The Impact of Bank Credit on Labor Reallocation and Aggregate Industry Productivity

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Using a difference-in-difference methodology, we find that the state-level deregulation of local U.S. banking markets leads to significant increases in the reallocation of labor within local industries towards small firms with higher marginal products of labor. Using plant-level data, we propose and examine an approach that quantifies the industry productivity gains from labor reallocation and find that these gains are economically important. Our analysis suggests that labor reallocation is a significant channel through which local banking markets affect the aggregate productivity and performance of local industries.

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A growing body of evidence suggests that improvements in financial markets can significantly contribute to economic growth.[1] Given the role of financial markets in moving resources towards the best economic opportunities, previous research has focused on how financing frictions may impact the allocation of resources and aggregate productivity. However, there is only limited direct evidence on the mechanism of how finance affects growth and productivity. We examine the detailed sources of potential gains from improvements in financial markets. Two main channels have been posed and debated. Financing frictions can lower aggregate productivity by leading to a misallocation of capital across existing firms or by distorting firms' entry and exit decisions.[2] While labor is a central factor used in production, limited attention has been paid to the role of financing markets in facilitating the reallocation of labor towards the most productive firms. Indeed, existing research typically assumes that financing frictions do not directly affect firms' ability to adjust their labor decisions, and that these frictions influence the allocation of labor only indirectly through their impact on the allocation of capital. According to this view, financial markets will not have a first-order effect on aggregate productivity by facilitating the reallocation of labor towards the most productive firms.

In this paper, we study the role of financial markets in influencing aggregate productivity by shaping the reallocation of labor and capital. Using a difference-in-difference analysis, we examine how reforms in U.S. local banking markets through major state-level banking deregulations affect the aggregate productivity of local industries by shaping the reallocation of labor and capital across firms. We find that these state-level banking deregulation events are associated with significant increases in the within industry reallocation of labor towards higher marginal product of labor firms and that labor reallocation is associated with large gains in aggregate industry productivity.

Intuitively, labor reallocation will only affect the aggregate productivity of an industry to the extent that these reallocations are correlated with differences in firms' marginal products of labor. We propose and estimate an approach to formalize this intuition and measure the overall impact of within-industry labor reallocation on industry productivity growth, which we label labor reallocation gains. We build on previous research that uses plant-level data to decompose

[1] See Jayaratne and Strahan (1996) and Guiso, Sapienza, and Zingales (2004) and the references therein.
[2] This focus results from the importance of aggregate productivity in explaining cross-country differences in income per capita (Caselli (2005)) and the role of resource misallocation in driving aggregate productivity (Hsieh and Klenow (2009), Bartelsman, Haltiwanger, and Scarpeta (2013), and Collard-Wexler and De Loecker (2014)).

aggregate industry productivity growth into its different determinants and isolate the contribution of labor reallocation to this growth.

We motivate our analysis by contrasting two opposing views on how financial markets can affect labor reallocation and aggregate productivity. One view is that the allocation of labor is a sideshow and financial markets do not significantly affect labor reallocation gains. According to this view, financial markets might affect the allocation of labor, but this is not a main source of changes in aggregate productivity. One common argument in support of this view is that financial markets only have a direct impact on firms' capital decisions and affect labor only indirectly through changes in capital decisions.

An alternative view is that financial markets can have a significant effect on aggregate productivity by affecting labor reallocation gains. We argue that this effect can potentially be significant for multiple reasons.[3] First, there are reasons to expect financing frictions to directly affect firms' ability to expand their employment. To begin, firms will need financing to employ more labor if there is a timing delay between payments to workers and the additional cash flows generated by the use of more labor – a net working capital channel that has been recently emphasized theoretically by Jermann and Quadrini (2012). Firms also often face training and hiring costs, and firm-specific investments by workers can be important, so expanding labor often requires upfront costs.[4] Unlike physical capital, which can serve as collateral, it can be harder for firms to finance expansion of their labor force at the same terms as they can finance new physical capital. Capital also has an additional financing advantage over labor as firms can lease physical capital directly from capital providers. As firms facing better economic conditions desire to expand their labor, financing frictions can limit this expansion and reduce the reallocation of labor towards firms with better economic prospects.

The impact of the relaxation of financing frictions on labor can especially have a large effect on aggregate productivity, given the large number of small firms and given these firms are the

[3] Previous research has examined the impact of finance on firm and aggregate employment (Benmelech et al. (2011), Pagano and Pica (2012), and Chodorow-Reich (2014)), but has not examined the impact of finance on aggregate productivity through the allocation of labor.

[4] Even if some of these returns are generated over short-term horizons, Paravisini et al. (2014) suggests that firms can face significant financing frictions in raising short-term working capital. Financing constraints can also expose workers to greater labor income risks and limit firms' ability to attract workers as shown by Agrawal and Matsa (2013) and Brown and Matsa (2013).

ones that potentially will expand labor significantly, as financial constraints can cause these small firms to have high ex ante marginal products of labor. An additional channel for the importance of labor can arise if bank deregulation makes the product market more competitive for small firms so they more efficiently hire labor given increased competition. Overall, if labor reallocation is important, these effects can be large given that labor is a significantly larger share of production relative to capital.

We focus on within-industry resource allocation.[5] Reallocation of labor is defined in broad terms to include any change in the shares of labor allocated to different firms in an industry. These changes in labor shares will incorporate both direct reallocations of labor across firms, where workers switch firms, but also the differential employment growth rates of firms within an industry. Labor reallocation gains are then the component of industry productivity growth that can be explained by changes in the labor shares of firms over time. We analyze how major state-level banking reforms affect these labor reallocation gains. This allows us to quantify the impact that banking markets have on industry productivity through changes in labor reallocation.

We implement this analysis with plant-level data from the U.S. Census Bureau on a broad sample of small U.S. manufacturing firms. An important requirement for the implementation of our approach is measuring gaps in the marginal products of plants within an industry-state-year. As a main approach, to minimize potential measurement and estimation issues, we measure such gaps in the marginal product of labor using differences in plant output per unit of labor within samples of small firms and controlling for differences in firm age and well as firm wages. We also consider alternative approaches in which we specify and estimate general production functions. When estimating production functions, we follow both simple approaches previously used in the finance literature as well as structural approaches building on previous research in empirical industrial organization (Ackerberg et al. (2006)).

We examine the within industry reallocation of labor and the magnitude of industry productivity changes after state-level deregulation in banking markets, when compared to industries in states that did not deregulate banking markets around the same time. The state-level

[5] Our approach follows Olley and Pakes (1996) and Hsieh and Klenow (2009). This focus on the within industry allocation of resources is often motivated by the existence of significant and persistent gaps in productivity within industries (Bartelsman, Haltiwanger, and Scarpeta (2013)).

deregulations that we study allowed banks to operate across broader geographic regions, such as different states or MSAs. During our sample period, small U.S. firms heavily relied on loans from local banks as a source of external financing (e.g., Petersen and Rajan (1994)). Previous research has suggested that these reforms affected local banking markets, leading to both reductions in the market power and improvements in the efficiency of local banks (e.g., Jayaratne and Strahan (1998), and Kroszner and Strahan (1999, hereafter KS)). This evidence also suggested that banking deregulation lead to higher local economic growth and mattered especially for small local firms (e.g., Jayaratne and Strahan (1996), and Cetorelli and Strahan (2006)). These state-level deregulations have the advantage that they are staggered across states over time. KS provide evidence suggesting that these differences in timing across states were not related to contemporaneous changes in state-level economic or banking conditions.[6]

We estimate that this state banking deregulation is associated with economically important increases in labor reallocation gains. Our results are robust across multiple robustness tests and different specifications. Across different specifications, these increases represent between 60%-88% additional increases in productivity over time relative to pre-deregulation changes in productivity. We quantify that these additional labor reallocation gains associated with banking deregulation increase the annual value-added growth of local industries between 0.6-1.0 percentage points. Additionally, we estimate how this increased growth translates into increases on the level of aggregate industry productivity and output during the sample and find that these gains are economically significant.

We also examine how state-level banking deregulation is associated with changes in capital reallocation. We estimate that these events are associated with limited changes in the reallocation of capital towards firms with higher marginal products of capital or labor. This evidence suggests that our labor reallocation effects are important relative to productivity effects through changes in capital reallocation. It also suggests that increases in labor reallocation towards higher marginal product of labor firms are not a consequence of an increased capital reallocation across these same firms.

[6] Kroszner and Strahan (1999) argue that these reforms were triggered by national-level technological changes, which weakened local banking monopolies and reduced their incentives to fight against deregulation, and that differences in the timing of deregulation across states largely capture long-term state characteristics predicting the response of interest groups to these national-level changes.

Our identification hinges on the assumption that state-level banking deregulation is not related to other changes differentially affecting the growth of higher versus lower marginal product firms within local industries. We find that state banking deregulation is associated with significant changes in this differential growth among small firms with operations concentrated in one state and is not associated with prior changes in this differential growth. We contrast this effect with the one estimated in samples of local plants that belong to larger and more geographically dispersed multi-establishment firms, which are less dependent on local banks as a source of financing for their operations in a given state as these large firms have access to national financial markets. We find that deregulation is *not* associated with changes in the differential growth of higher marginal product firms for these large multi-establishment firms.

We also examine these findings in depth by comparing a sample of geographically and economically closely matched industries. For each local industry in a state that deregulated credit markets during our sample (treated industry), we construct a group of control industries which include only geographically close industries located in states that did not deregulate credit markets around the same period. We find that, relative to matched control industries, treated industries significantly increase their resource reallocation towards higher marginal product firms in the years immediately after their deregulation episodes. Moreover, we find that the magnitudes of these effects match the ones from our basic results.

Our evidence is consistent with banking deregulation having a direct effect on small firms by improving their access to credit and relaxing the financing constraints of high marginal product of labor firms in expanding employment. We also argue that there are two plausible explanations for the relative importance of labor versus capital reallocation effects during these events. Changes in the quality of financial intermediation might have mattered more for the financing of labor, as opposed to physical capital, or adjustment costs might have limited the responsiveness of firm capital decisions to uncertain and possibly temporary differences in local banking markets.

We examine the impact of banking deregulation on industry productivity through alternative channels. Our main results examine how banking deregulation is associated with changes in industry productivity growth through changes in the intensive-margin reallocation of resources. By considering different decompositions of industry productivity growth, we compare the economic importance of our previous effects to other channels through which banking deregulation can affect the productivity of local industries. These alternative channels include changes in firm-level productivity growth and changes in the entry and exit decisions of firms.

Consistent with Krishnan, Nandi and Puri (2014), we find that banking deregulation is associated with increases in firm-level productivity. However, for the average industry, we find that the contribution of the resource reallocation towards aggregate industry productivity is more than three times the magnitude of the contribution of increases in firm-level productivity. These results highlight the importance of studying the implications of financing frictions for productivity at the industry level and the importance of labor reallocation.

While we find evidence that firms' entry and exit decisions change with deregulation, our analysis suggests that the implications of these effects for industry productivity are limited when compared to the intensive margin effects we document. These limited findings for entry and exit are consistent with Kerr and Nanda (2009) and the idea that it is hard to predict the quality of new firms before they start operating and producing results.

Overall, our paper makes two main contributions to a growing literature on the impact of finance on resource allocation and aggregate productivity. First, we provide evidence that changes in labor reallocation can be an economically important channel through which banking markets affect aggregate productivity. Second, we provide direct evidence that changes in financial markets can have economically important effects on aggregate productivity through their impact on the intensive-margin allocation of resources. Our results show that such effects can be significant even in the context of the U.S.

1. Financial Markets and the Reallocation of Resources

In this section, we discuss in detail the connection between our paper and previous research on how financial markets affect the allocation of resources and aggregate productivity. Previous research has estimated calibrated models with financing frictions and used them to quantify the channels through these frictions affect aggregate productivity (Buera, Kaboski, and Shin (2011), Midrigan and Xu (2014), and the references therein). A first way that the analysis in this paper complements these papers is by considering the role of the labor reallocation channel. These exercises typically assume that financing frictions do not directly affect firms' ability to expand employment and have no first-order effects on aggregate productivity through labor misallocation. In practice, there is a range of frictions potentially distorting the allocation of resources within an industry, such as labor and product market regulations, and political institutions. For tractability, calibrated exercises also typically assume these frictions are not present and attribute all deviations from benchmarks in resource allocation to financing frictions.[7] A final way that our analysis complements these exercises is providing direct evidence on how significant changes in banking markets affect the determinants of industry productivity growth.

Other papers have also connected financial market markets reforms or measures of financial development to differences in resource allocation within and across industries. Wurgler (2000) relates cross-country differences in financial development to a measure of how efficiently countries allocate capital across their industries. Bertrand, Schoar, and Thesmar (2007) analyze how French banking deregulation reforms affect the entry and exit decisions of firms and the link between their product market shares and operating performance. Guiso, Sapienza, and Zingales (2004) provide evidence that local financial development in Italy leads to increases in firm entry and product market competition. Cetorelli and Strahan (2006) and Nanda and Kerr (2009) study how U.S. state-level banking deregulations affect the size distribution of firms and their entry and exit decisions, respectively. While the effects documented in this previous research are likely to have implications for aggregate productivity, these implications are not analyzed. In the absence of such analysis, the quantitative implications of these results for the different channels through which financial markets affect aggregate productivity are unclear.

A related literature has connected cross-country differences in financial development to economic growth and country-level measures of total factor productivity but has not examined the detailed sources of increases in aggregate productivity.[8] Larrain and Stumpner (2013) explicitly analyze how cross-country differences in financial development across Eastern European countries affect different components of aggregate industry productivity. They do not consider the role of financial markets in affecting aggregate industry productivity through the reallocation of labor and assume that firms' marginal products of labor are equalized to wages, what implies that such gains are equal to zero. Their analysis also does not separates the effect of financial markets on industry

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[7] While we do not have a calibrated model, Moll (2014) emphasizes that tractability issues limit researchers' ability to evaluate the robustness of such quantitative exercises to different specifications of the environment and illustrates how changes in some commonly used assumptions, such as a focus on steady-state outcomes, can have first-order effects on the results.
[8] For example, see Levine (1997) and Beck, Levine, and Loayza (2000).

productivity through intensive margin reallocations from their effects through changes in the entry and exit decisions of firms in the data due both to market selection and data coverage.

2. Methodological Framework

In this section, we describe our methodology to quantify the significance of the labor reallocation channel in greater detail and then present the results implementing our methodology.

2.1. Measuring Marginal Reallocation Gains

We start by illustrating how to isolate the contribution of labor and capital reallocation to marginal changes in industry productivity using first-order approximations for changes in industry output over time. We define industry productivity growth as the industry value-added growth in excess of what can be predicted by the aggregate growth of industry production factors. We focus on value added because it avoids double counting output across industries. In our main results, percentage differences in industry value added are measured at a fixed price for firms' real output.[9] Our measure of industry productivity growth can be derived from the framework proposed by Levinsohn and Petrin (2012, hereafter LP) to measure economy-wide productivity growth with plant-level data.[10]

We build from firm-level production functions and the aggregation of output across firms. We implement our main results without estimating production functions and our magnitude analysis requires only simple assumptions about some key production function parameters (see Sections 2.3 and 4.4). For robustness, we also estimate production functions using multiple different methods for estimating firm-level production functions.

A firm *i* in industry *j* and time *t* can produce output Y_{ijt} with a production function given by:

$$Y_{ijt} = A_{ijt}F(K_{ijt}, L_{ijt}, M_{ijt}), \tag{1}$$

[9] We also consider measuring differences in industry productivity using simple differences in industry total sales minus material costs. Evaluating differences in output at fixed prices is common in measures of aggregate productivity incorporating heterogeneous goods (e.g., Basu and Fernald (2002), and Petrin and Levinsohn (2012)). Intuitively, relative prices capture relative marginal valuations of different goods and allow us to compare changes in real quantities across them.

[10] The framework proposed by LP allows one to measure the contribution of an industry to aggregate productivity growth, which might come from expanding industry aggregate factors. We are only interested in productivity gains conditional on the aggregate factors of an industry and show in the Internet Appendix that our measure of industry productivity growth can be derived as a component of the PL measure that only captures this effect.

where A_{ijt} is a time-variant and firm-specific productivity component, K_{ijt} is the firm's capital stock, L_{ijt} denotes the labor used in production, and M_{ijt} denotes materials. As is common in the productivity literature, productivity A_{ijt} is modeled as a Hicks-neutral term. As is also common in this literature, we define firms' output as their total revenues deflated with an industry-specific price deflator. Firm total factor productivity (TFP) is defined as A_{ijt}. Notice that differences in firm output $Y_{ijt} = P_{ijt}Q_{ijt}$ in Equation (1) can reflect differences in the physical quantity of output Q_{ijt} but also capture differences in firm-specific relative prices P_{ijt} (as emphasized by Foster, Haltiwanger and Syverson (2008)).

We are interested in analyzing how the reallocation of resources across an industry's existing firms contributes to industry productivity growth. In our analysis of marginal changes in industry productivity, we focus on industry gains conditional on a given sample of industry firms. When we quantify the cumulative impact of these intensive-margin reallocations on industry productivity, we explicitly take into account the fact that this sample of firms changes over time due to entry and exit. Let I_{jt} denote a fixed set of firms that exist in industry j around time t. For expositional simplicity, in the material following, we assume that output prices are constant within an industry-year but show in Appendix A.2 how we accommodate differentiated products and firm-specific prices. We briefly discuss the intuition for this general case at the end of this section. If output prices are constant within an industry-year, then Y_{ijt} gives us firms' real output and we can measure industry output as $Y_{jt} = \sum_{i \in I_{jt}} Y_{ijt}$.

For any production factor X_{ijt}, let $X_{jt} = \sum_{i \in I_{jt}} X_{ijt}$ denote the industry aggregate factor. Notice that, in general, the aggregation of firms' production functions will not necessarily lead to an industry production function with a separable TFP term as in (1). In general, the simple aggregation of firms' individual outputs gives us:

$$Y_{jt} = G(N_{jt}, \{A_{ijt}, SK_{ijt}, SL_{ijt}, SM_{ijt}\}, K_{jt}, L_{jt}, M_{jt}), \tag{2}$$

where $SF_{ijt} = \frac{F_{ijt}}{F_{jt}}$ is a firm's industry share of production factor F, N_{jt} is the number of firms in I_{jt}, and $\{A_{ijt}, SK_{ijt}, SL_{ijt}, SM_{ijt}\}$ is the joint distribution of these variables across N_{jt} observations.

The allocation of resources in this framework is defined in broad terms and captures any differences in the shares of factors allocated to different firms within an industry.[11] Changes in these shares, which we label resource reallocation, will incorporate both direct reallocations of resources across firms, such as asset sales, but also the differential growth rates of firms within an industry. By using a first-order approximation, we can isolate the importance of changes in the allocation of resources in explaining marginal changes in industry productivity over time. More formally, industry productivity growth is defined as:

$$IPG_{jt} = \left(\frac{1}{1-sm_{jt}}\right)\left(\frac{dln(Y_{jt})}{dt} - \alpha_{jt}\frac{dln(K_{jt})}{dt} - \beta_{jt}\frac{dln(L_{jt})}{dt} - \gamma_{jt}\frac{dln(M_{jt})}{dt}\right), \quad (3)$$

where sm_{jt} is the ratio of industry material costs to industry revenue and α_{jt}, β_{jt} and γ_{jt} denote industries' capital, labor and materials' elasticity, respectively. The elasticity of each of these factors is computed using the marginal product of the aggregate factor in (2). For example, industry capital elasticity can be defined as $\alpha_{jt} = \frac{K_{jt}}{Y_{jt}}\frac{\partial Y_{jt}}{\partial K_{jt}}$. This will tell us the increase in aggregate output predicted by an increase in aggregate factors, holding constant these other determinants of aggregate output. The term $\left(\frac{1}{1-sm_{jt}}\right)$ coverts these percentage industry output gains into percentage value added gains measured at current output prices. Note that $\left(\frac{1}{1-sm_{jt}}\right) = \frac{P_{jt}Y_{jt}}{VA_{jt}}$, where VA_{jt} is industry value added and $P_{jt}$is the (common) output price in the industry. In the simple case where the industry production function has a separable TFP term as in (1), then (3) will estimate industry productivity growth as TFP growth scaled by $\left(\frac{1}{1-sm_{jt}}\right)$.

