Pricing Behavior and the Response of Hours to Productivity Shocks*

Domenico J. Marchetti
Banca d'Italia, Research Department

Francesco Nucci
University of Rome "La Sapienza"

Abstract

Recent contributions have suggested that technology shocks have a negative impact on hours, contrary to the prediction of standard flexible-price models of the business cycle. Some authors have interpreted this finding as evidence in favor of sticky-price models, while others have either extended flexible-price models or disputed the empirical finding itself. In this paper we estimate a variety of alternative TFP measures for a representative sample of Italian manufacturing firms and on average find a negative effect of productivity shocks on hours. Using the reported frequency of price reviews, we show that the contractionary effect is stronger for firms with stickier prices and weaker or not significant for firms with more flexible prices. Price stickiness remains a crucial factor in the response of hours even if product storability or market power are allowed for. Our results hold under alternative assumptions for the stationarity of hours per capita.

*We thank Susanto Basu, Mark Bils, Jordi Galí, Saul Lach, Valerie Ramey, Plutarkos Sakellaris, Alessandro Secchi, Chad Syverson, Michael Woodford and seminar participants at the Bank of Italy, the University of Rome "La Sapienza", the CEPR-Banco de Espana-ECB Conference on "Prices, Productivity and Growth" and the 2004 NBER Summer Institute for helpful discussions and suggestions at different stages of our research. Of course, responsibility for any errors is entirely our own. The views in this paper are those of the authors and do not necessarily reflect those of the Bank of Italy. E-mail: domenico.marchetti@bancaditalia.it; francesco.nucci@uniroma1.it

1 Introduction

In recent years the comovement of technology and labor at business cycle frequencies has come under growing scrutiny. In standard flexible price models the correlation is positive because, after a technology shock, prices fall, aggregate demand increases and hours worked rise. By contrast, in a widely cited paper, Galí (1999) reported a negative correlation between productivity and labor, and interpreted it as evidence in favor of sticky-price models. Arguably, after a technology shock, if nominal rigidities prevent prices from falling as much as they would with flexible prices, aggregate demand remains stable or increases only modestly and firms may satisfy it by employing a smaller volume of inputs, which have become more productive. Later work has emphasized that this occurs unless monetary policy fully accommodates technological shocks by lowering interest rates (e.g., Dotsey, 1999).

Because of the important implications for business cycle theory, Galí's results have fueled a growing debate in the literature. On the one hand, a number of authors have provided evidence that corroborates the finding of a negative response of labor input to technology shocks. In particular, while Galí (1999) estimated a structural VAR on productivity and hours and identified technology shocks as those having a permanent impact on productivity, Francis and Ramey (2002) extended Galí's identification scheme by imposing additional long-run restrictions and considering a wider set of variables. Basu, Fernald and Kimball (2004) developed an extended production function framework with proxies for changes in unobserved capital and labor utilization.[1]

On the other hand, several contributions have either disputed Galí's empirical finding or challenged his theoretical interpretation. On empirical grounds, Christiano, Eichenbaum and Vigfusson (2003a) have argued that the negative correlation found in the above studies is driven by over-differencing of the hours worked data. If hours per capita are assumed to be stationary and the level of this variable is considered, a positive effect of technology on hours is found (this contribution, in turn, has stimulated a debate on the statistical properties of hours per capita; see Francis and Ramey, 2004a and b, Fernald, 2004, and Galí, 2004). A positive effect of productivity shocks on hours is also found by Chang and Hong (2003), who use

[1] Other contributions include Shapiro and Watson (1988) and Shea (1998); see, also, Galí and Rabanal (2004) for a detailed summary.

data of US 4-digit manufacturing sectors, and Fisher (2002), who allows for investment-specific technological shocks in addition to neutral technological shocks.[2]

On theoretical grounds, a variety of alternative explanations of Galí's finding are consistent with flexible prices. One class of possible explanations refers to mechanisms through which the adoption of technological progress may somehow disrupt current production, eventually resulting in a decrease in worked hours. For example, reaping the benefits of productivity improvements may require the replacement of existing equipment (Cooper and Haltiwanger, 1993), changes in the labor organization (Hall, 2000), retraining of the firm's labor force (Campbell, 1998) or reallocation of labor across firms (Davis and Haltiwanger, 1990). Another type of explanation, suggested by Francis and Ramey (2002), calls for habit formation in consumption and capital adjustment costs. In this model, aggregate demand is largely unaffected by technology shocks (so that firms would employ fewer workers to produce the same amount of output) because consumers have inertial behavior. In principle, another alternative explanation of the contractionary effect of productivity innovations hinges on market power. A firm with high market power is expected to face an inelastic demand. Even if prices are fully flexible, a low price elasticity of demand may cause output to increase modestly after the price reduction induced by a productivity shock; accordingly, labor input, which has become more productive, might decline.[3] Moreover, the relevance of price stickiness in the transmission mechanism of technology shocks has recently been questioned by Chang, Hornstein and Sarte (2004). As originally suggested by Bils (1998), they argue that the labor response to a productivity improvement depends crucially on the degree of storability of the firms' products. In particular, following a technology shock, demand may increase only modestly because of price stickiness, but, if goods are storable and the cost of holding inventories is not too high, firms may still choose to increase output and therefore employment.

We contribute to the debate by exploiting the unusual richness of a detailed dataset on a representative sample of Italian manufacturing firms. We

[2] See also Ulhig (2004), where productivity shocks are identified in a principal component perspective.

[3] Another possible explanation has been suggested in an open economy framework by Collard and Dellas (2003). If substitutability between domestic and foreign goods is low, a domestic technology shock drives down the prices of domestic goods relative to those of foreign goods, thus discouraging domestic output and employment growth.

find on average a negative response of hours worked to a productivity shock. Moreover, we use data on the reported frequency of price reviews to discriminate between sticky and flexible-price interpretations of this result. We find that the contractionary effect is strong for firms with stickier prices, while it is weaker or not significant for firms with more flexible prices.

In contrast with a previous contribution (Marchetti and Nucci, 2005), where we focused on one particular approach to productivity measurement (Basu et al., 2004), in this paper we compute a variety of different TFP measures, that together span a large spectrum of theoretical assumptions and models. These estimates are the standard (revenue-based) Solow residual, the cost-based Solow residual and a model-based measure proposed in the industrial organization literature (Olley and Pakes, 1996). For comparative purposes, we also include Basu et al.'s estimate of productivity.

In assessing whether the response of hours to a productivity shock depends on the degree of price stickiness, we investigate whether our results survive once alternative explanations or additional transmission mechanisms are explicitly allowed for in the empirical framework. In particular, we analyse the role of products' storability and inventory holdings, and their alleged role in shaping the response of labor to technology shocks. Furthermore, we assess the relevance of market power versus nominal rigidity as an alternative explanation of the contractionary effect of productivity shocks. We do so by using survey data on the price elasticity of demand reported by each firm.

Finally, we investigate whether our finding is a figment of a specification error due to over-differencing of hours. As mentioned above, according to Christiano et al. (2003a) hours per capita is a stationary variable; therefore, its level should be considered in the empirical analysis rather than its first difference. In light of this controversy, we document the response of hours per employee to productivity shocks under alternative assumptions on the stationarity of hours. We also report empirical findings where the number of workers is used as a measure of labor input.

The remainder of the paper is organized as follows. Section 2 describes the data and documents the relevance of price rigidity across different sectors and degrees of market concentration. Section 3 discusses the various TFP measures used in the empirical analysis. Section 4 investigates the response of hours to productivity innovations and the role of price stickiness. Section 5 controls for alternative explanations of the contractionary finding and for additional shock propagation mechanisms. Section 6 deals with the robustness of the results, in particular with respect to alternative assumptions on the stationarity of hours per employee. Section 7 draws some conclusions.

2 The Data

2.1 Data sources

We use comprehensive panel data on a representative sample of Italian manufacturing firms. The main source is the Survey of Italian Manufacturing (SIM), carried out annually by the Bank of Italy. The data are of unusually high quality, being directly collected by interviewers who are officials of the local branches of the Bank of Italy, and often have a long-standing work relationship with the firm's management. Each year since 1984 roughly 1,000 firms have been surveyed; because of entries and exits, the balanced panel consists of almost 300 firms. Sample composition is mantained by the statisticians of the Research Department of the Bank of Italy to ensure representativeness with respect to the whole manufacturing sector in terms of composition by branch, firm size and geographical location. Data drawn from SIM include figures on employment and hours, labor compensation, investment and capital stock, plus qualitative information on a number of variables that are crucial for economic analysis but are hard to find in the existing surveys. These variables include the typical frequency of price reviews, the extent of the firm's market power and the degree of concentration of its main market.

Data on gross production (sales plus inventory change), purchases of intermediate goods and inventories of finished goods are drawn from the Company Accounts Data Service (CADS), which is the most important source of balance sheet data on Italian firms. It covers about 30,000 firms and is compiled by a consortium that includes the Bank of Italy and all major Italian commercial banks.

Merging the SIM and CADS datasets resulted in an unbalanced panel of almost 1,000 firms and 8,000 observations, ranging from 1984 to 1997. The period considered includes three manufacturing-wide expansions (1985-1990, 1994-1995 and 1997) and two recessions (1991-1993 and 1996). Further details on data sources and the definition of the variables can be found in Appendix I.

