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Credit within the Firm

LUIGI GUISO
EIEF
LUIGI PISTAFERRI
EIEF, Stanford University and NBER
and
FABIANO SCHIVARDI
EIEF and University of Cagliari

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We use variation in the degree of development of local credit markets and matched employer–employee data to assess the role of the firm as an internal credit market. We find that firms operating in less financially developed markets offer lower entry wages but faster wage growth than firms in more financially developed markets. This helps firms finance their operations by implicitly raising funds from workers. We control for local market fixed effects and only exploit time variation in the degree of local financial development induced by an exogenous liberalization, so that the effect we find is unlikely to reflect unobserved local factors that systematically affect wage–tenure profiles. We estimate that the amount of credit generated by implicit lending within the firm is economically important and can be as large as 30 percent of the bank lending. Consistent with credit market imperfections opening up trade opportunities within the firm, we find that the internal rate of return of implicit loans lies between the rate at which workers savings are remunerated in the market and the rate that firms pay on their loans from banks.

Key words: Implicit contracts, Financial frictions, Tenure profile, Wage setting

JEL Codes: J3, L2, G3.

1. INTRODUCTION

In an economy with financial frictions, the firm ceases to be merely a place where production occurs. The pooling of assets and human capital, besides allowing production of goods and services, naturally creates a "market" where implicit labor contracts can be designed to redistribute factor rewards across states or over time, partially overcoming the consequences of imperfect insurance and financial markets. In the implicit contract literature, differences in preferences for risk makes it optimal for risk neutral entrepreneurs to offer insurance to risk averse workers (Azariadis 1975; Baily 1974; Knight 1921). In this setting, firms effectively smooth worker's consumption across *states* when insurance markets fail to work due to moral hazard or limited commitment problems. In a similar spirit, the wage contract may reflect opportunities to redistribute factor rewards across *time* when access to the credit market is limited or too costly. By tilting the wage profile relative to its frictionless counterpart, the payment of wages over the course of an employer—employee relationship can be front- or back–loaded, thus making funds available to the party—the worker or the firm—who currently needs them most.

This article focuses on the role of the employment relationship as an implicit credit relation and tests how credit frictions in local financial markets are reflected in the wage contract that firms and workers agree upon. The key idea is that the relative ease to access credit should be reflected in the shape (slope and location) of the wage profile. If firms have easier access to the loan market than workers and/or are less in need of cash (e.g., because they are well endowed with collateral, can produce hard information, or can—more easily than workers—establish repeated relationships with their lenders), they can lend implicitly to their credit-constrained workers by offering a wage profile that, over the workers' tenure with the firm, is flatter than the profile the same workers would face in a frictionless environment. In other words, the worker achieves consumption smoothing by having the firm offering a smooth income profile rather than borrowing from financial intermediaries. This case is examined by Azariadis (1988), who studies a setting where, due to extreme adverse selection, workers are excluded from the loan market while firms have perfect access to it. Since loan repayment promises, as reflected in the wage–tenure profile, are not enforceable, what makes it possible for lending within the firm to take place is workers' investment in firm-specific human capital, which reduces workers' incentive to leave the firm before repayment.

Implicit lending opportunities need not be limited to the case of workers borrowing from the firm. After all, firms even more than workers are users of capital to finance their investment plans and firms may also have limited access to the financial market, as a large literature on firm borrowing constraints suggests [see e.g., the surveys in Hubbard (1998) and Stein and Center (2003). Michelacci and Quadrini (2009) study this case and show that a credit-constrained firm can at least partly make up for the shortage of capital by reshaping the workers' wage contract relative to its frictionless equivalent. In particular, the firm may pay workers less at the beginning of their tenure and more toward the end, resulting in a steeper wage profile relative to the case in which access to credit is unimpeded. In this setting, contracts can be made self-enforceable by firms' investment in worker-specific human capital, which limits firms' incentives to fire a worker that has lent implicitly to it. But alternative mechanisms, such as reputational concerns or institutional constraints, may in practice also serve the same scope.

To test the role of the firm as an internal credit market and establish the direction of implicit credit flows, we use two sources of data. First, we rely on matched Italian longitudinal firm–employee data. The data report the workers' wages and employment histories over a long span of time (1974–1997) allowing us to construct tenure and experience profiles. Second, we exploit systematic differences in financial development across local markets that exist at the start of our sample period (due to various "accidents of history") and, most importantly, exogenous changes in these differences induced by the financial market liberalization of the 1990s. Under the well-grounded assumption that firms, particularly small ones, and *a fortiori* workers can only borrow locally (Petersen and Rajan 2002), workers and firms' abilities to borrow from financial intermediaries are directly tied to the degree of *local* credit market development. Variations in the latter should then be reflected in the slope and location of the wage profile experienced by workers joining the firm in different time periods. We construct a measure of financial market backwardness as the "excess" spread between loan rates and deposit rates that a given firm faces in its local credit market relative to what an observationally equivalent firm borrowing from an observationally equivalent bank would face in the most developed local credit market. We then attach to each worker–firm relationship the degree of backwardness in their local credit market at the time of hiring, and use a two-step estimation strategy to identify how the slope and intercept of the wage–tenure profile respond as the degree of financial development varies over time and spatially across local markets. This two-step strategy allows us to address the endogeneity of tenure and labor market experience and the endogenous sorting into firms. We find that wage profiles are steeper and have a lower intercept when firms face a less developed local credit market, which is consistent with the hypothesis that workers lend implicitly to their firms.

To give a sense of the magnitudes involved, we calculate that the entry wage of a worker matched with a firm facing a local market that has a measure of financial backwardness 50 percent above the median is 18 percent lower than that of a (observationally equivalent) worker matched with a (observationally equivalent) firm facing the median local credit market. Moreover, her wage grows at a rate that is 0.33 percent faster than for each month of tenure. This implies that a typical worker will be lending to the firm over the first 55 months (about 4.5 years) of his tenure before starting to be "paid back".

These implicit wage contractual differences can generate substantial flows of funds from workers to firms. A representative firm located in the median developed credit market raises from workers as much funds as 11 percent of what it gets from banks. This share increases to 30 percent for a firm located in a market at the 75th percentile of our measure of financial market backwardness. Our estimates of the internal rates of return on these implicit loans ranged between 2 and 5 percent, depending on the degree of financial development. Interestingly, these rates always sit between the rate on deposits (a measure of the return to workers' savings) and the rate on bank loans (the cost of firm debt), confirming the mutual advantage for contracting by workers and firms in imperfect financial markets.

Since the response of wage contracts to financial market imperfections should depend on firm and worker characteristics, we rely on observable heterogeneity to further corroborate our findings. We find that firms with plausibly more problematic access to the loan market, as measured by the credit score they receive from credit bureaus, react to a deterioration in the conditions of the local credit market by offering even steeper wage profiles than firms with easier access to loans. Similarly, we find that workers who have presumably better access to the loan market and thus better alternative sources of consumption smoothing, as measured by their job position, face a steeper wage–tenure profile in response to financial market frictions than workers with more problematic access to the market. We also argue that the correlation we find between financial development and wage–tenure profiles cannot be explained by factors underlying traditional theories of wage growth by tenure, such as incentive effects (Lazear 1981) or human capital accumulation (Becker 1962).

To assess the robustness of our findings, we perform a large number of sensitivity checks. First, we account for the possibility that firms might differ in the workers' turnover policy and hence experience a different effect on the wage–tenure relationship. We also account for nonlinear tenure effects, differences by firm size, sample selection, local business cycle conditions, and study how local credit market imperfections affect job duration. Finally, we consider the role of firm heterogeneity in returns to tenure, using a strategy to estimate firm-specific returns to tenure in the spirit of Abowd *et al*. (2006). This exercise requires an alternative data set, which supplies further validation to our findings. Our results are remarkably robust to all these modifications.

Several papers have studied the occurrence of lending within the firm from a theoretical perspective. Besides Azariadis (1988) and Michelacci and Quadrini (2005, 2009), Bernhardt and Timmis (1990) were among the first to formalize the idea, already noticed in Azariadis (1975), that the employment relationship can help to "complete" financial markets when the workers cannot use human capital as collateral and are thus excluded from credit markets or when firms face borrowing restrictions due to financial frictions. More recently, Burdett and Coles (2003) study a labor market where firms post wage–tenure contracts and show that in equilibrium wages increase with tenure, and the structure of the contracts reflects both the workers' preferences as well as the parameters of market environment that firms and workers face, including financial frictions. Our article is, as far as we know, the first to systematically undertake the empirical task of showing how credit market frictions shape the wage contracts. The closest predecessor to our article is Brandt and Hosios (1996). In a fascinating empirical contribution, they use data on wage/employers contract for some villages in 1936 rural China where presumably financial markets were absent. They show that wage contracts do indeed generate lending, whose direction—from the employer to the worker or vice versa—depends on preference parameters and the initial endowments of the two parties. However, in Brandt and Hosios (1996) credit frictions are given and taken as a realistic feature of the environment. In contrast, our main contribution is to establish how wage contracts respond to observed and measured differences in the financial markets that firms and workers face in a modern economy. Michelacci and Quadrini (2009) exploit the joint implications of their model on the wage process and firm dynamics to test for the existence of credit flows from workers to firms. In their model, credit-constrained firms pay initially lower wages and grow faster than unconstrained firms as they borrow from workers to reach the optimal size. Moreover, wages also grow faster with firm growth, as workers are "paid back" as the firm approaches the optimal size and credit constraints are relaxed. Using Finnish-matched data, they find that wage growth is positively related to firm growth and starting wages are negatively related to future firm growth, consistent with the model's predictions. Differently from these estimates, which use firms' dynamics to proxy for financial constraints, we directly relate the wage–tenure profile to measured frictions in local financial markets. More broadly, our article contributes to a literature that studies the interrelations between labor and financial markets [i.e.,Benmelech, Bergman, and Enriquez (2010); Wasmer and Weil (2004)]. Our article is also related to the debate on the returns to tenure (Altonji and Shakotko 1987; Altonji and Williams 2005; Topel 1991) and on the factors that affect them [see Dustmann and Meghir (2005) and references therein]. In particular, Abowd *et al*. (2006) document both heterogeneity in returns to tenure and a tendency for returns to be higher in low starting wage firms. Recognizing that the wage–tenure profile also reflects implicit credit flows contributes to explaining both findings.

The reminder of the article is organized as follows. Section 2 illustrates our empirical strategy and reviews some of the relevant literature. Section 3 describes the data and discusses the sources of variation in local financial market development. Section 4 illustrates the identification strategy behind our two-step estimator. Section 5 shows the main estimation results, exploit heterogeneity in firms and workers to further corroborate our findings, and discusses how our results compare to the classical theories of wage growth. We discuss a series of robustness checks in Section 6 and account for firm -level heterogenity in returns to tenure in Section 7. Section 8 estimates the size of implicit lending within the firm and Section 9 concludes.

2. "CREDIT REGIMES" AND WAGE–TENURE PROFILES

To illustrate how inefficiencies in local credit markets can affect (implicit) wage contracts, consider the following log wage equation:

$$\ln w_{ij(t_0)pt} = \rho + \beta T_{ij(t_0)pt} + \delta L_{pt_0} + \gamma T_{ij(t_0)pt} \times L_{pt_0} + \varepsilon_{ij(t_0)pt} \tag{1}$$

The actual wage equation we estimate below controls for a variety of other characteristics; here we use equation (1) for illustrative purposes. The subscripts $i, j(t_0), p$, and t index the worker i, the firm j that the worker joined in year t_0, the local credit market p, and the reference year t, respectively. We argue in Section 3.1 that the local credit market is defined by the boundaries of the province, an administrative unit roughly comparable to a US county. In equation (1), $T_{ij(t_0)pt}$ is tenure [hence $T_{ij(t_0)pt} = (t - t_0)$] and L_{pt_0} is a continuous measure of the degree of financial market imperfection in province p. Without loss of generality, we normalize $L_{pt_0} = 0$ in the most developed credit market. We assume that the relevant credit market imperfections for the wage contract set with worker i are those that prevail at the time of hiring (t_0); hence, we do not consider the possibility of renegotiation.[1]

Figure 1 illustrates the possible cases of interest. In the baseline case ($L_{pt_0} = 0$), the initial wage is ρ and it grows at rate β per month of tenure with the firm. The signs of δ and γ determine the

1. See Section 6 for a test of this assumption.

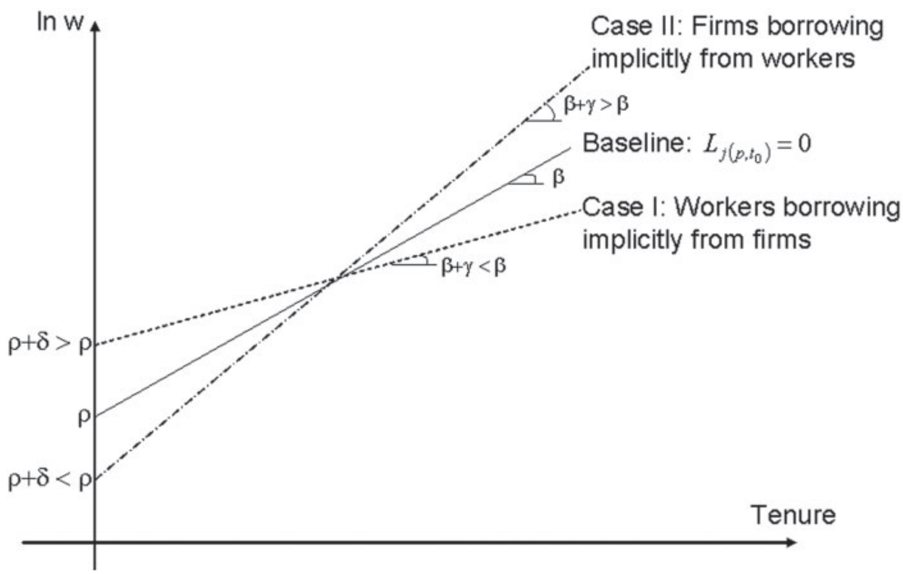

FIGURE 1

Credit flows and tenure profiles

type of "credit regime" in which workers and firms operate. Consider first case I, in which $\gamma < 0$ and $\delta > 0$, implying that the wage profile in more backward credit markets is flatter than in more developed markets. Here, workers are implicitly borrowing from the firm. Their wage payments are front loaded. This tilting of the wage profile may be interpreted as a response to credit market imperfections when workers and firms can establish long-term relationships through, for example, firm-specific human capital investments. In a perfect credit market, individuals with the growing wage–productivity profile depicted in the baseline case would borrow from banks at the start of their relationship with the firm to smooth consumption intertemporally. However, acquiring reliable information about aspects of the exchange relation between employer and employees may be costly for banks, who respond by limiting credit (in the extreme, denying access to it altogether). Azariadis (1988) and Bernhardt and Timmis (1990) were among the first to suggest that in this case the firm can act as a "lender of last resort" for its workers, *implicitly* lending to them by offering a wage profile that is flatter than in the frictionless case. In other words, in underdeveloped financial markets consumption smoothing is achieved through wage smoothing (or implicit borrowing), rather than through (or in addition to) formal borrowing.