In Appendix A.1 we show that one can write (3) as:

$$IPG_{jt} = \left(\frac{1}{1-sm_{jt}}\right)\left(\sum_{i\in I_{jt}}\frac{Y_{ijt}}{Y_{jt}}\frac{dln(A_{ijt})}{dt} + LRG_{jt} + KRG_{jt} + MRG_{jt}\right), \quad (4)$$

where $LRG_{jt} = \frac{L_{jt}}{Y_{jt}}\sum_{i\in I_{jt}}\frac{\partial Y_{ijt}}{\partial L}\frac{dSL_{ijt}}{dt}$ denotes labor reallocation gains and the other two terms are defined analogously based on capital and materials. The first term in (4) captures the contribution of firm-level productivity growth to industry growth. The other three terms capture the

[11] This broad definition of resource allocation is commonly used in studies of industry productivity growth (e.g., Olley and Pakes (1996)) and the literature linking within-industry resource allocation to aggregate productivity (e.g., Hsieh and Klenow (2009)).

contribution of resource allocation to industry productivity growth, which we label as reallocation gains. These gains capture the additional growth in industry output due to shifts in firms' factor shares. More precisely, they capture the difference between the realized marginal growth of industry output and the growth we would observe in the absence of any changes in factor shares. To illustrate the intuition for these gains, consider the case of labor reallocation gains. Since $\frac{dSL_{ijt}}{dt}$ has to add up to zero in the industry, these gains capture an industry covariance between firms' marginal products and $\frac{dSL_{ijt}}{dt}$. Intuitively, reallocation gains are positive (negative) only to the extent that higher marginal product firms grow faster (slower) within an industry.

We emphasize the different potential determinants of reallocation gains. In Appendix A.1 we show that one can approximate LRG_{jt} as:

$$LRG_{jt} \approx \frac{Var\left(\frac{\partial Y_{ijt}}{\partial L}\right)}{E\left(\frac{\partial Y_{ijt}}{\partial L}\right)} \frac{L_{jt}}{Y_{jt}} LRSens_{jt}, \tag{5}$$

where $Var(.)$ and $E(.)$ capture variance and expected values measured using the industry distribution and $LRSens_{jt}$ is the sensitivity of labor reallocation to the marginal product of labor in the industry. $LRSens_{jt}$ is the additional increase in $\frac{dlog(SL_{ijt})}{dt}$ predicted by a given percentage increase in $\frac{\partial Y_{ijt}}{\partial L}$. More formally, is the coefficient on the log of $\frac{\partial Y_{ijt}}{\partial L}$ in a linear regression of $\frac{dlog(SL_{ijt})}{dt}$ on the previous variable and a constant.[12] This sensitivity measures the extent to which industries reallocate resources in response to a given gap in the marginal product of its firms and, intuitively, captures differences in the way industries allocate resources across given opportunities.

As Equation (5) illustrates, the impact of changes in $LRSens_{jt}$ on LRG_{jt} depends on the degree of dispersion in marginal products within the industry and the labor-to-output ratio in the industry. The same sensitivity of reallocation to gaps in marginal products translates into higher productivity gains when there are larger gaps in marginal products in the first place. The output gains from changing these shares are also more important when the industry relies more on the factor per unit of output. These effects are measured by $\frac{LRG_{jt}}{LRSens_{jt}}$, which captures differences in the potential

[12] The approximation comes from the fact that we replace a regression coefficient in levels by one measured in logs adjusted based on the average value of the variables.

industry productivity gains from reallocating resources across opportunities in a given way. We label this ratio as the potential reallocation gains.

In Appendix A.2 we consider the general case where firms have differentiated products and face firm-specific prices within an industry. We show that industry productivity growth can be decomposed into components analogous to the ones in our previous analysis if firms face the same elasticity of demand for their differentiated products ε_j within each industry – which is very plausible. In this decomposition, the sensitivity of resource reallocation to marginal products remains the same as before, and potential gains from reallocation can be obtained by multiplying our previous value by $\left(\frac{\varepsilon_j}{\varepsilon_j - 1}\right)$.[13]

2.2. Examining the Impact of Banking Reforms

We examine the impact of a significant banking market reform on our previous reallocation gains. We can then evaluate how these changes in banking markets affect industry productivity through their impact on the intensive-margin reallocation of resources.[14] To the extent that banking markets matter by influencing the allocation of resources across given opportunities, we should expect them to affect reallocation gains through the sensitivity of resource reallocation to each factor's marginal product.

The banking market reforms we examine are state-level banking deregulations. Prior to the 1970s most U.S. states had restrictions on banks' ability to operate within and across state borders that had remained historically stable. Given that small U.S. firms mostly relied on geographically close banks as a source of external financing until the early 1990s (Petersen and Rajan (2002)), these restrictions created local banking monopolies (Kroszner and Strahan (1999, hereafter KS)). Between the early 1970s and early 1990s states relaxed these restrictions in a staggered way. Following previous research on U.S. state banking deregulation, we focus on two main types of

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[13] This assumption can be interpreted as an approximation and is common in recent models linking within-industry resource allocation to aggregate productivity (e.g., Hsieh and Klenow (2009) and Bartelsman, Haltiwanger, and Scarpeta (2013)). Hottman, Redding, and Weinstein (2015) provide evidence that most firms are well approximated by this benchmark of within-industry constant markups. Under this plausible assumption, we can address the absence of extensive data on firm-specific prices P_{ijt} and real quantities Q_{ijt} across industries and still rely on the industry-deflated total value of shipments $Y_{ijt} = P_{ijt} * Q_{ijt}$.

[14] After presenting our main analysis, we also provide some evidence on the relative importance of this channel versus other channels through which productivity can be impacted by banking markets, such as changes in firm-level productivity and firms' entry and exit decisions.

restrictions imposed by states. First, states imposed restrictions on intrastate branching. For example, these included restrictions on the ability of multibank holding companies to convert branches of acquired subsidiary banks into branches of a single bank, as well as restrictions on banks' ability to open new branches. As in Jaraytane and Strahan (1996), we choose the date of intrastate deregulation as the date in which a state permits branching through mergers and acquisitions.

Second, the Douglas amendment to the Bank Holding Act of 1956 prevented a bank holding company from acquiring banks in another state unless that state explicitly permitted such acquisitions by statute. No state allowed such acquisitions until the late 1970s. States then entered reciprocal regional or national arrangements that allowed their banks to be acquired by banks in any other state in the arrangement. Except for Hawaii, all states had entered such agreements in 1993. These episodes of interstate deregulation culminated with the passage of the 1994 Riegle-Neal Interstate Banking and Branching Efficiency Act, which codified these state-level changes at the national level. Our data is available from 1977 and, motivated by the above timeline, we end our sample in 1993.

We follow Amel (1993) and KS in determining the dates of interstate and intrastate deregulation. We show these dates in Table IA.6 of the Internet Appendix and note the large number of interstate deregulation episodes during our sample period. Previous research has documented that these deregulation episodes are associated with significant reductions in monopoly power of local banks and increases in the efficiency of local banking markets. For example, these events were associated with reduced loan losses by local banks, which cannot be explained by shifts towards safer loans. Local banks experienced drops in operating costs and more efficient local banks increased their market share after deregulation. These changes were matched with lower spreads on local loans and higher local economic growth, despite the absence of significant increases in aggregate local investment. Taken together, this evidence suggests that this reform not only reduced the cost of financing for small local firms but also potentially increased the quality of local financial intermediation.

Given this previous evidence, we focus our analysis on the industry productivity consequences of these deregulation episodes and, as our data section describes, focus on small single-establishment firms from the Census.[15] We are interested in linking changes in aggregate industry productivity to overall banking market conditions faced by an industry. Therefore, the unit of analysis in our results will be an industry-state, which we label as a local industry. Local banking markets should mostly for small firms with a strong geographic exposure to a given state and we focus on these small single-establishment plants in most of our analysis. As we discuss below, more than 75% of manufacturing plants belong to these small firms in our sample period. We contrast our results with a set of plants of large multi-establishment firms whose operations span multiple states. Given that these large firms have access to national capital markets, we should observe limited effects for these large plants if local banking deregulation is responsible for our findings.

2.3. Measuring Gaps in Marginal Products

In order to implement the previous analysis, we need to relate changes in the factor shares of firms within a local industry to differences in their marginal products. This requires measuring gaps in the marginal products of small firms within an industry-state-year. As a first approach, we measure such gaps in the marginal product of labor using differences in plant output per unit of labor. We include measures of hours worked when measuring units of labor and control for differences in firm age as well firm wages in some specifications. We measure a plant's output using both the industry-deflated value of shipments and sales minus material costs (value added).

In order to see the intuition for this approach, notice the following. In general, one can write the log of a firm's marginal product of labor as $MPL = \log(\beta) + \log\left(\frac{Y}{L}\right)$, where β is the labor elasticity and $\frac{Y}{L}$ is the output per unit of labor. If differences in the labor elasticity of firms within an industry-state-year are limited then one can use $\log\left(\frac{Y}{L}\right)$ to capture gaps in MPL. Notice that this approach requires these differences to be examined only within our sample of small firms and after including the previously discussed controls. This approach can be grounded in plant-level production functions if we use a Cobb-Douglas specification, a commonly used specification in

[15] See Jayaratne and Strahan (1996, 1998), Krozsner and Strahan (1999), and Cetorelli and Strahan (2006) and the references therein for a more detailed discussion of this evidence. In theory, larger banks could limit the access to finance of small business but this evidence suggests that this was not the case during the events studied in this paper.

studies of plant or firm productivity.[16] Notice that the parameters of this Cobb-Douglas production function could be arbitrarily different across each industry-state-year observation. The key point for our purposes is that these parameters do not need to be empirically estimated. Independently of the value of such parameters, gaps in MPL within an industry-state-year will be given by gaps in $\log\left(\frac{Y}{L}\right)$.[17]

For robustness, as a second alternative approach, we specify and estimate a translog production function as a second-order approximation to any production function specified in (1). In this approach, we measure gaps in the marginal product of labor as $MPL = \log(\beta) + \log\left(\frac{Y}{L}\right)$, where $\log(\beta)$ depends on production inputs and estimated parameters. The key advantage of this approach is that it allows the elasticity of labor and capital to be different across firms within an industry-state-year. The major limitation of this approach is that it requires the estimation of production function parameters. Notice that the choice of these parameters will only matter to the extent that it leads to gaps in the elasticity of labor and capital of firms. We interpret this approach as a robustness check on our previous assumption that differences in these factor elasticities are similar within an industry-state-year.

We estimate these parameters using different strategies. We first follow a simple strategy similar to the one typically used in the finance literature examining productivity. We estimate these parameters separately for each industry-year (3-digit SIC code) using ordinary least squares (OLS) estimates and data from the previous five years. We include year fixed effects in this estimation and use plant-level data, which we describe in greater detail in Section 3. We term this strategy as OLS.[18] We then consider a structural estimation strategy that builds on previous research in empirical industrial organization. We follow the broad framework outlined in Ackerberg, Benkard, Berry, and Pakes (2006, hereafter ABBP) for the estimation of production functions and propose

[16] For example, see Foster, Haltiwanger and Syverson (2008) and Syverson (2011) and the references therein. The assumption here is that percentage changes in labor units lead to similar percentage changes in output across firms. The assumption is not that the marginal and average returns to units of labor are the same. As firms use more labor, the same increase in labor units represents less in percentage terms and has a more limited impact on the output.

[17] In the analysis that follows we include industry-state-year fixed effects. Even if we included estimated values for $\log(\beta)$ in each industry-state-year, these values would not affect the analysis since we demean MPL by industry-state-year.

[18] The finance literature examining firm or plant productivity typically estimates production function parameters using rolling windows and OLS estimates or panel data approaches that add firm or plant fixed effects (e.g., Maksimovic, Phillips, and Wang (2013), Krishnan, Nandi and Puri (2014), and Giroud and Mueller (2015)).

a specific strategy adapted to our analysis. In this approach, we use explicit assumptions on economic primitives to construct control variables that address the simultaneity and selection biases involved in the estimation of production functions. This strategy is consistent with our analysis and takes into account the possibility that financing frictions directly affect both firm investment and labor decisions.

We also explicitly allow state banking deregulation to affect firm financing constraints, firm productivity, state economic conditions, as well as firm entry and exit decisions. As in the industrial organization literature using structural estimation strategies, we estimate production function parameters separately for each industry (3-digit SIC code) but do not allow these parameters to change over time.[19] Appendix B describes this estimation strategy, its intuition, and its implementation in greater detail. We label this strategy as a structural approach.

3. Data and Summary Statistics

Our main data sources are the Longitudinal Business Database (LBD), the Census of Manufacturers (CM) and the Annual Survey of Manufacturers (ASM) from the U.S. Census Bureau. We focus primarily on small single-unit establishments, as is previously discussed in Section 2.2, state banking deregulation should matter mostly for small firms with a strong geographic exposure to a given state. These single-unit firms are more likely to be dependent on local banking markets for financing. We compare these results to multi-establishment firms who have operations that span multiple states given these firms are likely to have access to national sources of financing and thus should be less dependent on local banking markets.

We thus construct two samples of firms. First, we construct a sample of single-establishment firms in manufacturing using the LBD. In our initial database at the firm-industry-state-year level, 88% of the observations belong to single-plant firms. Overall, across all manufacturing firms in the LBD over our sample period, we found that 76% of establishments belong to single-plant firms. Table 1 provides summary statistics on our sample of single-plant firms, the main sample used in our analysis. The average firm employment in this sample is 22 workers. This contrasts with the

[19] See ABBP and the references therein for a discussion of this literature. The estimation of parameters with these strategies requires panel data and the construction of several control variables, what makes it unfeasible to implement it without a large sample. Note that the elasticity of labor and capital could still change over time as they depend on the choice of inputs, which might change over time, and that we allow parameters to change over time when we estimate them with simpler strategies.

average firm employment of multi-plant firms that equals 737 workers. While these single-plant firms are small, in aggregate they represent close to 50% of the overall sales and employment of their industry-state on average across all years.

Second, we also construct an additional sample for our analysis by examining multi-plant firms. We would expect that state banking deregulation should not matter for these large multi-plant firms as these firms should have access to capital in the public markets and access to capital in more national markets from multiple states.

The CM provides information on the sales and inputs used by all manufacturing firms every five years (i.e., Census years). Our analysis tracks over time the allocation of resources within industries across small firms, what requires data over time on a comprehensive number of small firms in these industries. Higher frequency data on small firms is useful in our analysis as it allows one to more precisely link changes in banking markets to changes in resource allocation. The ASM allows one to track this same information for a subsample of manufacturing firms in non-census years through rotating five-year panels. However, while large plants are sampled with probability one, small plants are sampled randomly with probabilities that decline with their size. When compared to samples of local industries in the CM, samples of local industries constructed in this way capture less than 10% of the firms of interest for our purposes. This issue is particularly relevant in the context of this paper because we need to measure within industry correlations over time.

We address this challenge by combining the CM with the LBD. The LBD provides annual employment and payroll information for every private establishment from 1976 onward. The underlying data are sourced from U.S. tax records and Census Bureau surveys. We use the LBD to annually track over time the within-industry reallocation of labor and link to the CM to relate this reallocation to firm marginal products and firm productivities. We measure firms' marginal products and productivities in a given year using data from the last available Census and address the potential measurement issues associated with this approach in the Internet Appendix.[20] We also use the LBD to track entry and exit.

[20] For example, we provide direct evidence that differences in marginal products within an industry-state are highly persistent at the horizon considered in this approach.

We construct our initial sample of data by matching firms in the LBD and the last available CM. We first identify single-plant manufacturing firms in the LBD and match them to the CM at the firm-year level. We then identify multi-plant firms operating in manufacturing in the LBD and match them to the CM at the firm-industry-state-year level. We focus on the reallocation of resources across different firms in a local industry (industry-state) and collapse both data sources at the firm-industry-state-year level. On average, this initial dataset covers 152,000 unique firm-industry-state observations each year between 1977 and 1993. We end our sample in 1993 because of the timeline of national-level banking deregulation discussed in Section 2.2. We start our sample in 1977 because this is the first year in which both LBD and CM data are available.

Table 1 also shows the within-industry-state-year dispersion of the marginal product of labor and capital implied by different approaches in our sample of single-plant firms. In our first approach outlined in Section 2.3., we construct measures of marginal product gaps by simply using data from the CM. In our second approach, where we rely on estimated production functions, we combine the data from the CM with estimated parameters. Since some of the methods outlined in Section 2.3 require panel data, we estimate the industry- or industry-year-level parameters of the production functions specified in (1) using the ASM. Across both approaches, we estimate marginal product gaps at the plant level. In our sample of single-plant firms this translates directly into firm marginal products. In our samples of multi-plant firms, we construct marginal products at the firm-state-industry level as a weighted average of plant marginal products, using plants' factor shares as weights. These marginal products can be interpreted as capturing firm factor expansions conditional on the within-firm allocation of the factor across plants. Variable definitions are in Appendix C.

4. Results

4.1. Labor Reallocation

Following our methodological framework, we examine how local banking deregulation relates to changes in within-industry labor reallocation gains. We start by examining how the sensitivity of labor reallocation to the marginal product of labor in local industries relates to local banking deregulation. A first approach to examine this relationship is to estimate:

$$\Delta EmpShare_{isjt} = \alpha_{sjt} + \beta_0 \times MPL_{isjt} + \beta_1 \times Dereg_{st} \times MPL_{isjt} \qquad (6)$$

$$+ \delta \times X_{isjt} + \varepsilon_{isjt},$$

where $\Delta EmpShare_{isjt}$ is the change in the employment share of firm i in industry j, state s and time t, α_{sjt} is a state-industry-year fixed effect, MPL is the log of firm marginal product of labor, $Dereg$ is a banking deregulation index, and X denotes age controls. The banking deregulation index is the sum of two indicators that equals one if interstate or intrastate banking deregulation has been passed in the state. Employment share is the ratio of firm employment to the overall employment of a firm's industry-state. $\Delta EmpShare_{isjt}$ is measured as the log difference of this share between year t and t-1. Only firms present in the industry-state in both year t and t-1 are included in the sample and the computation of the employment share.

Notice that β_0 tells us the sensitivity of employment reallocation to the marginal product of labor for industries located in states that have not deregulated banking markets, i.e. it measures an average value of $LRSens$ across these industries (see Section 2.1). Also notice that the state-industry-year fixed effects ensure that this relationship captures a correlation within an industry-state-year.

The coefficient of interest is β_1 and tells us the differential value of this sensitivity for industries located in states that deregulated their banking markets. The age controls *X* include the one-year lag of age, its squared value, as well as the interactions of both these variables with deregulation indicators. There are important life-cycle patterns in productivity, and we want to capture differences between the marginal products of firms at the same stage of their life cycle.[21]

One potential issue with this approach is that β_1 might be capturing cross-state differences and times-series trends in the employment reallocation of industries. We address these issues by controlling for both fixed differences across states and time-series changes in the employment reallocation of local industries. This is done by adding state and year fixed effects interacted with MPL as controls in the estimation of (6). After we add these controls, the estimation of β_1 can be thought as a difference-in-differences estimation of how state banking deregulation affects the labor reallocation sensitivity of local industries. Intuitively, one can think about this estimation as

[21] One potential issue with our approach is that the link between changes in employment shares and *MPL* could be biased by measurement error if both variables are constructed using the same underlying data. We note that these variables are constructed using different data sources (LBD versus CM) and that we are interested in differences in this relationship across local industries.

involving two steps. First, we estimate the sensitivity of labor reallocation to the marginal product of labor within each industry-state-year. We then estimate how deregulation affects this relationship using a difference-in-differences specification. We are implementing these two steps together in a single regression.[22] If differences in the timing of deregulation across states capture long-term differences across them, as argued by Krozsner and Strahan (1999), this approach will isolate the impact of deregulation on $LRSens$.

In addition to these controls, we also include firm-state-industry fixed effects to control for fixed differences across firms in their local employment growth. This leads us to estimate:

$$\Delta EmpShare_{isjt} = \alpha_{sjt} + \mu_{isj} + \gamma_s \times MPL_{isjt} + \theta_t \times MPL_{isjt} \qquad (7)$$

$$+\beta_1 \times Dereg_{st} \times MPL_{isjt} + \delta \times X_{isjt} + \varepsilon_{isjt},$$

where μ_{isj} denotes firm-state-industry fixed effects, θ_t denotes year fixed effects, γ_s denotes state fixed effects, and the other variables are defined as in Equation (6).

Table 2 reports results of the estimation of Equations (6) and (7) in our sample of single-plant firms. As previously discussed, this sample captures small firms with greater exposure to local banking markets. We consider both approaches outlined in Section 2.3 to measure gaps in firms' marginal products. Namely, we first use output per unit of labor, where labor units include hours worked, and output is measured using both gross output and value added. We then also incorporate an estimated labor elasticity based on a translog specification and both OLS and structural approaches. While $Dereg$ combines the effect of both intrastate and interstate deregulation, we also separately estimate the effect of each type of deregulation by including separate indicators for each of these events together in the same specification.