2.2 Empirical regularities on price stickiness

The information on the degree of price stickiness characterizing the individual firms of our sample was provided by the replies to a question included in the 1996 SIM survey. Firms were asked the following question, with reference to their main product: "How frequently does your firm typically review selling prices?". The managers interviewed could choose among five possible responses: "Several times a month", "Every month", "Every three months", "Every six months" and "Once a year or less frequently". The replies obtained from 955 firms are summarized in Table 1, first row. The survey found that roughly 30 per cent of the firms typically reviewed prices every three months or more often, 35 per cent every six months and another 35 per cent of firms once a year or less often. Therefore, the median frequency of price reviews is twice a year, as in the case of the US firms surveyed by Blinder, Canetti, Lebow and Ruud (1998) and somewhat lower than the quarterly frequency reported for UK manufacturing firms by Hall, Walsh and Yates (2000).[4] In principle, for the purposes of this paper, information on the frequency of actual price changes (or, better yet, on the time elapsed between a shock and the corresponding price revision) would be preferable as measure of price stickiness, since the frequency of price reviews is only one aspect of firms' pricing behavior, though an important one. Unfortunately, such information is not provided by the 1996 SIM survey. However, Blinder et al. (1998) document a strong positive correlation at the firm level between the frequency of price reviews and that of price changes (see also Hall et al., 2000, Table 1). Indeed, the Bank of Italy interviewers who conducted the survey used in this paper reported that the re-examination of prices had often coincided with their actual change. Furthermore, the data of the recent survey conducted by the Bank of Italy on a different sample of Italian manufacturing firms confirm a close relationship at the firm level between the frequency of price reviews and that of actual price changes.[5] The evi-

[4] The median frequency of price reviews reported in the SIM survey is also broadly consistent with that reported in another survey of Italian firms, recently carried out by the Bank of Italy as part of a reserch project on inflation persistence launched by the euro-area Central banks (Eurosystem Inflation Persistence Network; see Fabiani, Gattulli and Sabbatini, 2004). According to this survey, 55 per cent of the manufacturing firms typically review their prices once a year, 45 per cent quarterly or more often.

[5] The correlation, measured by the Goodman-Kruskal (1954) gamma statistic, is .63 (with an asymptotic standard error of .09). The Goodman-Kruskal statistic is a measure of association relevant for ordinal variables; like the conventional correlation coefficient, it

dence on price reviews reported in this paper is also broadly consistent with that on price changes reported in the literature, which points to an average frequency of 1-2 price changes per year, depending on the country, the sector and the type of product and survey.[6]

In Table 1 we also document the frequency of price reviews disaggregated by category of industrial product and sector of economic activity. Firms producing consumer goods do not review prices more often than producers of intermediate and investment goods. At sectoral level, food and textiles and apparel are the branches characterized by more frequent price reviews (consistently with the evidence reported by Kashyap, 1995, and Bils and Klenow, 2004). On the other hand, price reviews are less frequent, and prices are presumably stickier, among firms producing transportation equipment, nonferrous metals, machinery, electric machinery and chemicals.

We also find that firms operating in more competitive markets review prices more often, as in the case of the UK firms surveyed by Hall et al. (2000) and consistently with the evidence on US price changes reported by Carlton (1986). The intuition is that the consequences (in terms of lower profits) for setting an inappropriate price are more severe in markets where demand is more sensitive to prices and competition is stronger.[7] Table 2 reports the main results. According to all measures of market power and market competition considered, firms operating in more competitive markets or having a lower degree of market power tend to review prices more often.

ranges from -1 to 1. The survey is described by Fabiani et al. (2004).

[6]Earlier contributions such as Carlton (1986), Cecchetti (1986) and Kashyap (1995) indicate average spells of price rigidity equal to approximately one year or more (the latter in the case of magazine prices, reviewed by Cecchetti; see Taylor, 1999, for a comprehensive review). More recent contributions, such as Blinder et al. (1998), Hall et al. (2000) and Bils and Klenow (2004) report, on average, somewhat shorter spells of price rigidity (roughly equal to, respectively, eight, six and five months). The scope for comparing the results of the cited studies is limited, however, since some contributions refer to prices of single firms, others to prices of single products, others to disaggregated consumer price indices.

[7]The degree of market competition and the firm's market power are measured, respectively, by the share of market sales of the largest four firms (so-called four-firm ratio), the price elasticity of demand perceived by the firm, the firm's own position in the market (i.e., leader, among the top four firms, among the top ten firms) and a standard measure of the firm's markup (i.e., the ratio of production value minus labor compensation minus nominal cost of materials over production value; see e.g. Domowitz, Hubbard and Peterson, 1986). All measures but the latter one were drawn from firms' replies to specific questions in the 1996 SIM survey.

For example, 33 per cent of firms facing a perceived demand elasticity greater than the mean (in absolute value) review prices at least every three months, compared with only 24 per cent of the other firms.

3 Our measures of productivity change

In this paper we employ four alternative measures of total factor productivity (TFP) growth, namely the Solow residual, in both the revenue-based and cost-based versions, and two model-based measures, proposed respectively by Basu and Kimball (1997) and Olley and Pakes (1996). The revenue-based Solow residual is the most utilized measure of TFP growth since the pioneering work of Solow (1957). The other measures represent some of the main attempts in the literature to overcome its shortcomings, either on theoretical or empirical grounds or both. Together they span a wide range of theoretical assumptions, satisfying most of the properties of an ideal measure of TFP growth. All the measures used in this paper are computed at the firm-level, to avoid the well-known aggregation bias which is likely to affect estimates obtained from aggregate data (Basu and Fernald, 1997). Furthermore, we adopt a gross-output rather than value-added framework, to avoid potential model mispecification and omitted variable bias (Basu and Fernald, 1995). Below we briefly introduce the four TFP measures.[8]

Consider a firm's production function subject to a technology disturbance, where gross output, Y, is produced using labor, capital and intermediate

[8] In the approaches followed in this paper productivity is measured as a "residual", i.e. the portion of output which is unaccounted for by the change in inputs. An alternative approach to the measurement of productivity shocks is that based on long-run restrictions in a structural VAR model, proposed by Galì (1999) and used, among others, by Francis and Ramey (2002) and Christiano et al. (2003a). Technology shocks are identified as the only shocks which have a permanent effect on labor productivity. This approach has the advantage that the resulting estimates of productivity shocks are, by construction, orthogonal to demand variables. On the other hand, a disadvantage is that any non-technology shocks with permanent effects on productivity, such as a change in capital income tax, are spuriously labeled as "technology shocks" under this identification scheme. Furthermore, Faust and Leeper (1997) have shown that the results of long-run restrictions are crucially affected by the number of variables included in the VAR and the assumptions on their time series properties. See Christiano, Eichenbaum and Vigfusson (2003b) for a combination of the "residual"-based approach to productivity measurement and the VAR-based approach.

inputs:

$$Y = F(L, K, M, Z), \tag{1}$$

where L is the labor input, measured by the product of the number of employees, N, and the number of hours per worker, H, i.e. $L = NH$; K is the capital stock; M is the quantity of materials and energy inputs and Z is an index of technology; time subscripts are omitted for simplicity.

With competitive goods and factor markets, perfect factor mobility and constant returns to scale, profit maximization implies that productivity change can be expressed as:

$$dz = dy - s_L dl - (1 - s_L - s_M)dk - s_M dm, \tag{2}$$

where lower-case letters represent logs, s_X is factor X's share of the firm's revenues and the output elasticity to technology has been normalized to one. Expression (2) is the Solow residual; the estimate used in this paper is henceforth denoted as sr.

The Solow residual has been extensively used in the literature, particularly in its value-added version, because the methodology is simple and the data required are readily available. However, to the extent that the underlying assumptions listed above are violated, the Solow residual reflects other economic phenomena besides productivity change, since it is affected by any shock that changes the optimal mix of output and input.[9] These considerations have induced Hall (1988 and 1990) and others to allow for market power and increasing returns. In particular, if one controls for imperfect competition in the product market, expression (2) becomes:

$$dz = dy - c_L dl - c_K dk - c_M dm, \tag{3}$$

where c_X is the cost-based share of factor X; this is the so-called cost-based Solow residual. The computation of c_L, c_K and c_M requires estimates of the imputed cost of capital. The estimate of the cost-based Solow residual used in this paper is henceforth denoted as $cbsr$.[10]

[9] In fact, contrary to the predictions of the underlying theory, the Solow residual is typically closely correlated with demand variables, such as military expenditure (Hall, 1988), monetary aggregates (Evans, 1992) and government consumption (Burnside, Eichenbaum and Rebelo, 1993).

[10] We used firm-level estimates of the user cost of capital, obtained by applying Auerbach's (1983) version of the Hall-Jorgenson approach to highly-disaggregated data (see Appendix 1 for details).

A further extension is to allow for increasing returns to scale, by estimating the scale elasticity parameter γ in the following regression:

$$dy = \gamma(c_L dl + c_K dk + c_M dm) + dz, \tag{4}$$

where dz is the regression residual.