There is an opposite view about the shape of the wage–tenure profile, articulated in Michelacci and Quadrini (2005, 2009). The intertemporal exchange may involve a liquidity-constrained firm implicitly borrowing from its workers. This can be achieved by back-loading wages, i.e., paying lower wages at the beginning of the worker–firm relationship (relative to the frictionless case) in exchange for higher wages at a later stage. This corresponds to case II in Figure 1. Here $\gamma > 0$ and $\delta < 0$ and the wage profile in less developed credit markets is steeper than in more developed credit markets.[2]

2. One may wonder why workers are willing to lend to firms in a less developed credit market. One answer is that workers have access to a cheaper monitoring technology than a bank, because for them information collection about the firm is a by-product of going to work. This makes workers' signals more precise, reducing conditional uncertainty and making them more willing to lend.

Which of the two "credit regimes" shapes the wage–tenure profile? The answer depends on the signs of δ and γ. We estimate these two parameters below using variation over time in access to credit in the location where the employment relationship takes place. The estimation procedure allows us to distinguish between the two above hypotheses.[3]

What makes the contracts discussed above enforceable? Given that they involve implicit promises, they are not legally enforceable. However, as remarked by, among others, Azariadis (1988), Lazear (1981), and Michelacci and Quadrini (2009), *specific* human capital investments can be sufficient to make these contracts self-enforceable. In case I, workers have little incentive to quit before repayment if they have made firm-specific human capital investments. In case II, firms have little incentive to fire workers before their loans are fully repaid if they have made worker-specific investments. If specific human capital were not enough to make these contracts self-enforcing, e.g., because human capital is (mostly) specific to the industry rather than to the firm [as argued by Neal (1995)], or specific to an occupation rather than to a worker, enforcement may be provided by other mechanisms. For instance, reputational concerns may facilitate implicit contract enforcement if the borrower's behavior is public information. Firing and hiring costs on the firms' side and mobility costs on the workers' side may also contribute to reduce the incentive to terminate the employment relationship to avoid repayment.

3. DATA

To identify the effect of credit market imperfections on wage contracts, we need local credit markets that differ in efficiency and longitudinal data on workers' histories with the firms they have worked for. Italy offers both. First, due to a number of "accidents of history" dating back to at least the 1930s, the development of Italian credit markets has differed markedly across localities as small as provinces (the equivalent of a US county). As a consequence of these initial disparities, the process of credit market liberalization that took place over the 1990s (and which was prompted by an external shock, the EU First Banking Directive mandating free entry, and introducing other measures aimed at increasing competitiveness) differentially affected local credit markets. Hence, access to external finance for workers and firms differ greatly depending on their location, and this differentially affects their incentives to make up for these inefficiencies in the wage contracts. Second, we use administrative data from the Italian Social Security Administration to obtain information on workers' earnings histories at a given firm. In what follows, we illustrate the procedure to construct our measure of local financial market development and the main features of the workers' data, referring the interested reader to the data appendix for further details.

3.1. *Measuring Financial Development*

To implement our test, we require that agents (firms and their workers), though located within the borders of the same country, have differential access to the credit markets. Variation of this sort may arise if: (1) credit markets are geographically segmented so that a worker or a firm located in a certain local market is bound to borrow in that market; and (2) local credit markets differ in their degree of development.

There is ample evidence that firms, particularly small businesses like the ones in our sample (and thus *a fortiori* single individuals) are tied to their local credit markets. For instance, Petersen and Rajan (2002) show that lending to small businesses is a highly localized activity

3. Of course, there are two other cases that may emerge empirically: $\gamma > 0$ and $\delta > 0$, and $\gamma < 0$ and $\delta < 0$. These cases would be inconsistent with our interpretation that differences in the wage–tenure profile across local financial markets reflect implicit credit flows between firms and workers, thus providing a direct falsification test for our story.

as proximity between borrowers and lenders facilitates information acquisition.[4] Segmentation of local credit markets is thus very likely to occur. Our geographical unit is the province, an administrative unit roughly comparable to a US county. Provinces are a proper measure of local markets in banking for different reasons. First, this was the definition of a local market used by the Bank of Italy to decide whether to authorize the opening of new branches when entry was regulated. Second, according to the Italian Antitrust authority the "relevant market" in banking for antitrust purposes is the province. At the time of our data, there were 95 provinces.

Due to a number of historical legacies surveyed in detail by Guiso *et al.* (2004, 2006), Italian local credit markets differed markedly in their degree of development at the start of the sample period we focus on in our analysis (1990). Such differences can be traced back to the different political traditions before the country was unified at the end of the nineteenth century. Moreover, they were perpetuated by a 1936 banking law that, in response to the 1929 crisis, heavily regulated the sector, with strong limits to entry and expansion of banks. This regulatory system was maintained almost unchanged until the late 1980s, preserving and actually amplifying the differences in financial development across local markets that existed in the early 1930s. Hence, when at the beginning of the 1990s, the process of financial liberalization started (prompted by a plausibly exogenous external shock, the reception in the Italian legislation of the EU First Banking Directive), it displayed its effects on a set of heterogeneously developed local credit markets. As a consequence of these different initial conditions, financial liberalization was relatively more beneficial to local markets that were lagging behind as of 1990. We will exploit these differential geographical effects of financial liberalization to identify the effect of credit market imperfections on wage contracts.

A good measure of financial development would be the ease with which individuals who need external funds can obtain them and/or the premium (adjusted for risk) they have to pay for these funds; in fact, in a perfect credit market this premium would be zero. The key idea is to exploit the geographic variation in access to the credit market or in its cost to estimate the ease with which otherwise equal firms or workers can obtain lending in two different local markets. Here, we follow Guiso *et al*. (2006) and use the variation across firms in the cost at which they can borrow to obtain a measure of the efficiency of the local credit market. A similar measure could be constructed using information on the interest rate charged on personal loans, but unfortunately these data are not collected.

To construct a measure of local credit market development, we use firm data from the Company Accounts Data Service (*Centrale dei Bilanci*, or CB for brevity) matched with Credit Register (CR) data for the same firms. The CB data span from 1982 onward and give detailed information on a large number of balance sheet items together with a full description of firm characteristics (location, year of foundation, sector, ownership structure), plus other variables of economic interest usually not included in balance sheets, such as flow of funds. Company accounts are collected for approximately 30,000 firms per year; as we explain in detail in the appendix, the data quality are high and the sample covers a large fraction of overall production. For our purposes, the most important feature of the matched CB/CR data set is that it provides for each firm and for all the years from 1990 to 1997 two types of relevant information. First, the interest rate on credit lines charged by each bank that lends to the firm. Second, a credit score measure used by banks

4. Bofondi and Gobbi (2006) show direct evidence of the informational disadvantage of distant lenders in Italy. They find that banks entering new markets suffer a higher incidence of nonperforming loans. This increase, however, is more limited if they lend through a newly opened local branch than if they lend at a distance. Degryse and Ongena (2005) find that small firms' loan conditions depend on distance. Lerner (1995) documents the importance of distance in the venture capital market. Anecdotical evidence also suggests that the bankers' rule of thumb is to avoid lending to a client located more than three miles from the branch.

TABLE 1
Descriptive statistics

Variable	Mean		(St.dev.)
Panel A: Firms characteristics			
Employment	71.6		(608)
Assets	13,402		(386,991)
Turnover	1,4026		(95,258)
Interest rate on loans	15.0		(3.9)
Bank debt	886		(14,519)
Score	5.2		(1.8)
North	0.69		(0.46)
South	0.12		(0.33)
Manufacturing	0.54		(0.50)
Construction	0.08		(0.27)
Panel B: Workers characteristics			
Monthly earnings (euro)	767		(544)
Age	37		(11)
Male	0.65		(0.48)
Productions	0.60		(0.49)
Clericals	0.39		(0.49)
South	0.22		(0.41)
North	0.59		(0.49)
Experience (in months)	117		(81)
Tenure (in months)	66		(71)
Mover	0.13		(0.34)
Displaced due to firm bankruptcy	0.02		(0.15)
Firm size	2,506		(12,199)
Median firm size		32	

Note: Panel A shows summary statistics for the firms used to compute the credit market backwardness variable (approximately 250,000 firm-year observations). Monetary values are expressed in 1995 thousand euros. Panel B Shows summary statistics for the workers in the whole sample used in estimation (454,666 records). Standard deviations in parenthesis. Data on firm size are available only for a subset of those (411,293). Monthly earnings are gross earnings expressed in 1995 euros.

when screening firms and allocating credit. As we will discuss later, this is a particularly attractive measure of firm-level creditworthiness, which will prove useful when we look at heterogeneity in firms' motives for relying on internal lending. Table 1, Panel A reports summary statistics for the sample of firms used to construct the measure of financial development (see below).

We use the interest rate on credit lines charged by each bank that lends to the firm to compute the spread with respect to the rate on deposits in the province where the firm is located. This spread is a measure of the mark-up on loans. Our assumption is that banking markets characterized by larger mark-ups are, *ceteris paribus*, less financially developed.[5] More formally, let s_{jbpt} denote the interest rate spread (relative to the province's average deposit rate) paid by firm j to bank

5. One criticism to this measure is that it is based on the cost of obtaining a loan rather than the amount one can obtain; another is that it relies on firms' ease in accessing local credit markets and ignores workers' access. Guiso *et al.* (2004) use a similar methodology but rely instead on data from the Survey of Households Income and Wealth (SHIW), a survey run by the Bank of Italy biannually on a representative sample of 8,000 Italian households. The survey contains information on whether a loan application was accepted or turned down at the household level. They use this information to construct a measure of financial development across Italian regions (which are larger geographical units than the province). This measure of financial development is highly correlated with the one we use; as shown by Guiso *et al.* (2006), this indicator can explain ∼25 percent of the variation in the interest rate spread across provinces in 1990. Thus, in local markets where, *ceteris paribus*, workers are more likely to be turned down when applying for loans, the pricing of a granted loan to firms deviates more from the competitive benchmark. Furthermore, the variation over time in the fraction of households that were turned down is consistent with that in the spread indicator. Casolaro, Gambacorta, and Guiso (2006) document that while the fraction of applicants that were turned down was ∼ 50 percent before financial liberalization,

b in market p in year t, F_{jpt} denote a vector of firm controls and B_{bpt} denote a vector of bank controls.[6] We run the regression

$$s_{jbpt} = \beta_t F_{jpt} + \gamma_t B_{bpt} + f_{pt} + \eta_{jbpt} \tag{2}$$

where f_{pt} is a vector of province-year fixed effects that captures the time variation at the local level in financial development, following the process of financial liberalization over the 1990s. We estimate equation (2) separately for each year between 1990 and 1997, and retrieve the estimated fixed effect $\widehat{f_{pt}}$ for each one of the 95 provinces in which the country is divided and for each year between 1990 and 1997. Finally, we define our indicator of financial backwardness by recentering the measure as: $L_{pt} = \widehat{f_{pt}} - \min(\widehat{f_{pt}})$. Hence, the value of L_{pt} is normalized to be zero in the most competitive local credit market (the baseline), and for the other local credit markets it can be interpreted as the excess interest rate spread relative to the baseline.

Table 2 shows for each province the average value of our measure of financial market backwardness, the value in 1990—the first year in the sample—and the change in the indicator between 1990 and 1997 (our last sample year). There is ample variation across areas with a clear geographical pattern that shows more financially backward provinces, both at the beginning of sample and on average, in the Southern regions.[7] This is more clearly visible in Figure 2, which reports the map of our 1990 measure by province. While a North–South divide is a clear feature of the data, there is considerable variation in financial development within the Center–North and the South.

At the beginning of the sample period, before the liberalization process started, the least financially developed province was Cosenza (in the Southern region of Calabria), whereas the most developed was Ravenna (in Emilia, one of the Northern regions); the interest rate spread between these two local markets was close to 400 basis points with a standard deviation across all markets of 81 basis points, implying highly segmented local credit markets and substantial dispersion in financial development. Differences across provinces in variation over time are also very pronounced (third column) with a standard deviation of 71 basis points. This is reassuring, since we will primarily use the time variation in the degree of financial backwardness to identify its effect on wage contracts. Interestingly, provinces that were more backward just before the liberalization started are the ones where the improvement in financial development has been

it fell to 25 percent in 1998 and to 10 percent in 2002. Unfortunately, there are not enough data in the SHIW to obtain reliable estimates of the variation over time at the *province* level in the fraction of people that were turned down and use this as an alternative indicator of the change in local financial development.

6. Firm controls make sure that province fixed effects do not reflect borrowers' differences in riskiness, while bank controls make sure that they do not reflect differences across banks in the cost of making loans. Firm controls include the firm return on sales, its leverage (as a proxy for financial fragility), its size (measured by log assets) to capture the fact that smaller firms are more likely to fail, and the firm credit score. The latter is a particularly good control for firm riskiness as it is used by the banks that belong to the CB consortium to decide whether to grant a loan and to price it. As bank controls we use: the size of the lending bank (measured by log assets), its return on assets, the ratio of nonperforming loans on total loans outstanding, and dummies for state or local government bank ownership. Since the same firm often borrows from multiple banks [see (Detragiache *et al*. 2000)], as an alternative to these bank controls, we insert a full set of bank dummies obtaining very similar results.

7. Sicily is an exception as it shows lower values of the index of financial backwardness than other provinces in the South. This is most likely a consequence of a different regime of regulations that prevailed in Sicily since the post war period. In Sicily, the authorization to open new banks and new bank branches was granted by the regional government rather than by the Bank of Italy. As a consequence, the number of local bank branches over a 20 year period went up by 586 percent compared to a national average of 83 percent and the number of banks went up by 21 percent while it was shrinking in the rest of the country. This is hard to explain with economic convergence but is consistent with the less stringent regulatory regime. At any rate, excluding Sicily from our sample does not change the results.

