0: $b_1 = b_2$: *p*-value = 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 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.