Although an estimate of productivity change obtained by estimating equation (4) constitutes an important refinement with respect to the traditional Solow residual, it may still incorporate significant measurement errors in labor and capital inputs. In particular, if there are adjustment costs in hiring and firing and in capital accumulation, the unobserved rate of utilization of labor and capital is likely to fluctuate over time. Basu and Kimball (1997) have proposed a number of proxies for capturing these fluctuations, derived from a model of cost minimization with adjustment costs in labor and capital. The Basu and Kimball regression equation is an augmented version of (4):

$$\begin{aligned} dy &= \gamma dx + \beta(c_L dh_{it}) + \eta\left[c_K\left(dp_M + dm - dp_I - dk\right)\right] \\ &\quad +\theta\left[c_K(di - dk)\right] + dz, \end{aligned} \tag{5}$$

where dx represents the weighted average of changes in the observed inputs (i.e. $dx = c_L dl + c_K dk + c_M dm$); di is investment growth, dp_I and dp_M are the rate of growth of the price of, respectively, capital and intermediate goods; dz is, again, the regression residual, which corresponds to a very refined measure of productivity growth. The measure computed in this paper is henceforth referred to as bk.[11]

A different approach has been taken by Olley and Pakes (1996). In their analysis of the US telecommunication equipment industry, they propose an algorithm to explicitly address two different problems. The first problem is the traditional simultaneity bias, which arises in production function regressions because the unobserved productivity shock is typically correlated with factor demand; the second problem is the selection bias that arises because firms' shutdown decisions may be endogenously affected by productivity.[12]

[11] Following Basu et al. (2004), it was obtained by estimating equation (5) separately for durables and non-durables sectors and allowing for sector-specific returns-to-scale parameters, γ (see Appendix 2 for details).

[12] For example, a larger capital stock is associated, *ceteris paribus*, with larger profits

Olley and Pakes propose a multi-step procedure. In the first step, the simultaneity bias is taken care of by including in the regression proxies of the unobservable productivity term, derived from a model of firms' optimizing behavior (namely, investment and capital). In the second step, firms' survival probability is estimated and used to extract information on expected productivity and its relationship with capital accumulation. This information is used in the third step to control for the effect of expected productivity on the capital coefficient. Productivity growth can then be computed as:

$$dz = dy - \widehat{\beta}_L dl - \widehat{\beta}_K dk - \widehat{\beta}_M dm \quad (6)$$

where $\widehat{\beta}_L$ and $\widehat{\beta}_M$ are consistently estimated in the first step and $\widehat{\beta}_K$ in the third step (see Appendix 3 for details). In the rest of this paper the TFP measure computed according to equation (6) is denominated *op*.[13]

The main descriptive statistics and cyclical properties of *sr*, *cbsr*, *bk* and *op* are summarized in Table 3. A notable feature displayed in the table is the similarity in the distribution of the alternative measures, despite the different underlying assumptions and models. The median values of TFP growth range from .7 to 1 per cent per annum, whereas the 25-th and the 75-th percentiles are all around -2.5 e 4 per cent, respectively. Unsurprisingly, the Solow residual in both versions is the most procyclical measure, as shown by the coefficients estimated by regressing each measure of TFP growth on GDP growth. Procyclicality is significantly reduced — by at least one half according to this criterion — by controlling for unobservable factor utilization as shown by Basu and Kimball; it basically disappears if productivity

and this may increase firms' ability to survive after a negative productivity shock, thus affecting sample composition and the observed relationship between capital endowments and productivity realizations.

[13]Some concerns about the Olley-Pakes approach have been raised by Syverson (1999). He argues that when demand conditions have a significant idiosyncratic component, they may affect investment decisions besides productivity. In this case, the Olley-Pakes algorithm may provide inconsistent parameter estimates as well as productivity measures that are a mixture of demand and technology components. Syverson argues that this potential problem is more severe in imperfectly competitive markets, where the degree of specificity of firms' demand is likely to be higher. To tackle this issue, as a sensitivity exercise, we re-applied the Olley-Pakes approach only to firms with lower market power, i.e. those reporting a price elasticity of demand higher in absolute value than the median. We replicated all the empirical investigations of this paper focusing on this sub-sample only, for which the Syverson critique applies to a minor extent. Overall, the results obtained were qualitatively unchanged.

is measured following Olley and Pakes.[14] Further insight is provided by the cross-correlation pattern shown in Table 4. Interestingly, and to some extent surprisingly, all the measures are strongly correlated, with correlation coefficients ranging from .80 to .94.

The combined evidence of Tables 3 and 4 suggests that, on the one hand, the bulk of the underlying dynamics of productivity is captured by all the TFP measures and, on the other, each measure captures some (cyclically relevant) components of productivity which are missed by the other measures (or, symmetrically, is free of some noise or measurement error possibly included in other measures). There are no clear grounds for sistematically preferring any given measure to the others. Which one is the most appropriate will ultimately depend on the appropriateness of the respective assumptions and the accuracy of the relevant data for the firm and period being considered. For this reason, and for the sake of robustness, throughout the remainder of the paper we report the results obtained with all the four measures.

4 The response of hours to technology shocks

We employed the information on the firms' pricing behavior reported in Section 2 and the measures of TFP growth described in Section 3 to estimate the relationship between technology shocks and labor input, and investigate whether it is affected by the degree of price stickiness. The controversy on the sign of such relationship and its implications for business cycle models motivates our investigation.

As mentioned before, in a sticky-price model the prediction of a negative response of labor to technology shocks depends on the type of monetary policy assumed. For example, with a Taylor (1993) or a Clarida, Galí and Gertler (2000) rule, in the wake of a technology improvement monetary policy "mimicks" the expansionary effect of declining prices by fully accommodat-

[14]The coefficient estimated by regressing *op* on GDP growth remains positive, but has no statistical significance. On the one hand, this result might suggest that, after properly controlling for both the simultaneity and selection biases, much, if not all, of the procyclicality of measured productivity vanishes. Alternatively, one might argue that the proxy suggested by Olley and Pakes fails to capture cyclical fluctuations of productivity adequately (see Levinsohn and Petrin, 2003, for reasons why this might happen). We followed Levinsohn and Petrin's insight and used intermediate inputs as a proxy for unobserved productivity; however, the estimates of the main parameters fail to converge if polynomials of reasonably low degree are used.

ing the shock, and this induces a significant increase in output and labor. In such cases, the response of labor to a technology shock in a sticky-price model is observationally equivalent to that in a flexible-price model. From this point of view, the data used in this study are particularly suitable. In the period considered (i.e., the second half of the eighties and the first half of the nineties), monetary policy in a number of European countries including Italy was constrained by German monetary policy (e.g. Clarida, Galí and Gertler, 1998); in that period, therefore, domestic productivity shocks in Italy were very unlikely to be fully accomodated by the central bank.[15] Furthermore, our use of firm-level data significantly reduces the relevance of monetary policy in the investigation. The reason is that monetary policy may accommodate aggregate productivity shocks, but can hardly respond to firm-specific shocks, unless they have a very large common component. These considerations motivate our empirical investigation, to which we now turn.

4.1 Results for the whole sample

We first document the response of labor to productivity shocks for the entire sample. As in Basu et al. (1998) and Marchetti and Nucci (2005), the innovations, $\varepsilon(.)$, to our series of productivity change (i.e. sr, $cbsr$, op and bk) are obtained by estimating an AR(2) model for each of them. Table 5 presents the results obtained by regressing change in total hours on each of these innovations.

The findings document a negative impact of productivity shocks on hours growth. Looking at regressions with only the contemporaneous productivity innovation, the effect is always negative and statistically significant. The estimated effect tends to be larger for the innovations to the revenue-based Solow residual, $\varepsilon(sr)$: the estimated parameter is -.306 (with a standard error of .032) while it is -.135 (s.e. of .042) for the Olley-Pakes measure, -.109 (s.e.

[15] In particular, Dotsey (1999) shows that if the central bank follows a modified Taylor rule, responding to output growth rather than to deviations of output from its potential level, the response of labor to technology shocks is closer to that obtained under a constant money growth rule (which is negative if prices are sticky). Monetary policy in Italy in the period of interest is described by Dornbusch, Favero and Giavazzi (1998) by means of a rule in which the short-term interest rate depends on the German short-term rate plus the difference in inflation and that in output growth between the two countries. This type of rule resembles closely the modified Taylor rule described above, which assigns zero weight to the domestic output gap.

of .033) for the Basu-Kimball productivity impulse and -.076 (s.e. of .030) for the cost-based Solow residual.[16]

All the results reported in Table 5 and the following ones refer to regressions which include, on the right-hand side, year, industry and size dummies. As a robustness check, we also ran regressions without these groups of dummies; the results remained virtually unchanged.[17] Moreover, in order to verify that the results of Table 5 are not simply due to omitted variable bias, we also ran regressions with some proxies of the firms' demand and supply conditions, such as their sales growth or sectoral output growth, included in the specification. Again, the results were qualitatively unchanged.

By adding lags of productivity innovations as regressors, we broadly document that the negative response of hours (though in most cases not statistically significant) is limited to the first period only, with a recovery occurring over time, presumably as the frictions responsible for the contractionary effect disappear.