TABLE 2
Financial market backwardness

Province	$\overline{L}_p$	$L_{p,1990}$	Δ	Province	$\overline{L}_p$	$L_{p,1990}$	Δ	Province	$\overline{L}_p$	$L_{p,1990}$	Δ
Agrigento	1.21	1.14	0.38	Frosinone	1.87	1.63	0.24	Pistoia	1.78	1.63	0.10
Alessandria	1.08	1.36	−0.79	Genova	1.93	1.78	−0.14	Pordenone	1.97	1.64	−0.32
Ancona	1.65	0.59	1.42	Gorizia	2.18	2.32	−0.43	Potenza	2.21	1.98	0.45
Aosta	1.31	1.38	−0.16	Grosseto	1.22	1.47	0.45	Ragusa	1.69	1.62	0.29
Arezzo	2.45	2.14	0.07	Imperia	0.66	1.20	−0.54	Ravenna	0.42	0.00	0.36
Ascoli	2.69	2.55	0.04	Isernia	2.99	2.73	−0.38	Reggio C.	3.02	2.46	0.70
Asti	0.63	0.84	−0.55	L'Aquila	2.26	2.03	0.18	Reggio E.	1.23	0.79	−0.03
Avellino	3.04	3.31	−0.14	Laspezia	1.64	1.60	0.14	Rieti	2.38	2.26	−0.09
Bari	1.75	1.28	0.58	Latina	2.43	2.51	−0.89	Roma	1.78	2.06	−0.48
Belluno	1.72	1.92	−0.90	Lecce	2.40	2.10	1.32	Rovigo	2.02	2.40	−1.26
Benevento	2.89	3.74	−1.70	Livorno	0.79	0.73	0.41	Salerno	2.59	2.65	−0.18
Bergamo	1.25	1.65	−0.72	Lucca	1.74	1.79	−0.39	Sassari	1.66	1.63	0.30
Bologna	1.07	0.76	0.11	Macerata	1.64	0.60	1.79	Savona	0.61	0.74	−0.64
Bolzano	1.44	1.32	−0.63	Mantova	0.63	0.87	−0.43	Siena	1.53	1.50	−0.65
Brescia	2.42	2.51	−0.68	Massa	1.34	1.27	−0.15	Siracusa	1.65	0.99	1.30
Brindisi	2.42	2.24	0.49	Matera	1.79	2.14	−0.33	Sondrio	0.59	1.61	−1.28
Cagliari	1.75	1.58	0.08	Messina	1.32	1.41	0.07	Taranto	2.73	3.42	−1.11
Caltanisetta	1.39	0.96	1.41	Milano	2.06	1.84	0.21	Teramo	2.51	2.52	−0.52
Campobasso	1.42	1.49	−0.90	Modena	1.06	0.33	0.62	Terni	2.28	1.88	0.52
Caserta	3.33	3.57	−0.75	Napoli	3.31	3.30	−0.48	Torino	1.62	1.82	−0.67
Catania	1.98	1.79	0.73	Novara	1.69	1.59	0.17	Trapani	1.22	0.54	1.10
Catanzaro	2.18	3.05	−0.86	Nuoro	1.98	2.16	−0.13	Trento	1.65	1.06	0.58
Chieti	2.81	2.36	0.20	Oristano	1.71	0.61	0.83	Treviso	2.15	1.96	−0.46
Como	1.92	1.90	−0.26	Padova	2.20	2.17	−0.69	Trieste	1.54	1.59	−0.26
Cosenza	2.79	3.92	−1.74	Palermo	1.55	1.45	0.40	Udine	2.08	1.36	0.39
Cremona	1.64	1.41	0.21	Parma	0.41	0.20	−0.10	Varese	1.91	1.78	−0.05
Cuneo	0.40	0.85	−0.85	Pavia	1.43	1.46	−0.10	Venezia	1.95	1.92	−0.20
Enna	1.56	0.66	1.71	Perugia	2.23	1.76	0.60	Vercelli	2.23	2.37	−0.49
Ferrara	1.62	1.28	−0.05	Pesaro	1.48	0.04	1.86	Verona	1.98	2.05	−0.61
Firenze	1.74	1.40	0.73	Pescara	2.18	1.95	0.22	Vicenza	1.95	1.72	−0.18
Foggia	1.30	0.98	−0.36	Piacenza	0.81	0.54	0.10	Viterbo	0.57	0.21	0.39
Forlí	0.66	0.82	−0.29	Pisa	1.81	1.63	−0.65	Average	1.76	1.66	−0.35

Note: The table reports the time average of indicator of credit market imperfection in each Italian province ($\overline{L}_p$), its value in 1990, the first year of our sample just prior to the start of the liberalization ($L_{p,1990}$), and the change in the indicator between 1990 and 1997, the last sample year (Δ).

more marked. This is consistent with our contention that less developed markets benefit more from financial liberalization, providing the basis for our identification strategy. Convergence is apparent from Figure 3, which plots the change in the backwardness indicator between 1990 and 1997 against its value in 1990. We also document formally the convergence induced by the liberalization process in growth-type regressions of the change in financial backwardness between 1990 and 1997 on the initial value (shown in the top right corner of Figure 3). The estimated negative coefficient on the initial level of L_p implies that a province with a level of financial backwardness that was one standard deviation above the mean in 1990 has experienced a decline in the interest rate spread of 40 basis points.[8]

8. To investigate further the idea that heterogeneity across provinces in the effects of financial liberalization on financial development is due to the differences in the level of financial development that prevailed just before the liberalization started and that were largely the unintended consequence of the 1936 banking regulation, we also run IV regressions instrumenting the 1990 level of financial backwardness with measures of the structure of the banking industry in the region in 1936 as constructed by Guiso *et al.* (2004). The IV estimates are similar to the OLS estimates showing convergence after liberalization.

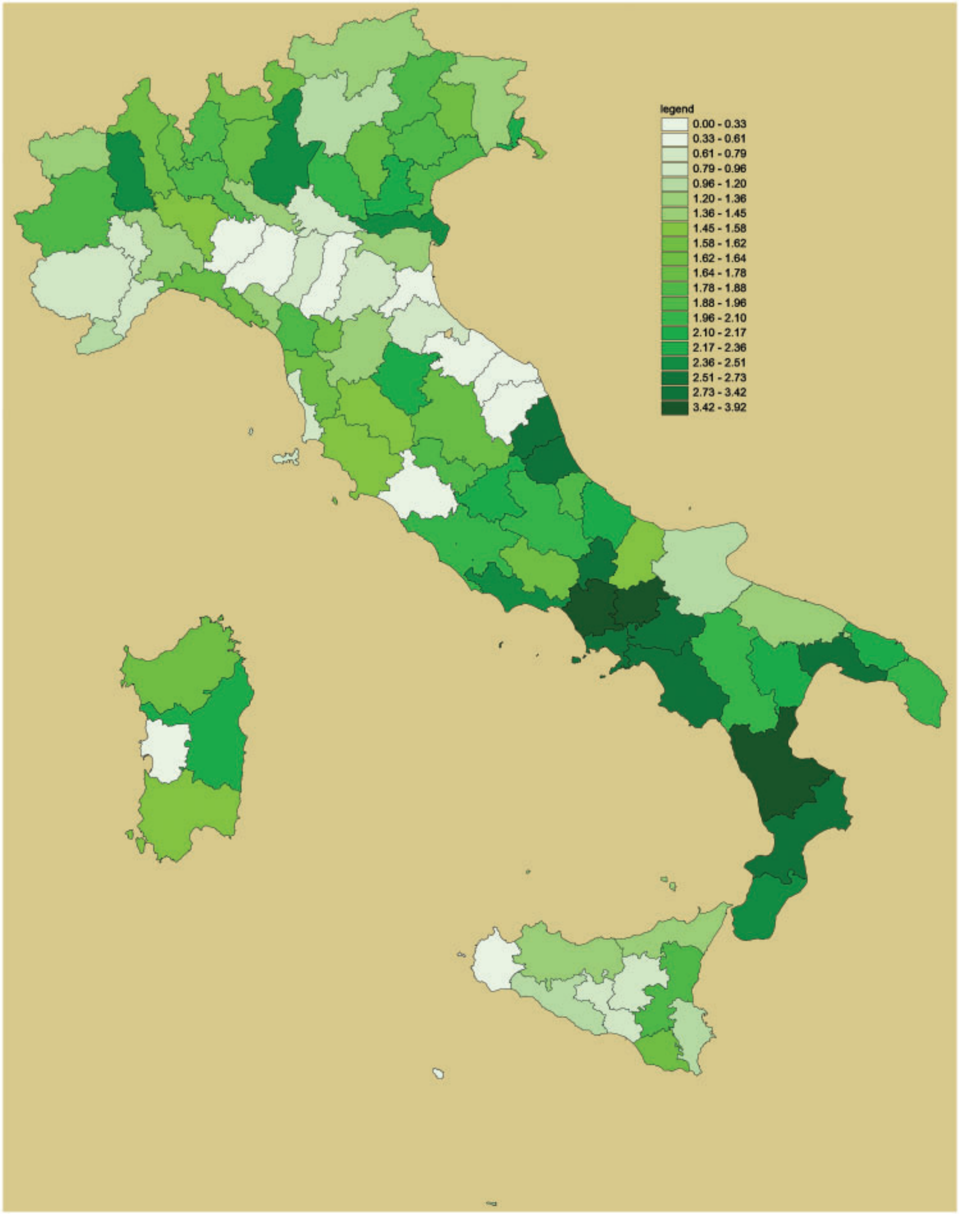

FIGURE 2
Credit market backwardness, Italian provinces, 1990

3.2. *Workers Data*

We obtain worker-level data from the Italian Social Security registry (INPS) that provides information on total compensation and its components for a sample of workers. The INPS data are provided for the entire population of workers registered with the social security system whose birthday falls on either March 1*st* or October 1*st*. Data are available on a continuous basis from 1974 to 2002. The data cover private sector employees (but not the self-employed or public

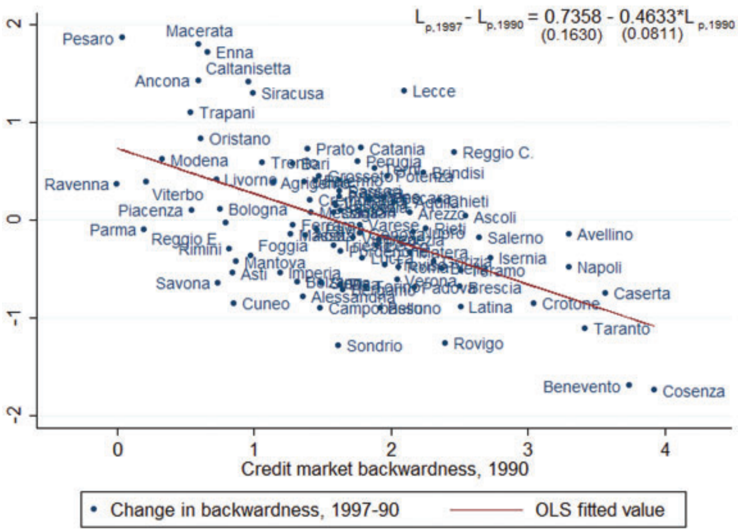

FIGURE 3

The convergence process

employees) and derive from employer forms roughly comparable to those collected by the Social Security Administration in the US.[9] Misreporting is prosecuted. Besides providing information on workers earnings, the INPS data also contain some demographics. However, as is typical of administrative data, demographics information is scant and limited to age, gender, place of birth, and job category (blue collar, white collar, or manager). INPS also supplies some basic information (such as employment and location) for all Italian firms. This information can be matched to the workers' data and we will use it for some of the exercises below.

For our estimates, we restrict the sample to workers aged between 18 and 60 years observed over the years 1990-1997. We do not use data after 1997 because there are consistency issues with the worker identifier as released by INPS.[10] We do not use data before 1990 because (as we explained above) we do not have information on local credit market imperfections before that date. However, we use the INPS data before 1990 to construct measures of actual labor market experience and tenure with the firm. Each record in the original data set is a social security contribution record for a given worker/firm/year observation. For each record, there is information about which months the worker was employed at that firm. Apart from self-employment or public employment spells, our measures of labor market experience and tenure should thus be free from measurement error (at least for those observed after 1974). We construct gross monthly earnings (our measure of wage in the regressions below) as total yearly earnings with a firm divided by the number of months the worker has been employed by the firm in that year. Table 1, Panel B reports summary statistics for the sample of workers used in estimation. Further details are in Appendix 9.

In equation (1), variables are indexed by the province p, denoting the relevant local credit market for the implicit wage contract between the firm and its workers. We have two types of

9. While the US administrative data are usually provided on a grouped basis, INPS has truly individual records. Moreover, US earnings records are censored at Social Security cap, while the Italian dataset is not subject to top-coding.

10. Worker identifiers for the post-1997 data cannot be matched with those in previous years, due to a change in the coding of the individual identifier released by INPS.

information that can help us to identify p in the data. First, the data include the province where the worker is employed. Second, we can reconstruct (from the firm numerical identifier) the province where the firm maintains its legal registered office ("sede legale"), which for multiplant firms is also normally their headquarters. In the vast majority of cases ($\sim$ 90 percent of our final sample), the two definitions coincide. For these observations, the firm and the worker face the same local credit market (under the assumptions that workers live in the same province where they are employed and firms only bank locally). For the remaining cases, we assume that the relevant p is the province where employment takes place. We explore the sensitivity of our results to this choice in Section 6.

In Section 5.2, we test whether heterogeneity in firms' motives for relying on internal lending affects the shape of the wage profile. For this purpose, we need to match the INPS workers data set with the CB data set containing information on the firm credit score. The merge is made possible by the fact that the INPS data provide us with the employer's tax code. Since the CB data set only covers a subsample of firms, in the matching process we lose observations. The resulting data set has 106,277 records with information for 15,179 firms and 24,639 workers (note, however, that some firms have missing records on the credit score variable in some years).[11]

4. IDENTIFICATION

We now discuss identification of the shift in the slope and intercept of the wage–tenure profile induced by local credit market imperfections. We expand equation (1) and rewrite it as:

$$\begin{aligned}\ln w_{ij(t_0)pt} = {} & \rho + X'_{ij(t_0)pt}\alpha + Z'_i\phi + \mu E_{it} + \beta T_{ij(t_0)pt} + \delta L_{pt_0} \\ & + \gamma T_{ij(t_0)pt} \times L_{pt_0} + \eta T_{ij(t_0)pt} \times h_p + \vartheta h_p + \varepsilon_{ij(t_0)t}\end{aligned} \tag{3}$$

where $X'_{ij(t_0)pt}$ and Z'_i are two vectors of time varying and time-invariant worker characteristics, respectively, E_{it} a measure of the worker overall labor market experience and h_p a vector of provincial dummies. This specification allows for province effects both in the level of wage rates and in the returns to tenure.[12] In particular, this formulation implies that any effects that differences in the average level of financial backwardness have on the intercept and the slope of the wage contract are captured by the province fixed effect. The vector $X'_{ij(t_0)pt}$ also includes year effects. Hence, the identification of the effect of L_{pt_0} in equation (3) comes from: (1) differential *evolution* in credit market imperfections across provinces induced by the process of financial market liberalization discussed in Section 3.1 and (2) the fact that new workers are hired at different points (and hence have different t_0) during the period in which this process unfolds (as reported in Panel B of Table 1, $\sim$13 percent of the observations in our sample are new hires and we verified that they are distributed across all provinces).