Panels A and B of Table 2 report the estimated coefficients for β_1, which capture changes in $LRSens$, with Panel A including state-industry-year fixed effects and Panel B also including firm-state-industry fixed effects and state and year fixed effects interacted with the marginal products. Panel C of Table 2 quantifies the magnitude of the percentage changes in $LRSens$ implied by these

[22] Notice that the sample of firms used to estimate this relationship is changing over time and can be affected by deregulation. Motivated by our analysis in Section 2.1, we are interested in analyzing how an industry measure ($LRSens$) changes with banking deregulation. At any given year, this measure has to be computed using all existing firms in an industry.

effects. We compare our estimates for β_1 to the average sensitivity of employment reallocation to the marginal product of labor prior to deregulation. We find that local banking deregulation is associated with both economically and statistically significant differences in the sensitivity of labor reallocation to the marginal product of labor. We find that banking deregulation (joint effect of interstate and intrastate deregulations) leads to a percentage increase in $LRSens$ between 60%-88%. Alternatively, deregulation leads a one standard deviation in MPL (within-industry-state-year) to predict an additional employment growth equal to 0.89-1.12 times the average employment growth in the sample. Table 3 shows that these results and magnitudes remain similar after controlling for within-industry-state differences in worker skill across firms using the one-year lag of firm average wages in an analogous way to our age controls. This evidence suggests that banking deregulation is associated with significant changes in the extent to which industries reallocate resources in response to a given gap in marginal products. Furthermore, the results suggest that this conclusion is robust to approaches taking into account potential gaps in the elasticity of labor within an industry-state-year. The results become economically more important when we use estimated values for the labor elasticity. Motivated by these findings, we use our first approach measuring gaps in marginal products with output per unit of labor in the remaining of the analysis.

4.2. Results for Multi-Plant Firms

We next contrast our previous findings with the same results estimated in samples of multi-plant firms. In Section 3 we illustrate the significantly larger size of multi-plant firms in our data when compared to the single-plant firms previously analyzed. One average, these firms have 55% of their employment in a given deregulated state. Because of their greater size and geographic dispersion, these firms are less likely to depend on local banks when financing their operations in deregulated states. To the extent that this dependence is small, and our previous results capture changes in local banking markets, we should expect these same results to become insignificant when estimated in these samples of multi-plant firms. In contrast, if the previous results capture changes in local factor markets or local demand in product markets that are correlated with banking deregulation, we might expect deregulation to be associated with significant effects in these samples.

Table 4 presents results using the main specifications used in Table 2 (Panel B) for the previous sample of multi-plant firms. The results show that banking deregulation is not associated with an economically or statistically significant change in the differential growth of higher marginal product of labor firms. The magnitudes of these coefficients are directly comparable to the ones in Panel B of Table 2 and are small when compared to them. This analysis provides support for our previous focus on single-plant firms as the group mostly affected by deregulation. It also provides support to the view that our single-plant results capture the direct effect of changes in local banking conditions. [23]

4.3. Potential Gains from Reallocation

We focus on single-plant firms in the remaining analysis and now examine whether banking deregulation is associated with changes in the potential reallocation gains in Equation (5). As Equation (5) illustrates, reallocation gains are the product of potential gains and $LRSens$. We examine the extent to which banking deregulation is associated with percentage changes in potential labor reallocation gains. By combining these results with our previous estimates for the percentage changes in $LRSens$, we can analyze the extent to which banking deregulation is associated with overall changes in labor reallocation gains.

One reason to expect changes in potential reallocation gains is that, as resources move towards higher marginal product firms, marginal products might become more equalized across firms. However, in practice, the significance of this effect is unclear for at least two reasons. First, the magnitude of the previous results suggest that this effect is likely to be limited when compared to the changes in $LRSens$.[24] Second, banking deregulation might also affect the distribution of firm productivity in an industry, for example, because of changes in individual firm-level productivity.

Following our previous analysis, we propose a simple approach to examine this issue which does not require estimating production functions. If firms have a constant labor elasticity β_{jt} within an industry-year, we can use Equation (5) to write the potential gains from reallocating

[23] Kerr and Nanda (2009) also use this contrast between single- and multi-plant firms to better isolate the effect of state banking deregulation. We have found that transitions between single-plant and multi-plant firms have a small magnitude which is unlikely to significantly affect the productivity of these groups.

[24] For example, suppose that $LRSens$ increases by 0.01 and the labor elasticity is 0.3. A 10% gap in *Y/L* will now be compensated by an additional 0.1% increase in labor each year and this effect will reduce the original gap in *Y/L* by 0.07% each year. This effect will approximately reduce the original gap in *Y/L* by seven percent after ten years.

labor in an industry-state-year as $\beta_{jt} \frac{Var\left(\frac{Y_{ijt}}{L_{ijt}}\right)}{E\left(\frac{Y_{ijt}}{L_{ijt}}\right)} \frac{L_{jst}}{Y_{jst}}$. We note that the values for β_{jt} can be omitted if we include industry-year fixed effects while examining percentage (log) changes in these gains.[25]

We examine how deregulation affects potential reallocation gains by estimating:

$$Log(Potential\,LRG)_{sjt} = \alpha_{sj} + \theta_{tj} + \beta \times Dereg_{st} + \varepsilon_{sjt}, \tag{8}$$

where $Potential\,LRG$ are potential labor reallocation gains in industry j, state s, and time t, α_{sj} is a state-industry fixed effect, θ_{tj} are industry-year fixed effects, and $Dereg_{st}$ is defined as before. This approach is similar to the one in our previous results where our analysis is equivalent to estimating a difference-in-differences specification with $LRSens$.

Table 5 reports the results. We find that percentage changes in potential reallocation gains are significantly smaller in magnitude than our previously estimated percentage increases in $LRSens$. In our main specification, we estimate that deregulation is associated with drops in $Potential\,LRG$ between 1-14% and increases in $LRSens$ between 60%-88%.[26] We conclude that the percentage changes in $LRSens$ associated with banking deregulation mostly translate into percentage increases in labor reallocation gains. Therefore, banking deregulation is associated with significant percentage changes in labor reallocation gains and this effect is driven by changes in the sensitivity of reallocation to marginal products.

4.4. Magnitude of Labor Reallocation Gains and Cumulative Productivity Increases

We quantify the incremental industry value-added growth implied by the previous changes in labor reallocation gains. We estimate this incremental growth using our estimated changes in $LRSens$ combined with the average value of $Potential\,LRG$ prior to deregulation. When estimating initial values for $Potential\,LRG$, we use the same expression used in Section 4.3 but now need to include estimates for β_{jt}. We estimate this elasticity using the strategies discussed in

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[25] The fixed effects will lead us to demean the (log) gains at the industry-year level. $Var(.)$ and $E(.)$ represent moments of an industry-state-year. We have found that this analysis is also robust to using estimated translog production functions.

[26] While our previous results did not control for industry-year shocks affecting $LRSens$, we show in Section 5 that our labor reallocation results remain similar after including such controls.

Section 2.3 and a Cobb-Douglas specification.[27] As discussed in Section 2.1, Equation (5) captures these gains when output prices are constant within an industry. To allow for differentiated products within an industry, we scale reallocation gains using Equation (5) by $\left(\frac{\varepsilon}{\varepsilon-1}\right)$, where ε is the average demand elasticity for firms' products across industries. The literature uses values for the average demand elasticity between three and five and we set this value equal to four.[28]

Table 6 reports these magnitudes. We denote these gains estimated under the assumptions of constant or heterogeneous output prices within an industry as *Ind_Prod_Growth_1* and *Ind_Prod_Growth_2*, respectively. Our null hypothesis is that reforms in banking markets cannot lead to sizeable productivity gains through the labor reallocation channel. We estimate that the increase in labor reallocation gains associated with banking deregulation leads to an increase in the annual industry value-added growth of local industries between 0.6% and 1.0%. Across different states, these gains are estimated over an average period of approximately ten years. These gains capture the combined effect of both interstate and intrastate deregulation and are significant when compared to U.S. real GDP per capita growth over the sample period. They are also important relative to previously estimated effects of local financial development on local economic growth in other countries.[29]

One natural issue raised by the previous magnitudes is the cumulative impact of this increased value-added growth on the level of industry productivity after several years of banking deregulation. This initial increased reallocation effect is likely to diminish over time as the distribution of productivity across existing firms in an industry will change over time. Additionally, entry and exit will also reduce the importance of this initial increased reallocation effect. In Appendix A.3, we propose a simple approach to estimate the cumulative effects of increased marginal labor reallocation gains after banking deregulation on the level of industry productivity. Our approach explicitly incorporates the fact that marginal reallocation gains on the

[27] The labor elasticity is here only a scaling factor that converts gains measured as a percentage of labor units into gains measured as a percentage of output. We note that different values for this elasticity within a range considered or estimated in the literature do not affect our main conclusions.
[28] See Hsieh and Klenow (2009), Bartelsman, Haltiwanger, and Scarpeta (2013), and Asker, Collard-Wexler, and De Loecker (2014). Using the previous expression, it is simple to see that our estimates will be not very sensitive to different choices within this range of values. Note that our results with constant output prices within an industry can be interpreted as the case where ε is large.
[29] Real GDP per capita growth is 1.9% per annum in the U.S. over the sample period. Guiso, Sapienza, and Zingales (2004) find that in the most financially developed regions in Italy, this growth rate is 1.2 percentage points higher than in the least financially developed ones.

intensive margin will most likely have an impact on the future level of productivity that fades away over time. We show that, under plausible conditions, one can estimate these cumulative gains as a discounted sum of increased marginal reallocation gains. Moreover, this discount rate captures simple moments in the data that we can directly measure in our sample. Intuitively, this discount rate is determined by the persistence of within-industry differences in firm productivity and the importance of new entrants as a share of aggregate industry sales.

We follow this approach to quantify the cumulative industry value-added gains associated with the previous increases in labor reallocation gains. Table 6 also reports the magnitude of these cumulative productivity gains. The cumulative gains associated with *Ind_ Prod_Growth_1* and *Ind_Prod_Growth_2* are denoted as *Cum_Prod_Gain_1* and *Cum_Prod_Gain_2*, respectively. We estimate that the increase in labor reallocation gains associated with banking deregulation leads to an increase in the value-added of local industries between 2.6% and 4.5% over a horizon of approximately ten years. To place these estimates in perspective, we note that Hsieh and Klenow (2009) estimate that fully equalizing firms' marginal products across all factors in the U.S. manufacturing sector during the late 1980s would lead to increases in industry productivity of approximately 31%. These estimated gains are directly comparable to *Cumulative Ind. Productivity Gain_2.* Relative to this benchmark, our results suggest that, in ten years, changes in labor reallocation associated with banking deregulation generate approximately 12% of possible long-term gains from reallocating all production factors.

Together, this evidence suggests that the productivity gains due to labor reallocation after banking deregulation are significant for the average U.S. industry.

4.5. Capital Reallocation

We now examine how banking deregulation relates to changes in within-industry capital reallocation gains. We follow analogous steps to the ones used in our labor reallocation analysis. First, we estimate how the sensitivity of capital reallocation to the marginal product of capital changes with banking deregulation. We estimate a specification analogous to Equation (7), with the growth of firms' industry capital share as the outcome. Panel A of Table 7 reports the results. We find that banking deregulation is not associated with economically or statistically important increases in the sensitivity of capital reallocation to the marginal product of capital. Deregulation leads a one standard deviation in *MPK* (within-industry-state-year) to predict an additional capital growth between -0.3 and -0.2 percentage points.

One potential issue with these results is that data on the growth of firms' capital stock for a large number of firms in our sample is only available every five years and this significantly reduces our sample size. We address this issue by estimating our labor reallocation results in this same sample. Panel B of Table 7 reports this analysis, which shows that our labor reallocation results remain important with this approach.[30]

Panel B of Table 7 also reports the following additional evidence contrasting labor and capital reallocation effects using the sample restricted to census years. First, banking deregulation is also not associated with increases in the sensitivity of capital reallocation to the marginal product of labor. Additionally, our labor reallocation results remain economically important and similar in magnitude if we replace changes in firms' employment shares with the differential growth of these shares relative to their capital shares. This evidence suggests that our previous labor reallocation gains play a central role in determining the overall effect of banking deregulation on aggregate productivity through the reallocation of labor and capital. These results also suggest that the increased reallocation of labor towards firms with higher marginal products of labor was unique to labor and not a reflection of an increased reallocation of capital towards these same firms.

We further examine these issues by analyzing the average changes in firms' labor and capital growth associated with banking deregulation. We estimate a specification analogous to Equation (8) using plant-level data for our sample of single-plant firms. We estimate labor results using both our entire sample and restricting the analysis to the census years used in the capital results. We include both plant and industry-year fixed effects. We estimate capital results using both firms' net capital growth and investment as outcomes. Panel C of Table 7 reports the results. Across all these specifications, we find that banking deregulation is associated with significant increases in the employment growth of small firms but limited increases in their capital growth. This evidence

[30] While there is some drop in the magnitude of the effects, note that these estimates capture longer-term effects of deregulation given the lower frequency of the data. This magnitude is very similar to the one that we have found when we separately estimated the longer-term effects of banking deregulation in our broader sample.

suggests that the contrast between labor and capital reallocation results is a reflection of a broader asymmetry in the effects of banking deregulation on labor and capital.[31]

5. Identification of Banking Deregulation Effects

Our identification of banking deregulation effects hinges on the assumption that these events are not correlated with other state-level changes that affect the relative growth of higher marginal product firms. We address potential concerns with this assumption in multiple ways.[32] We start by highlighting the contrast in our previous analysis between the results for single-plant and multi-plant firms. As discussed in Section 4.2, because of their greater size and geographic dispersion, multi-plant firms are less likely to depend on local banks when financing their operations in a given state. However, the local plants of these firms in states that deregulate will still be exposed to local economic changes possibly correlated with deregulation such as changes in factor prices or local demand in output markets. The fact that we find significant changes in the relative growth of higher marginal product firms only among firms with significant exposure to local banking markets supports the view that our previous findings capture state banking deregulation.

We further address these identification concerns by examining trends in $LRSens$ prior to banking deregulation in our sample of single-plant firms. We analyze this issue by adding $Dereg(-1\ to - 5)$ to the estimation of (7). This variable is an indicator that equals one in the five years prior to deregulation and is included in an analogous way to $Dereg$. We follow this approach both with our deregulation index, which includes both types of deregulation, and with the separate effect of interstate deregulation. We examine interstate deregulation events because they are associated with stronger effects in our main results and happen with higher frequency during our sample period. Columns (1) and (2) in Panel A of Table 8 show that states do not experience differential changes in $LRSens$ in the five years prior to deregulation. Figure 1 breaks down these effects across the five years prior to deregulation, normalized by our previously estimated effects associated with $Dereg$. These results further show that deregulation is not associated with a positive differential trend in $LRSens$ in the years prior to deregulation. These results provide

[31] We discuss potential reasons for such asymmetry in Section 6. We have addressed measurement issues by also using firm investment as an outcome in all capital reallocation results. Jaraytane and Strahan (1996) also find that state banking deregulation is not associated with significant changes in aggregate local manufacturing investment.

[32] In the analysis that follows, we focus on our main specification measuring gaps in labor marginal products with gross output per unit of labor. However, we have found similar conclusions with specifications using alternative approaches to measure gaps in marginal products, such as replacing gross output with value added.

support to the view that deregulation is not capturing previous positive trends differentially affecting higher marginal product firms.

We refine our findings by comparing only industries that are geographically and economically close to each other – but are in different, adjacent states which did not experience deregulation at the same time. We first include Census region fixed effects interacted with MPL as well as industry fixed effects interacted with MPL as additional controls in the estimation of (7). In this analysis, when we estimate the effect of state banking deregulation on local industries, we are only comparing local industries to other industries in the same region but different states. We are also controlling for industry-wide changes in reallocation in this analysis. As a consequence, the effects of banking deregulation are estimated by comparing the local labor reallocation of the same industry over the same year across different locations exposed to different degrees of banking deregulation.

This approach is motivated by the idea that, among the small manufacturing firms in our sample period, banking markets are more local than product markets. Petersen and Rajan (2002) estimate that the average distance between small firms and their bank lenders is approximately 50 miles during our sample period. Moreover, their estimate for this distance in early 1990s is 68 miles. Using plant level data from the commodity flow survey, Holmes and Stevens (2012) estimate average shipment distances for manufacturing plants in the size range of our sample between 330 and 420 miles in 1997. Therefore, industries that are geographically close within a certain distance range are arguably exposed to different banking markets but face similar product market conditions. By using only variation within an industry and only across geographically close regions, we attempt to further control for changes in product market conditions while estimating the effect of local banking deregulation.

We further refine this specification by also examining the effects of banking deregulation in a shorter window of time around deregulation events. Intuitively, we estimate the effect of banking deregulation by analyzing changes in labor reallocation in a five-year window after deregulation, relative to the five years prior to deregulation. We use industries in states that did not deregulate banking markets over this same window as a benchmark. More formally, we add $Dereg(-1\ to -5)$ to the estimation of (7) and replace $Dereg$ with $Dereg(1\ to\ 5)$ and $Dereg(6+)$. These last two variables are indicators that equal one in the five years after deregulation and from six years after deregulation on, respectively. We include these variables in an analogous way to $Dereg$. We then use the difference between the estimated coefficients of $Dereg(1\ to\ 5) \times MPL$ and $Dereg(-1\ to - 5) \times MPL$ to estimate the effects of deregulation. This shorter time window further addresses the concern that the results capture differential trends in reallocation across states.

Panel B of Table 8 reports results following this approach and from specifications that both restrict and do not restrict the analysis to shorter windows of time around banking deregulation events. As in Panel A of Table 8, we report results examining both the overall effect of deregulation and the separate effect of interstate deregulation. Across all specifications, we find that state banking deregulation is associated with economically and statistically significant increases in $LRSens$. The magnitude of these effects is similar and directly comparable to the ones in Panel B of Table 2. These results show that our findings are robust to comparing only industries which are geographically and economically close as well as using only shorter windows of time around deregulation.

Finally, we also consider a matching approach as an alternative strategy to implement these same ideas. We identify local industries that experienced deregulation in their states and construct a matched sample of geographically close industries in adjoining states that did not experience deregulation over that same period.[33] We then examine if the sensitivity of labor reallocation to the marginal product of labor differentially changed in treated industries, when compared to matched industries, around the time of their deregulation episode.

The main advantage of this approach relative to our previous analysis is that we can further narrow the distance between treated and control industries in the same region. For each industry that experiences deregulation during our sample, we first find the ten closest industries based on three-digit SIC codes that have the same 2-digit SIC code and Census region but in different states that did not experience a deregulation episode around the same time. More precisely, we only consider industries that did not experience a deregulation episode in a seven-year period centered in the treated industries' deregulation year. We then impose upper bounds of 1,000 and 500 miles in the maximum allowed distance between treated and control industries. By imposing these

[33] An example would be examining the Washington area SMSA and comparing the same industry in the adjacent states of Maryland and Virginia.

bounds, we construct two groups of treated and control industries with average distances equal to 292 and 215 miles, respectively. [34]

After constructing these samples of matched treated and control industries for each interstate deregulation episode, we estimate the following specification:

$$\Delta EmpShare_{isjct} = \alpha_{sjct} + \alpha_0 \times Treated_c \times MPL_{isjt} + \alpha_1 \times Post_{ct} \times MPL_{isjt} \quad (9)$$
$$+ \beta \times Treated_c \times Post_{ct} \times MPL_{isjt} + \delta \times X_{isjct} + \varepsilon_{isjct},$$

where $\Delta EmpShare_{isjt}$ is the change in the employment share of firm i in industry j, state s, time t, and episode c. The deregulation of the banking markets faced by each industry-state is indexed as a separate episode c. For any given episode, both the treated industry and the matched controls for that episode are included and the data covers a seven-year period centered in the deregulation year of the treated industry. The data for all episodes is then stacked.[35] α_{sjct} is a state-industry-episode-year fixed effect, $Treated$ is an indicator that equals one for the treated industry in a given episode, $Post$ is an indicator that equals one during the years after the treated industry's deregulation, *MPL* is the log of firm marginal product of labor, and *X* denotes age controls.

The coefficient of interest is β and tells us whether the sensitivity of labor reallocation to the marginal product of labor differentially changes in treated industries after their deregulation, relative to geographically and economically close control industries. As in the context of Equation (7), one can think about the estimation of this effect as capturing a differences-in difference estimator of changes in $LRSens$ around deregulation.

The main limitation of this approach is that we use less data and fewer states to estimate our results. As the number of states in this analysis is reduced, we cannot double cluster our standard errors at the state and industry level as in our previous results. Instead, we cluster our standard errors at the industry-state level in these results. We interpret these findings as an additional

[34] We focus on interstate deregulation episodes which are associated with stronger effects in our previous analysis and happen more frequently during our sample period. We measure the distance between two local industries as the average distance between their plants. Notice that these distances are lower than the previously discussed average shipment distances for manufacturing plants in the size range of our sample. This reinforces the idea that treated and control industries are exposed to similar product market conditions.