Overall, Table 5 provides a picture that points to a negative correlation between labor and productivity. Thus, our evidence seems to reinforce the similar finding obtained in other contributions. However, as explained above, the interpretation of this result is problematic. While Galí (1999) and Basu et al. (2004) interpret it as evidence in favor of price stickiness, one can think of a number of alternative explanations which are consistent with flexible prices. These range from habit persistence in consumption to retraining, reorganization and reallocation effects, to market power.

The available empirical evidence typically does not allow a distinction to be made between flexible- and sticky-price interpretations of the contractionary effect of productivity shocks. In the following section, we use the survey information on pricing behavior to address this issue.

[16] In our panel regressions we use generated regressors, since the productivity measures on the right-hand side are generally obtained as residuals of production function estimation. However, if one includes unlagged generated residuals in a regression, the consistency and efficiency of the estimators are preserved and the validity of the standard inference is unaffected (see Pagan, 1984).

[17] The estimated parameters for these dummies and the results of the tests for their joint significance are available from the authors upon request.

4.2 The role of nominal rigidity

If the sticky-price explanation is correct, the observed relationship at the firm level between productivity impulses and labor input should differ depending on the slowness of the firm's price adjustments. Under the sticky-price interpretation, we would expect a stronger negative response of hours to technology the less frequent the price reviews (and, presumably, changes) at the firm level. Eventually, the response should turn positive if price reviews are frequent enough, i.e. if prices are sufficiently flexible.

Table 6 documents the regression results obtained separately from two sub-samples: the first consists of firms that typically review their prices every three months or more often; the second comprises firms that typically review their prices every six months or less often.[18] The results lend support to the sticky-price interpretation. Firms with stickier pricing behavior (i.e., less frequent price reviews) experience a sharper decline in hours associated with a productivity improvement. On the other hand, for firms that review their prices more frequently, the impact of technology shocks is typically positive or not statistically significant. For example, looking at regressions with only the contemporaneous TFP innovation, the impact effect of $\varepsilon(bk)$ on total hours growth is estimated to be -.150 (with a standard error of .054) for firms with stickier prices and -.013 for the other firms (with a s.e. of .075). Similarly, the estimated effect of the Olley-Pakes technological shock is equal to -.306 (with a s.e. of .070) in the sub-sample of firms with less frequent price reviews and -.016 (with a s.e. of .082) in the other sub-sample. In the case of firms with more flexible prices, even when the effect is negative and statistically significant (this occurs only in the regression with the contemporaneous value of $\varepsilon(sr)$), its absolute value is much lower compared to the case of firms with stickier prices. Considering regressions with a distributed lag of productivity impulses, the impact effect of technology innovations on hours is again always negative, and in general statistically significant, for firms with stickier pricing behavior, with a recovery of hours over time. By contrast, the effect of productivity shocks for firms with more frequent price

[18] In principle, it might be misleading to assess the degree of price stickiness of a given firm based only on the frequency of price reviews (or, for that matter, price changes), since this frequency is clearly affected by that of cost and demand shocks, which in turn depends on the specific market and production process characteristics. In order to control for this potential source of bias, we replicated the analysis described in this section by using as the splitting criterion the fact that a given firm reviews prices more or less frequently than the median firm in the same sector. The results remained substantially unchanged.

reviews is positive or not statistically different from zero. For example, when total hours growth is regressed on distributed lags of the innovations to the Olley-Pakes measure, the contemporaneous effect is -.220 (with a s.e. of .077) in the sample of firms with stickier prices and .191 (with a s.e. of .119) in the other sample. For robustness, we also employed another sample-splitting criterior and considered five different sub-samples, one for each possible answer to the survey question on the frequency of price reviews. The results broadly confirmed the general picture.[19]

5 Controlling for product storability and market power

5.1 Product storability

A recent line of research has questioned the importance of price stickiness in the transmission mechanism of technology shocks. By developing an idea first proposed by Bils (1998), Chang et al. (2004) argue that, with sticky prices, the response of labor input to a productivity shock depends on how much goods depreciate in storage and the cost of holding inventories. In particular, in the aftermath of a technology shock that reduces marginal cost, price stickiness may prevent demand from rising in the short run. However, if firms produce storable goods, they will increase output and build up inventories in anticipation of higher future sales and this would imply an employment increase even in the short run. By using U.S. manufacturing industry data, with information on average inventory holdings and durability of goods, Chang et al. provide evidence supporting their prediction.

The productivity shock propagation mechanism devised in their paper dwarfs the role of price stickiness and emphasizes that of inventory buildup. We took this issue seriously and tested its empirical relevance with our data. To this purpose, we extended our dataset by including information at the firm level on the end-of-period stock of nominal inventories of fin-

[19] The first sub-sample refers to firms reviewing prices several times a month, the fifth to firms reviewing prices once a year or less frequently. If, for example, total hours growth is regressed on the innovations to the Solow residual, the effect is .004 (s.e. of .187) in the first sub-sample and -.150 (s.e. of .144) in the second sub-sample. By contrast, the estimated effect is -.217 (s.e. of .093) in the third sub-sample and -.553 and -.164, respectively, in the fourth and fifth sub-samples (s.e. of .068 and .072).

ished goods and work in process (consistently with the theory, we excluded materials and other intermediate inputs). We then computed a firm-level, time-varying inventory-sales ratio, that we used as a proxy of product storability. We conducted the empirical investigation taking two different routes. First, conditioning on the presence of price rigidity, we investigated whether the different degree of storability gives rise to a different response of hours to productivity shocks. We did so by selecting the group of firms with stickier prices and then splitting it further on the basis of our storability proxy (the splitting criterion we used was the median of the average inventory-sales ratios of firms in this group, which is equal to .15). We ran our usual regressions separately for each sub-sample.

The results in Table 7a show that, for firms with stickier prices, the contractionary effect of technology shocks is found no matter what the degree of storability is. Indeed, for all the measures of productivity used, the negative effect on hours is statistically significant both in the subsample with a high inventory-sales ratio and in the other (with the partial exception of the cost-based Solow residual, for which the effect in the sample with a high inventory-sales ratio is negative but not statistically significant). Moreover, for each TFP measure the estimated effect is not statistically different across the two sub-samples. For example, with the Olley-Pakes productivity measure the response of hours is -.313 (with a s.e. of .086) in the sample of firms with high inventory-sales ratio and -.257 (with a s.e. of .097) in the other.

We also performed a second test. We conditioned on high product storability and investigated whether a different degree of price rigidity is still associated with a different labor response to a productivity shock. We first selected the group of firms with an average inventory-sales ratio greater than (or equal to) the overall median, split this sample on the basis of price stickiness and again regressed hours on productivity shocks separately for each sub-sample. The results are reported in Table 7b. The evidence is that, even considering firms with high product storability, a negative and, in general, statistically significant response of hours to productivity shocks is found for firms with stickier prices, while in general the estimated effect is not statistically significant in the other firms. These results show, on empirical grounds, that product storability does not significantly affect the role of price stickiness in shaping the response of hours to productivity shocks.

5.2 Market power

An alternative explanation of the contractionary effect of technology shocks, consistent with flexible prices, hinges on the presence of market power. This is usually measured by the price-marginal cost margin (Lerner index), which in turn depends upon the price elasticity of demand. Even if prices are fully flexible, an inelastic demand may cause output to increase modestly after the price reduction induced by a productivity improvement. Accordingly, labor input may decline if demand elasticity is low. Notice that, in Section 2, we documented that price stickiness is associated with a higher degree of market power and this finding is robust with respect to the use of alternative indicators of the latter variable. Hence, it could be argued that the reported different response of hours to productivity shocks depends on market power and demand elasticity rather than on price rigidity.

Our data allow us to investigate which of these two competing interpretations is predominant. In particular, as mentioned in a previous section, the survey data used in this paper contain figures on the price elasticity of demand perceived by each firm.[20] We combined this information with that on the frequency of price reviews and conducted two different tests. First, we conditioned on high market power and investigated whether a different degree of price stickiness continues to yield a different response of labor input to a productivity impulse. If the explanation based on market power is predominant, we should find a negative response no matter what the degree of firms' price rigidity is. In order to implement the test, we first selected the firms with high degree of market power, i.e. those with a price elasticity of demand smaller than (or equal to), in absolute value, the overall median (4). This sample was divided further into two subsamples according to the degree of price stickiness and the model was estimated separately for each group.

The empirical findings are reported in Table 8a. The picture which emerges is that, even if we consider only firms with a low price elasticity of demand, the estimated response of hours to technology shocks depends crucially on the degree of price rigidity. In particular, within this subsample of firms with inelastic demand, whilst the contractionary effect on hours is

[20]More precisely, firms were asked: "If you raised your selling prices by 10 per cent, what would be the change of your sales in nominal terms, assuming no change in the prices of your competitors and other things being constant?". Figures on the implicit price elasticity of demand were computed from the replies.

found for all the TFP measures when sticky-price firms are considered, for flexible-price firms, by contrast, the estimated effect is not statistically different from zero (with the exception of the regression with the revenue-based Solow residual, where the effect is negative and statistically significant). For example, the estimated response of hours to an innovation in the cost-based Solow residual is .090 (with a s.e. of .074) for firms with more flexible prices, while it is -.121 (with a s.e. of .057) in the subsample of firms with stickier prices.