Assume that the structure of the error term in equation (3) is as follows:

$$\varepsilon_{ij(t_0)t} = a_i + b_{ij(t_0)} + c_{it} \tag{4}$$

Here, a_i is an individual fixed effect ("ability"), $b_{ij(t_0)}$ is a firm–worker match effect, and c_{it} is an i.i.d. shock. We could allow for time-varying firm-specific shocks [such as in Guiso, Pistaferri, and Schivardi (2005)] by appropriately re-defining the term c_{it}. The experience and tenure variables are likely correlated with the error term. For example, more able people

11. See Guiso *et al*. (2005) for more details on the matched employer–employee data set.

12. In Section 7, we discuss in detail how to account for firm-specific heterogeneity in returns to tenure.

(i.e., with high realizations of a_i) may have stronger labor market attachment and hence longer overall labor market experience. Moreover, more experienced workers may be in better matches because they have had the opportunity to search longer while on the job. As for tenure, one might expect firms to fire less able workers more frequently than high-ability workers. In addition, firms are more likely to fire (or workers more likely to quit) when the value of the match is low. This discussion means that OLS applied to equation (3) will give biased and inconsistent estimates.

Our identification strategy is a variant of those proposed by Topel (1991) and Dustmann and Meghir (2005). Let M_{it} be an indicator variable denoting whether the worker moves between period $t-1$ and period t. Consider the first differenced version of equation (1) for individuals who stay with the same employer between $t-1$ and t ($M_{it}=0$). For these workers:

$$\Delta \ln w_{ij(t_0)pt} = (\mu+\beta) + \Delta X'_{ij(t_0)pt}\alpha + \gamma L_{pt_0} + \eta h_p + \Delta c_{it} \tag{5}$$

The advantage of this specification is that the sources of endogeneity (the correlation between tenure/experience and the individual/match fixed effects) have been removed. If Δc_{it} is independent of M_{it} (conditional on the observables), then an OLS regression is all that is needed to consistently estimate the parameters of equation (5), in particular γ and $(\mu+\beta)$. Note that μ and β cannot be separately identified. If Δc_{it} depends on M_{it} (even after conditioning on the observables), then this creates a standard sample selection issue that can be addressed making distributional assumptions about the unobservable Δc_{it} and finding exclusion restrictions for identification. We use three exclusion restrictions. The first is whether the *current* job is one found following exogenous loss of *previous* job due to firm closure. Those who are displaced due to firm closure must start searching for a new job sampling from the unconditional distribution of match values. Those who moved voluntarily to the current firm did it because they improved their match value, i.e., they sampled from the conditional distribution. Hence, the probability of being a mover out of the *current* job must be higher for the displaced worker than for the average worker (matches found after involuntary displacement are on average of lower quality than those found by voluntary moves). Since firm closure is an exogenous event affecting the entire work force of the firm, it is also likely to be independent of shocks affecting individual wages at some point in the future at *another* firm, i.e., of Δc_{it}, suggesting it is a valid exclusion restriction. The second exclusion restriction uses the time to earliest retirement date, taking into account not only the age-based retirement, but also the retirement based on the number of years one has contributed to the social security system.[13] In fact, mobility choices are taken trading off current mobility costs against future wage benefits. The longer the horizon, the higher the effect in net present value terms of a given wage increase following a job change and hence more likely is a move. Under the assumption that equation (5) is correctly specified, it is reasonable that residual horizon is orthogonal to Δc_{it}. However, if returns to tenure vary with tenure, as for instance suggested by Dustmann and Meghir (2005), this assumption might fail. We come back to this issue in Section 6 where we discuss robustness and show that our results are robust to using only the displacement indicator as an exclusion restriction. Finally, given that mobility costs can differ substantially across workers because of differences in the thickness of their local labor market, to capture heterogeneity in mobility costs and thus in the effect of residual horizon on mobility decision, we let the latter vary across provinces by including an interaction between the horizon length and province dummies (as proxies for labor market thickness).

13. For a worker of age A we define the residual work horizon (in months) as $\min\{360\text{-}E,(R-A)\times 12\}$, where E is months of labor market experience, and R is the statutory retirement age (60 for males and 55 for females). This definition accounts for the fact that over the period spanned by our data workers could retire either because of seniority (30 years or 360 months of paid work) or because of old age (60 for men, 55 for women).

As explained above, identification of γ and δ is achieved by comparing the slope and intercept of the wage contract of a worker in province p who starts tenure at time t_0 (facing financial constraints L_{pt_0}) with that of an otherwise similar worker in the same province who starts tenure at time t_1 (facing financial constraints $L_{pt_1} \neq L_{pt_0}$).[14] Note that in order to consistently estimate γ (even controlling for selection into staying with the same firm), we need to assume that $E\left(\Delta c_{it}|L_{pt_0}\right)=0$. Let us be clear about what this assumption entails. Since average wage growth in the province is absorbed by the province dummies h_p, it requires that shocks to the growth rate of individual wages, net of any common component, at any time *after* the worker starts its tenure with the firm are orthogonal to the degree of financial development in the province where the job is located at the time of hiring, L_{pt_0}. We regard this as a very weak and reasonable requirement.

To obtain an estimate of the other parameter of interest—i.e., δ, the intercept of the wage—tenure relationship, which turns out to be key for measuring the extent of borrowing that goes on within the firm—we use the estimates of α, γ, $(\mu+\beta)$, and η from equation (5) to construct a tenure/experience-adjusted wage level for individuals in their first job in the labor market. For these individuals, $E_{it}=T_{ij(t_0)pt}$. Define this adjusted wage as:

$$\begin{aligned} e_{ij(t_0)pt} &= \ln w_{ij(t_0)pt} - X'_{ij(t_0)pt}\alpha - (\mu+\beta)E_{it} - \gamma E_{it}\times L_{pt_0} - \eta T_{ij(t_0)pt}\times h_p \\ &= \rho + Z'_i\phi + \delta L_{pt_0} + \vartheta h_p + \varepsilon_{ij(t_0)t} \end{aligned} \tag{6}$$

If $E\left(\varepsilon_{ij(t_0)t}|L_{pt_0},Z_i,h_p\right)=0$, regression (6) can be estimated by OLS using the estimated adjusted wage $\widehat{e}_{ij(t_0)pt}$ in place of the unobserved $e_{ij(t_0)pt}$. What does this assumption entail? From equation (4), $\varepsilon_{ij(t_0)t}$ may be correlated with the level of financial development of the local market if more able workers sort into provinces that are on average more financially developed, or that have some other attractive features that happen to be correlated with financial development. Alternatively, $\varepsilon_{ij(t_0)t}$ may be correlated with financial development if matches are of higher average quality in more financially developed provinces. In both cases, OLS estimates will be biased downward. If these effects were permanent (plus an inconsequential random error), the province fixed effects h_p would capture them and no bias would emerge. That is, suppose there are only two periods (t_0 and t_1) and two provinces (p and p', with $L_{pt_0} > L_{p't_0}$ without loss of generality, so that province p is initially less financially developed than p'). Our assumption is that, with regard to ability, $E\left(a_i|L_{pt_1}\right)=E\left(a_i|L_{pt_0}\right)<E\left(a_i|L_{p't_0}\right)=E\left(a_i|L_{p't_1}\right)$ (and similarly for match effects). In this case, province fixed effects would capture the (permanent) composition effects. These effects can be assumed permanent because the process of convergence across credit markets is slow or it takes time to attract talent so as to shift the distribution of ability or match values across provinces. In the absence of convincing instruments for L_{pt_0}, this will be our identifying assumption.

14. Since we can estimate the degree of local financial development only starting in 1990, for workers joining the firm before 1990, we assign the degree of financial development of the province in 1990. This assumption is consistent with our observation (see Section 3.1) that the structure of the local banking market had been frozen by the 1936 legislation, with little entry and expansion until the 1990s (possibly with the exception of Sicily, see note 7). We also checked whether the results changed if we were to use only jobs that start during our sample period 1990–1997, and find qualitatively similar results.

5. RESULTS

5.1. *Main results*

Table 3, Panel A reports the estimates of equation (5) using only the sample of job stayers. Since the job staying/moving decision can be endogenous (for example, because workers who receive positive transitory shocks, such as a bonus, may postpone a move), we present estimates with and without controlling for selection into new firms (Columns 2 and 3, respectively). In Column 1, we report the probit estimates for firm mobility. These are used to construct an estimate of the inverse Mills ratio. The exclusion restrictions in the mobility probit (variables that affect the decision to move but do not affect wage growth) have the expected effects: the plant closure indicator, equal to one if the current job is found after a displacement due to plant closure in the previous job, has a strong positive effect on mobility and so does the residual worker horizon in all provinces.[15] Jointly, the instruments are highly statistically significant (the value of the χ^2 test statistic is 17,269 with 96 degrees of freedom) suggesting that the estimates do not suffer from weak exclusion restriction problems.[16]

In our regressions, we control for worker job position (dummies for blue and white collar) and for year dummies. The latter, in particular, absorbs any time variation in the interest rate that spreads over the sample period, which is due to nation wide movements in interest rates. Furthermore, since we can identify the effect of credit constraints using province-specific time variation, we can insert a full set of province dummies as controls. Thus, any systematic differences across provinces (for instance, in average productivity) that is reflected in wage growth is captured by these controls. Without province-specific time variation in financial development, identification of the effect of financial frictions on wage contracts using only cross-sectional geographical variation in the *level* of financial development would be problematic. In fact, insofar as financial development also spurs average productivity growth, it would also capture differences in the latter on wage growth.[17]

Column 2 shows the results when no adjustment for sample selection is made. The financial frictions indicator has a positive and highly significant impact on a worker wage rate growth implying that in provinces with more backward financial markets firms and workers settle on a steeper wage profile over the tenure horizon. Adjusting for selection (Column 3), results in a substantially smaller coefficient of the liquidity constraints indicator—pointing to the importance of accounting for movers' decisions—but the coefficient remains highly statistically significant (a *p*-value of 3.2 percent).

To fully characterize the effect of financial market imperfections on the shape of the wage contract, we need to identify not only their effect on the slope but also on the intercept of the wage–tenure profile. In fact, as described in Figure 1, if in less developed financial markets workers lend to their firm, the wage profile should have a higher slope and a lower intercept relative to a frictionless counterpart. Table 3, Panel B reports the OLS estimates of equation (6). Each column uses the parameters estimated in the corresponding column in Panel A to compute the left-hand side of equation (6). We find a negative and highly statistically significant effect on the intercept of the wage profile [the parameter δ in equation (6)]. It is important to stress that

15. The estimated effect of the residual work horizon varies between a minimum of 0.001 and a maximum of 0.005 and is statistically significant at the 5 percent confidence level in 74 out of 95 provinces.

16. The value of the χ^2 test statistic for the interaction between the residual work horizon and province dummies is 450 (with 94 degrees of freedom).

17. This is not the case when identifying the bite of financial frictions out of time variation within each province, because the source of variation is the exogenous liberalization imposed by the EU directive and because its heterogeneous effects across provinces are the consequence of the different initial levels of financial development, themselves the accidental reflection of historical heritage and of the 1936 legislation as shown in Figure 3.

TABLE 3
Main Results

Panel A: Wage growth equation

	Probit	Wage growth	
	(1)	(2)	(3)
Liquidity constraint	0.2699*** (0.0106)	0.0129*** (0.0015)	0.0033** (0.0015)
ΔWhite collar	0.1199*** (0.0225)	0.0082** (0.0034)	0.0045 (0.0034)
ΔManager	0.5245*** (0.0824)	0.0516*** (0.0096)	0.0398*** (0.0096)
ΔYear dummies	Yes	Yes	Yes
Province dummies	Yes	Yes	Yes
Inverse Mills ratio			0.0744*** (0.0032)
Plant closure dummy	0.7597*** (0.0081)		
Residual horizon	0.0033*** (0.0010)		
Province dummies × residual horizon	Yes		
N	380,380	329,772	329,772

Panel B: Wage-level equation (first-job workers sample)

	(1)	(2)
Liquidity constraint	−0.1498*** (0.0090)	−0.1805*** (0.0090)
Male	0.2304*** (0.0058)	0.2448*** (0.0058)
Province dummies	Yes	Yes
N	51,415	51,415

Note: Panel A shows the estimation results of the wage growth equation for the sample of workers that stay with the firm. Column (1) shows the results of the probit estimates for the workers that have moved. Columns (2) and (3) report the estimates of equation (3). Panel B shows the estimation results of the wage-level equation for the sample of workers in their first job. The left-hand side variable is computed using the parameters estimates from Panel A as in equation (6). Estimates in column (1) use parameters from column (2) of Panel A to construct the left-hand sid variable; estimates in column (2) use parameters from column (3) of Panel A. Robust standard errors are reported in parenthesis. The superscripts ***,**, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

although we use some of the estimated parameters of the wage growth regression to compute the left-hand side of equation (6), there is nothing in our exercise that would mechanically produce a negative estimate of δ.[18]

The wage profile shifts counter clockwise in provinces with less developed financial markets. This is consistent with the view that firms circumvent credit market constraints by borrowing implicitly from their workers (back-loading compensation). Economically, the values of γ and δ using the selection-adjusted estimates imply that the wage of a worker that begins tenure matched with a firm facing a local market that has an index of financial backwardness 50 percent above the median value in 1997 is $\sim$ 18 percent lower than that of an otherwise equal worker in the median local market for financial development; but it grows at a rate that is 0.33 percent faster for each month of tenure ($\sim$4 percent a year). Thus, by itself, heterogeneity in access to finance across firms is sufficient to generate significant cross-sectional heterogeneity in observed wages paid by (otherwise) similar firms to (otherwise) similar workers. This helps address the wage heterogeneity puzzle documented by, among others, Krueger and Summers (1988), Abowd and Kramarz (2000), and Van den Berg (1999).

18. From equation (6), measurement error in L_{pt_0} alone would generate a *positive* relationship between $e_{ij(t_0)pt}$ and L_{pt_0} (since $\widehat{\gamma} > 0$).

Of course, since the main effect of credit market imperfections is to twist the wage profile, this same worker will receive a higher wage at the end of his tenure. Hence, there exists a value of tenure at which the wage in a financially developed market equals that in a financially depressed market (where profiles cross). Using equation (1), this value, which we denote as T^*, is equal to $-\delta/\gamma$, i.e., ~55 months using our estimates in the third column of Table 3. Hence, during the first 4.5 years of tenure, the worker is typically lending to the firm before being paid back.