[35] Notice that, by construction, control industries do not experience deregulation during a given episode. Therefore, a given industry-state-year cannot be used as treated local industry in one episode and a control local industry in another episode. However, it might be used as a control for different episodes and appear multiple times in the data. We address the implications of this issue for statistical inference by clustering standard errors.

robustness check and notice that our previous results, which do not face this issue, are similar in spirit to this analysis.[36]

Table 9 reports the results. We find a significant increase in $LRSens$ for treated industries versus control industries in the years immediately following deregulation. The magnitude of this increase is directly comparable and similar to the ones in Panel B of Table 8. These results provide further evidence that our main findings are robust to controlling for product market conditions while estimating the effect of local banking deregulation.

Together, this evidence provides support to the view that our previous analysis connecting banking deregulation to increases in labor reallocation gains captures the effect of banking deregulation.

6. What Explains the Importance of Labor Reallocation Effects?

We next examine the potential economic mechanisms explaining why banking deregulation might lead to significant labor reallocation gains. We argue that our collective evidence is mostly consistent with one explanation for this effect. Namely, the idea that local banking deregulation directly affects the employment of small local firms with higher marginal returns to using labor. This effect could be driven by an overall improvement in the access to credit of local small firms or by an improvement in the quality of local credit. As small firms with better economic prospects are less financially constrained in expanding their employment, labor is reallocated towards these firms.

However, in principle, different mechanisms could lead changes in local banking markets to increase labor reallocation gains. For example, local banking markets could affect workers' behavior or the contestability of local product markets. They could also affect local firms through other channels such as changes in local input markets. We argue that it is challenging for these explanations to match two important results in our analysis. First, the lack of reallocation effects for the local plants of large and geographically diversified multi-plant firms as discussed in Section 4.2. Second, the robustness of our results to comparing only geographically and economically close industries as discussed in Section 5.

[36] We have found similar conclusions when we clustered standard errors at the industry level in this analysis.

It is natural to imagine that these alternative interpretations would predict *some* effect on the local plants of multi-plant firms. For example, the incentives for the workers of these firms and the contestability of their product markets would also change with local banking markets. Additionally, as we discussed in Section 5, differences in local banking markets are arguably related to limited differences in product markets in our results comparing only geographically and economically close industries. Even if significant differences in product market conditions across banking markets remain in this analysis, one would expect the results to significantly reduce in importance if they are driven by product market effects. Note that our results also provide direct evidence that labor effects are not driven by contemporaneous changes in capital (see Section 4.5).[37]

Another related question raised by our analysis is the reason for the asymmetry between our labor and capital reallocation results. In Section 4.5 we found that banking deregulation is associated with significant changes in the average employment growth of local firms but with no significant changes in their average capital growth or investment. This suggests that the previous asymmetry in reallocation results is a reflection of a broader asymmetry in the effects of banking deregulation across labor and capital.[38] We emphasize two possible explanations for this asymmetry. One possibility is that, as suggested by previous research, banking deregulation is associated with improvements in the quality of financial intermediation, such as increases in the market share of banks better able to predict credit risks (see Section 2.2). These improvements could matter mostly for the financing of labor. Because physical capital can serve as collateral or be leased directly from capital providers, these improvements might be less important for the financing of capital. Another possibility is that adjustment costs limit firms' capital responses to differences in local banking conditions. As differences in local banking market conditions are uncertain and might not persist, firms can become reluctant to make decisions which are costly to reverse in response to these conditions. Previous research has suggested that these considerations are significantly more important for capital than labor among U.S. firms (Bloom (2009)). This possibility predicts that our labor reallocation results should be more important when labor

[37] As discussed in Section 2.2, while we do not show the between link banking deregulation and local banking conditions, this relationship has been extensively documented in previous research.

[38] This asymmetry is unlikely to be driven only by data or measurement issues. It holds when we estimate all results using the same sample. It also holds when we estimate all capital results using firm investment instead of firm capital growth, which is arguably more exposed to measurement issues.

adjustment costs are less important. Previous research suggests that these costs are likely to be more important for more skilled workers (Hamermesh and Pfann (1996)) and we found evidence that our labor results are more important for industries that use more unskilled workers.[39]

7. Alternative Channels

We close our analysis by considering alternative channels through which banking deregulation might affect the aggregate productivity of local industries. We compare the importance of intensive-margin reallocation changes to labor, the focus of our previous analysis, to firm-level productivity changes and extensive margin changes through entry and exit decisions. We consider decompositions of industry productivity growth that isolate the contribution of these different changes to industry productivity growth. Because of space limitations, we only discuss our main findings and basic approach. We show our analysis in detail in the Internet Appendix.

In this analysis, we follow Olley and Pakes (1996) and measure industry productivity as a weighted average of firm productivity A_{ijt} and specify both gross-output and value-added production functions in (1). The weights in this measure capture firms' industry shares and, motivated by data availability and our previous results, we use firm employment shares. Following Foster, Haltiwanger, and Kriznan (2001) and others, we then use decompositions of industry productivity changes that isolate the contribution of the previous sources of industry productivity growth. As in our previous analysis, we estimate the impact of banking deregulation on specific components of annual industry productivity growth.

We first consider changes in the intensive-margin reallocation of resources in the context of this analysis. Reallocation gains now capture shifts in industry shares across firms with diverging productivities, as opposed to marginal products, but otherwise can be analyzed in a similar way to our previous results. We find that banking deregulation is associated with significant increases in marginal reallocation gains, with similar magnitudes to our previous analysis.

We then consider the role of changes in firm productivity. Previous research has provided evidence that banking deregulation is associated with increases in firm-level productivity (Krishnan, Nandi, and Puri (2014), hereafter KNP). One interpretation for such effect is that

[39] We measure differences in worker skill across local industries by using state-year-adjusted gaps in the average wage of local industries.

financing constraints limit firms' ability to adopt different technologies or management practices.[40] We find that banking deregulation is associated with increases in firm-level productivity, with magnitudes similar to the one reported in KNP. We quantify the effect of banking deregulation on industry productivity growth through this channel. When compared to our previous reallocation effects, these firm-level effects are the same sign but smaller in magnitude. These estimates suggest that the intensive margin productivity increases associated with deregulation mostly capture reallocation gains. These findings emphasize the importance of studying the implications of financial markets for productivity at the industry level, as opposed to only at the firm level.

These two previous channels capture the intensive margins through which industry productivity can change. The third component of our analysis captures the effect of banking deregulation on industry productivity through extensive margin effects due to changes in firms' entry and exit decisions. This potential effect is determined by the productivity gap between entrants or exiting firms and incumbent firms, and the level of entry and exit in the industry. We find changes in entry and exit along the lines of Kerr and Nanda (2009). However, also consistent with their findings, our results suggest that these effects had a limited impact on industry productivity growth. One simple explanation for these findings is it can be hard to predict the quality of new firms before they start operating and producing results. Therefore, changes in credit markets have a limited impact in improving the selection of firms at birth and matter more by shaping this selection at later stages.

Table 10 reports results summarizing these findings. Using the previously discussed estimates, we decompose the importance of the three previous channels in driving the overall estimated increase in industry productivity growth after credit market deregulation. These results illustrate that intensive-margin reallocation effects represent the central channel through which state banking deregulation events affect industry productivity.

8. Conclusions

We study how state banking deregulation in the U.S. affects the aggregate productivity of local industries by shaping the allocation of labor among firms. We find that the deregulation of local U.S. banking markets leads to significant increases in the reallocation of labor within local

[40] Krishnan, Nandi, and Puri (2014) also analyze the effect of state banking deregulation but focus on measures taken by states to limit their exposure to national legislation allowing banks to operate across states from 1994 on.

industries towards firms with higher marginal products of labor. We propose an approach to quantify the industry productivity gains from such increased reallocation by using plant-level data and industry productivity growth decompositions.

We find that labor reallocation effects lead to significant increases on the aggregate productivity of the average U.S. industry. We provide evidence that these labor effects are important relative to the effect of banking deregulation on industry productivity through changes in capital reallocation. We also provide evidence that the labor reallocation effects are not driven by changes in capital reallocation across the same set of firms.

Our results are robust to multiple approaches addressing concerns about the identification of banking deregulation effects. We find that our results are important for small single-plant firms that rely on local banks but are not important for the local plants of large multi-plant firms, which can arguably access national capital markets. Our results are also robust to conducting a difference-in-difference approach in geographically close markets that span states that have deregulated at different times.

Our evidence supports the view that banking deregulation relaxed financing constraints faced by small firms with high marginal products of labor in expanding their employment. We also analyze the effect of banking deregulation on industry productivity through changes in firm productivity or the entry and exit of firms. We find evidence that the labor reallocation channel is also significant when compared to these channels.

Overall, our analysis suggests that changes in labor reallocation can be an important channel through which financial markets affect aggregate productivity. This contrasts with a view that the allocation of labor is a sideshow for understanding how financial markets matter for aggregate productivity and growth. While in other contexts financial market reforms might have stronger effects on the allocation of capital, our results suggest the importance of analyzing how finance affects labor decisions, in addition to capital decisions.

Our results also provide direct evidence that financial markets can have first-order effects on aggregate productivity through changes in the intensive-margin allocation of resources, as opposed to only changes in firm entry or exit. Finally, they suggest that such effects can be economically important even in the United States which has relatively well-developed financial markets and where resource misallocation is often believed to be limited.

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Appendix A – Industry Productivity Growth Measure and Decomposition

A.1 Industry Productivity Growth Decomposition: Simple Case

We first consider the case where output prices are constant within an industry-year discussed in the text. Note that we can write equation (2) in the text as $Y_{jt} = \sum_{i\in I_{jt}} A_{ijt}F(SK_{ijt} \times K_{jt}, SL_{ijt} \times L_{jt}, SM_{ijt} \times M_{jt})$. The first-order condition for changes in Y_{jt} can therefore be written as:

$$\frac{dln(Y_{jt})}{dt} = \alpha_{jt}\frac{dln(K_{jt})}{dt} + \beta_{jt}\frac{dln(L_{jt})}{dt} + \gamma_{jt}\frac{dln(M_{jt})}{dt} + \sum_{i\in I_{jt}}\frac{dA_{ijt}}{dt}\frac{Y_{ijt}}{A_{ijt}Y_{jt}} + \sum_{i\in I_{jt}}\frac{\partial Y_{ijt}}{\partial L}\frac{L_{jt}}{Y_{jt}}\frac{dSL_{ijt}}{dt} \tag{A.1}$$

$$+ \sum_{i\in I_{jt}}\frac{\partial Y_{ijt}}{\partial K}\frac{K_{jt}}{Y_{jt}}\frac{dSK_{ijt}}{dt} + \sum_{i\in I_{jt}}\frac{\partial Y_{ijt}}{\partial M}\frac{M_{jt}}{Y_{jt}}\frac{dSM_{ijt}}{dt},$$

which leads to equation (4) in the text. In the Internet Appendix, we then show that we can approximate this expression for the reallocation gains of factor F as $\frac{Var\left(\frac{\partial Y_{ijt}}{\partial F}\right)}{E\left(\frac{\partial Y_{ijt}}{\partial F}\right)}\frac{F_{jt}}{Y_{jt}}FRSens_{jt}$, where $FRSens_{jt} = \frac{Cov\left(MPF_{ijt}, \frac{d\ln(SF_{ijt})}{dt}\right)}{Var(MPF_{ijt})}$, $MPF_{ijt} = ln\left(\frac{\partial Y_{ijt}}{\partial F}\right)$, and $Cov(.)$ and $Var(.)$ denote a covariance and variance in the industry, respectively. The approximation comes from the fact that we replace a regression coefficient in levels by one measured in logs adjusted based on the average value of the variables.

A.2 Industry Productivity Growth Decomposition: General Case with Firm-Specific Prices

Industry productivity growth will be given by the component of $\frac{1}{VA_{jt}}\left(\sum_{i\in I_{jt}} P_{ijt}\frac{dQ_{ijt}}{dt}\right)$ that cannot be predicted by the growth of industry aggregate factors. Note that we are assuming that firms face similar prices for materials. Under this assumption, valued-added growth at constant prices will also have a cost of materials term, but this term will only depend on the growth of aggregate industry materials and will not matter for industry productivity growth. The previous term can be expressed as $\left(\frac{1}{1-sm_{jt}}\right)\sum_{i\in I_{jt}} RS_{ijt}\frac{dln(Q_{ijt})}{dt}$, where $RS_{ijt} = \frac{P_{ijt}Q_{ijt}}{\sum_{i\in I_{jt}} P_{ijt}Q_{ijt}}$ captures industry revenue shares. We now define industry output growth as weighted average of firm real output growth or $\frac{dY_{jt}}{dt} = \sum_{i\in I_{jt}} RS_{ijt}\frac{dln(Q_{ijt})}{dt}$. Suppose that the real output production function of firms is given by $Q_{ijt} = B_{ijt}H(K_{ijt}, L_{ijt}, M_{ijt})$. Then we can write

$$\frac{dln(Q_{ijt})}{dt} = \frac{dB_{ijt}}{dt} + \alpha^0_{ijt}\frac{dln(K_{ijt})}{dt} + \beta^0_{ijt}\frac{dln(L_{ijt})}{dt} + \gamma^0_{ijt}\frac{dln(M_{ijt})}{dt}, \tag{A.2}$$

where α^0_{ijt}, β^0_{ijt} and γ^0_{ijt} denote the firm labor, capital, and materials real output elasticity, respectively. For any factor F, we can also write $\frac{dln(F_{ijt})}{dt} = \frac{dln(SF_{ijt})}{dt} + \frac{dln(F_{jt})}{dt}$. We can combine this last result with (A.2) and rewrite industry output growth as:

$$\frac{dln(Y_{jt})}{dt} = \alpha^0_{jt}\frac{dln(K_{jt})}{dt} + \beta^0_{jt}\frac{dln(L_{jt})}{dt} + \gamma^0_{jt}\frac{dln(M_{jt})}{dt} + \sum_{i\in I_{jt}}\frac{d\ln(B_{ijt})}{dt}\frac{Y_{ijt}}{Y_{jt}} \tag{A.3}$$

$$+\sum_{i\in I_{jt}}\beta_{ijt}^{0}\frac{Y_{ijt}}{Y_{jt}}\frac{d\ln(SL_{ijt})}{dt}+\sum_{i\in I_{jt}}\alpha_{ijt}^{0}\frac{Y_{ijt}}{Y_{jt}}\frac{d\ln(SK_{ijt})}{dt}+\sum_{i\in I_{jt}}\gamma_{ijt}^{0}\frac{Y_{ijt}}{Y_{jt}}\frac{d\ln(SM_{ijt})}{dt}.$$

The last three terms capture reallocation gains. For any factor F, the reallocation gain term in (A.3) can be written as $\sum_{i\in I_{jt}}P_{ijt}\frac{\partial Q_{ijt}}{\partial F}\frac{dSF_{ijt}}{dt}\frac{F_{jt}}{Y_{jt}}$. Intuitively, reallocation gains are now evaluated by replacing $\frac{\partial Y_{ijt}}{\partial F}$ with $P_{ijt}\frac{\partial Q_{ijt}}{\partial F}$. Let ε_{ijt} denote the elasticity of demand for a firms' product. We have that $P_{ijt}\frac{\partial Q_{ijt}}{\partial F}=\frac{\partial Y_{ijt}}{\partial F}\left(\frac{\varepsilon_{ijt}}{\varepsilon_{ijt}-1}\right)$. We can therefore rewrite the reallocation gain term in (A.3) as $FRG_{jt}^{0}=\sum_{i\in I_{jt}}\left(\frac{\varepsilon_{ijt}}{\varepsilon_{ijt}-1}\right)\frac{\partial Y_{ijt}}{\partial F}\frac{dSF_{ijt}}{dt}\frac{F_{jt}}{Y_{jt}}$. If this elasticity is constant within an industry and given by ε_{jt}, then we can write that $FRG_{jt}^{0}=\left(\frac{\varepsilon_{jt}}{\varepsilon_{jt}-1}\right)FRG_{jt}$, where FRG_{jt} denotes the factor reallocation gains with our previous output measure. A further decomposition of reallocation gains analogous to our previous one will lead to the same value for $FRSens_{jt}$ as before and potential reallocation gains which are now given by $\left(\frac{\varepsilon_{jt}}{\varepsilon_{jt}-1}\right)\frac{Var\left(\frac{\partial Y_{ijt}}{\partial F}\right)}{E\left(\frac{\partial Y_{ijt}}{\partial F}\right)}\frac{F_{jt}}{Y_{jt}}$.

A.3 Estimating Cumulative Effects from Increased Marginal Reallocation Gains

We formalize this analysis by asking the following question. Suppose that we hold constant over time (between years t and $t+\tau$) changes in an industry's total factors, firm-level productivity, as well as its firms' entry and exit decisions, including the output produced by firms in the first year they enter the industry. We also hold constant all industry conditions at year $t-1$, including the initial allocation of factors. How does the industry value added growth between $t-1$ and $t+\tau$ (measured at fixed current prices) changes after a given increase in $LRSens$ (with no change in potential gains)? As in the marginal decomposition analysis, this tells us an additional value added growth (at constant prices) due to changes in the reallocation of factors.

We denote the scenario with higher reallocation and the scenario with lower reallocation as R and N, respectively. We focus on the case where output prices are constant within an industry-year and rely on our previous analysis showing how to estimate reallocation gains in a more general case using the gains estimated in this special case. Denote $Y_{t+\tau}^{k}$ as the industry output produced in scenario k by firms that exist in the industry at year $t+\tau$. Note that the answer to our previous question is given by $\frac{Y_{t+\tau}^{R}-Y_{t+\tau}^{N}}{Y_{t+\tau}^{N}}$ and only depends on the output produced by the firms present in year $t+\tau$. In the Internet Appendix we show that, under plausible assumptions, we can write:

$$\frac{Y_{t+\tau}^{R}-Y_{t+\tau}^{N}}{Y_{t+\tau}^{N}}\approx(1-s)RG_{jt+\tau}+(1-s)^{2}\left(\frac{\theta}{1+\mu}\right)RG_{jt+\tau-1}+\cdots+(1-s)^{\tau}\left(\frac{\theta}{1+\mu}\right)^{\tau-1}RG_{jt},\qquad\text{(A.4)}$$

where s denotes the share of industry output produced by new entrants, μ is the annual growth rate of firm TFP, θ measures the annual persistence of industry cross-sectional differences in TFP, and RG_{jt} denotes the previous reallocation gains. As discussed in the text, the discount rates used in this sum can be directly measured using simple moments in our sample.

Appendix B – Structural Estimation of Production Functions

We follow the general framework for the estimation of production functions outlined in Ackerberg, Benkard, Berry, and Pakes (2006, hereafter ABBP). ABBP present a basic framework and then consider some ways in which they can be adapted to accommodate different assumptions. We adapt our assumptions to explicitly incorporate the issues considered in our analysis. In this way, our analysis estimating production functions is internally consistent with our other results. After describing our set up and key assumptions, we propose and discuss the implementation of an approach to estimate the production function specified in (1). Our approach explicitly addresses both simultaneity and selection biases involved in this estimation.

Set Up

We specify a Translog production function at the plant-level. When we estimate production functions, we use sample weights to make the sample representative of the universe of plants. We note that more than 75 percent of manufacturing plants in this universe are single-plant firms. For simplicity, we denote i as a firm and use the same notation as in Section 2.1. We denote $x_{ijt} = log(X_{ijt})$. We write the production function in (1) as:

$$y_{ijt} = \beta_0 + \beta_a age_{ijt} + \alpha_k k_{ijt} + \alpha_l l_{ijt} + \alpha_m m_{ijt} + \alpha_{kl} k_{ijt} l_{ijt} + \alpha_{km} k_{ijt} m_{ijt}$$
$$+\alpha_{lm} l_{ijt} m_{ijt} + \frac{\alpha_{ll}}{2} l_{ijt}^2 + \frac{\alpha_{kk}}{2} k_{ijt}^2 + \frac{\alpha_{mm}}{2} m_{ijt}^2 + \omega_{ijt} + \epsilon_{ijt}, \qquad \text{(A.5)}$$

where ϵ_{ijt} is a shock revealed to firms at time t after all decisions have been made, age_{ijt} is the firm's age and ω_{ijt} is a productivity component observed by the firm before making decisions in year t. As discussed in the text, we estimate the production function (A.5) separately for each industry (3-digit SIC code). For expositional simplicity, we omit the industry subscript from production function parameters. Both ω_{ijt} and ϵ_{ijt} are unobservable to the econometrician. We assume that firms' capital stock k_{ijt} is determined in year $t-1$ but that firms can adjust their labor (facing potential costs or constraints) in year t.