We also constructed a second test of the market power interpretation. We selected the group of firms with more flexible prices and examined whether differences in market power across firms in this group led to a different estimated response of hours to productivity shocks. In this case, the explanation based on market power would be consistent with a negative response of hours when price elasticity is low and a positive response when it is high. On the contrary, the results indicate that the estimated effect on hours is, in general, statistically not different from zero or positive, regardless of the extent of market power (see Tab. 8b). This finding holds true across all the measures of productivity but one (the revenue-based Solow residual), thus suggesting that the factor prevailing is again the degree of nominal rigidity.

6 Other tests

6.1 Alternative assumptions on the stationarity of hours

While many contributions to the literature have provided alternative explanations of Galì's finding of a negative comovement of productivity and hours, Christiano et al. (2003a) have questioned the finding itself. Their argument is that the reported contractionary effect of productivity shock is a figment of a specification error due to over-differencing of hours worked. Because hours per capita is commonly assumed to be a stationary variable, its (log) level should be considered in the empirical analysis rather than its first difference or a detrended series. According to Christiano et al., the finding of a contractionary effect of productivity on hours is not robust because a re-estimation of the Galí-type of VAR, using the level of hours, yields a rise in hours after a technology shock.

The results of Christiano et al., in turn, have generated an intense debate on the statistical properties of hours, which is still open.[21] An investigation of this issue is beyond the scope of this paper. Furthermore, our empirical framework is very different from that of the above studies since we use a panel of firm-level data and do not use the VAR methodology. However, given its importance for the matter addressed here, we also examine the issue of the stationarity of hours per capita in our analysis. In particular, we alternatively assume that hours per employee are difference stationary, level stationary, stationary around a linear trend and stationary around a quadratic trend. In Table 9 we document the response of hours per employee to technology shocks under these alternative assumptions concerning the dependent variable and across all the measures of technology shocks used thus far. Overall, the picture emerging from the results is that productivity innovations seem to have a contractionary impact on hours per employee no matter what assumption is made concerning the stationarity of hours. In the whole sample, as well as in the subsample of sticky-price firms, the effect of productivity shocks on hours per employee is always negative, although it is statistically not significant in a number of cases. In particular, the contractionary effect is statistically significant with the Olley-Pakes and the revenue-based Solow measures, and this holds true across all the assumptions concerning the stationarity of hours. By contrast, the effect is negative but in general not statistically significant with the Basu-Kimball measure and the cost-based Solow residual.

6.2 The employment response

As an extension of our analysis we investigated the impact of technology shocks on employment (i.e., the number of workers) and, again, the role of price stickiness. A priori, regardless of whether the employment response is positive or negative, we would expect the estimated effect of productivity on employment to be smaller (in absolute value) than that on hours. The reason for this lies in the significant adjustment costs characterizing employment, which may induce firms to operate more intensively on the hours margin. This is especially true for the Italian economy, where rigidities in the labor market are widely documented to be important.

The estimation results are reported in Table 10. When the entire sample is considered, the estimated effect of productivity impulses on employment is

[21] See Francis and Ramey (2004a and b), Fernald (2004) and Galí (2004).

negative across all the TFP measures (although it is statistically significant with the revenue-based Solow residual and the Basu-Kimball measure and insignificant in the other two cases). When the subsample of firms with stickier prices is considered, the estimated effect becomes larger and is statistically significant in three of the four cases. In the other subsample, by contrast, the estimated employment response is not statistically different from zero across all the TFP measures. Importantly, as expected, if we compare the magnitude of the estimated employment response with the response of hours documented in Tables 5 and 6, the impact employment response is much smaller (in absolute value). This holds true for all the measures of productivity shocks. As argued before, labor hoarding provides a natural intuition for this empirical finding.

7 Conclusions

Recent contributions have suggested on empirical grounds that technology shocks have a negative short run effect on labor input, contrary to the predictions of standard flexible-price models of the business cycle. This finding is currently under debate; some studies seem to confirm it, others to reject it. Its interpretation is controversial, too. Some authors interpret it as evidence in favor of sticky-price models, while others have extended flexible-prices models in a number of ways, in order to generate predictions consistent with the evidence.

In this paper, we document a negative impact of productivity shocks on labor in a representative panel of Italian manufacturing firms. Furthermore, by combining time series of productivity and hours with information on pricing behavior, we shed some light on the empirical merit of sticky vs. flexible-price explanations of the finding. Given the complexity of productivity measurement, we do not rely on one specific estimate but, rather, derive a variety of TFP measures spanning a wide range of theoretical assumptions and empirical approaches. Nominal rigidity continues to play a crucial role in the propagation mechanism of productivity shocks even if we control explicitly for product storability or market power. Also, our results are robust with respect to alternative assumptions on the stationarity of hours per employee, an issue which has recently become central in this debate. Overall, while our evidence does not in itself rule out the relevance of mechanisms such as habit formation or retraining and reallocation, it indicates that price stickiness does count in driving the short-run contractionary effect of technology shocks reported in several contributions to the literature.

A Appendix 1: Data sources and description of variables

Data Sources. Data are primarily drawn from two sources: the Bank of Italy Survey of Investment in Manufacturing (SIM) and the Company Accounts Data Service (CADS). The SIM data have been collected since 1984. At the beginning of each year the firms included in the sample receive the questionnaire with questions referring to the year just ended. In order to ensure data consistency over time, the questions also refer to the previous year. Officials of the Bank of Italy conduct the interviews and it is their responsibility to verify the accuracy of the information collected. Sample stratification is based on sector of economic activity (three-digit Ateco-91 level), firm size and geographical location. Size refers to the number of employees and four classes are considered: 50-99, 100-199, 200-999, 1000+ employees; firms with fewer than fifty employees are not included in the SIM sample because it is more difficult to ensure high quality in the collection of their data. Firm location refers to the Italian regions (nineteen). Appropriate statistical techniques have been used in order to deal with outliers and missing data within the sample. CADS (*Centrale dei Bilanci*), a data service established by the Bank of Italy and a consortium of banks which are interested in pooling information about their clients, contains detailed financial statement data on around 30,000 Italian firms. The data have been collected since 1982 and are reclassified to ensure comparability across firms.

Industry classification. The industry detail considered in the analysis (for example, for the estimation of sectoral coefficients or the computation of sectoral means) refers to thirteen manufacturing branches: food and tobacco products; textiles and clothing; leather and footwear; wood and furniture; paper and publishing; chemicals; rubber and plastic products; non metallic minerals; metal products; machinery for industry and agriculture; electrical machinery (including computers and office equipment); transportation equipment and other manufactures.

Variable description. Gross output is measured as the value of firm-level production (source: SIM) deflated by the sectoral output deflator computed by ISTAT. Employment is the firm-level average number of employees over the year (source: SIM); firm-level manhours include overtime hours (source: SIM). Intermediate inputs are measured as firm-level net purchases of intermediate goods of energy, materials and business services (source: SIM), deflated by the corresponding industry deflator computed by ISTAT. Investment is firm-level total fixed investment in buildings, machinery and equipment and vehicles (source: SIM), deflated by the industry's ISTAT investment deflator. Capital is the beginning-of-period stock of capital equipment and non-residential buildings at 1997 prices. To compute it, we applied the perpetual inventory method backwards by using firm-level investment data from SIM and industry depreciation rates from ISTAT. The benchmark information is that on the capital stock in 1997 (valued at replacement cost), which was collected by a special section of the SIM Survey conducted for that year. The capital deflator is the industry capital deflator computed by ISTAT.

The series of the required remuneration of capital, rP_KK – used for the estimation of bk – was constructed using the firm-level, time-varying estimates of the user cost of capital computed at the Bank of Italy by De Mitri, Marchetti and Staderini (1998) on data drawn from both SIM and CADS. An additional statistical source for this variable is provided by the Credit Register (CR) data, which are collected by a special unit of the Bank of Italy (*Centrale dei Rischi*) and contain detailed information on firms' bank borrowing. De Mitri et al. (1998) adopted the Auerbach's (1983) version of the Hall-Jorgenson approach, which is specific to firms that are financed through both equity and debt. The expression for the user cost of capital is the following:

$$r = \frac{(1-S)}{(1-\tau)}\left[gi(1-\tau) + (1-g)e - \pi + \delta\right]; \qquad \text{(A. 1)}$$

τ is the general corporate tax rate. S refers to local and other specific tax rates, investment tax credits, depreciation allowances and any relevant subsidy, all of which are set to the appropriate firm-specific value according to Italian law in the given year and to a number of firms' characteristics; g is the firm-level ratio of financial debt over total liabilities (source: CR); i is the average debt interest rate paid by firms (source: CR); e is the required return to equity (i.e., the opportunity cost associated with holding part of firms' equity). It is approximated by the average yield of Italian Treasury bonds (BTPs), on the ground that the Italian equity premium has usually been estimated to be negligible, or even negative, during most of the period considered; π is the industry-specific expected increase of capital goods prices (source: SIM) and δ is the industry rate of capital depreciation (source: ISTAT).