5.2. *The effect of firm and worker heterogeneity*

Suppose that we can identify a set of firms (or workers) that are highly sensitive to borrowing frictions, for instance, because they are highly dependent on outside finance (e.g., have little net worth) or have no source of external funding other than bank borrowing. For these firms (workers), we should expect, *ceteris paribus*, the wage contract to become even steeper (flatter) than for the average firm (worker) as the local credit market loses efficiency, i.e., as L_{pt_0} increases. Formally, this can be studied by allowing the parameters of our wage equation to shift with observable firm or worker characteristics measuring heterogeneity in access to credit. This heterogeneity in the values of δ and γ is directly implied by the firm-as-an-internal-credit-market model (Michelacci and Quadrini 2005) and can be used to assess its empirical validity. Here, we focus on two sources of heterogeneity that we can confidently measure.

Firm creditworthiness: The first source of heterogeneity we analyze is firms' differences in creditworthiness, which may result in differential access to credit. As a measure of firm creditworthiness, we use a firm-specific credit score, a variable directly available to the banks that belong to the CB consortium, and used as a key input in determining whether and how much to lend to a firm. Hence, the score provides a measure of the creditworthiness of any firm on the same metric, irrespective of the firm's age or size (often used as coarse proxies of firm creditworthiness). Consistent with credit scoring measuring differences in firms creditworthiness, we find that, *ceteris paribus*, good-score firms pay lower interest rates, as discussed in Section 3.1.

The thought experiment we perform is the following. Suppose we fix the creditworthiness of the firm and ask how an exogenous shock to the conditions of the local credit market would impact the wage profile. This is an experiment that uses the exogenous variability in our data (i.e., shifts in L_{pt_0} induced by the process of financial market liberalization). To implement this thought experiment, we reestimate equation (5) by adding interaction terms between the measure of local financial frictions and the relevant measure of firm heterogeneity, controlling for any direct effect that this heterogeneity may have on wage setting. [19]

The first two columns of Table 4 shows the result when we interact financial backwardness with firms' credit scores in the wage growth equation (first column) and in the wage-level equation (second column), respectively. We divide the credit score into three categories: bad, medium, and good scores (the excluded category).[20] Controlling for any direct effect, the score level may exert on workers wage growth, we find that medium score and, even more so, bad-score firms offer significantly steeper wage profiles than good-score firms as the conditions of the local credit market deteriorate. Moreover, the estimate of the intercept becomes smaller for lower score firms as required for the change in the wage–tenure profile to be interpreted as an implicit credit contract.

19. In each case, we re-estimate the mobility probit adding the extra variables that appear in the wage growth equation and construct a new Mills ratio to control for selection into staying with the same firm.

20. The credit score is a number between 1 and 9 with higher values signaling lower creditworthiness. We assign firms with a number between 7 and 9 to the "bad score" category (19 percent); firms with a number between 4 and 6 to the "medium score" category (47 percent); the remainders are in the excluded category (34 percent).

TABLE 4
The effect of firm and worker characteristics

	Firm creditworthiness		*Worker occupation*	
	First diff.	Levels	First diff.	Levels
Liq. constr.	0.0011 (0.0032)	−0.0934*** (0.0178)	0.0026 (0.0016)	−0.1479*** (0.0092)
Liq. constr. × bad score	0.0083*** (0.0028)	−0.0936*** (0.0209)		
Liq. constr. × medium score	0.0057*** (0.0021)	−0.0720*** (0.0151)		
Bad score	−0.0142*** (0.0050)	0.0388 (0.0437)		
Medium score	−0.0053 (0.0039)	−0.0160 (0.0302)		
Liq. constr. × manager			0.0090** (0.0041)	−0.3029*** (0.0872)
Liq. constr. × white collar			0.0029*** (0.0010)	−0.0796*** (0.0084)
Manager			0.0220*** (0.0078)	1.3469*** (0.1671)
White collar			0.0128*** (0.0019)	0.2534*** (0.0170)
N	83,150	9,318	329,772	51,415

Note: The table shows the estimation results of the wage growth equation (3) and the wage-level equation (6) allowing for the response in the slope of the tenure profile to credit market frictions to differ across different types of firms and workers. Robust standard errors are reported in parenthesis. The superscripts ***,**, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

We want to stress again that this result is in no way an artifact of our two-step estimator. These estimates imply that low-score firms tend to rely more on borrowing from workers in response to an increase in local credit market imperfections than good-score firms. Quantitatively, a bad-score firm would respond to a deterioration in access to the local credit market by adjusting the steepness of the wage profile offered to its workers 1.4 times more than a medium-score firm, and 8.5 times more than a good-score firm.

Worker differential access to the loan market: Following a similar logic, we classify workers according to their willingness to supply credit, as measured by the likelihood of facing borrowing constraints. For workers, we do not have as good a measure of their creditworthiness as we have for firms and thus have to rely on a coarser indicator. We proxy for it with a worker's job type using the distinction between blue collar, white collar and managers and assume that access to the loan market is more problematic for blue collar workers than it is for white collar workers and managers. This assumption is backed up by evidence from estimates of the conditional probability that a loan applicant is turned down by an intermediary obtained using the Italian Survey of Households Income and Wealth (SHIW), described in note 5, which shows that blue collar workers are significantly more likely to be denied credit than white collar workers and managers, conditioning on a variety of controls.[21] In these regressions, we control for the interaction of tenure and occupational dummies to account for permanent differences in productivity profiles across occupations.

The last two columns of Table 4 shows the result when the indicator of local credit market frictions is interacted with the manager and white collar dummies, as proxies for workers' creditworthiness (blue collar workers are the excluded group). We find that in response to a

21. We pool the 1998 and 2000 waves and run probit regressions for whether an application was turned down controlling for demographics, measures of workers' economic resources, geographic, time, and occupational dummies. Relative to the sample mean, being a blue collar worker raises the probability of being denied access to the loan market by 8.75 percent; being a white collar worker and a managers lowers it, respectively, by 37 percent and 49 percent.

deterioration of the conditions of the local credit market, wage profiles become steeper for all types of workers but, consistent with our interpretation, those of managers (deemed to face easier access to the local credit market) become even steeper than those of other types of workers. In fact, the estimates of the shift in the slope (γ) are 0.0116, 0.0055, and 0.0026, whereas the point estimates of the shift in the intercept (δ) are $-0.451, -0.228$ and -0.148, respectively for managers, white collars, and blue collars.

5.3. *Discussion*

How does the firm-as-a-credit market mechanism that we identify square up with the leading theories of increasing wage–tenure profiles? Three broad classes of models have been proposed to explain upward sloping wage–tenure profiles: models based on incentives, on human capital accumulation, and selection (Gibbons and Waldman 1999).[22] We discuss them in turn.

According to Lazear (1981), firms tilt upward the wage profile relative to the worker's productivity profile to induce workers to exert effort. If one assumes that in the baseline, case wages coincide with productivity, it would appear that the finding that the wage profile is steeper in more backward credit markets relative to the baseline (which turns out to be the empirically relevant case) can be made consistent both with an incentive story à la Lazear and a liquidity-constraint story à la Michelacci-Quadrini. Notice, however, that while in Lazear it is true that the firm "implicitly" borrows from its workers at the beginning of their relationship, the borrowing is *incidental* (it is the only way to implement the incentive aspect of the wage profile) and it is *independent* of whether the firm is liquidity constrained. As we have seen, this is not the case empirically: stronger reliance on implicit borrowing is found among firms located in less developed credit markets.

Unfortunately, we cannot test whether a Lazear-type mechanism is at work because we do not observe productivity. However, we stress that the mechanism at work in the Lazear's model can be seen as a complement, not as a substitute for the mechanism (liquidity constraints) that we emphasize. Figure 4 illustrates this point graphically. The heavy dotted line represents the unobserved worker's productivity profile. In the baseline (no liquidity constraints or $L_{pt_0} = 0$), the wage profile is steeper than the productivity profile to supply incentives for workers. If firms are subject to liquidity constraints (case II above), they would make the wage profile even steeper for the additional purpose of borrowing implicitly from workers. The case of workers borrowing from firms (case I above) is aptly described by the following quote from Lazear's paper: "if workers have utility functions, which are time separable and concave in income, then the optimal [wage] path will remain upward sloping,[23] even if all borrowing is prohibited, but will tend to be somewhat *flatter* than it is when no borrowing constraints are imposed" (emphasis added).[24]

Lazear-type mechanisms may pose a different problem for our estimates. Since we rely on geographical variation in access to the credit market, one may argue that heterogeneity in local credit market development may be correlated with shirking. If in a province individuals are more prone to shirk on the job, they may also be more likely to default on loans, making banks less willing to lend. However, our identification strategy relies on geographical heterogeneity in variation *over time* in financial development. The effect of persistent local factors on wage–tenure

22. A further mechanism is promotions and learning. We have no information on promotions, so we cannot address this aspect. We note, however, that we find that financial backwardness leads to a steeper wage profile even *within* occupations (Table 4). Hence, our findings are robust to the possibility of being promoted from one occupation to another.

23. In Lazear's case, productivity is flat due to absence of specific human capital.

24. Given that we find that the wage profile is steeper when credit markets work less well, this case is clearly inconsistent with the data.

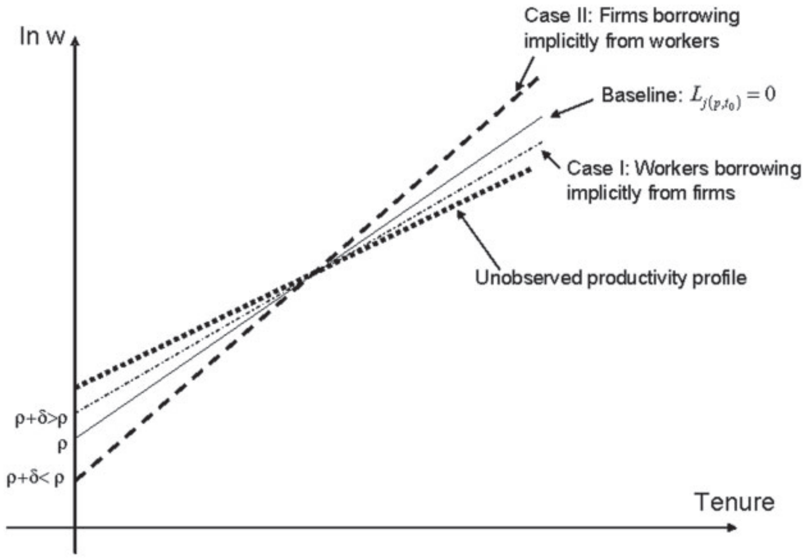

FIGURE 4
Credit flows and tenure profiles with incentives considerations.

profiles, including diversity across areas in shirking attitudes, are captured by province dummies. As for the dynamics, the only way an incentives story could fit our evidence is if changes in shirking behavior across provinces were synchronized with changes in credit market development over the 1990s. We find this highly implausible.

A second strand of literature follows Becker (1962) and attributes wage growth to the accumulation of human capital and, through this, increasing productivity. While we have argued that the time variation in access to external markets we rely upon stems from exogenous shocks, one might object that even exogenous differences in access to the loans market may result in differences in the workers' productivity profile and our regressions would be picking up the latter rather than changes in the wage–tenure profile related to borrowing and lending within the firm. This story implies that when capital markets improve, workers or firms can invest more in specific human capital and hence the wage profile becomes steeper.[25] However, we find that when credit markets improve, the profile becomes flatter, the exact opposite. In the borrowing constraints model this is because firms substitute credit from workers with credit from formal sources.

A more direct way of testing the human capital accumulation story would be to look at the correlation between investment in on-the-job-training and local financial backwardness. If the training mechanism was driving the results, it should be the case that in poorly functioning credit markets, there is more investment in on-the-job-training, resulting in steeper profiles. While our data set offers no information for tackling this issue, we can use other data sets to at least address it descriptively. We use again the SHIW. In the 1991 wave, the survey asked a number of questions about on-the-job-training programs. In particular, respondents were asked if they had attended a training program organized by their company, and, if so, the duration of the program (in weeks). We run a simple probit regression on our indicators of credit market backwardness and control

25. Whether investing less in specific human capital implies flatter pay profiles may depend on workers' mobility choices and their bargaining position within the firm (which determines how much of their specific human capital investment returns they manage to capture). See Abowd *et al*. (2006) for a discussion of these issues.

for a series of individual characteristics (age, education, married, male, occupation dummies). We also run a tobit for the weeks of training. The evidence (available on request) is that in markets with poorly functioning credit markets, firms invest significantly *less* in training workers on the job, not more. While this evidence is descriptive, it suggests that the on-the-job-training story cannot fit basic aspects of the data.

Finally, Salop and Salop (1976) develop a model in which firms use returns to tenure to sort workers. The idea is that if workers differ in the probability of quitting and firms differ in the cost of training, then firms with high training costs will offer an increasing wage–tenure profile to attract the low mobility workers. For the selection mechanism to explain our results, we should observe that firms in more backward credit markets invest more in training and therefore offer steeper wage–tenure profiles. As discussed in the previous paragraph, the evidence is exactly the opposite. It is therefore unlikely that the positive correlation that we find between wage growth and the index of local credit market backwardness is due to this mechanism.

6. ROBUSTNESS

In this section, we assess the robustness of the previous results along various margins. The key question we want to address is: is the finding of a downward shift and an upward tilt of the wage profile in more backward credit markets sensitive to changes in the regression specification, variable definition, or sample selection?

Worker's turnover: the existence of implicit credit flows rests on the fact that workers expect with a certain probability to remain with the firm long enough to be "paid back." Suppose that, for reasons unrelated to local credit market conditions, certain firms turn over their workforce more intensively than other firms, e.g., because, due to rapid innovation, workers-specific capabilities become rapidly obsolete or because they face more variable demand. These firm-specific mobility policies may affect the structure of the labor contract both directly Abowd *et al*. (2006) and through the implicit credit contracts, as workers may be less willing to lend to firms where they have *ex ante* unstable employment prospects. To test whether our findings are robust to firm-specific turnover, policies, we construct a proxy of "labor churning" at the firm level, defined as (hires+fires)/employment. We use the monthly employment data, available from the INPS archives, to construct a yearly measure of churning. We approximate hires+fires with the sum of the absolute value of the monthly employment changes; employment is computed as the January value plus the positive employment changes over the other months.[26] We then take the firm-level average over the available years. As an alternative, we use the yearly standard deviation in the growth rate of the monthly employment. We then define a firm as "low churning" if the chosen measure of churning is below the sample median. Results are shown in Table 5 (we loose some observations because for some firms the employment information is missing). To save on space in this and the following tables, we only report the estimates of the relevant coefficients. The results show that firms with lower churning pay higher wages upon entry as in Postel-Vinay and Robin (2002). More importantly for our results, in response to a deterioration in credit market conditions, they also offer a relatively lower wage at entry and faster growth suggesting that low turnover makes reliance on implicit contracting easier. However, the effects are small and, for the slope, not precisely estimated. A very similar picture emerges when using the standard deviation of monthly employment as the indicator of firm-level mobility. In sum, allowing for heterogeneity in turnover does not change our conclusions.