Note that the labor elasticity is given by $\beta_l = \alpha_l + \alpha_{kl} k_{ijt} + \alpha_{lm} m_{ijt} + \alpha_{ll} l_{ijt}$ and can be computed using this expression once we have estimated production function parameters. Firm tfp is given by $TFP_{ijt} = \beta_0 + \beta_a age_{ijt} + \omega_{ijt} + \epsilon_{ijt}$ and can be inferred using (A.5) if we know production function parameters.

We first specify a process for the unobserved productivity ω_{ijt}. A central reason to expect potential biases while estimating (A.5) is that firms have expectations about ω_{ijt} when making decisions and these expectations predict differences in future output. Even if we address potential biases in estimating (A.5) using past decisions as an instrument for current decisions we still face issues. Therefore, it is important to specify how firms can predict future productivity with their current information set. ABBP assume that ω_{ijt} follows a first-order Markov process. Under this condition, once we have controlled for ω_{ijt-1}, we can use past decisions as instruments. The reason is that any other factor affecting their decisions in the last year will not predict the current output. Note that in a fixed effects model in panel data this omitted factor is constant over time. In ABBP this omitted factor can change over time.

We relax this assumption and assume that:

$$p(\omega_{ijt+1}|I_{ijt}) = p(\omega_{ijt+1}|\omega_{ijt}, Dereg_{ijt}) \quad \text{(A.6)}$$

where $Dereg_{ijt}$ is our banking deregulation index for the state in which the plant is located, I_{ijt} is the entire information set of the firm in year t, and $p(.)$ a probability distribution. Our motivation is to allow banking deregulation to affect the productivity process and growth within an industry. Previous research and our own evidence suggest the importance of allowing for this possibility.

We next specify that, conditional on the sample of firms with positive investment $i_{ijt} > 0$, we can write:

$$\omega_{ijt} = h_t(age_{ijt}, k_{ijt}, i_{ijt}, l_{ijt}, Dereg_{ijt}). \quad \text{(A.7)}$$

This condition can be derived from the two following assumptions that we make. The first assumption is that investment and employment decisions in year t only depend on age_{ijt}, k_{ijt}, $Dereg_{ijt}$ and ω_{ijt}. The second assumption is that conditional on positive investment, age_{ijt}, k_{ijt}, and $Dereg_{ijt}$, then (i_{ijt}, l_{ijt}) is different for different values of ω_{ijt}. In other words, one can recover ω_{ijt} from the joint investment and labor decisions. Intuitively, as firms expect higher levels of future productivity they monotonically adjust these decisions.

This condition is imposed by Olley and Pakes (1996, hereafter OP) without the l_{ijt} and $Dereg_{ijt}$ terms. In their setting, firms can freely adjust labor in each period and there are no effects from state banking deregulation. Their assumption is that only age_{ijt}, k_{ijt} and ω_{ijt} will matter for investment decisions. Additionally, conditional on age_{ijt} and k_{ijt}, investment will be a monotonic function of ω_{ijt} for firms with positive investment.

Given the emphasis in our analysis that firms need to finance some of their labor, we are allowing firms to potentially face costs or constraints in adjusting their labor. As a consequence, both investment and employment decisions are affected by firms' expectations about future productivity and appear in (A.7). This extension of OP is discussed by ABBP. We also allow banking deregulation to affect firms' investment and employment decision and change the link between these choices and both firm productivity, age and initial capital.

It is important to clarify why the two previous assumptions leading to (A.7) are consistent with the potential effect of financing constraints on both the investment and labor decisions of firms.

First, there is a concern is that financing frictions would lead firm decisions to depend on more conditions, in addition to age_{ijt}, k_{ijt}, $Dereg_{ijt}$ and ω_{ijt}. These variables capture fundamental aspects of financing frictions. As long as differences in the importance of financing frictions are largely reflected in these variables, this assumption is plausible. Note that size and age, the two main firm characteristics associated with financing constraints, are captured by k_{ijt} and age_{ijt}, respectively. Note that financing frictions could also depend on the cash flows of firms and this will be reflected in ω_{ijt}. Another source of differences in the financing conditions faced by firms is local credit conditions and $Dereg_{ijt}$ is an important source of variation in these conditions.

Second, there is a concern that financing constraints would limit firms' ability to respond to better conditions by investing and expanding their labor, leading (i_{ijt}, l_{ijt}) to not increase monotonically with ω_{ijt}. We note that this assumption only requires that there is some positive increase in at least one of these decisions as ω_{ijt} increases. This increase can be arbitrarily small. More importantly, we note that higher values for ω_{ijt} will lead to higher cash flows today, and the assumption is that firms will either invest more or expand their employment today. In other words, the assumption is not about some change in the information set not reflected in current cash flows that financially constrained firms are unable to respond to. Intuitively, increases in cash flows provide a source of financing for financially constrained firms to respond to increases in ω_{ijt}.

Finally, our third condition deals with potential selection problems in conditioning in the sample of firms that operate. We assume that firms make the decision to operate or not in a year before observing ϵ_{ijt} (realized only after production) but after observing the value of ω_{ijt}. Therefore, there is a potential selection on ω_{ijt}. We then assume that firms operate only if ω_{ijt} is sufficiently high or, more precisely, under the following condition:

$$\omega_{ijt} \geq \pi_t(age_{ijt}, k_{ijt}, l_{ijt-1}, Dereg_{ijt-1}). \tag{A.8}$$

This condition essentially assumes that firms' decisions to operate is monotonic in ω_{ijt} and that the only relevant state variables are age_{ijt}, k_{ijt}, $Dereg_{ijt}$ and l_{ijt-1}. OP assume a similar condition without the $Dereg_{ijt}$ and l_{ijt-1} terms. We extend their condition to allow for the costly adjustment of labor and for the possibility that deregulation affects firms' ability and incentives to enter or exit the industry. Note that, as previously discussed, these variables will also capture financial conditions. Therefore, we are also allowing financing conditions to possibly affect firms' decisions to operate.

Identification

We now discuss how we identify the parameters in (A.5) in the previous set up. Let P_{ijt} denote the probability that a firm operates conditional on I_{ijt-1}. Notice that, given assumptions (A.6) and (A.8), we can write:

$$P_{ijt} = \varphi_t(age_{ijt-1}, i_{ijt-1}, k_{ijt-1}, l_{ijt-1}, Dereg_{ijt-1}). \tag{A.9}$$

Let X_{ijt} denote an indicator that equals one if firms decide to operate. A key implication from the previous assumptions is that we can write $E\left(\omega_{ijt} \middle| I_{ijt-1}, X_{ijt} = 1\right) = g\left(\omega_{ijt-1}, Dereg_{ijt-1}, P_{ijt}\right)$. This is essentially extending (A.6) to incorporate selection effects and shows that conditioning on P_{ijt} is enough to incorporate such effects. To see this result, note that (A.6) and (A.8) imply that $E\left(\omega_{ijt} \middle| I_{ijt-1}, X_{ijt} = 1\right) = g\left(\omega_{ijt-1}, Dereg_{ijt-1}, \pi_t(.)\right)$. If we assume that $p\left(\omega_{ijt} \middle| \omega_{ijt-1}, Dereg_{ijt-1}\right)$ always has positive density (possibly over a bounded set) then there is a one-to-one mapping between $\pi_t(.)$ and P_{ijt} conditional on ω_{ijt-1} and $Dereg_{ijt-1}$. We can then write our previous condition using P_{ijt}. Note also that $E\left(\epsilon_{ijt} \middle| I_{ijt-1}, X_{ijt} = 1\right) = 0$ since firms decide to operate or not before observing the shock ϵ_{ijt}.

The previous two results imply that we can write (A.5) as:

$$y_{ijt} = \beta_0 + \beta_a age_{ijt} + \alpha_k k_{ijt} + \alpha_l l_{ijt} + \alpha_m m_{ijt} + \alpha_{kl} k_{ijt} l_{ijt} + \alpha_{km} k_{ijt} m_{ijt}$$

$$+\alpha_{lm} l_{ijt} m_{ijt} + \frac{\alpha_{ll}}{2} l_{ijt}^2 + \frac{\alpha_{kk}}{2} k_{ijt}^2 + \frac{\alpha_{mm}}{2} m_{ijt}^2 + g(\omega_{ijt-1}, Dereg_{ijt-1}, P_{ijt}) + \mu_{ijt}, \quad \text{(A.10)}$$

where

$$E(\mu_{ijt} | I_{ijt-1}, X_{ijt} = 1) = 0. \quad \text{(A.11)}$$

Conditions (IA.14) and (IA.15) tell us that, if we can control for ω_{ijt-1} and P_{ijt}, we can estimate production function parameters using decisions made by firms until $t-1$ as instruments for firms' input choices in year t. This approach holds despite the fact that we estimate it using the sample of firms that (endogenously) decided to operate in year t. (A.11) implies that all such decisions will be orthogonal to μ_{ijt} conditional on $X_{ijt} = 1$.

While we do not directly observe ω_{ijt-1} and P_{ijt}, conditions (A.7) and (A.9) determine them as a function of observable variables. Therefore, by controlling for these observables and using decisions made by firms until $t-1$ as instruments for firms' input choices in year t, we can identify production function parameters using (A.10) and (A.11). More formally, we assume that the conditions (A.7), (A.9), (A.10) and (A.11) uniquely identify the production function parameters.

Implementation

We propose and implement a simple procedure to find a solution to the moment conditions (A.7), (A.9), (A.10) and (A.11) in the sample. We assume that all unknown functions in these conditions are well behaved and model them as third-degree polynomials so that any sequence of solutions to these conditions applied to the sample converges to their unique solution as the sample size increases. We therefore focus on simple procedure that provides one sequence of such solutions.

Our procedure uses an initial guess for the production function parameters and then searches for a fixed point using a feedback loop. We use as our initial guess the OLS estimates of (A.5). We add only year fixed effects to this specification. We then use the following loop which has three steps. In the first step we estimate predicted values for ω_{ijt} using (A.7). While we do not observe ω_{ijt} we use our guess for production parameters and (A.5) to compute $\omega_{ijt} + \epsilon_{ijt}$. We note that ϵ_{ijt} is orthogonal to all variables used to predict ω_{ijt} in (A.7). We therefore estimate (A.7) using $\omega_{ijt} + \epsilon_{ijt}$ as the dependent variable and obtain predicted values $\widehat{\omega}_{ijt}$. In the second step, we estimate predicted values for P_{ijt} using (A.9). We obtain predicted values $\widehat{P}_{ijt}$. Finally, in the third step we estimate (IA.14) using firms' past input choices as instruments for current input choices and replacing ω_{ijt} and P_{ijt} with $\widehat{\omega}_{ijt}$ and $\widehat{P}_{ijt}$, respectively. This leads to new estimates for the production function parameters which we use as an updated guess in a new first step. We stop our procedure only when it leads to a fixed point.

Our first step is estimated in the sample where $i_{ijt} > 0$ because of condition (A.7). This leads us to estimate the results in the sample with $i_{ijt-1} > 0$ in step 3 as we use $\widehat{\omega}_{ijt-1}$ as a control. We note that using this selected sample does not affect our analysis because (A.11) implies that i_{ijt-1} is orthogonal to μ_{ijt}. In other words, we are conditioning on a variable unrelated to μ_{ijt}.

Appendix C – Variable Definitions

As described in Section 2, our main data sources are the Longitudinal Business Database (LBD), the Census of Manufacturers (CM), and the Annual Survey of Manufacturers (ASM) from the U.S. Census Bureau. Across all variables, industries are defined as 3-digit SIC codes.

Employment – total firm employment from the LBD. Given our sample of single-plant firms, this is the same as total establishment employment.

Employment Growth - change in the log of firm employment between years t and $t-1$.

Employment Share – share of the industry-state employment.

Employment Share Growth – change in the log of the share of industry-state employment between years t and $t-1$. For any given year t, this variable is only defined for the sample of firms in the data in both years t and $t-1$. Total industry-state employment in both year t and year t-1 are computed only including these firms.

Sales – total value of shipments from the CM adjusted with industry deflator.

Age – firm age measured using the LBD.

MPL – log of the marginal product of labor. In our main results, we measure gaps in *MPL* using the log of output per unit of labor (total hours). We measure output using both gross output and value added. We compute these values using the CM. As an alternative approach, we also estimate production function parameters for each industry or industry-year using the methods outlined in Section 2.3. We then compute marginal products using the CM and estimated parameters. For any given year, this variable uses the estimated marginal product using data from the latest CM. In the sample of single-plant firms, this is computed as the plant's *MPL*. In the samples of multi-plant firms, this is computed as the weighted average of plant-level values, where the weights capture the employment share of plants in the firm-state-industry.

MPK – log of the estimated marginal product of capital. We follow the same approach as in the construction of *MPL.*

Capital Share Growth – change in the log of the capital share between years t and $t-1$. For any given year t, this variable is only defined for the sample of firms in the data in both years t and $t-1$. Total industry-state capital in both year t and year t-1 are computed only including these firms.

Capital Share – share of the industry-state capital stock.

Investment – ratio of capital expenditures to the one-year lag of the capital stock.

MPL and MPK dispersion (within industry-state-year) - we first estimate the values of MPL and MPK as previously described. We then compute the difference between each of these variables and their average value in their industry-state-year. Finally, we compute the standard deviation of these demeaned variables.

Table 1

This table presents summary statistics for different variables and estimates used in the paper. Table A shows summary statistics for the main sample used in the paper, which covers single-plant firms. Variable definitions are in Appendix C. Panel B and C report the within industry-state dispersion in the marginal product of labor and marginal product of capital using different approaches to measure gaps in marginal products, respectively. Y/L and Y/L - VA denote the approaches using output per unit of labor with gross output and value added, respectively. The number of observations has been rounded to the nearest hundred following the Census Bureau's disclosure policy.

Panel A: Summary Statistics				
Variable	Mean	Std	Nobs	
Employment Growth	0.0089	0.4621	2,287,100	
Employment Share	0.0272	0.0834	2,287,100	
Employment Share Growth	-0.0131	0.4570	2,287,100	
Employment	22.28	46.23	2,287,100	
Sales ($1K 1987)	1,446	4,047	2,287,100	
Age	7.06	4.49	2,287,100	
Dereg	1.1833	0.8408	2,287,100	
Intrastate_Dereg	0.6810	0.4661	2,287,100	
Interstate_Dereg	0.5023	0.5000	2,287,100	
Exit	0.0685	0.2526	2,287,100	
Entry	0.0819	0.2743	2,795,000	
Panel B: Dispersion in MPL (within industry-state-year)				
	Y/L	Y/L - VA	OLS	Structural
Std of *MPL*	0.5234	0.5361	0.4527	0.4280
Panel C: Dispersion in MPK (within industry-state-year)				
	Y/K	Y/K - VA		
Std of *MPK*	0.4681	0.5096		

Table 2
Banking Deregulation and the Sensitivity of Labor Reallocation to Marginal Products

This table presents results linking the sensitivity of labor reallocation to the marginal product of labor within an industry-state (*LRSens*) to credit market deregulation. The results are all based on the sample of single-plant firms. Panels A and B report results from estimating equations (6) and (7), respectively. The dependent variable is the annual change in the log of the firm's industry-state employment share. For a given year t, this change in share is computed including only firms present in both year t and $t-1$. *MPL* is the log of the marginal product of labor, which can be based on output per unit of labor or translog production functions, with parameters estimated using the OLS or Structural approaches . Y/L and Y/L - VA denote the approaches using output per unit of labor with gross output and value added, respectively. *Dereg* is a banking deregulation index that equals the sum of *Intrastate_Dereg* and *Interstate_Dereg*. *Intrastate_Dereg* and *Interstate_Dereg* are indicators that equal one if the state has passed intrastate and interstate banking deregulation, respectively. The control variables in all regressions include the one-year lag of age, its squared value, as well as the interactions of both these variables with *Intrastate_Dereg* and *Interstate_Dereg*. The number of observations has been rounded to the nearest hundred following the Census Bureau's disclosure policy. Standard errors are heteroskedasticity robust and double clustered at the state and industry level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively. Panel B includes firm fixed effects in addition to state-industry-year fixed effects of Panel A. Panel C reports the percentage changes in *LRSens* implied by the effects in Panel B. These percentage changes are computed as the ratio of the effects in Panel B to the estimated value of *LRSens* in the subsample that has not passed deregulation.

Panel A: Initial Evidence

	Outcome: Change in Log of Employment Share			
	Y/L	Y/L - VA	OLS	Structural
	(1)	(2)	(3)	(4)
MPL	0.0235***	0.0195***	0.0217***	0.0191***
	(0.0022)	(0.0019)	(0.0024)	(0.0034)
MPL × *Dereg*	0.0084***	0.0089***	0.0070***	0.0103***
	(0.0017)	(0.0015)	(0.0018)	(0.0018)
Nobs	2,287,100	2,287,100	2,287,100	2,287,100
R-square	0.01	0.01	0.01	0.01
State-Industry-Year FE	Yes	Yes	Yes	Yes

Panel B: Main Specification								
Outcome: Change in Log of Employment Share								
	Y/L		Y/L - VA		OLS		Structural	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MPL × *Dereg*	0.0076***		0.0093***		0.0114***		0.0116***	
	(0.0014)		(0.0017)		(0.0020)		(0.0017)	
MPL × *Intrastate_Dereg*		0.0047***		0.0066***		0.0075***		0.0071***
		(0.0015)		(0.0014)		(0.0019)		(0.0020)
MPL × *Interstate_Dereg*		0.0098***		0.0115***		0.0147***		0.0159***
		(0.0036)		(0.0038)		(0.0046)		(0.0042)
Nobs	2,287,100	2,287,100	2,287,100	2,287,100	2,287,100	2,287,100	2,287,100	2,287,100
R-square	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
State-Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE x MP	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE x MP	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-State-Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Magnitude of Changes in Labor Reallocation - Main Specification				
Percentage Change in *LRSens*				
	Y/L	Y/L - VA	OLS	Structural
Dereg (Change from 0 to 2)	60.3%	88.4%	98.1%	106.2%
Intra_Dereg	18.7%	31.2%	32.2%	32.7%
Inter_Dereg	38.9%	54.2%	63.3%	73.0%

Table 3
Results Controlling for Differences in Worker Skill

This table presents the results in Panels B and C of Table 2 with additional controls for differences in worker skill across firms. In addition to age controls, we now also include the average wage of firms (wage) as controls in the estimation of (7). These additional control variables are the one-year lag of wage, its squared value, as well as the interactions of both these variables with *Intrastate_Dereg* and *Interstate_Dereg*. Y/L and Y/L - VA denote the approaches using output per unit of labor with gross output and value added to measure gaps in *MPL*, respectively. The number of observations has been rounded to the nearest hundred following the Census Bureau's disclosure policy. Standard errors are heteroskedasticity robust and double clustered at the state and industry level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Changes in Labor Reallocation Sensitivity

Outcome: Change in Log of Employment Share

	Y/L		Y/L - VA		OLS		Structural	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MPL × Dereg	0.0076***		0.0093***		0.0113***		0.0115***	
	(0.0014)		(0.0017)		(0.0020)		(0.0017)	
MPL × Intrastate_Dereg		0.0047***		0.0066***		0.0074***		0.0071***
		(0.0015)		(0.0014)		(0.0018)		(0.0020)
MPL × Interstate_Dereg		0.0098***		0.0114***		0.0146***		0.0158***
		(0.0035)		(0.0038)		(0.0045)		(0.0042)
Nobs	2,287,100	2,287,100	2,287,100	2,287,100	2,287,100	2,287,100	2,287,100	2,287,100
R-square	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
State-Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE x MP	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE x MP	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-State-Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Magnitude of Changes in Labor Reallocation

Percentage Change in *LRSens*

	Y/L	Y/L - VA	OLS	Structural
Dereg (Change from 0 to 2)	60.1%	88.0%	97.7%	106.2%
Intra_Dereg	18.6%	31.1%	32.1%	32.7%
Inter_Dereg	38.6%	53.8%	62.9%	72.9%

Table 4
Labor Reallocation Results with Multi-Plant Firms

This table presents results replicating the analysis in Panel B of Table 2 using multi-plant firms. The results link the sensitivity of labor reallocation to the marginal product of labor within an industry-state (*LRSens*) to credit market deregulation. The dependent variable is the annual change in the log of the firm's industry-state labor share. The results include all multi-plant firms in our database. The number of observations has been rounded to the nearest thousand following the Census Bureau's disclosure policy. Standard errors are heteroskedasticity robust and double clustered at the state and industry level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Outcome: Change in Log of Employment Share				
	Y/L		Y/L - VA	
	(1)	(2)	(3)	(4)
MPL× Dereg	0.0009		0.0015	
	(0.0024)		(0.0028)	
MPL× Intrastate_Dereg		-0.0008		0.0013
		(0.0037)		(0.0041)
MPL× Interstate_Dereg		0.0022		0.0012
		(0.0028)		(0.0036)
Nobs	330,000	330,000	330,000	330,000
R-square	0.003	0.003	0.003	0.003
State-Industry-Year FE	Yes	Yes	Yes	Yes
State FE x MP	Yes	Yes	Yes	Yes
Year FE x MP	Yes	Yes	Yes	Yes
Firm-State-Industry FE	Yes	Yes	Yes	Yes

Table 5

Banking Deregulation and Potential Labor Reallocation Gains

This table presents results linking the potential gains from labor reallocation within an industry-state to credit market deregulation. The results are from estimating equation (8) and are estimated in the sample of single-plant firms. Potential gains from labor reallocation are computed using equation (5). Y/L and Y/L - VA denote the approaches estimating these gains using output per unit of labor with gross output and value added, respectively. *Dereg* is a banking deregulation index that equals the sum of *Intrastate_Dereg* and *Interstate_Dereg*. *Intrastate_Dereg* and *Interstate_Dereg* are indicators that equal one if the state has passed intrastate and interstate banking deregulation, respectively.The number of observations has been rounded to the nearest thousand following the Census Bureau's disclosure policy. Standard errors are heteroskedasticity robust and double clustered at the state and industry level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Outcome: Log of Potential Labor Reallocation Gains								
	Y/L				Y/L - VA			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dereg	-0.007		-0.011		-0.020		-0.028	
	(0.033)		(0.034)		(0.033)		(0.033)	
Intrastate_Dereg		0.025		0.025		0.019		0.018
		(0.039)		(0.039)		(0.037)		(0.038)
Interstate_Dereg		-0.085*		-0.100*		-0.113*		-0.138**
		(0.051)		(0.052)		(0.067)		(0.062)
Nobs	11,000	11,000	11,000	11,000	11,000	11,000	11,000	11,000
R-square	0.12	0.12	0.01	0.01	0.13	0.13	0.01	0.01
State-Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE			Yes	Yes			Yes	Yes
Year FE	Yes	Yes			Yes	Yes		

Table 6

Magnitude of Industry Productivity Gains from Increased Labor Reallocation

This table presents results quantifying the industry productivity gains implied by the changes in labor reallocation gains among single-plant firms. These gains are additional percentage increases in value added due to the additional intensive margin reallocation of labor, and are estimated using equation (5). Panels A and B reports the gains implied by the results in Tables 2 and 5. We quantify the additional industry productivity growth implied by these results as well as the cumulative productivity gains that take place during the sample as a consequence of these effects. In order to quantify these gains, we use estimated values for the elasticity of labor based on two approaches (OLS and Structural). We specify a Cobb-Douglas production function when estimating this elasticity. Panel C reports the average values of this elasticity across different approaches.