B Appendix 2: Measuring productivity change à la Basu-Kimball (1997)

Since the rate of utilization of labor and capital is typically unobservable, in the empirical analysis one needs to express them as a function of observables, by adding structure to the model and exploring the equilibrium relationships between factor utilization and the firm's observable inputs. To this end, Basu and Kimball (1997) assume that cost-minimizing firms face adjustment costs in labor and capital, the employee is remunerated for his effort along with the number of hours worked, and capital depreciates at a rate which depends on its utilization. In particular, they formulate the following firm's cost minimization problem:

$$\underset{H,E,A,I,U,M}{Min} \int_0^\infty \left[NWG(H,E) + NW\Psi\left(\frac{A}{N}\right) + P_I KJ\left(\frac{I}{K}\right) + P_M M \right] e^{-rt} dt$$

subject to

$$Y = F(NHE, UK, M, Z)$$
$$\overset{\circ}{K} = I - \delta(U)K; \text{ and } \overset{\circ}{N} = A,$$

where W is the base wage; $WG(H,E)$ is the total compensation paid to each worker, which depends on both the number of hours and the level of effort and $NW\Psi\left(\frac{A}{N}\right)$ measures the adjustment cost of varying the number of workers; investment also encounters adjustment costs, which are captured by the function $J\left(\frac{I}{K}\right)$; the product of this term and $P_I K$ gives the expenditure for capital, where P_I is the price of investment goods; δ is the rate of capital depreciation, which is an increasing function of capital utilization, U; P_M is the price of intermediate inputs; and E and U are the rate of utilization of, respectively, labor (i.e., hourly effort) and capital.

First-order conditions for this problem are reported in Basu and Kimball (1997). Exploiting the resulting equilibrium relationships yields expressions for labor utilization as a function of hours per employee and for capital utilization as a function of investment, intermediate goods and their respective prices. After appropriate substitutions, one obtains the regression model (5) reported in the text:

$$
\begin{aligned}
dy &= \gamma dx + \beta(c_L dh) + \eta\left[c_K\left(dp_M + dm - dp_I - dk\right)\right] \\
&\quad +\theta\left[c_K(di - dk)\right] + dz.
\end{aligned}
\tag{A.2}
$$

As in Basu et al. (2004) and Marchetti and Nucci (2005), we estimated equation (A.2) separately for industries producing durables and non-durables , and allowed for sector-specific returns-to-scale parameters, as suggested by Burnside (1996). We also included dummies in the specification to control for time, sector, size and the occurrence of mergers and acquisitions. The estimation was conducted using the Arellano and Bond (1991) generalized method of moments (GMM) procedure, in order to take into account the correlation between input demand and the productivity residual. The instrumental variables used were the lagged values of the endogenous explanatory variables, dated period t-2 and t-3.[22] We also used external, demand-side instruments, which appear relevant on economic grounds, are presumably uncorrelated with firm-level technology variation and have been utilized in the literature (see, for example, Hall, 1988, Burnside, 1996, and Basu et al., 2004). These additional instruments are the rate of growth of sectoral materials prices, the rate of increase in the real exchange rate, the expected change in sectoral order-book levels (from the business surveys of ISAE − Institute for Economic Research and Analysis − a public body providing technical support to the Italian Treasury) and a measure of unanticipated monetary shock based on a vector autoregression (VAR) model.[23] Changes in intermediate input prices at the industry level are presumably not affected in a systematic way by firm-level technology change. On the other hand, factor prices should affect input use but, at the same time, do not shift the production function in the short run. The rate of change in the real exchange rate is likely to induce movements in world demand for Italian goods and therefore to be a powerful instrument, given the importance of exports for a small open economy such as Italy's. On the other hand, fluctuations of technical change are

[22] We truncated the set of these instruments at the third lag to attenuate the potential bias arising when all the available linear orthogonality conditions are exploited (Ziliak, 1997).

[23] The measure of monetary shock is obtained from a monthly recursive VAR model estimated over the period 1975-1997 by Dedola and Lippi (2000). The specification includes the industrial production index, the CPI, an index of commodity prices, the three-month interbank rate, the nominal effective exchange rate and M2.

not driven by exchange rate swings and the latter are unaffected by firm-level productivity shocks. The expected sectoral variations in the order-book level should capture future aggregate demand movements and are likely to be uncorrelated with the highly dispersed (across firms) technical changes.

The results are reported in Table A.1. The measure of technology variation bk used throughout the paper was obtained from these estimates; in particular, it was computed as the sum of the regression residuals and the parameters associated with the year, sector and size dummy variables. The latter were included in bk because, given our analytical framework, they capture the sector, the year and the size-specific components of firm's technological growth.

Returns to scale (i.e. parameter γ in equation A.2) were found to be constant in a majority of sectors (seven out of thirteen); estimates range from 0.86 in Other manufacturing to 1.14 in Chemicals. The other coefficients reported in the table can be used to derive the sectoral estimates of the structural parameters implied by the theoretical framework (see also Marchetti and Nucci, 2004). In all sectors the elasticity of effort with respect to hours per employee, ζ, was found to be negative, while the elasticity of the marginal depreciation of capital with respect to its utilization, Δ, was found to be positive, supporting the view that the depreciation function is convex. The marginal installment cost of capital was found not to be increasing with the rate of investment.

The instruments' validity was assessed through the Sargan statistic of over-identifying restrictions. It is worth noting that the results proved robust with respect to the choice of instrument. As a robustness check, we ran equation (A.2) after excluding the external instruments, either together or singly, from the set of instruments; the results remained qualitatively unchanged.[24]

[24] In addition, since we deflate nominal output at the firm-level using sectoral price indices, our estimates are potentially affected by the "omitted price bias" pointed out by Klette and Griliches (1996). We addressed this issue by using their correction and by following Muendler's (2001) insight, i.e. by adding sectoral output growth as regressor and including in the measure of bk the deviation of sectoral output growth from its time average, weighted by the sectoral price elasticity of demand (see Muendler, 2001, for details). While the estimates of returns to scale were considerably higher, as expected, the pattern of the comovement of the innovations to bk and labor was qualitatively unchanged (see Marchetti and Nucci, 2005).

Table A.1

Estimating results of equation (A.2): the Basu-Kimball Model

Specification:	Non-durables sectors	Durables sectors
dx (returns-to-scale parameter γ):		
Food and tobacco products	.974** (.017)	-
Textiles and clothing	.904** (.023)	-
Leather and footwear	1.034** (.068)	-
Paper and publishing	.865** (.032)	-
Chemicals	1.140** (.030)	-
Rubber and plastic products	1.135** (.030)	-
Wood and furniture	-	1.020** (.190)
Non metallic mineral products	-	.940** (.034)
Basic metals	-	1.008** (.034)
Machinery for industry and agriculture	-	1.004** (.022)
Electrical machinery	-	.996** (.039)
Transportation equipment	-	1.054** (.025)
Other Manufacturing	-	.861** (.032)
$c_L dh$	-.202**(.083)	-.420** (.070)
$c_K \left(dp_M + dm - dp_I - dk\right)$	.722** (.070)	.822** (.057)
$c_K(dhdh - dk)$	.025**(.010)	.027** (.010)
Sargan test of over-identifying restrictions	193.15 (192; .463)	218.48 (212; .365)
Wald test for weak instruments	707.16 (396; .000)	1041.16 (400; .000)

Legend: sample period 1984-1997; GMM estimation. Heteroschedasticity-consistent s.e. for parameter estimates are shown in brackets. The instrument set includes: lagged values of the endogenous explanatory variables at time t-2 and t-3; growth rate of intermediate input prices; rate of growth of the real exchange rate; variation of sectoral order-book levels drawn from the ISAE business survey; a VAR-based measure of monetary shock. For the Sargan test, degrees of freedom and p-values are reported in brackets. The specifications include time, sectoral, size and major corporate operations dummies; Wald test results (not reported) indicate that the dummies of each group are found to be jointly statistically significant.

**Significant at the 5-percent level.

C Appendix 3: Measuring productivity change à la Olley-Pakes (1996)

This appendix closely follows Olley and Pakes (1996). In measuring productivity, they address the issue of simultaneity and selection bias. To do this, they first approximate unobserved productivity semiparametrically and get consistent estimates of the part of the production function unaffected by it; they then estimate the exit behavior of firms to extract information on the relationship between expected productivity and capital accumulation. Finally, by controlling for this effect, they obtain consistent estimates of the capital coefficient.

The Olley-Pakes model is slightly modified here to fit the case in which firms' production is measured as gross output, rather than valued added, and intermediate inputs are therefore included in the production function, in addition to capital and labor. Firms are assumed to use the following Cobb-Douglas technology:

$$y = \beta_0 + \beta_A a_t + \beta_L l_t + \beta_K k_t + \beta_M m_t + \omega_t + \eta_t, \tag{A.3}$$

where a is the firm's age, ω and η are unobservable productivity disturbances.While ω is known to the firm when it decides how much labor to use (i.e. it is a state variable in the firm's optimization problem), η is not known.[25] In each period firms decide whether to stay in business or shut down; in the first case, they also choose the amount of variable factors (labor and intermediate inputs) and the level of investment.