26. Ideally, one would like to compute churning based on workers' flow. Unfortunately, we have too few workers per firm to construct a reliable measure of mobility based on the matched data. Note that, as the time interval goes to zero, so that any employment change is also a job change, our measure converges to that based on workers flow.

TABLE 5
Robustness Analysis: Workers' mobility

	Measure of churning			
	(Hires+Fires)/Employm.		*S D of the employment growth*	
	First diff.	Levels	First diff.	Levels
Liq. constr.	0.0029* (0.0017)	−0.2146*** (0.0121)	0.0027 (0.0017)	−0.2007*** (0.0112)
Liq. constr.× Low churning	0.0010 (0.0011)	−0.0620*** (0.0122)	0.0020* (0.0010)	−0.0912*** (0.0107)
Low churning	0.0101*** (0.0021)	0.7442*** (0.0254)	0.0042** (0.0020)	0.6393*** (0.0223)
N	291,686	46,347	291,686	46,347

Note: The table shows the estimation results of the wage growth equation (3) and the wage-level equation (6) when introducing a measure of firm-level workers' mobility. In the first two columns, the measure of churning comes from the average of the (hires+fires)/employment ratio; in the last wto columns, from the standard deviation of monthly employment growth. "Low churning" firms are those with values below the sample median (see the main text for further details). Robust standard errors are reported in parenthesis. The superscripts ***,**, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

Nonlinearity in returns to tenure: another issue, mentioned at the end of Section 4, is that returns to tenure may not be constant as implied by our main specification, but depend on attained tenure itself (Dustmann and Meghir 2005). To allow for non-linear returns to tenure, we modify equation (1) to include a quadratic term in tenure and its interaction with our measure of financial market backwardness. The estimation strategy remains unchanged. The wage growth equation, estimated only on the sample of firm stayers, includes two extra variables, a linear term in tenure and its interaction with the financial market backwardness variable [which identify the effect of the quadratic term in wage (log)-level equation]. Results for this specification are reported in Table 6. Due to the different specification, we report the full results for the wage growth equation and the wage-level equation separately. The negative and significant effect of the linear term in tenure in the wage growth equation implies that the wage profile is concave in tenure, consistent with Dustmann and Meghir's findings (Panel A). However, this does not affect the previous conclusion that wage–tenure profiles are steeper in firms facing more backward credit markets. In fact, the effect of credit market imperfections on wage growth is positive at all levels of tenure (the quadratic term is small and insignificant). Furthermore, even for this specification, the estimate of the wage-level equation shows that the intercept of the wage–tenure profile is lower in less developed local credit markets confirming the previous results (Panel B).

Plant vs. firms: when firms have multiple plants across provinces, a question arises as to what is the credit market of reference for the employer–employee implicit wage contract.[27] In our baseline, we have assumed that it is the province where employment takes place. This may be justified on a number of grounds. First, the contracts that the firm can offer are constrained by those offered by its competitors in the local labor market, which in turn reflect the conditions of the local credit market. Moreover, the plant may have administrative autonomy relative to the headquarters. Finally, the legal registered office's location may have little connection with the concept of production unit (e.g., Lombard firms that maintain their legal registered office in Milan for purely logistical reasons).

27. Although important in principle, in practice, this is not likely to be a big concern in our data. Italian firms feature the smallest average size among industrialized countries (Pagano and Schivardi 2003). Reflecting this, multi plant firms are rare as confirmed by the fact that 90 percent of our final sample of workers are employed in the same province where the firm maintains its legal registered office.

TABLE 6
Robustness Analysis: Quadratic tenure profile

Panel A: Wage growth equation	
Liquidity constraint	0.0039** (0.0016)
ΔWhite collar	0.0056* (0.0034)
ΔManager	0.0455*** (0.0096)
ΔYear dummies	Yes
Province dummies	Yes
Inverse Mills ratio	0.0423*** (0.0037)
Tenure/100	−0.0052*** (0.0006)
Liquidity constraint*Tenure/100	0.0002 (0.0003)
N	329,772
Panel B: Wage-level equation, first-job sample	
Liquidity constraint	−0.0943*** (0.0079)
Male	0.2382*** (0.0050)
Province dummies	Yes
N	51,415

Note: The table shows the estimation results of the wage-growth equation (3) and the wage-level equation (6) allowing for a quadratic in tenure in the level equation. Robust standard errors in parenthesis. The superscripts ***,**, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

Here, we assess the sensitivity of our results in two ways. First, we assume that for the observations where the worker's province of employment differs from the province where the firm is headquartered (∼10 percent of our sample), the relevant p is where the firm is located. Second, we drop the observations where the two provinces differ. The results for the three different definitions of p (including for completeness those obtained in the baseline case, Panel A) are reported in Table 7. Moving from a worker- to a firm-based definition gives virtually identical results (see Panel B). In the other experiment (Panel C), the point estimates hardly move, although they are less precise due to smaller sample size.[28]

Other concerns: in Table 8, we check the robustness of our results vis-à-vis a number of additional issues. Our identification strategy relies on variation in financial development across local credit markets. Clearly, local credit market conditions are binding for firms only if they cannot borrow outside the boundaries of the local market. This is less likely to be the case for large firms, which can raise funds outside the borders of the province where they are located (through, say, remote bank lending or issuance of corporate bonds). Though most firms in our sample are small (median employment is thirty), the firm size threshold that qualifies a firm for borrowing outside its province is unobserved, and this introduces measurement error in the matching between a firm and the relevant credit market condition that it and its workers face. To account for this, we repeat our estimation exercise restricting the sample to workers employed in

28. These two conclusions are confirmed if we replicate the entire regression analysis, not just the basic regression. We also conduct an informal test of our assumption that the relevant definition of p is where employment takes place rather than where the firm maintains its legal residence. To do so, we include in our baseline regression the financial development variable referring to the worker's location as well as the one referring to the firm headquarter's location. Identification comes from observations where the worker is employed in a province different from the one in which the firm is headquartered. In the growth specification, the worker-based measure has a similar estimate as in the baseline specification, while the firm-based measure is close to zero and statistically insignificant. In the level specification, both measures are statistically significant, but the impact is much larger for the worker-based measure. This evidence provides support for our preferred specification. All results available on request.

TABLE 7
Sensitivity of results to the definition of local credit market

	Panel A: Baseline		*Panel B: Firm's legal registered office*		*Panel C: Workers and firms in same province*	
	First diff.	Levels	First diff.	Levels	First diff.	Levels
$L_{p(w)t_0}$	0.0033** (0.0015)	−0.1805*** (0.0090)			0.0030* (0.0017)	−0.2081*** (0.0096)
$L_{p(f)t_0}$			0.0034** (0.0017)	−0.2088*** (0.0090)		
N	329,772	51,415	331,278	51,894	295,162	48,161

Note: Robust standard errors in parenthesis. ***,**, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

TABLE 8
Robustness Analysis: Other concerns

	Panel A: Initial size ≤ 15		*Panel B: Using a first-job, first-year sample*		*Panel C: Reducing the set of exclusion restrictions*	
	First diff.	Levels	First diff.	Levels	First diff.	Levels
Liq. constr.	0.0060* (0.0034)	−0.2923*** (0.0156)	0.0033** (0.0015)	−0.0758*** (0.0137)	0.0176*** (0.0016)	−0.1258*** (0.0114)
N	54,148	25,437	329,772	17,425	329,772	51,415
	Panel D: Controlling for local unempl. rate at hiring		*Panel E: Testing for renegotiation*		*Panel F: Controlling for firm fixed effects*	
	First diff.	Levels	First diff.	Levels	First diff.	Levels
Liq. constr.	0.0032** (0.0015)	−0.1224*** (0.0084)	0.0035** (0.0016)	−0.1911*** (0.0096)	0.0072** (0.0029)	−0.1056*** (0.0226)
N	329,715	51,415	329,772	47,020	329,772	48,418

Note: The table shows the estimation results of the wage growth equation (3) and the wage-level equation (6) for six different samples or specifications. Panel A shows the estimation results restricting the sample to workers employed by "small" firms. Firm size is defined using the number of workers employed by the firm as of the first time we observe the match in the 1990–97 period. In Panel B, we estimate the level equation for a sample of individuals in their first year in the labor market (i.e., with zero labor market experience). In Panel C, we use only the firm bankruptcy dummy as an exclusion restriction in the mobility probit. In Panel D, we add to our regression the unemployment rate and its interaction with tenure (with the same timing of the financial market backwardness variable). In Panel E, we add to our regression the variable $\min\{L_{p,t_0}, L_{p,t_0+1}, \ldots, L_{p,t}\}$ and its interaction with tenure. In Panel F, we estimate the main specification including firms' fixed effects in the wage growth specification. Robust standard errors in parenthesis. The superscripts ***,**, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

firms with fifteen employees or less at the beginning of the sample—a rather stringent criteria.[29] Panel A shows the estimates, adding the estimates for the whole sample to ease comparison. Though our selection leaves us with only ∼16 percent of the original sample (resulting in lower statistical precision), we still find that tenure profiles are steeper in less developed credit markets and wages are lower at the beginning of tenure and higher toward the end. Furthermore, consistent with local credit market conditions binding more for smaller firms, the effect on the slope and the intercept are stronger in this sample of very small firms than in the overall sample.

29. This threshold reflects the institutional feature that employer protection legislation is less stringent for firms with fifteen employees or less.

Second, we need to consider a subtle form of nonrandom attrition. The estimates of equation (6) use a variable number of observations for each individual. Individuals who stay longer with the initial firm naturally contribute more observations. However, longer tenures are typically associated with higher match values or higher ability, creating a possible form of sample selection bias. One simple way of addressing this problem is to "balance" the panel artificially by using only the first year in the labor market for each individual. This is what we do in Panel B (the first difference results are the same as for the main sample, and reported for completeness). The results show that even when accounting for this type of attrition, and notwithstanding the substantially smaller sample, credit market inefficiency still results in a lower wage level at the beginning of tenure.

Third, one may wonder whether the results are robust to the exclusion restrictions imposed in the worker mobility selection equation. While we cannot test the exclusion restrictions directly, we can check informally whether the results are sensitive to adopting a more parsimonious strategy, and hence impose only the least controversial restriction. Specifically, we retain only the firm closure dummy as the identifying variable in the selection equation. Estimation results based on the restricted instrument set are shown in Panel C; the pattern of the estimated coefficients are the same as in Table 3.

Another concern relates to the effects of the local business cycle conditions on the wage profile. Suppose that workers who join a firm during a weak local labor market receive lower initial wages, but later they experience faster wage growth due to a mean reversion effect. If the spread between the rates on loans and deposits increases when local labor market conditions are poor, due for example to an (uncontrolled for) increase in the risk premium, our specification may be capturing this effect rather than implicit borrowing. To allay this worry, we augment our regressions with the (logged) local unemployment rate at the time of hiring, U_{pt_0}, and its interaction with tenure.[30] If the effect we are describing is at play, we should find that U_{pt_0} is positive in the wage growth equation and negative in the level equation and that the credit market backwardness variable becomes insignificant. In fact, the effect of U_{pt_0} is negative both in the wage growth equation and in the level equation. Moreover, the estimates of γ and δ are robust to the introduction of this further control (Table 8, Panel D).

Another issue is the possibility that the implicit loan contracts we are considering are subject to renegotiation, for example, to reflect improved conditions in the credit market. In this case, the variable that should appear in the wage regression is not the condition of the local credit market at the time the contract starts, but the best conditions over the life of the contract. Against the idea that firms try to renegotiate the implicit terms of the contract is the loss of reputation, which may deter participation by some workers. To test whether renegotiation takes place, we follow Beaudry and DiNardo (1991) and include in our equation (3) the variable $\min\{L_{pt_0}, L_{pt_0+1}, \ldots, L_{pt}\}$ and its interaction with tenure. This variable measures the *best* conditions of the local credit market since the start of the contract. If implicit loan contracts are subject to renegotiation, rather than being stipulated once and for all at the start of the relation, we should find that this variable knocks out the significance of L_{pt_0}. The results, reported in Panel E of Table 8, militate against the idea that renegotiation is important. First, the estimates of the effect of L_{pt_0} hardly change relative to the main specification. Second, the variable $\min\{L_{pt_0}, L_{pt_0+1}, \ldots, L_{pt}\}$ is insignificant in the first difference specification (a coefficient of -0.0028 with a standard error of 0.0023).

In sum, the results display remarkably little sensitivity to accounting for heterogeneity in workers mobility at the firm level, nonlinear wage tenure relations, definition of local credit

30. We do not have provincial unemployment rates before 1993, so we use regional unemployment rates for people hired before 1993.

TABLE 9
Job duration regressions

	Cox Prop. Hazard model (1)	*Log-normal survival mod.* (2)	*Gamma survival mod.* (3)
Liq. constr.	-0.0595^{**} (0.0236)	0.0965^{***} (0.0347)	0.1051^{***} (0.0327)
N	52,076	52,076	52,076

Note: In all regressions, we also control for a variety of observables (a quadratic in age, gender, occupation, province dummies, and aggregate effects as captured by the provincial unemployment rate). All variables are also measured at the time of hiring. Robust standard errors in parenthesis. ***,**, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

market, exclusion of larger firms, non random attrition, variations in the set of exclusion restrictions used for identification, local business cycle conditions when starting a job, and renegotiation. Across a large variety of samples and specifications, we confirm evidence of a lower initial wage and a steeper wage–tenure profile in financially backward credit markets, consistent with the idea that firms borrow implicitly from their workers.

Job duration: workers who receive deferred (or back-loaded) wages as an implicit way of lending to their firm should have an incentive to stay with their firm longer than workers who do not. We present an explicit test of this prediction. We begin by selecting all jobs that started during our sample period, 1990–1997, and calculate the maximum months of tenure achieved with the firm. Since our sample period is only 8 years, most jobs are still in progress by the time our sample period ends (~60 percent of all jobs). Our maximum likelihood estimation methods account for this right-censoring problem in a standard way (i.e., maximum likelihood contributions will differ for censored and uncensored spells).

We estimate a series of duration models where the dependent variable is the duration of the job. Our key control is L_{pt_0}, the degree of credit market backwardness at the time of hiring. We also control for a variety of observables (a quadratic in age, gender, occupation, province dummies, and aggregate effects as captured by the provincial unemployment rate), also measured at the time of hiring.

We estimate a Cox proportional hazard model [column (1) of Table 9] as well as parametric survival models (the last two columns of Table 9) for robustness. Regardless of the estimation method, all the results tell the same story: jobs last long (i.e., are less likely to terminate in the Cox proportional hazard model case or more likely to survive in the parametric survival model cases) in more backward credit markets, consistent with the idea that in these markets workers receive back-loaded wages and hence need to stay longer with the firm in order to recoup their early investment.