Panel A: Gross-Output Specification

	Banking Deregulation Effect (Change in *Dereg* from 0 to 2)	
	OLS Labor Elasticity	Structural Labor Elasticity
Ind_Prod_Growth_1 (%VA)	0.61%	0.59%
Ind_Prod_Growth_2 (%VA)	0.82%	0.78%
Cum_Prod_Gain_1 (%VA)	2.69%	2.58%
Cum_Prod_Gain_2 (%VA)	3.59%	3.43%

Panel B: Value-Added Specification

	Banking Deregulation Effect (Change in *Dereg* from 0 to 2)	
	OLS Labor Elasticity	Structural Labor Elasticity
Ind_Prod_Growth_1 (%VA)	0.77%	0.72%
Ind_Prod_Growth_2 (%VA)	1.03%	0.96%
Cum_Prod_Gain_1 (%VA)	3.40%	3.15%
Cum_Prod_Gain_2 (%VA)	4.54%	4.20%

Panel C: Average Values for Estimated Labor Elasticity

	OLS	Structural
Gross-Output Specification	0.344	0.352
Value-Added Specification	0.740	0.727

Table 7
Banking Deregulation and Capital Reallocation

This table presents three sets of results based on the sample of single-establishment firms. Panel A presents results linking the sensitivity of capital reallocation to the marginal product of capital within an industry-state (*KRSens*) to banking deregulation. The results are based on the estimation of a specification analogous to Equation (7), replacing labor variables with capital variables. The dependent variable is the annual change in the log of the firm's industry-state capital share. For a given year *t*, this change in share is computed including only firms present in both year *t* and *t-1*. *MPK* is the log of the marginal product of capital. We measure gaps in *MPK* using differences in output per unit of capital. Y/K and Y/K - VA denote the approaches using output per unit of capital with gross output and value added, respectively. *Dereg* is a banking deregulation index that equals the sum of *Intrastate_Dereg* and *Interstate_Dereg*. *Intrastate_Dereg* and *Interstate_Dereg* are indicators that equal one if the state has passed intrastate and interstate banking deregulation, respectively. The control variables in all regressions include the one-year lag of age, its squared value, as well as the interactions of both these variables with *Intrastate_Dereg* and *Interstate_Dereg*. Panel B presents results replicating the effects in Panel B of Table 2 (columns (1) and (3)) with different outcome variables and the sample restricted to CM years. Panel C presents results linking the average employment growth, capital stock growth, and investment of firms to banking deregulation. These results are based on a linear regression of each of these outcome variables on *Dereg* and both firm-industry-state and industry-year fixed effects. The number of observations is rounded to the nearest hundred following the Census Bureau's disclosure policy. Standard errors are heteroskedasticity robust and double clustered at the state and industry level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Changes in the Sensitivity of Capital Reallocation to the Marginal Product of Capital

	Outcome: Change in Log of Capital Share			
	Y/K		Y/K - VA	
	(1)	(2)	(3)	(4)
MPK× Dereg	-0.0018**		-0.0032*	
	(0.0008)		(0.0020)	
MPK× Intrastate_Dereg		-0.0025		-0.0028
		(0.0017)		(0.0021)
MPK× Interstate_Dereg		-0.0020		-0.0067*
		(0.0034)		(0.0037)
Nobs	397,700	397,700	397,700	397,700
R-square	0.141	0.059	0.120	0.059
State-Industry-Year FE	Yes	Yes	Yes	Yes
State FE x MP	Yes	Yes	Yes	Yes
Year FE x MP	Yes	Yes	Yes	Yes
Firm-State-Industry FE	Yes	Yes	Yes	Yes

Panel B: Changes in the Sensitivity of Labor versus Capital Reallocation to the Marginal Product of Labor

	Sample Restricted to Census Years					
	Outcome: Δlog(Emp Share)		Outcome: Δlog(K Share)		Outcome: Δlog(Emp Share) - Δlog(K Share)	
	Y/L	Y/L - VA	Y/L	Y/L - VA	Y/L	Y/L - VA
	(1)	(2)	(3)	(4)	(5)	(6)
MPL× Dereg	0.0047**	0.0053**	-0.0015**	-0.0018**	0.0064***	0.0068***
	(0.0023)	(0.0022)	(0.0006)	(0.0008)	(0.0024)	(0.0024)
Nobs	397,700	397,700	397,700	397,700	397,700	397,700
R-square	0.016	0.015	0.002	0.002	0.012	0.012
State-Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE x MP	Yes	Yes	Yes	Yes	Yes	Yes
Year FE x MP	Yes	Yes	Yes	Yes	Yes	Yes
Firm-State-Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Average Changes in Employment and Capital Stock Growth After Banking Deregulation

	Outcome: Employment Growth		Outcome: Capital Stock Growth	Outcome: Investment
	All Years	Census Years	Census Years	Census Years
	(1)	(2)	(3)	(4)
Dereg	0.0110**	0.0150**	0.0017*	0.0020**
	(0.0046)	(0.0078)	(0.0010)	(0.0009)
Nobs	397,700	397,700	397,700	397,700
R-square	0.008	0.014	0.002	0.002
Industry-Year FE	Yes	Yes	Yes	Yes
Firm-State-Industry FE	Yes	Yes	Yes	Yes

Table 8
Identification of Banking Deregulation Effects: Parallel Trends and Region Controls

This table presents results addressing the identification of the effect of banking deregulation on the sensitivity of labor reallocation to the marginal product of labor (*LRSens*) using the sample of single-plant firms. The results address the robustness of the effects in Panel B of Table 2 (columns (1) and (2)). The results in columns (1) and (2) of Panel A add *Dereg (-1 to -5)* and *Interstate_Dereg (-1 to -5)*, respectively. These results also include the interaction of the previous variables with *MPL* and age controls. *Dereg (-1 to -5)* equals the sum of *Interstate_Dereg (-1 to -5)* and *Intrastate_Dereg (-1 to -5),* indicators that equal one in the five years prior to interstate and intrastate deregulation, respectively. The results in Panel B add region-year and industry-year fixed effects as well as their interaction with *MPL*. The result in column (2) of Panel B replaces *Dereg* with *Dereg(1 to 5)* and *Dereg (6+),* indicators that equal one in the five years after deregulation and from six years after deregulation on, respectively. Column (2) of Panel B reports the difference between the estimated coefficients of *MPL× Dereg (1 to 5)* and *MPL× Dereg (-1 to -5),* and column (4) reports analogous results for *Interstate_Dereg.* Standard errors are heteroskedasticity robust and double clustered at the state and industry level. The number of observations has been rounded to the nearest hundred following the Census Bureau's disclosure policy. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Are Banking Deregulation Episodes Associated with Pre-Trends in Labor Reallocation?

	Outcome: Change in Log of Employment Share	
	Y/L	
	(1)	(2)
MPL × Dereg	0.0052***	
	(0.0014)	
MPL × Dereg (-1 to -5)	-0.0017	
	(0.0012)	
MPL × Interstate_Dereg		0.0099**
		(0.0049)
MPL × Interstate_Dereg (-1 to -5)		0.0006
		(0.0020)
Nobs	2,287,100	2,287,100
R-square	0.01	0.01
State-Industry-Year FE	Yes	Yes
State FE x MP	Yes	Yes
Year FE x MP	Yes	Yes
Firm FE	Yes	Yes

Panel B: Differential Deregulation Windows and Region Controls				
Outcome: Change in Log of Employment Share				
	Y/L			
	(1)	(2)	(3)	(4)
MPL × *Dereg*	0.0065***			
	(0.0013)			
MPL × *Dereg (1 to 5) -* *MPL* × *Dereg (-1 to -5)*		0.0076*** (0.0015)		
MPL × *Interstate_Dereg*			0.0094***	
			(0.0034)	
MPL × *Interstate_Dereg (1 to 5)-* *MPL* × *Interstate_Dereg (-1 to -5)*				0.0087*** (0.0035)
Nobs	2,287,100	2,287,100	2,287,100	2,287,100
R-square	0.01	0.01	0.01	0.01
State-Industry-Year FE	Yes	Yes	Yes	Yes
Region-Year FE x MP	Yes	Yes	Yes	Yes
Industry-Year FE x MP	Yes	Yes	Yes	Yes
State FE x MP	Yes	Yes	Yes	Yes
Year FE x MP	Yes	Yes	Yes	Yes
Firm-State-Industry FE	Yes	Yes	Yes	Yes

Table 9
Regional Matching Across States

This table presents results addressing the identification of the effect of banking deregulation on the sensitivity of labor reallocation to the marginal product of labor (*LRSens*) using a matching approach. Sample 1, 2, matches establishments with the same two-digit industry in the same Census region but in *different* states using a maximum distance of 1000 miles, 500 miles respectively. These results are the output from the estimation of equation (12). *Treated* is an indicator that equals one for industries in states that deregulate banking markets. *Post* is an indicator that equals one after banking deregulation dates. *MPL* is the marginal product of labor. We also include interactions of age controls with *Treated, Post*, and *Treated* × *Post*. Standard errors are heteroskedasticity robust and clustered at the state-industry level. The number of observations has been rounded to the nearest hundred following the Census Bureau's disclosure policy. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Outcome: Change in Log of Employment Share		
Interstate Deregulation		
	Y/L	
	Sample 1	Sample 2
	(1)	(2)
MPL × *Treated* × *Post*	0.0136***	0.0101*
	(0.0050)	(0.0057)
Nobs	914,500	704,000
R-squared	0.01	0.01
State-Industry-Year-Episode FE	Yes	Yes

Table 10

Banking Deregulation and Different Channels for Productivity Gains

This table reports results summarizing the estimated effects of state banking deregulation on the different components of industry productivity growth. The results are all based on the sample of single-plant firms. We first estimate the effect of state banking deregulation on three components of industry productivity growth: intensive-margin reallocation gains, firm-level productivity gains and extensive-margin reallocation gains. We then quantify the percentage of increased productivity growth associated with each of these three channels.

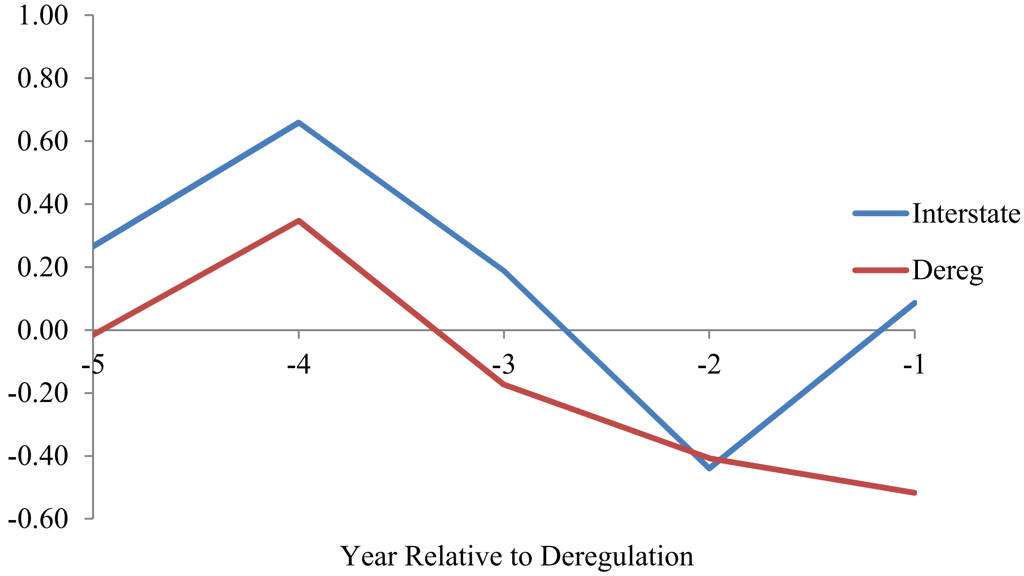
	Banking Deregulation Effect (Change in *Dereg* from 0 to 2)
Percentage of Gains from Intensive-Margin Reallocation Channel	61.7%
Percentage of Gains from Firm-Level Channel	32.0%
Percentage of Gains from Extensive-Margin Reallocation Channel	6.4%

Figure 1

Differences in Labor Reallocation Prior to Banking Deregulation

Change in Log of Employment Share Predicted by *MPL*

This figure presents results addressing the identification of the effect of banking deregulation on the sensitivity of labor reallocation to the marginal product of labor (*LRSens*). The results break down by year the effects of *Dereg (-1 to -5)* × *MPL* and *Interstate_Dereg (-1 to -5)* × *MPL* reported in Panel A of Table 8 (columns (1) and (2)). These results are estimated by replacing *Dereg (-1 to -5)* and *Interstate_Dereg (-1 to -5)* with five separate indicator variables for each of the five years prior to deregulation. These five coefficients are normalized by the estimated effect of deregulation in Panel B of Table 2 (columns (1) and (2)).



Internet Appendix for

"The Impact of Bank Credit on Labor Reallocation and Aggregate Industry Productivity"

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This appendix contains tables and proofs that supplement the analysis in the paper. Section 1 and Table IA.1 of this internet appendix implement robustness checks on the measurement of firm marginal product gaps. Section 2 and Tables IA.2 to IA.5 report the analysis of the alternative channels through which banking deregulation might affect the aggregate productivity of local industries. Table IA.6 of Section 3 shows the dates for interstate and intrastate banking deregulation episodes. Section 4 provides the proof for some of the results described in Appendix A. Finally, Section 4 also establishes the formal link between the industry productivity growth measure used in the paper and the approach in Petrin and Levinsohn (2012) to measure aggregate productivity growth.

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1. Measurement Issues

One concern with our measurement of marginal products comes from the fact that, as previously discussed in Section 3, we measure firms' marginal products using data from the last available Census of Manufacturers. We first note that the average distance between the last census and the current year in our sample is two years. We then provide direct evidence that differences in marginal products within an industry are highly persistent at such horizon and also show that our analysis is robust to including only years which are closer in time to the years in which marginal products are measured.

Table IA.1 reports the results. Panel A shows results analyzing the persistence of estimated MPLs across subsequent CMs. The results are the output of a linear regression linking MPL to the five-year lag of MPL and industry-state-year fixed effects. Panel B describes the annual persistence implied by the estimates in Panel A. Panel B also reports the persistence implied for the results in the paper. This last persistence is computed using the average distance of two years in the sample between MPL and the date of its measurement (previous CM). The results show that our measure of MPL is highly persistent at this horizon. The previous estimates imply that, on average, a within industry-state increase in the historical MPL by 10% predicts an increase between 5.7%- 6.6% in our measured MPL.

The previous finding shows that differences in our measured MPL will overestimate differences in the current MPL. This might lead us to underestimate the value of $LRSens$ and its changes in absolute value. As a consequence, this might also leads us to underestimate the absolute magnitude of industry productivity gains. We examine the significance of this bias and the extent to which is detectable by replicating our previous results in a subsample where this measurement issue is less important.

Panel C of Table IA.1 reports results replicating the specifications in Table 3 (columns (1) and (2) of Panel B) in a subsample of years at most two years apart from the previous CM. When compared to our results in Table 3, the average distance between our measured MPL and the latest CM now drops from two years to one year. The results show that our estimates remain economically and statistically similar to the ones in the paper and suggest that the previous bias is not economically large. Taken together, these findings provide evidence that this source of misspecification does not significantly affect our analysis.

2. Alternative Channels

In Section 7 we discuss our analysis of alternative channels through which banking deregulation might affect the aggregate productivity of local industries. We here show this analysis in greater detail. We compare the importance of intensive-margin reallocation changes, the focus of our previous analysis, to firm-level productivity changes and extensive margin changes through entry and exit decisions. We consider decompositions of industry productivity growth that isolate the contribution of these different changes to industry productivity growth.

In this analysis, we follow Olley and Pakes (1996) and measure industry productivity as a weighted average of firm productivity A_{ijt} in (1). The weights in this measure capture firms' industry shares and, motivated by data availability at an annual frequency and our main results, we use firm employment shares. Following Foster, Haltiwanger, and Kriznan (2001) and others, we then use decompositions of industry productivity changes that isolate the contribution of the previous sources of industry productivity growth. As in our previous analysis, we estimate the impact of banking deregulation on specific components of annual industry productivity growth. We also focus on the combined effect of interstate and intrastate deregulation as in our main results. One issue with these decompositions of industry productivity growth is that, in contrast with our previous approach, they evaluate productivity gains due to resource reallocation using differences in firm TFP instead of differences in marginal products (Petrin and Levinsohn (2012)). However, they allow one to incorporate the effects of entry and exit on industry productivity in a simple and tractable way and we use it to complement our previous analysis which addressed this issue.[1] Since we measure gaps in TFP in this analysis, we need to rely on estimated production functions and we follow the approaches discussed in Section 2.3.

2.1. Industry Productivity Decompositions

[1] Recent examples of papers following this approach to analyze the contribution of entry and exit to industry productivity include Collard-Wexler and De Locker (2014).

We first present the industry productivity decomposition used in our analysis. Let I_{jt} denote the set of firms that operates in industry j in year t. We also define the following groups of firms. B_{jt} is the set of firms that operate in the industry in both year t and $t-1$. This captures a set of continuing firms that appeared in our previous intensive-margin analysis. C_{jt} is the set of firms that operate in the industry in year $t-1$ but not in year t. D_{jt} is the set of firms that do not operate in the industry in year $t-1$ but do operate in year t. These last two groups of firms capture firms exiting and entering the industry, respectively.

We denote the (labor) share of a firm in the industry as $s_{ijt} = \frac{L_{ijt}}{L_{jt}}$ where $L_{jt} = \sum_{i \in I_{jt}} L_{ijt}$. Given this notation, we note that our previously discussed measure of industry productivity is given by $A_{jt} = \sum_{i \in I_{jt}} s_{ijt} A_{ijt}$. We then define industry productivity growth as $IPG_{jt} = \frac{A_{jt}}{A_{jt-1}} - 1$ and intensive-margin industry productivity growth as $IPG_{jt}^{B} = \frac{\sum_{i \in B_{jt}} s_{ijt-1}}{\sum_{i \in B_{jt}} s_{ijt}} \frac{\sum_{i \in B_{jt}} s_{ijt} A_{ijt}}{\sum_{i \in B_{jt}} s_{ijt-1} A_{ijt-1}} - 1$. The key difference between these measures is that IPG_{jt} computes productivity over time including all firms in each year while IPG_{jt}^{B} includes only a fixed subset of firms present over subsequent years. The difference between these growth rates is driven by entry and exit.