Firms optimize by comparing the sell-off value they would receive if they sell their plants with the expected discounted value of future net cash flows attainable if they continue operations. The equilibrium is characterized by an exit rule $\chi_t(a_t, k_t) = \{0, 1\}$ and by an investment rule $i_t = i(a_t, k_t, \omega_t)$. The exit rule is such that firms continue operations (i.e., $\chi = 1$) if $\omega_t \geq \underline{\omega}(a_t, k_t)$. If the profit function π is increasing in capital, then $\underline{\omega}$ is decreasing in capital. The intuition is that firms with larger capital stocks are likely to generate larger profit flows, *ceteris paribus*, and are thus better equipped to survive after a low productivity shock. This generates a selection bias, which leads to an under-estimation of the capital coefficient in (A.3).

[25] An alternative interpretation of η is measurement error.

Provided that $i > 0$, the investment rule can be inverted, leading to an expression for unobservable productivity, ω_t, as a function of observables, i.e.

$$\omega_t = h(i_t, a_t, k_t). \tag{A.4}$$

This allows us to control, at least partially, for simultaneity bias in the estimation of equation (A.2); in particular, by substituting (A.4) into (A.3) gives:

$$y_t = \beta_L l_t + \beta_M m_t + \phi(i_t, a_t, k_t) + \eta_t \tag{A.5}$$

where

$$\phi(i_t, a_t, k_t) = \beta_0 + \beta_A a_t + \beta_K k_t + h(i_t, a_t, k_t). \tag{A.6}$$

Equation (A.5) can be estimated by approximating ϕ with a polynomial in (i, a, k); this is the first step of the Olley-Pakes procedure. In our estimation, we followed Olley and Pakes (1996) and used a fourth-order polynomial, after verifying that there was no significant change in the estimates going from a third to a fourth-order polynomial. The estimation of equation (A.5) provides consistent estimates of β_L and β_M; however, β_A and β_K remain unidentified. In order to identify them, estimates of the survival probabilities can be used:

$$P = \Pr\left\{\chi = 1 \mid \underline{\omega}_{t+1}, J\right\} = \varphi(i_t, a_t, k_t). \tag{A.7}$$

The probit estimation of equation (A.7) is the second step of the Olley-Pakes algorithm. We estimated the survival probability $\widehat{P}$ by approximating φ with a fourth-order polynomial in (i, a, k); as before, there was no significant change in the overall fit of the model going from the third to the fourth-order approximation. Like Olley and Pakes, we allowed for changes over time in exit behavior by including dummies for three different periods: 1985-1990 (continuing expansion throughout manufacturing industry), 1991-1993 (recession) and 1994-1995 (recovery).

The estimate of the survival probability yields information on the relationship between expected productivity and capital accumulation that generates the downward bias in the estimates of β_K. It can be shown that the conditional expectation of ω_{t+1}, which roughly represents the "bias" in the capital coefficient, can be expressed as a function of P_t and h_t, i.e. $g(P_t, h_t)$. We thus obtain the third-stage regression of the Olley-Pakes's approach:

$$y_{t+1} - \widehat{\beta_L} l_{t+1} - \widehat{\beta_M} m_{t+1} = \beta_A a_{t+1} + \beta_K k_{t+1} + g(\widehat{P}_t, \widehat{\phi}_t - \beta_A a_t + \beta_K k_t) + \eta_{t+1} \tag{A.8}$$

In estimating equation (A.8), which is nonlinear, we used a third-order polynomial approximation of $g(P, h)$. Since estimates of the parameters of interest did not change significantly going from the second to the third order and proved to be robust with respect to the choice of the starting values, the approximation is deemed to be accurate enough.

The main results of the whole estimating procedure are reported in Table A.2, where the first column refers to a simple regression of output on inputs (i.e., equation A.3), the second column refers to the Olley-Pakes first-stage regression (i.e., equation A.5) and the third column refers to the Olley-Pakes third-stage regression (i.e., equation A.8), where the coefficients on labor and intermediate inputs are derived from the first stage and imposed. The results show that in our sample the simultaneity bias has a negligible effect on the estimate of the labor and materials coefficients, whereas the downward effect of selection bias on the capital coefficient is more pronounced. This is broadly consistent with the pattern reported by Olley and Pakes for US telecommunications equipment firms.

Table A.2
Olley-Pakes procedure

Specification:	Regression of output on all inputs Equation (A.3)	First-stage regression Equation (A.5)	Third-stage regression Equation (A.8)
l	.171** (.004)	.170** (.005)	.170** (.005)
m	.792** (.004)	.790** (.004)	.790** (.004)
k	.038** (.004)	-	.045** (.001)
a	.001** (.000)	-	.001** (.000)
Other variables	-	Third order polynomial in (i, a, k)	Third order polynomial in P and h

Note: Panel data estimation, sample period 1984-1997. Heteroschedasticity-consistent s.e. are shown in brackets. Labor and materials coefficients in third-stage regression (third column) are derived from the first-stage regression (second column) and imposed. **Significant at the 5-percent level.

Table 1
Frequency of price reviews
by sector of economic activity

Category of firms	Average spell of price rigidity (percent share of firms)					Number of firms
	Less than 1 month	1 month	3 months	6 months	1 year or more	
Whole sample	6.6	6.9	16.0	35.6	34.9	955
Consumer goods	3.2	3.2	13.9	41.3	38.4	310
Interm. and inv.goods	8.3	8.7	16.8	32.9	33.3	630
Food	16.9	11.3	22.5	22.5	26.8	71
Textiles and apparel	5.4	3.6	17.4	60.5	13.2	167
Wood and furniture	14.3	14.3	7.1	35.7	28.6	14
Paper and printing	23.8	4.8	28.6	16.7	26.2	42
Chemicals	5.3	15.8	17.5	19.3	42.1	57
Rubber and plastic	10.7	3.6	17.9	35.7	32.1	28
Non ferrous ores	0.0	11.8	15.7	29.4	43.1	51
Metals and metal prod.	6.9	20.7	13.8	25.9	32.8	58
Machinery	3.4	2.6	12.1	37.9	44.0	116
Electric machinery	5.9	3.9	9.8	37.3	43.1	51
Transportation equip.	0.0	2.6	15.4	30.8	51.3	39
Other manufacturing	3.3	6.7	10.0	26.7	53.3	30

Table 2
Frequency of price reviews,
concentration and market power

Category of firms	Average spell of price rigidity (percent share of firms)					Number of firms
	Less than 1 month	1 month	3 months	6 months	1 year or more	
Whole sample	6.6	6.9	16.0	35.6	34.9	955
Operating in markets:						
- highly concentrated	5.7	3.8	14.0	29.9	46.5	157
- less concentrated	8.1	7.4	16.4	36.4	31.7	568
Firm's position in the market:						
- leader	3.7	7.9	15.8	29.5	43.2	241
- among top four firms	8.1	5.6	16.8	33.6	35.8	321
- among top ten firms	7.6	5.4	17.3	44.3	25.4	185
Price elasticity of demand:						
(absolute value)						
- lower than 4	2.5	6.8	14.2	38.2	38.2	353
- greater than or equal to 4	9.9	6.2	17.2	35.3	31.4	354
Markup:						
(over labor and materials)						
- greater than 9 per cent	6.6	6.0	15.1	34.0	38.4	365
- lower than 9 per cent	7.5	8.9	17.0	38.8	27.7	358

Note: highly concentrated markets are defined as those where the four largest firms' aggregate share of total sales exceeds 80 per cent.

Table 3
Alternative measures of TFP growth:
Main statistical and cyclical properties

Measure of productivity growth	Median	25-th perc.	75-th perc.	Coefficient estimate from regressions on GDP growth
Revenue-based Solow Residual (sr)	.007	-.024	.039	.30 (.07)
Cost-based Solow Residual ($cbsr$)	.008	-.026	.040	.51 (.07)
Measure à la Olley-Pakes (op)	.007	-.024	.038	.05 (.08)
Measure à la Basu-Kimball (bk)	.010	-.023	.043	.15 (.07)

Note: Sample period 1984-1997.

Table 4
Alternative measures of TFP growth:
Cross-correlation

Measure of productivity growth	sr	$cbsr$	op	bk
Revenue-based Solow Residual (sr)	1	.94	.85	.91
Cost-based Solow Residual ($cbsr$)	.94	1	.80	.90
Measure à la Olley-Pakes (op)	.85	.80	1	.89
Measure à la Basu-Kimball (bk)	.91	.90	.89	1

Note: Sample period 1984-1997.

Table 5
Productivity shocks and hours

Dependent variable: $dn_t + dh_t$		
$\varepsilon(sr)_t$	$\varepsilon(sr)_{t-1}$	$\varepsilon(sr)_{t-2}$
-.306** (.032)	-	-
-.205** (.042)	.309** (.043)	.148** (.044)
$\varepsilon(cbsr)_t$	$\varepsilon(cbsr)_{t-1}$	$\varepsilon(cbsr)_{t-2}$
-.076** (.030)	-	-
.037 (.039)	.287** (.040)	.125** (.041)
$\varepsilon(op)_t$	$\varepsilon(op)_{t-1}$	$\varepsilon(op)_{t-2}$
-.135** (.042)	-	-
-.036 (.049)	271** (.052)	.066 (.053)
$\varepsilon(bk)_t$	$\varepsilon(bk)_{t-1}$	$\varepsilon(bk)_{t-2}$
-.109** (.033)	-	-
-.001 (.043)	.269** (.045)	.153** (.046)

Note: Panel data estimation on the entire sample. Each row corresponds to a regression. Sample period is 1984-1997. Fixed effects or random effects estimator is used, according to the results of the Hausman test. Parameter estimates are reported with standard errors in brackets. Regressions include year, size and sectoral dummies.