7. HETEROGENEITY IN THE RETURN TO TENURE

One possible criticism of our specification is that we assume that the returns to tenure depend only on observable characteristics (province-specific dummies and the condition of efficiency of local credit market at the time of entry). Abowd *et al.* (2006) find substantial heterogeneity in returns to tenure across French firms. More generally, there might be firm-level unobservable characteristics that influence returns to tenure and that we do not control for properly. We address this criticism in this final results section using two different empirical approaches.

The first approach is simply to allow the return to tenure to have a firm-specific component. Denoting the latter ξ_j, we rewrite the wage equation (3) as:

$$\begin{aligned}\ln w_{ij(t_0)pt} &= \rho + X'_{ij(t_0)pt}\alpha + Z'_i\phi + \mu E_{it} + \left(\beta + \xi_j\right) T_{ij(t_0)pt} + \delta L_{pt_0} \\ &\quad + \gamma T_{ij(t_0)pt} \times L_{pt_0} + \eta T_{ij(t_0)pt} \times h_p + \vartheta h_p + \varepsilon_{ij(t_0)t} \end{aligned} \tag{7}$$

Taking first differences, the wage growth for stayers is now:

$$\Delta \ln w_{ij(t_0)pt} = (\mu + \beta) + \Delta X'_{ij(t_0)pt}\alpha + \gamma L_{pt_0} + \eta h_p + \xi_j + \Delta c_{it}.$$

This specification shows that firm-level heterogeneity in returns to tenure can be accounted for in our framework by adding a firm fixed effect ξ_j in the wage growth equation. In this way, we control for all potential fixed determinants of a firm's compensation policy, including firm-specific turnover rates.

We re-estimate our model including firm fixed effects and obtain the results reported in Panel F of Table 8. The estimates confirm qualitatively our previous findings. Even controlling for firm-specific returns to tenure the wage–tenure profile is on average steeper and the initial wage is lower whenever credit markets are less efficient.

Specification (7) is still restrictive because it allows for firm-specific returns to tenure but constrains the shifts in the wage–tenure profile to be common across firms in the same province year. An alternative, fully unconstrained route to account for firm-level heterogeneity is proposed by Abowd *et al*. (2006), who estimate returns to tenure on a firm-by-firm basis using a large matched employer–employee data set for France. In our case, the relevant exercise is to estimate *firm–cohort*-specific parameters (where a cohort is defined by the year of hiring), and test whether the estimated parameters change in response to changes in the conditions of the local credit market in ways that are consistent with the evidence discussed above. The problem with this approach is that data requirement are severe, as the estimation of returns to tenure at the cohort–firm level requires a substantial number of workers for each cohort–firm group. Our main data set is inappropriate for this task because the number of matches per firm is limited (we only observe workers born in two days of the year). We therefore rely on another data set, the Bank of Italy's annual INVIND survey of manufacturing firms. INVIND is an open panel of around 1,200 firms per year representative of manufacturing firms with at least 50 employees. The Social Security Administration provided the complete work histories of *all* workers that ever transited in an INVIND firm for the period 1980–1997, including spells of employment in which they were employed in firms not listed in the INVIND survey. The worker information is the same as in the data set we use in our main analysis. We have records for about one million workers per year, more than half of whom are employed in INVIND firms in any given year.[31] Given that we have all workers in a firm, we can estimate firm–cohort returns to tenure. The drawback is that INVIND includes mostly medium- to large-size firms: the average number of workers is 625, compared to 71 in the previous sample. As we have argued, large firms are less likely to depend on local credit market conditions. Hence, while this data set allows for a more precise estimates of the coefficients of interest, it is also skewed toward firms that might not be ideal in terms of the economic mechanism we are trying to capture.[32]

31. We refer the interested reader to Iranzo *et al.* (2008) for a detailed description of the data set.

32. In this data set, the estimates of equation (3) confirm qualitatively the findings of the INPS data set. In particular, the estimates of the shift in the slope is 0.0082 (s.e. 0.0007) and the estimate of the shift in the intercept is −0.8484 (0.0051). Moreover, we have also repeated the analysis of Table 5 on the role of workers' turnover. For this data set, in fact, we can compute exact measures of the turnover rate, as we observe the entire workforce, including hirings and separations. Again, we found very similar results.

This second approach is carried out in three steps. We rewrite equation (3) as:

$$\ln w_{ij(t_0)pt} = \rho + X'_{ij(t_0)pt}\alpha + Z'_i\phi + \mu E_{it} + \gamma_{j(t_0)p} T_{ij(t_0)pt} + \vartheta h_p + \varepsilon_{ij(t_0)t} \tag{8}$$

where

$$\varepsilon_{ij(t_0)t} = a_i + \psi_{ij(t_0)} + c_{it}$$
$$\psi_{ij(t_0)} = \delta_{j(t_0)} + b_{ij(t_0)}$$

For this exercise, we decompose the match effect $\psi_{ij(t_0)}$ into a firm–cohort-specific effect $\delta_{j(t_0)}$ and a residual match effect $b_{ij(t_0)}$ orthogonal to $\delta_{j(t_0)}$. We use a simplified version of the procedure suggested by Abowd *et al*. (2006). In a first step, we consider equation (8) at the time of hiring (t_0) and estimate $\delta_{j(t_0)}$, the firm–cohort-specific intercept of the wage–tenure profile. In the second step, we use the residuals of this equation to estimate $\gamma_{j(t_0)}$, the firm-cohort-specific return from tenure, controlling for the endogeneity of tenure. Finally, in the last step, we test whether $\delta_{j(t_0)}$ and $\gamma_{j(t_0)}$ change in response to changes in the efficiency of local credit markets. Note that defining $\gamma_{j(t_0)} = \beta + \gamma L_{pt_0} + \eta h_p$ and $\delta_{j(t_0)} = \delta L_{pt_0}$ gives our main specification (3) as a special case.

More in detail, since tenure is zero at the time of hiring, the entry wage equation is:

$$\ln w_{ij(t_0)pt_0} = \rho + X'_{ij(t_0)pt_0}\alpha + \mu E_{it_0} + \vartheta h_p + a_i + \delta_{j(t_0)} + \left(b_{ij(t_0)} + c_{it_0}\right). \tag{9}$$

We use the numerical procedure proposed by Abowd and Kramarz (2000), implemented with the Stata package `a2reg` due to Ouazad (2008) to estimate a_i and $\delta_{j(t_0)}$, along with α, μ, and ϑ (we have omitted $Z'_i\phi$ due to multicollinearity with the individual fixed effects). Since the INVIND data set starts in 1980, our measure of labor market experience is downward biased due to censoring. We hence impute experience at the time of entry in our sample assuming that individuals have worked without interruptions since the age of 18 years (if blue collar) or age of 23 years (otherwise). As in the main specification, the regression controls for year dummies, dummies for occupation, and province dummies. To eliminate bias induced by potential endogeneity of labor market experience, we include as a control function the number of previous jobs [see Abowd *et al*. (2006)].[33]

We next use the estimates of equation (9) to construct the residual for years $t > t_0$:

$$\begin{aligned} u_{ij(t_0)pt} &= \ln w_{ij(t_0)pt} - \left(\rho + X'_{ij(t_0)pt}\alpha + \mu E_{it} + h_p + a_i + \delta_{j(t_0)}\right) \\ &= \gamma_{j(t_0)} T_{ij(t_0)pt} + \left(b_{ij(t_0)} + c_{it}\right) \end{aligned}$$

and estimate the firm–cohort-specific return to tenure $\gamma_{j(t_0)}$. Since the error term includes the match-specific effect $b_{ij(t_0)}$, tenure might be endogenous and the estimates of $\gamma_{j(t_0)}$ biased. We therefore instrument tenure with the number of previous jobs, the duration of the last job, and dummies for the worker's position in the age distribution of the firm. We estimate $\gamma_{j(t_0)}$ separately for each firm–cohort combination. We use only observations with uncensored tenure records (those who start their jobs after 1980 or is aged 18 years in 1980). Using only estimates that are statistically significant at the 10 percent level, we find that the median estimated return is 0.0051 per month of tenure. The heterogeneity in returns to tenure is comparable to that found in Abowd *et al*. (2006).

33. The results, omitted for brevity, are available upon request.

How does the shape of the cohort-specific wage–tenure profile [as captured by $\delta_{j(t_0)}$ and $\gamma_{j(t_0)}$] change when local credit market conditions vary? Figure 5 provides a first graphical answer. Panel (a) plots the province average change in the slope of the wage–tenure profile $(\overline{\Delta\widehat{\delta}_{j(t_0)}})$ against the change in credit market inefficiency (ΔL_{pt_0}), where $\Delta\widehat{\delta}_{j(t_0)} = \widehat{\delta}_{j(t_0)} - \widehat{\delta}_{j(t_0-1)}$ and similarly for ΔL_{pt_0}. Panel (b) repeats the exercise for the province average change in the intercept of the wage–tenure profile $(\overline{\Delta\widehat{\gamma}_{j(t_0)}})$. We also report the p-value of the test that the relationship is absent against the alternative that credit market inefficiencies shift the wage–tenure profile downward and counter clockwise (as found in the empirical analysis above). The graphical evidence for the returns to tenure parameter is clear cut: in all years we find that the returns to tenure decrease as financial markets development improves. For the intercept, there is evidence that an improvement in the conditions of the local credit market increases the starting wage of a given firm–cohort in all years considered.

A more formal way to address the question is to consider the regressions:

$$\Delta\widehat{\delta}_{j(t_0)} = \delta\,\Delta L_{pt_0} + e_{\delta,j(t_0)}$$
$$\Delta\widehat{\gamma}_{j(t_0)} = \gamma\,\Delta L_{pt_0} + e_{\gamma,j(t_0)}$$

We also control for year of entry dummies and a full set of provincial dummies to account for time and local effects. Since the estimates of $\widehat{\delta}$ and $\widehat{\gamma}$ are obtained using a variable number of observations per cohort, our regressions use only cohorts with at least 100 observations. We present unweighted as well as weighted regressions. For the latter, we weight with the number of observations used to compute estimates of the cohort-specific return to tenure (for the first difference specification) and the cohort-specific match effect (for the levels specification). The results are reported in Table 10. Both types of regressions tell a similar story, although the unweighted regressions are naturally less precise. Consistent with the graphical analysis and with the main message of the article, we find that a worsening in local credit market conditions ($\Delta L_{pt_0} > 0$) makes the wage–tenure profile experienced by a new cohort steeper than that experienced by the previous cohort ($\delta < 0$ and $\gamma > 0$). It is remarkable that the point estimates of δ and γ that we find here are very much in the ballpark of our reference estimates in Table 3, Panel A, column (3), despite the fact that both the sample and the estimation strategy have changed. Overall, this evidence appears fully consistent with the one discussed in Section 5, confirming the robustness of our findings.

8. ASSESSING IMPLICIT INTEREST RATES AND CREDIT FLOWS

We now assess the implied interest rate and the size of the implicit credit flows, comparing them with prices and quantities observed in the credit market (see Appendix B for more details). We pool observations over all available years to estimate the distribution of tenure, average wages at each level of tenure within each province, and the degree of credit market imperfections faced by workers at the time they joined the firm. In what follows, therefore, we drop the time, firm and individual subscripts and use $T = t - t_0$ to indicate the tenure of a worker, independently of the year she started the job.[34] From equation (3), a firm located in province p pays a wage

34. Ideally, we would construct time-varying measures of average wage by tenure and of the tenure distribution. Unfortunately, for many provinces, we do not have enough observations to do that. Financial development must be measured as of the year when the worker entered the job. Given that we take the cross-sectional average, for each province, we construct financial development for those with tenure T as the weighted average of financial development in the province in the year they started the job: $L_{pT} = \sum_t \frac{n_{ptT}}{n_{pT}} L_{p(t-T)}$ where n_{ptT} is the number of workers with tenure T in year t, $n_{pT} = \sum_t n_{ptT}$ is the total number of workers with tenure T over our sample period, and $L_{p(t-T)}$ is the credit market backwardness index measured in the year the worker joined the firm.

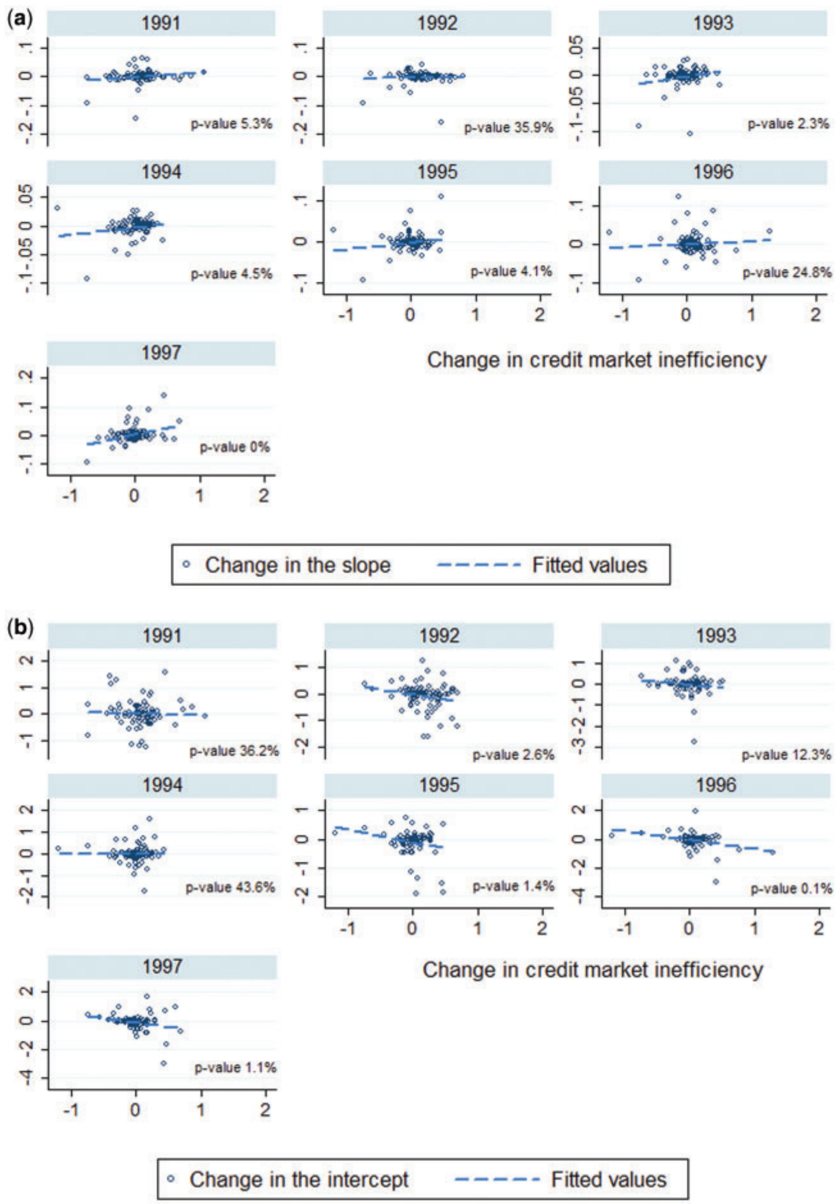

FIGURE 5

Changes in wage–tenure profiles and in credit market efficiency by year (a) Change in the slope γ and (b) Change in the intercept δ

$w_{pT} = e^{(\delta+\gamma T)L_{pT}} w_{p^*T}$ to a worker of tenure T, with w_{p^*T} being the wage that the same firm would pay if located in the most competitive credit market (where $L_{pT} = 0$). It follows that the amount of implicit borrowing from a worker with tenure T in province $p \neq p^*$ is:

$$\mathbf{Borrowing}_{pT} = \left(e^{-(\delta+\gamma T)L_{pT}} - 1\right) w_{pT} \tag{10}$$

TABLE 10

The effect of credit market backwardness on the wage–tenure profile: alternative estimation

	Unweighted		Weighted	
	First diff.	Levels	First diff.	Levels
$\Delta LC_{p,t_0}$	0.0087 (0.0071)	−0.1611* (0.0952)	0.0032*** (0.0005)	−0.0973*** (0.0070)

Note: All regressions include province and year of hiring dummies. Robust standard errors in parenthesis. The weighted regressions use as weights the number of observations used to compute estimates of the cohort-specific return to tenure (for the first difference specification) and the cohort-specific match effect (for the levels specification). ***,**, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

By construction, $\mathbf{Borrowing}_{pT}$ is positive as long as $T \leq T^*$ (the tenure at which the worker begins being repaid). It represents savings on wage payments due to the different tenure profile compared with the province with the most developed financial market. In terms of Figure 1, $\mathbf{Borrowing}_{pT}$ is the vertical distance between the baseline line and that for the case of firms borrowing from workers.