We show below that one can write industry productivity growth as:

$$IPG_{jt} = \frac{A_{jt-1}^{B}}{A_{jt-1}} \left(IPG_{jt}^{B} - SC_{jt-1} ExitGap_{jt-1} + SD_{jt} EntryGap_{jt} \right) + \Delta_{jt}, \qquad \text{(IA.1)}$$

where $A_{jt-1}^{B} = \frac{1}{\sum_{i \in B_{jt}} s_{ijt-1}} \sum_{i \in B_{jt}} s_{ijt-1} A_{ijt-1}$ is the productivity of continuing firms B_{jt} in year $t-1$; $SC_{jt-1} = \sum_{i \in C_{jt}} s_{ijt-1}$ measures the labor share of exiting firms C_{jt} in year $t-1$; $SD_{jt} = \sum_{i \in D_{jt}} s_{ijt}$ is the labor share of new entrants D_{jt} in year t; $ExitGap_{jt-1} = \left(\frac{A_{jt-1}^{C} - A_{jt-1}^{B}}{A_{jt-1}^{B}} \right)$ captures the gap in productivity between C_{jt} and B_{jt} in year $t-1$; $EntryGap_{jt} = \left(\frac{A_{jt}^{D} - \widetilde{A_{jt}^{B}}}{A_{jt-1}^{B}} \right)$ is the gap in productivity between D_{jt} and B_{jt} in year t; $A_{jt-1}^{C} = \frac{1}{\sum_{i \in C_{jt}} s_{ijt-1}} \sum_{i \in C_{jt}} s_{ijt-1} A_{ijt-1}$ is the productivity of C_{jt} in year $t-1$, $A_{jt}^{D} = \frac{1}{\sum_{i \in D_{jt}} s_{ijt}} \sum_{i \in D_{jt}} s_{ijt} A_{ijt}$ is the productivity of D_{jt} in year t, and $\widetilde{A_{jt}^{B}} = \frac{1}{\sum_{i \in B_{jt}} s_{ijt}} \sum_{i \in B_{jt}} s_{ijt} A_{ijt}$ is the productivity of B_{jt} in year t. Δ_{jt} is a second-order term that depends on the product of multiple changes and can be omitted in a first-order approximation for IPG_{jt} with annual data.

Intuitively, this expression illustrates how overall industry productivity growth is determined by the intensive-margin productivity growth and the contributions of entry and exit. These extensive margin effects will matter only to the extent that firms entering or exiting the industry have different productivities than continuing firms. Their contribution will be the product of the magnitude of these entry or exit productivity gaps by the level of entry or exit. When exiting firms have higher (lower) productivity than continuing firms then higher exit levels leads to lower (higher) productivity growth. When new entrants have higher (lower) productivity than continuing firms then higher exit levels leads to higher (lower) productivity growth.

To see the formal derivation of this result first note that we can write $IPG_{jt} = \frac{D_{jt}^{1} + D_{jt}^{2}}{A_{jt-1}}$ where:

$$D_{jt}^{1} = \sum_{i \in B_{jt}} s_{ijt} A_{ijt} - \sum_{i \in B_{jt}} s_{ijt-1} A_{ijt-1},$$

and

$$D_{jt}^{2} = \sum_{i \in D_{jt}} s_{ijt} A_{ijt} - \sum_{i \in C_{jt}} s_{ijt-1} A_{ijt-1}.$$

We then note that $\frac{D_{jt}^{1}}{A_{jt-1}} = g_{jt} \frac{\sum_{i \in B_{jt}} s_{ijt-1} A_{ijt-1}}{A_{jt-1}}$ where $1 + g_{jt} = \frac{\sum_{i \in B_{jt}} s_{ijt} A_{ijt}}{\sum_{i \in B_{jt}} s_{ijt-1} A_{ijt-1}}$. We define $1 + g_{jt}^{s} = \frac{\sum_{i \in B_{jt}} s_{ijt}}{\sum_{i \in B_{jt}} s_{ijt-1}}$ and also note that $1 + g_{jt} = (1 + g_{jt}^{s})(1 + IPG_{jt}^{B})$. By combining these results we have that $\frac{D_{jt}^{1}}{A_{jt-1}} = \left(g_{jt}^{s} + IPG_{jt}^{B}\right)\left(\frac{\sum_{i \in B_{jt}} s_{ijt-1} A_{ijt-1}}{A_{jt-1}}\right) + \Delta_{jt}^{1}$ where Δ_{jt}^{1} is a second-order term. We can further rewrite the previous expression as $\frac{D_{jt}^{1}}{A_{jt-1}} = \left(\frac{A_{jt-1}^{B}}{A_{jt-1}}\right)\left(SB_{jt-1} IPG_{jt}^{B} - SD_{jt} + SC_{jt-1}\right) + \Delta_{jt}^{1}$, where $SB_{jt-1} = \sum_{i \in B_{jt}} s_{ijt-1}$ measures the labor share of continuing firms B_{jt} in year $t-1$. After some additional algebra, the previous expression combined with the one for $\frac{D_{jt}^{2}}{A_{jt-1}}$ leads to equation (IA.1).

We then further decompose the intensive-margin growth in (IA.1) following an analysis analogous to the one in the paper. We can now write:

$$IPG_{jt}^{B} = \sum_{i \in B_{jt}} \frac{A_{ijt-1}}{A_{jt-1}^{B}} \frac{s_{ijt-1}}{\sum_{i \in B_{jt}} s_{ijt-1}} FPG_{ijt} + RG_{jt} + \mu_{jt}, \qquad \text{(IA.2)}$$

where $FPG_{ijt} = \left(\frac{A_{ijt}-A_{ijt-1}}{A_{ijt-1}}\right)$ is the firm-level productivity growth between year $t-1$ and t; $RG_{jt} = \frac{1}{A_{jt-1}^{B}} \sum_{i \in B_{jt}} A_{ijt-1} \Delta S_{ijt}^{B}$ denotes intensive-margin reallocation gains where $\Delta S_{ijt}^{B} = \frac{s_{ijt}}{\sum_{i \in B_{jt}} s_{ijt}} - \frac{s_{ijt-1}}{\sum_{i \in B_{jt}} s_{ijt-1}}$; μ_{jt} is a second-order term that depends on the product of multiple changes and can be omitted in a first-order approximation for IPG_{jt}^{B} with annual data.

This decomposition is similar to the one in our main analysis. The major difference relative to our previous approach is that we here evaluate the potential gains from reallocation using differences in TFP as opposed to marginal products. We can also further decompose these gains in a similar way to Equation (5):

$$RG_{jt} \approx \frac{Var(A_{ijt-1})}{E(A_{ijt-1})} \frac{1}{A_{jt-1}^{B}} TFPSens_{jt}, \qquad \text{(IA.3)}$$

where $TFPSens_{jt}$ is defined analogously to $LRSens_{jt}$ by replacing the marginal product of labor with firm tfp. This measure captures the extent to which industries reallocate resources towards higher tfp firms. The term $\frac{RG_{jt}}{TFPSens_{jt}}$ captures the potential gains from intensive-margin reallocation.

2.2. Changes in Intensive-Margin Reallocation

We follow the same steps used in our labor reallocation results in this context. We first estimate:

$$\begin{aligned} \Delta EmpShare_{isjt} = {} & \alpha_{sjt} + \mu_i + \gamma_s \times TFP_{isjt} + \theta_t \times TFP_{isjt} \\ & + \beta_1 \times Dereg_{st} \times TFP_{isjt} + \delta \times X_{isjt} + \varepsilon_{isjt}, \end{aligned} \qquad \text{(IA.4)}$$

where TFP_{isjt} is firm TFP, and all other variables are defined as in equation (7). We then estimate the implied percentage changes in $TFPSens_{jt}$ and combine them with direct estimates of percentage changes in potential reallocation gains. Finally, we use equation (IA.3) to estimate the increase in intensive-margin industry productivity gains associated with banking deregulation.

Panel A of Table IA.2 reports results from the estimation of equation (IA.4). We find that deregulation is also associated with significant increases in $TFPSens_{jt}$. As in the context of Table 3, one way to evaluate the magnitude of this effect is to consider the additional growth implied by one standard deviation in firm TFP. Banking deregulation leads one (within-industry-state-year) standard deviation in firm TFP to predict an additional employment growth between 0.80% and 0.91%. This additional growth equals 0.90-1.03 times the average employment growth in the sample. As in our previous analysis of labor reallocation, we have found that these effects represent significant percentage changes in $TFPSens_{jt}$ and are associated with much smaller changes between in the potential gains from reallocating factors. As before, changes in resource reallocation translate into significant percentage increases in marginal reallocation gains. We estimate these gains and combine them with the ones associated with other channels in Table IA.5. Table IA.2 also reports the absolute magnitude of these effects. As in our main results, we convert changes in gross output into value added at constant prices when estimating such magnitudes.[2] On average across different specifications, the results suggest that banking deregulation leads to an increase in annual industry value-added growth equal to 1.08%, a magnitude close to the one in our main results.

2.3. Changes in Firm-Level Productivity

We then consider changes in firm-level productivity. Previous research has provided evidence that banking deregulation is associated with increases in firm-level productivity (Krishnan, Nandi, and Puri (2014), hereafter KNP). One interpretation for such effect is that financing constraints limit firms' ability to adopt different technologies or management practices. We use a differences-in-difference specification to examine how deregulation is associated with changes in the productivity of a given firm in our sample.[3] More formally, we estimate:

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[2] This conversion allows us to relate these effects to value added growth, which aggregates across industries to GDP growth, and does not change the relative importance of the different channels here considered which are all scaled by the same factor.

[3] Krishnan, Nandi, and Puri (2014) also analyze the effect of state banking deregulation but focus on measures taken by states to limit their exposure to national legislation allowing banks to operate across states from 1994 on.

$$TFP_{isjt} = \mu_i + \theta_{jt} + \beta \times DeregYears_{st} + \delta \times X_{isjt} + \varepsilon_{isjt}, \tag{IA.5}$$

where *TFP* is firm tfp, μ_i denotes firm fixed effects, θ_{jt} are industry-year fixed effects, X are age controls, and *DeregYears* indicates the number of years since deregulation. This last variable is the sum of two variables separately constructed for each type of deregulation event (interstate or intrastate). For each event, this variable is equal to zero prior to deregulation and measures the number of years since the deregulation event.

The coefficient of interest β tells us how banking deregulation is associated with changes in firm productivity growth. Notice that this effect captures the additional increase (decrease) in firm productivity growth per year after deregulation.[4] Also note that, as in our previous results, the identification of this effect comes from differences in the timing of deregulation across states. What matters in our industry productivity growth decomposition is a weighted average of firm productivity growth. In our sample of small single-plant firms, we have found similar conclusions when we allowed the deregulation effects in (IA.5) to depend on firm revenue shares and examined changes in the weighted average of firm productivity growth.[5]

Table IA.3 presents the results. Consistent with the previous discussion, we find that deregulation is associated with significant increases in firm productivity growth. The magnitude of these effects is significant and similar to the one found in KNP. We combine this effect with the ones associated with other channels in Table IA.5. As in Section 2.3, we also measure the absolute value of these magnitudes by converting them into value added terms. The magnitude of these effects is comparable but smaller than the one from the previous intensive-margin reallocation results.

2.4. Changes in Extensive-Margin Reallocation

These two previous channels capture the intensive margins through which industry productivity can change. The third component of our analysis captures the effect of banking

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[4] For the purposes of our decomposition, we are interested in understanding how deregulation relates to the annual growth of industry productivity, as opposed to estimating how the average level of productivity changes after deregulation.

[5] As smaller firms are more affected and have a smaller weight, our simplified equally weighted approach will tend to overestimate the importance of firm-level changes in firm productivity. This approach is conservative given that we are interested in the relative importance of our previous intensive-margin effects.

deregulation on industry productivity through extensive margin effects due to changes in firms' entry and exit decisions. Intuitively, the contribution of entry and exit decisions to industry productivity growth is determined by two main factors. Namely, this contribution is determined by the productivity gap between entrants or exiting firms and incumbent firms, and the level of entry and exit in the industry. We use Equation (IA.1) to formalize this intuition and measure each of these terms. We then use once more a differences-in-difference specification to examine how deregulation is associated with changes in each of these terms.

Table IA.4 reports the results. We first examine changes in the productivity gap between entrants or exiting firms and incumbent firms. Equation (IA.1) tells us that we can measure these gaps at any point in time by simply comparing the within-industry-state-year percentage differences in TFP between firms entering (exiting) the industry and other firms. The results examine how these gaps change after banking deregulation. The entry results are estimated using the following specification:

$$TFP_{isjt} = \alpha_{sjt} + \gamma_s \times Entry_{isjt} + \theta_t \times Entry_{isjt}$$
$$+\ \beta \times Dereg_{st} \times Entry +\ \delta \times X_{isjt} + \varepsilon_{isjt}, \qquad \text{(IA.6)}$$

where TFP is firm tfp (measured in the next CM), α_{sjt} denotes state-industry-year fixed effects, γ_s are state fixed effects, θ_t are year fixed effects, $Dereg$ is a banking deregulation index (sum of interstate and intrastate indicators), $Entry$ is an indicator that equals one if the firm enters the industry in the current year and X denotes controls.

The coefficient of interest is β and tells us how the productivity gap for new entrants in a given state-industry changes after banking deregulation. As in the context of Equation (7), the estimation of β can be thought as a difference-in-differences estimation. Intuitively, one can think about this estimation as involving two steps. First, we estimate the productivity gap for new entrants within each industry-state-year. We then estimate how deregulation affects this relationship using a difference-in-differences specification. We are implementing these two steps together in a single regression.

The exit results are estimated using a similar specification:

$$TFP_{isjt} = \alpha_{sjt} + \gamma_s \times Exit_{isjt} + \theta_t \times Exit_{isjt}$$
$$+ \beta \times Dereg_{st} \times Exit + \delta \times X_{isjt} + \varepsilon_{isjt}, \qquad \text{(IA.7)}$$

where TFP is firm tfp (measured in the previous CM), $Exit$ is an indicator that equals one if the firm exits the industry in the current year and all other variables are defined in the same way as in Equation (IA.6). We include the same age controls used in our labor reallocation results in Table 3. As in the previous case, the coefficient of interest is β and its estimation can be thought as a difference-in-differences estimation.

Panels A of Table IA.4 report the results examining the productivity gaps of firms entering and exiting the industry, respectively. Across different specifications we find that banking deregulation is associated with changes in these productivity gaps. While not all results are statistically significant, both entry and exit effects tend to increase industry productivity. Deregulation is associated with an increase in the productivity gap of firms that enter the industry and a decrease in the productivity gap of firms that exit the industry.

Panel B reports results linking the level of entry and exit to banking deregulation using a difference-in-differences specification. The results are the output of a linear regression linking $Entry$ or $Exit$ to $Dereg$, year fixed effects and industry-state fixed effects. We find that deregulation is not associated with statistically significant changes in the level of entry and exit of small firms.[6]

Overall, this evidence suggests that deregulation did affect the entry and exit decisions of firms and that this change in entry and exit decisions contributed positively to industry productivity growth. Following Equation (IA.1), we multiply the estimated effects in Panel A by the average entry and exit rates in our sample, respectively. The sum of these values allows us to quantify how the previous changes in the productivity gaps of firms entering and exiting the industry affect industry productivity growth. We combine these effects with the ones associated with other channels in Table IA.5. As in Sections 2.3 and 2.4, we also measure the absolute value of these

[6] These estimates are analogous to the single-unit results in Panel A of Table 3 in Kerr and Nanda (2009). While Kerr and Nanda (2009) do not combine the two types of events into an average effect, the average of their effects for interstate and intrastate deregulation suggests similar conclusions to the one here discussed.

magnitudes by converting them into value added terms. The magnitude of the estimated effects is economically small when compared to the ones from the previous channels, especially when compared to the one from the intensive margin reallocation results.

These findings are consistent with the evidence in Nanda and Kerr (2009) in suggesting that deregulation plays a limited role in improving the quality (at least when measured by TFP) of the average small firms entering an industry. One simple explanation for these findings is it can be hard to predict the quality of new firms before they start operating and producing results. Therefore, changes in credit markets have a limited impact in improving the selection of firms at birth.[7]

2.4. Combining Different Channels

Table IA.5 reports results combining the previous magnitudes. Using the previously discussed estimates, we decompose the importance of the three previous channels in driving the overall estimated increase in industry productivity growth after banking deregulation. These results illustrate that intensive-margin reallocation effects represent a central channel through which state banking deregulation events affect industry productivity.

3. Banking Deregulation Dates

Table IA.6 shows the banking deregulation dates used in the paper. As described in the paper, we follow Amel (1993) and Kroszner and Strahan (1999) in determining the dates of interstate and intrastate deregulation. This table illustrates the large number of interstate deregulation episodes during our sample period.

4. Industry Productivity Growth Measure and Decomposition: Formal Results

We provide formal proofs for some of the results discussed in Appendix A of the paper. We also show the formal connection between the main industry productivity growth measure used in the paper and the approach in Petrin and Levinsohn (2012) to measure aggregate productivity growth.

[7] While our decomposition measures gaps in tfp at the date of entry or exit, we have found similar patterns when we examined measures of long-term differences in productivity between new entrants or exiting firms and incumbents.

4.1. Appendix A.1

We derive the approximation used to further decompose reallocation gains in greater detail. In Appendix A.1 we show that reallocation gains for factor F are given by $\sum_{i\in I_{jt}} \frac{\partial Y_{ijt}}{\partial L} \frac{L_{jt}}{Y_{jt}} \frac{dSL_{ijt}}{dt}$. Note that $\sum_{i\in I_{jt}} \frac{dSF_{ijt}}{dt} = 0$. Therefore, we can write reallocation gains as $\frac{F_{jt}}{Y_{jt}} N_{jt} Cov\left(\frac{\partial Y_{ijt}}{\partial F}, \frac{dSF_{ijt}}{dt}\right)$, where N_{jt} is the number of firms in I_{jt}, and $Cov(.)$ denotes a covariance in the industry. We can further rewrite these gains as $\frac{F_{jt}}{Y_{jt}} N_{jt} Var\left(\frac{\partial Y_{ijt}}{\partial F}\right) FRSensLevel_{jt}$, where $FRSensLevel_{jt} = \frac{Cov\left(\frac{\partial Y_{ijt}}{\partial F}, \frac{dSF_{ijt}}{dt}\right)}{Var\left(\frac{\partial Y_{ijt}}{\partial F}\right)}$. $FRSensLevel_{jt}$ is the additional increase in $\frac{dSF_{ijt}}{dt}$ predicted by a given increase in $\frac{\partial Y_{ijt}}{\partial L}$. More formally, $FRSensLevel_{jt}$ is the coefficient on $\frac{\partial Y_{ijt}}{\partial F}$ in a linear regression within the industry of $\frac{dSL_{ijt}}{dt}$ on the previous variable and a constant. $FRSensLevel_{jt}$ can be approximated using a sensitivity in percentage terms. We can approximate $FRSensLevel_{jt} \approx FRSens_{jt} \times \frac{E(SL_{ijt})}{E(\frac{\partial Y_{ijt}}{\partial F})}$, where $FRSens_{jt} = \frac{Cov\left(MPF_{ijt}, \frac{d\ln(SF_{ijt})}{dt}\right)}{Var(MPF_{ijt})}$ and $MPF_{ijt} = ln\left(\frac{\partial Y_{ijt}}{\partial F}\right)$. The approximation comes from the fact that we replace a regression coefficient in levels by one measured in logs adjusted based on the average value of the variables. $FRSens_{jt}$ can now be interpreted as the additional percentage change in factor shares (or factor growth) predicted by a given percentage difference in the marginal product of the factor. Since $E(SL_{ijt}) = \frac{1}{N_{jt}}$, we can approximate the factor's reallocation gains as $\frac{Var\left(\frac{\partial Y_{ijt}}{\partial F}\right)}{E\left(\frac{\partial Y_{ijt}}{\partial F}\right)} \frac{F_{jt}}{Y_{jt}} FRSens_{jt}$, what leads to equation (5) in the text. The potential gains from reallocating the factor are given by $\frac{Var\left(\frac{\partial Y_{ijt}}{\partial F}\right)}{E\left(\frac{\partial Y_{ijt}}{\partial F}\right)} \frac{F_{jt}}{Y_{jt}}$.