*Significant at the 10-percent level; **significant at the 5-percent level.

Table 6

Price rigidity and the comovement of productivity shocks and hours

Dependent variable: $dn_t + dh_t$			
Sample	$\varepsilon(sr)_t$	$\varepsilon(sr)_{t-1}$	$\varepsilon(sr)_{t-2}$
more rigid prices	-.380** (.050)	-	-
less rigid prices	-.171** (.075)	-	-
more rigid prices	-.337** (.068)	.393** (.067)	.195** (.068)
less rigid prices	-.090 (.089)	.199** (.084)	.044 (.084)
	$\varepsilon(cbsr)_t$	$\varepsilon(cbsr)_{t-1}$	$\varepsilon(cbsr)_{t-2}$
more rigid prices	-.082* (.049)	-	-
less rigid prices	.072 (.067)	-	-
more rigid prices	-.005 (.061)	.333** (.062)	.191** (.063)
less rigid prices	.255** (.079)	.209** (.075)	.113 (.076)
	$\varepsilon(op)_t$	$\varepsilon(op)_{t-1}$	$\varepsilon(op)_{t-2}$
more rigid prices	-.306** (.070)	-	-
less rigid prices	-.016 (.082)	-	-
more rigid prices	-.220** (.077)	.372** (.080)	.013 (.078)
less rigid prices	.191 (.119)	.155 (.115)	.122 (.120)
	$\varepsilon(bk)_t$	$\varepsilon(bk)_{t-1}$	$\varepsilon(bk)_{t-2}$
more rigid prices	-.150** (.054)	-	-
less rigid prices	-.013 (.075)	-	-
more rigid prices	-.141** (.064)	.327** (.064)	.187** (.064)
less rigid prices	.148* (.089)	.155** (.084)	.096 (.084)

Note: Panel data estimation. Each row corresponds to a regression. Sample period is 1984-1997. Fixed effects or random effects estimator is used, according to the results of the Hausman test. Parameter estimates are reported with standard errors in brackets. The sample is split according to the frequency of price reviews reported by the SIM Survey: "more rigid prices" indicates the sample of firms that typically review prices every six months or less often; "less rigid prices" the sample of firms that typically review prices more than twice a year. Regressions include year, size and sectoral dummies.

*Significant at the 10-percent level; **significant at the 5-percent level.

Table 7

Products storability and the comovement of productivity and labor

7a. Conditioning on the presence of price stickiness

	Sample of firms with stickier prices	
	High inventory-sales ratio	Low inventory-sales ratio
$\varepsilon(sr)$	-.407** (.069)	-.347** (.075)
$\varepsilon(cbsr)$	-.027 (.063)	-.155** (.073)
$\varepsilon(op)$	-.313** (.086)	-.257** (.097)
$\varepsilon(bk)$	-.125* (.069)	-.225** (.078)

7b. Conditioning on product storability being high

	Sample of firms with high inventory-sales ratio	
	More rigid prices	Less rigid prices
$\varepsilon(sr)$	-.390** (.067)	-.298** (.104)
$\varepsilon(cbsr)$	-.022 (.061)	-.062 (.092)
$\varepsilon(op)$	-.304** (.083)	-.068 (.111)
$\varepsilon(bk)$	-.117* (.067)	-.032 (.099)

Note: Panel data estimation. the results in each cell correspond to a regression; Sample period is 1984-1997. Fixed effects or random effects estimator is used, according to the results of the Hausman test. Parameter estimates are reported with standard errors in brackets. In table 7a the sample of firms with stickier prices (see legend to Table 6) is split according to the extent to which goods are storable. The splitting criterion is the median of the firms' time average of the inventory-sales ratio. In table 7b the sample of firms with a higher degree of products storability is split according to the degree of price rigidity. Regressions include year, size and sectoral dummies.

*Significant at the 10-percent level; **significant at the 5-percent level.

Table 8

Market power and the comovement of productivity and labor

8a. Conditioning on low price elasticity of demand

	Sample of firms with inelastic demand	
	Firms with stickier prices	Firms with more flexible prices
$\varepsilon(sr)$	-.385** (.060)	-.208** (.082)
$\varepsilon(cbsr)$	-.121** (.057)	.090 (.074)
$\varepsilon(op)$	-.376** (.074)	-.051 (.094)
$\varepsilon(bk)$	-.222** (.061)	.037 (.081)

8b. Conditioning on price flexibility

	Sample of firms with more flexibile prices	
	Firms with inelastic demand	Firms with elastic demand
$\varepsilon(sr)$	-.304** (.091)	.122 (.116)
$\varepsilon(cbsr)$	.009 (.083)	.358** (.102)
$\varepsilon(op)$	-.091 (.103)	.225* (.135)
$\varepsilon(bk)$	-.025 (.091)	.094 (.121)

Note: Panel data estimation. the results in each cell correspond to a regression; Sample period is 1984-1997. Fixed effects or random effects estimator is used, according to the results of the Hausman test. Parameter estimates are reported with standard errors in brackets. In table 8a the sample of firms with higher market power (those with a price elasticity of demand that, in absolute value, is smaller than (or equal to) the median value across all firms) is split according to the degree of price rigidity (see legend to Table 6). In table 8b the sample of firms with more flexible prices is split according to the degree of market power (the threshold is the median value of the price elasticities with respect to demand of firms in this subsample). Regressions include year, size and sectoral dummies.

*Significant at the 10-percent level; **significant at the 5-percent level.

Table 9
Productivity shocks and hours per employee:
Alternative assumptions on stationarity of hours

Dependent variable	Alternative measure of productivity impulses		
(a)	$\varepsilon(sr)$		
Hours per employee	All sample	More rigid prices	Less rigid prices
First difference (dh)	-.245** (.027)	-.252** (.045)	-.150** (.062)
Level	-.169** (.026)	-.194** (.044)	-.039 (.056)
Deviation from a linear trend	-.168** (.026)	-.190** (.045)	-.021 (.059)
Deviation from a quadratic trend	-.166** (.026)	-.187** (.045)	-.016 (.059)
(b)	$\varepsilon(cbsr)$		
Hours per employee	All sample	More rigid prices	Less rigid prices
First difference (dh)	-.070** (.026)	-.056 (.041)	.012 (.055)
Level	-.027 (.024)	-.019 (.041)	.100 (.050)
Deviation from a linear trend	-.011 (.024)	-.012 (.041)	.111 (.052)
Deviation from a quadratic trend	-.021 (.025)	-.009 (.041)	.113** (.053)
(c)	$\varepsilon(op)$		
Hours per employee	All sample	More rigid prices	Less rigid prices
First difference (dh)	-.107** (.035)	-.224** (.057)	.001 (.072)
Level	-.110** (.028)	-.122** (.045)	.072 (.060)
Deviation from a linear trend	-.076** (.030)	-.170** (.045)	.098 (.064)
Deviation from a quadratic trend	-.102** (.029)	-.169** (.045)	.026 (.062)
(d)	$\varepsilon(bk)$		
Hours per employee	All sample	More rigid prices	Less rigid prices
First difference (dh)	-.018 (.028)	-.017 (.044)	.053 (.062)
Level	-.024 (.025)	-.045 (.043)	.074 (.056)
Deviation from a linear trend	-.026 (.027)	-.047 (.043)	.097* (.058)
Deviation from a quadratic trend	-.020 (.026)	-.041 (.044)	.097* (.058)

Note: Panel data estimation. Each cell in the table corresponds to a regression. Sample period is 1984-1997. Fixed effects or random effects estimator is used, according to the results of the Hausman test. Parameter estimates are reported with standard errors in brackets. The sample is split according to the frequency of price reviews reported by the SIM Survey: "more rigid" indicates the sample of firms that typically review prices every six months or less often; "less rigid" the sample of firms that typically review prices more than twice a year. Regressions include year, size and sectoral dummies.

*Significant at the 10-percent level; **significant at the 5-percent level.

Table 10
Productivity shocks and employment

Sample	Dependent variable: dn_t $\varepsilon(sr)$	$\varepsilon(cbsr)$	$\varepsilon(op)$	$\varepsilon(bk)$
Whole sample	-.061** (.020)	-.007 (.019)	-.028 (.028)	-.091** (.021)
More rigid prices	-.113** (.035)	-.026 (.032)	-.082* (.046)	-.133** (.035)
Less rigid prices	-.021 (.048)	.060 (.044)	.083 (.057)	-.066 (.049)

Note: Panel data estimation. Each cell in the table corresponds to a regression. Sample period is 1984-1997. Fixed effects or random effects estimator is used, according to the results of the Hausman test. Parameter estimates are reported with standard errors in brackets. When the entire sample is split according to the frequency of price reviews reported by the SIM Survey, "more rigid prices" indicates the sample of firms that typically review prices every six months or less often; "less rigid prices" the sample of firms that typically review prices more than twice a year. Regressions include year, size and sectoral dummies.

*Significant at the 10-percent level; **significant at the 5-percent level.

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