A first check of the implications of our estimates is based on comparing implicit borrowing and repayment flows. In fact, the worker lends to the firm up to T^* and is repaid after that. If we impose the condition that the expected flows to and from the firm are actuarially equal, we can compute the internal rate of return (IRR), i.e., the interest rate that makes the expected stream of borrowing and repayments implicit in the tenure profiles equal to zero in net present value. Formally, the IRR is the unique value that solves:

$$\sum_T \frac{\pi_T}{(1+IRR)^T}\left(e^{-(\delta+\gamma T)L_{pT}}-1\right)w_{pT}=0$$

where π_T is the survival probability, i.e., the probability that a worker is still attached to the firm at tenure T, computed from the actual tenure distribution (assuming for simplicity that all tenure spells are completed). This expression represents the expected net present value of implicit borrowing and repayments.[35] The first row of Table 11 reports the results of this exercise for the most backward province (Cosenza), the one at the 75th percentile of the financial backwardness distribution (Lecce), at the 50th percentile (Ragusa), at the 25th percentile (Trento). We find that the average IRR is 3.3 percent, with considerable cross-province heterogeneity: it is 3.7 percent in Cosenza, 4.9 percent in Lecce, 2.4 percent in Ragusa, and 3.6 in Trento.[36] Ideally, the IRR should be above the interest rate that workers can obtain on their savings and below the interest rate that firms pay on bank loans.[37] In fact, in the period of 1990–97 the cross-country average real interest rate on loans was 10.1 percent and on deposits 0.6 percent; in Cosenza, they were 10.6 percent and –0.2 percent and in Trento 9.9 percent and 0.92 percent. Our values are exactly within these ranges: both workers and firms benefit from transacting. An IRR of 2–5 percent indicates that the surplus is split but firms appropriate a larger share.

We have considered how the implied IRR changes with different values of the parameters γ and δ. We found that proportional parameter changes that leave $T^* = -\delta/\gamma$ unchanged affect the IRR only marginally. For example, increasing both δ and γ by 20 percent implies an average

35. We are implicitly assuming that firms and workers are risk neutral.

36. Note that, due to differences in wages and in the tenure structure across provinces, the IRR need not be monotonic in the degree of credit market backwardness.

37. Note that since the distribution of tenure is right-censored (long tenures are not recorded), our estimate of the IRR is most likely downward biased.

TABLE 11
Size of the Credit Flows

	Degree of credit market backwardness:				
	Max (Cosenza)	75th pct (Lecce)	Median (Ragusa)	25th pct (Trento)	Average
IRR %	3.7	4.9	2.4	3.7	3.3
Maximum borrowing	11,899	6,776	5,515	5,785	6,801
Implicit borrowing per worker	2,773	1,732	1,287	1,424	1,669
Bank borrowing per worker	8,120	5,783	11,176	16,007	15,321
Borrowing from workers/from banks	0.34	0.30	0.12	0.09	0.11

Note: The table reports the estimates of the IRR and of the dimension of implicit borrowing for different values of the degree of local credit market development. The IRR is the interest rate that equalizes the expected flow of borrowing and lending; Maximum borrowing is the cumulated debt toward a worker of tenure T^*, when it reaches the maximum. Implicit borrowing per worker is the average implicit debt per worker.

IRR of 2.7 percent, whereas decreasing both parameters by 20 percent implies an average IRR of 4.0 percent. Proportional changes in the estimates of the parameters do not affect our results dramatically, so that we can focus on T^* to assess the sensitivity of the IRR to parameter changes. As one should expect, the implied IRR is more sensitive to changes in T^*. Setting $T^* = 50$, approximately a 10 percent drop, implies an average IRR of 5.6 percent, while a $T^* = 45$ implies an IRR of almost 9 percent. In fact, moving the crossing point in Figure 1 to the left requires a higher interest rate to equate the implicit borrowing and repayment flows. On the other side, increasing the crossing point to $T^* = 60$ (approximately a 10 percent increase) implies an average IRR of 1.1 percent, while further increases lead to an average IRR of zero.

We next assess the size of the implicit credit flows between workers and firms, comparing it to bank borrowing to gage the relevance of this financing channel. We describe the measures of borrowing briefly here and supply all the details in the appendix. We measure gross lending as the savings on wage payments that a firm facing a local market with financial frictions obtains from workers with tenure $T \leq T^*$ compared to being located in the most developed local financial market. We compute two measures of borrowing: first, maximum borrowing, that is, the stock of implicit debt that the firm has toward a worker of tenure T^*: in Figure 1, this is the area between the baseline and the case II line for $T \leq T^*$. Second, borrowing per worker, that is the average per worker stock of debt toward the workers with $T \leq T^*$, for an hypothetical firm with a tenure distribution equal to the actual tenure distribution at the provincial level. This measure can be directly compared with bank debt per employee. We obtain information on bank borrowing from the CB data service.[38] We take the average bank debt per employee at the provincial level, pooling observations over the 1990–97 years, to which the worker data refer. To avoid the effects of outliers, we drop the last percentile of the borrowing per worker distribution.

In Table 11, we report the results for the various statistics described above. For Cosenza, the maximum borrowing is almost 12,000 euros. This means that a worker with tenure T^* has accumulated a credit toward the firm on the order of 1.5 years of earnings. The value decreases with financial development, but it still amounts to 5,785 euros for the province of Trento, at the 25th percentile of the credit backwardness distribution. Implicit borrowing per worker is between 1,300 and 3,000 euros. In the most backward province, implicit borrowing per worker is 2,773 euros whereas bank borrowing per worker is around 8,000 euros. This means that borrowing

38. The CB is likely to overestimate the amount of bank borrowing per employee, as only firms with a certain degree of credit-worthiness are included. This implies that, if anything, the comparison between bank borrowing and borrowing from workers is biased toward finding a more important role for bank borrowing.

from the workers is approximately one-third of that from banks. And since bank borrowing per employee in the most financially developed province is approximately twice as large of that of Cosenza, around one-third of the difference in bank borrowing between the most and the least backward province is made up by borrowing from workers. As expected, the magnitude of implicit borrowing tends to decrease with financial development. For example, in Trento, at the 25th percentile of the financial backwardness distribution, implicit borrowing per worker is 1,424 euros, less than 10 percent of the loans from banks. The cross-province average is 1,669, 11 percent of the bank loans.

We conclude that implicit credit flows between workers and firms can supplement substantially explicit credit flows from financial intermediaries.

9. CONCLUSIONS

It has long been theoretically recognized that the repeated relationships established within the firm between workers, on the one hand, and entrepreneurs on the other can go a long way toward tempering the effects of credit and insurance market frictions. They can even go as far as providing a basis for the existence of the firm Bovenberg and Teulings (2002). Yet very little progress has been made in pinning down empirically the importance of the firm as an insurance provider and as an internal credit market. Our previous work (Guiso *et al*. 2005) shows evidence of the role of the firm as an insurance market against idiosyncratic shocks to a firm's productivity. This article shows that credit can take place within the firm and establishes that there is substantial lending flowing from workers to firms, with its size related to the degree of credit market frictions. Thus, the evidence we provide lends empirical support to the idea that the employment relationship helps tempering the adverse consequences of poorly functioning credit markets.

While we have focused on financial trades between employees and employers, the same factor that makes these trades possible—the human capital specificity that ties workers to their company—might also facilitate intertemporal monetary exchanges between workers in the same firm. The literature has so far studied the first type of exchanges, but the latter may, quantitatively, also be relevant. Even more interestingly, the possibility of financial exchanges between workers may reinforce financial exchanges between employees and employers: knowing that she can borrow from her colleagues when facing an adverse shock, a worker may be more willing to lend to her employer. We regard this as an interesting topic for future research.

APPENDIX A: DATA DETAILS

Our initial workers sample is composed of social security records for individuals aged 18–60 years with nonmissing social security codes, positive reported earnings, and consistent monthly employment codes (642,362 records and 112,303 individuals). A worker may have multiple social security contribution records in a given year (if, say, she had multiple employers in that year). We drop those with multiple concurrent jobs (because moves are hard to identify), those who receive social security contributions from a firm after it goes bankrupt (because it may signal a merger or acquisition rather than a closure), and those who have spells at a given firm separated by intervening spells at other firms (because the concept of tenure is not very clear cut). These selections reduce our sample to 508,333 records and 96,443 individuals. We drop individuals who have one or more outlier monthly earnings records (a decline greater than 70 percent or an increase greater than 400 percent). We lose 35,420 records and 6,095 individuals. Since we need to estimate wage growth equations, we also drop workers observed for only one year (15,596 records and individuals). Finally, we eliminate records with missing information on the province of work, because we cannot match them to information about local credit market imperfections (2,651 records).[39] Our final sample consists of 454,666 records corresponding to 74,519 individuals.[40]

The firm data are collected by the Company Accounts Data Services, which was established in the early 1980s jointly by the Bank of Italy, the Italian Banking Association, and a pool of leading banks to gather and share information on borrowers. Since the banks rely heavily on it in granting and pricing loans, the data are subject to extensive quality controls by a pool of professionals, so measurement error should be negligible. While the CB data are reasonably representative of the entire population in terms of distribution by sector and geographical area (Guiso and Schivardi 2007), the focus on level of borrowing skews the sample toward relatively larger firms: CB reporting firms account for approximately half of total employment and 7 percent of the number of firms in manufacturing.

APPENDIX B: CREDIT FLOWS DEFINITION

To compute the total debt toward a worker with tenure T, note that the firm has been borrowing from such worker $\mathbf{Borrowing}_{p0}$ in the first month of employment, $\mathbf{Borrowing}_{p1}$ in the second month, and so on. Given the interest rate r, the present value of $\mathbf{Borrowing}_{p0}$ is $(1+r)^T_{p0}\mathbf{Borrowing}_{p0}$ and similarly for subsequent flows. The cumulative borrowing from such worker is therefore:

$$\mathbf{Cumulative\,Borrowing}_{pT} = \sum_{s=0}^{T}(1+r)_{pt}^{(T-s)}\mathbf{Borrowing}_{ps}.$$

Following the results on the IRR, we will use an annual interest rate of 3 percent. Results are not very sensitive to alternative values of r, given that the average tenure (weighted by the share of workers at each tenure level) is about 6 years. $\mathbf{Cumulative\ Borrowing}_{pT}$ reaches a maximum at T^* after which the firm starts repaying the worker and $\mathbf{Borrowing}_{pT}$ turns negative. The maximum stock of debt that a firm can accumulate form a worker is therefore $\mathbf{Maximum\,Borrowing}_p = \mathbf{Cumulative\,Borrowing}_{pT^*}$.

The basic idea we use to construct a measure of borrowing from the workers can be illustrated with a simple example. Consider a model in which workers work for two periods and define them as junior in the first and senior in the second period. Junior workers lend implicitly to the firm, which pays them back when senior. A firm born at time 1 hires a junior worker, from whom it borrows implicitly B; at time 2, the firm hires another junior worker, from whom it also borrows B, and pays back the "loan" to the former junior workers, now turned senior; from period 3 onward, the senior worker retires, the junior turns senior and is paid back and the firm hires another junior worker from whom it borrows. In this scheme, starting in period 2, the flows of borrowing and repayments are equal in NPV terms. Still, the firm has an outstanding stock of debt equal to B, because in the first period it received the implicit loan B without any repayment being made. This loan will be repaid when the firm stops hiring in which case, it will make the payment to the senior worker without borrowing from the junior one. Until then, the firm is borrowing from its workers the amount B. This idea can be generalized to a firm with a given steady-state distribution of workers' tenures. We consider an hypothetical firm with a unit mass of workers, with a tenure distribution equal to the actual tenure distribution at the provincial level. Recall that the firm's borrowing from a worker with tenure T is $\mathbf{Cumulative\,Borrowing}_{pT}$. The total stock of debt for the hypothetical firm with one "representative" worker is therefore:

$$\mathbf{Implicit\,borrowing\,per\,worker}_p = \sum_{T\le T^*}\omega_{pT}\,\mathbf{Cumulative\,Borrowing}_{pT}.$$

Implicit borrowing per worker represents the stock of debt that the firm has accumulated toward workers with seniority smaller or equal to T^*, where each seniority level is weighted according to the actual seniority distribution in the province. Given that this hypothetical firm has one representative employee, the value is by construction in *per capita* terms.

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39. We impute the province of work when we find clear coding errors.

40. Note that, given that mobility requires knowledge about current and past employers, the first observation for each worker is lost and hence the probit regression in Table 3 uses only 380,380 observations. The wage growth regression uses only 329,772 observations because it conditions on staying with the same firm.

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