4.2. Appendix A.3

We now present the analysis leading to equation (A.4) in Appendix A.3 in greater detail. This expression is used to estimate the cumulative reallocation gains in the paper. We formalize this analysis by asking the following question. Suppose that we hold constant over time (between years t and $t+\tau$) changes in an industry's total factors, firm-level productivity, as well as its firms' entry and exit decisions, including the output produced by firms in the first year they enter the industry. We also hold constant all industry conditions at year $t-1$, including the initial allocation of factors. How does the industry value added growth between $t-1$ and $t+\tau$ (measured at fixed current prices) changes after a given increase in $LRSens$ (with no change in potential gains)? As in the marginal decomposition analysis, this tells us an additional value added growth (at constant prices) due to changes in the reallocation of factors.

We denote the scenario with higher reallocation and the scenario with lower reallocation as R and N, respectively. We focus on the case where output prices are constant within an industry-year and rely on our previous analysis showing how to estimate reallocation gains in a more general case using the gains estimated in this special case. Denote $Y_{t+\tau}^{k}$ as the industry output produced in scenario k by firms that exist in the industry at year $t+\tau$. Note that the answer to our previous question is given by $\frac{Y_{t+\tau}^{R}-Y_{t+\tau}^{N}}{Y_{t+\tau}^{N}}$ and only depends on the output produced by the firms present in year $t+\tau$. We label these firms as final firms. We denote Y_{t+s}^{k} as the output produced in year $t+s$ by the final firms that already exist in the industry in that same year, and Y_{At+s}^{k} as the output produced in year $t+s$ by final firms also present in year $t+s-1$. Additionally, let Y_{Bt+s}^{R}=Y_{Bt+s}^{N} denote the output produced in year $t+s$ by final firms that entered the industry in year $t+s$.

Let g_{t+s}^{k} denote the growth between year $t+s$ and $t+s-1$ of the output produced by final firms present in both of these years. We have that $1+g_{t+s}^{D} \equiv (1+g_{t+s}^{R})/(1+g_{t+s}^{N})$ captures the additional growth of final firms in year $t+s$ due to intensive margin reallocation. Finally, let $s_{t+s}^{k} \equiv Y_{Bt+s}^{k}/Y_{t+s}^{k}$ denote the share of total output produced by final firms that comes from new entrants.

Given this notation, we can approximate our answer as:

$$\frac{Y_{t+\tau}^{R}-Y_{t+\tau}^{N}}{Y_{t+\tau}^{N}} \approx (1-s_{t+\tau}^{N})\, g_{t+\tau}^{D} + (1-s_{t+\tau}^{N})(1-s_{t+\tau-1}^{N}) g_{t+\tau-1}^{D} + \cdots$$
$$+(1-s_{t+\tau}^{N}) \ldots (1-s_{1}^{N}) g_{1}^{D}. \tag{IA.8}$$

This approximation comes from the fact that we are ignoring compounding. This approximation will be accurate for the magnitudes we consider in the paper. To show (A.6), note that we can write

$\frac{Y_{t+s}^R - Y_{t+s}^N}{Y_{t+s}^N} = \left(\frac{Y_{At+s}^R - Y_{At+s}^N}{Y_{At+s}^N}\right)(1 - s_{t+s}^N)$ since $(1 - s_{t+s}^N) = \frac{Y_{At+s}^N}{Y_{t+s}^N}$. Note now that we can approximate $\frac{Y_{At+s}^R - Y_{At+s}^N}{Y_{At+s}^N} \approx \frac{Y_{t+s-1}^R - Y_{t+s-1}^N}{Y_{t+s-1}^N} + g_{t+s}^D$. This approximation comes from the fact that $Y_{At+s}^k = Y_{t+s-1}^k(1 + g_{t+s}^k)$. This leads to $\frac{Y_{t+s}^R - Y_{t+s}^N}{Y_{t+s}^N} \approx (1 - s_{t+s}^N)g_{At+s}^D + \left(\frac{Y_{t+s-1}^R - Y_{t+s-1}^N}{Y_{t+s-1}^N}\right)(1 - s_{t+s}^N)$. If we iterate this step, we arrive at (IA.8).

Intuitively, the terms g_{t+s}^D will capture marginal reallocation gains across final firms that exist in year $t + s$ and $t + s - 1$. We can use a first-order approximation, as in our previous analysis, to analyze these marginal gains. As before, we can write each factor's reallocation gain as $\frac{F_{jt}}{Y_{jt}} N_{jt} Cov\left(\frac{\partial Y_{ijt}}{\partial F}, \frac{dSF_{ijt}}{dt}\right)$. However, these gains now need to be estimated using final firms' productivity at the end of the period, as opposed to their productivity at the time of reallocation, as we are interested in understanding how they affect their final industry output. Moreover, this first-order condition for g_{t+s}^D will capture a sum over only final firms that existed in the industry in years $t + s$ and $t + s - 1$, as opposed to a sum across all industry firms that exist during that period.

Suppose now that current reallocation decisions are not correlated with future productivity shocks. To the extent that current reallocation is correlated with future productivity shocks, we will underestimate reallocation gains. Under this condition, we can write $g_{t+s}^D \approx RG_{t+s}^F \left(\frac{\theta}{1+\mu}\right)^{\tau - s}$ where RG_{t+s}^F is a first-order approximation to the reallocation gains of final firms computed with their productivity at the time of reallocation. Intuitively, θ captures the persistence of firm productivity and μ is the growth rate of firm-level productivity. In general, θ and μ can change by year. We have set them as constant for expositional simplicity. For any given factor F, let MPF_{t+s}^1 and MPF_{t+s}^2denote the marginal product of the factor under the productivity in the reallocation period and the final period, respectively. Note that all other determinants of marginal products are fixed in this comparison. We can write $A_{it+\tau} = \theta^{\tau - s} A_{it+s} + \varepsilon_{it+\tau}$, where $E(\varepsilon_{it+\tau}) = 0$. If current reallocation decisions are uncorrelated with future productivity shocks then $Cov\left(MPF_{t+s}^2, \frac{d\ln(SF_{ijt})}{dt}\right) = Cov\left(MPF_{t+s}^1, \frac{d\ln(SF_{ijt})}{dt}\right)\left(\frac{\theta}{1+\mu}\right)^{\tau - s}$ since $Cov\left(\varepsilon_{it+\tau}, \frac{d\ln(SF_{ijt})}{dt}\right) = 0$. Let Y_{t+s}^1 and Y_{t+s}^2 denote the output of final firms under the productivity in the reallocation period and the final period, respectively. As before, all other determinants of final firms' output are fixed in this comparison. We have that $Y_{t+s}^{2} = (1+\mu)^{\tau-s} Y_{t+s}^{1}$. Together, these two conditions lead to $g_{t+s}^{D} \approx \frac{F_{jt}}{Y_{t+s}^{2}} N_{jt} Cov\left(MPF_{t+s}^{2}, \Delta SF_{ijt}\right) = \frac{F_{jt}}{Y_{t+s}^{1}} N_{jt} Cov\left(MPF_{t+s}^{1}, \Delta SF_{ijt}\right) \left(\frac{\theta}{1+\mu}\right)^{\tau-s} =$ $RG_{t+s}^{F} \left(\frac{\theta}{1+\mu}\right)^{\tau-s}$.

Note that reallocation gains are computed as a percentage of output. An important condition we need for this analysis is that reallocation gains computed over the subset of final firms RG_{t+s}^{F} are similar to the ones computed across all firms in year $t+s$. This condition will hold if the dispersion of marginal products within final firms and within firms outside this subsample is significantly more important than the dispersion in marginal products across these two groups of firms. We have found that this is the case in our data. Under this condition, we can write:

$$\frac{Y_{t+\tau}^{R} - Y_{t+\tau}^{N}}{Y_{t+\tau}^{N}} \approx (1 - s_{t+\tau}^{N}) RG_{jt+\tau} + (1 - s_{t+\tau}^{N})(1 - s_{t+\tau-1}^{N}) \left(\frac{\theta}{1+\mu}\right) RG_{jt+\tau-1} + \cdots$$
$$+ (1 - s_{t+\tau}^{N}) \ldots (1 - s_{t+1}^{N}) \left(\frac{\theta}{1+\mu}\right)^{\tau-1} RG_{jt}. \tag{IA.9}$$

This equation leads to equation (A.8) in the paper.

4.3. Industry Productivity Growth Measure: Connection with LP Approach

As discussed in the text, our measure of industry productivity growth can be derived from the framework proposed by Levinsohn and Petrin (2012, hereafter LP) to measure economy-wide productivity growth with plant-level data. The framework proposed by PL allows one to measure the contribution of an industry to aggregate productivity growth (APG), which might come from expanding industry aggregate factors. We are only interested in productivity gains conditional on the aggregate factors of an industry and now show that our measure of industry productivity growth can be derived as a component of the PL measure that only captures this effect.

Using our previous notation, the contribution of an industry to APG is given by:

$$APG_{jt} = \frac{1}{VA_{jt}} \sum_{i \in I_{jt}} \left(P_{ijt} \frac{dQ_{ijt}}{dt} - P_{ijt}^{M} \frac{dM_{ijt}}{dt} - P_{ijt}^{L} \frac{dL_{ijt}}{dt} - P_{ijt}^{K} \frac{dK_{ijt}}{dt} \right), \tag{IA.10}$$

where P_{ijt}^{M}, P_{ijt}^{L}, and P_{ijt}^{K} denote the price of materials, labor, and capital, respectively. The sum of APG_{jt} across industries aggregates to the measure of economy-wide productivity growth in LP and Basu and Fernald (2002).

We assume that input prices are constant within an industry-year. In the context of labor, the major focus of our analysis, previous research has suggested that differences in wages across firms capture mostly differences in worker skill. Given our focus on the reallocation of production factors across firms, we are primarily interested in reallocation gains within a worker skill group. In our robustness checks, we show that our results are robust to controlling for differences in wages across workers.

If input prices are constant within an industry, then we can write (IA.10) as:

$$APG_{jt} = \frac{1}{VA_{jt}} \sum_{i \in I_{jt}} \left(P_{ijt} \frac{dQ_{ijt}}{dt} \right) - \frac{1}{VA_{jt}} \left(P_{jt}^{M} \frac{dM_{jt}}{dt} + P_{jt}^{L} \frac{dL_{jt}}{dt} + P_{jt}^{K} \frac{dK_{jt}}{dt} \right). \quad \text{(IA.11)}$$

We define industry productivity growth as the value of APG_{jt} in excess of what can be predicted by the growth of the aggregate factors. Given that the second term in (IA.11) can be fully predicted using aggregate factors, this definition is unchanged if we replace APG_{jt} with $\frac{1}{VA_{jt}} \left(\sum_{i \in I_{jt}} P_{ijt} \frac{dQ_{ijt}}{dt} \right)$. Note that this is exactly the definition of industry productivity growth that we used in our general case. Intuitively, the remaining component from APG_{jt} captures the gain from expanding industry aggregate factors, measured using the gap between the marginal product and the price of factors. This might measure economy-wide gains, but does not capture gains from using the same aggregate industry factors in a different way.

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Table IA.1

Persistence of Marginal Product Gaps and Measurement Error

This table presents results addressing the measurement concern that marginal products are based on data from the latest available Census of Manufacturers (CM). Panel A shows results analyzing the persistence of estimated *MPL* across subsequent CMs. The results are the output of a linear regression linking *MPL* to the five-year lag of *MPL* and industry-state-year fixed effects. Y/L and Y/L - VA denote the approaches measuring gaps in *MPL* using output per unit of labor with gross output and value added, respectively. Panel B describes the annual persistence implied by the estimates in Panel A. Panel B also reports the persistence implied for the results in the paper. This last persistence is computed using the average distance in the sample between *MPL* and the date of its measurement (previous CM). Panel C reports results replicating the specifications in Table 3 (columns (1) and (3) of Panel B) in a subsample of years at most two years apart from the previous CM. The number of observations has been rounded to the nearest hundred following the Census Bureau's disclosure policy. Standard errors are heteroskedasticity robust and double clustered at the state and industry level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Persistence of Marginal Product Gaps

	Outcome: MPL	
	Y/L	Y/L - VA
	(1)	(2)
MPL(t-5)	0.350***	0.246***
	(0.021)	(0.010)
Nobs	351,600	351,600
R-square	0.12	0.06
State-Industry-Year FE	Yes	Yes

Panel B: Persistence of Marginal Product Gaps - Magnitude

	Y/L	Y/L - VA
	(1)	(2)
Annual Persistence	0.811	0.756
Implied Persistence for Results	0.657	0.571

Panel C: Results in Subsample Closer to Census Years

	Outcome: Employment Share Growth	
	Y/L	Y/L - VA
	(1)	(2)
MPL × Dereg	0.0052***	0.0070***
	(0.0011)	(0.0014)
Nobs	1,570,000	1,570,000
R-square	0.01	0.01
State-Industry-Year FE	Yes	Yes
State FE x MP	Yes	Yes
Year FE x MP	Yes	Yes
Firm-State-Industry FE	Yes	Yes

Table IA.2
Alternative Channels: Changes in Intensive-Margin Reallocation

This table presents results linking the sensitivity of labor reallocation to firm TFP within an industry-state *(TFPSens)* to banking deregulation. The results are all based on the sample of single-plant firms and the estimation of equation (IA.4). The dependent variable is the annual change in the log of the firm's industry-state employment share. For a given year *t*, this change in share is computed including only firms present in both year *t* and *t-1*. *TFP* is the log of firm total factor productivity, which is based on a translog production function, with parameters estimated using the OLS or Structural approaches. *Dereg* is a banking deregulation index that equals the sum of *Intrastate_Dereg* and *Interstate_Dereg*, indicators that equal one if the state has passed intrastate and interstate banking deregulation, respectively. The control variables in all regressions include the one-year lag of age, its squared value, as well as the interactions of both these variables with *Interstate_Dereg* and *Intrastate_Dereg.* Panel A reports the results. The number of observations has been rounded to the nearest hundred following the Census Bureau's disclosure policy. Standard errors are heteroskedasticity robust and double clustered at the state and industry level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively. Panel B reports the additional industry value-added growth implied by the effects in Panel A. This additional growth is the average of the implied magnitudes based on the OLS and Structural approaches to estimate firm TFP.

Panel A: Labor Reallocation Sensitivity to TFP and Banking Deregulation

	Translog	
	OLS	Structural
	(1)	(2)
TFP × *Dereg*	0.0089***	0.0114***
	(0.0017)	(0.0020)
Nobs	2,287,100	2,287,100
R-square	0.01	0.01
State-Industry-Year FE	Yes	Yes
State FE x TFP	Yes	Yes
Year FE x TFP	Yes	Yes
Firm-State-Industry FE	Yes	Yes

Panel B: Magnitude of Changes in Intensive-Margin Reallocation Gains

	Banking Deregulation Effect (Change in *Dereg* from 0 to 2)
Industry Productivity Growth (%VA)	1.08%

Table IA.3
Alternative Channels: Changes in Firm-Level Productivity

This table presents results linking firm-level changes in TFP to banking deregulation. Panel A reports the results. The results are all based on the sample of single-plant firms and the estimation of equation (IA.5). The dependent variable is *TFP,* the log of firm total factor productivity, which is based on a translog production function, with parameters estimated using the OLS and Structural approaches. *Dereg_Years* equals the sum of two variables separately constructed for each type of deregulation event (interstate or intrastate). For each event, this variable is equal to zero prior to deregulation and measures the number of years since the deregulation event. The number of observations has been rounded to the nearest hundred following the Census Bureau's disclosure policy. Standard errors are heteroskedasticity robust and clustered at the industry level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively. Panel B reports the additional industry value-added growth implied by the effects in Panel A. This additional growth is the average of the implied magnitudes based on the OLS and Structural approaches to estimate firm TFP.

Panel A: Firm TFP Growth and Banking Deregulation

Outcome: Log of Firm TFP		
	Translog	
	OLS	Structural
	(1)	(2)
Dereg_Years	0.0020**	0.0010***
	(0.0010)	(0.0004)
Nobs	553,000	553,000
R-square	0.003	0.010
Industry-Year FE	Yes	Yes
Firm-State-Industry FE	Yes	Yes

Panel B: Magnitude of Changes in Firm Productivity Growth

	Banking Deregulation Effect (Change in *Dereg* from 0 to 2)
Industry Productivity Growth (%VA)	0.56%

Table IA.4
Alternative Channels: Changes in Extensive-Margin Reallocation

This table presents results linking the entry and exit decisions of firms to banking deregulation. The results are all based on the sample of single-plant firms. Panel A reports results examining how the productivity gaps between firms entering or exiting an industry and other firms in the same industry-state change after banking deregulation. The dependent variable is *TFP*, the log of firm total factor productivity, which is based on a translog production function, with parameters estimated using the OLS or Structural approaches (see text for more details). The results are based on the estimation of (IA.6) and (IA.7). Panel B reports results linking the level of entry and exit to banking deregulation. The results are the output of a linear regression linking the entry or exit indicator variables to *Dereg*, year fixed effects and industry-state fixed effects. The number of observations has been rounded to the nearest hundred following the Census Bureau's disclosure policy. Standard errors are heteroskedasticity robust and double clustered at the state and industry level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively. Panel C reports the additional industry value-added growth implied by the effects in Panel A. This additional growth is the average of the implied magnitudes based on the OLS and Structural approaches to estimate firm TFP.

Panel A: Banking Deregulation and Selection in Entry and Exit

Outcome: Log of Firm TFP				
	Translog			
	OLS	Structural	OLS	Structural
	(1)	(2)	(3)	(4)
Entry× Dereg	0.0063***	0.0017		
	(0.0016)	(0.0017)		
Exit× Dereg			-0.0062***	-0.0048***
			(0.0011)	(0.0018)
Nobs	2,795,000	2,795,000	2,287,100	2,287,100
R-square	0.01	0.01	0.01	0.01
State FE x Entry	Yes	Yes		
Year FE x Entry	Yes	Yes		
State FE x Exit			Yes	Yes
Year FE x Exit			Yes	Yes
State-Industry-Year FE	Yes	Yes	Yes	Yes

Panel B: Banking Deregulation and the Level of Entry and Exit

	Outcome: Entry	Outcome: Exit
	(1)	(2)
Dereg	-0.0008	-0.0026
	(0.0052)	(0.0021)
Nobs	2,795,000	2,287,100
R-square	0.01	0.01
Year FE	Yes	Yes
State-Industry FE	Yes	Yes

Panel B: Magnitude of Changes in Extensive-Margin Reallocation Gains

	Banking Deregulation Effect (Change in *Dereg* from 0 to 2)
Industry Productivity Growth (%VA)	0.11%

Table IA.5
Quantifying the Contribution of Different Channels

This table reports results summarizing the estimated effects of state banking deregulation on the different components of industry productivity growth. The results are all based on the sample of single-plant firms. In Tables IA.2 to IA.4 we estimate the effect of state banking deregulation on three components of industry productivity growth: intensive-margin reallocation gains, firm-level productivity gains and extensive-margin reallocation gains. We here quantify the percentage of increased productivity growth associated with each of these three channels.

Banking Deregulation Effect (Change in *Dereg* from 0 to 2)	
Percentage of Gains from Intensive-Margin Reallocation Channel	61.7%
Percentage of Gains from Firm-Level Channel	32.0%
Percentage of Gains from Extensive-Margin Reallocation Channel	6.4%

Table IA.6
State Banking Deregulation Dates

This table presents the dates of interstate and intrastate deregulation events used in our analysis. We follow Amel (1993) and Kroszner and Strahan (1999) in determining these dates. See Section 1.3 for more details.

State	*Intrastate* Deregulation Year	*Interstate* Deregulation Year
Alabama	1981	1987
Alaska	<1970	1982
Arizona	<1970	1986
Arkansas	1994	1989
California	<1970	1987
Colorado	1991	1988
Connecticut	1980	1983
Delaware	<1970	1988
DC	<1970	1985
Florida	1988	1985
Georgia	1983	1985
Hawaii	1986	>1993
Idaho	<1970	1985
Illinois	1988	1986
Indiana	1989	1986
Iowa	1997	1991
Kansas	1987	1992
Kentucky	1990	1984
Louisiana	1988	1987
Maine	1975	1978
Maryland	<1970	1985
Massachusetts	1984	1983
Michigan	1987	1986
Minnesota	1993	1986
Mississippi	1986	1988
Missouri	1990	1986
Montana	1990	1993
Nebraska	1985	1990
Nevada	<1970	1985
New Hampshire	1987	1987
New Jersey	1977	1986
New Mexico	1991	1989
New York	1976	1982
North Carolina	<1970	1985
North Dakota	1987	1991
Ohio	1979	1985
Oklahoma	1988	1987
Oregon	1985	1986

Pennsylvania	1982	1986
Rhode Island	<1970	1984
South Carolina	<1970	1986
South Dakota	<1970	1988
Tennessee	1985	1985
Texas	1988	1987
Utah	1981	1984
Vermont	1970	1988
Virginia	1978	1985
Washington	1985	1987
West Virginia	1987	1988
Wisconsin	1990	1987
Wyoming	1988	1987