Misallocation and Capital Market Integration: Evidence From India*

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Abstract

Using the staggered liberalization of access to foreign capital across highly disaggregated Indian industries, we show that reducing capital misallocation increases aggregate productivity. The natural policy experiment allows us to credibly identify changes in firms' input wedges, addressing major challenges in the measurement of misallocation. For domestic firms with initially high marginal revenue products of capital (MRPK), liberalization increased revenues by 18%, physical capital by 60%, wage bills by 26%, and reduced the marginal revenue product of capital by 43% relative to low MRPK firms. There were no effects on firms with low MRPK. The effects of liberalization are largest in areas with less developed local banking sectors, indicating that foreign investors may substitute for an efficient banking sector. Finally, we develop a method to use natural experiments to estimate the lower bound effect of changes in misallocation on manufacturing productivity and conclude that the liberalization increased the aggregate productivity of the Indian manufacturing sector by at least 6.5%.

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1 Introduction

The misallocation of resources across firms may have a meaningful effect on aggregate productivity, particularly in low-income countries (Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009). Yet, despite misallocation's potential importance for explaining cross-sector and cross-country dispersion in productivity, quantifying the severity of misallocation and identifying the best policy tools to reduce it is complicated by two challenges.

First, on the measurement side, it is common to attribute all – or much of – the cross-sectional dispersion in the observed marginal returns to firms' inputs to misallocation. This creates upward bias in measures of misallocation due to measurement error (Bils, Klenow, and Ruane, 2018; Rotemberg and White, 2017; Gollin and Udry, 2019), model mis-specification (Haltiwanger, Kulick, and Syverson, 2018), volatility of productivity paired with the costly adjustment of inputs (Asker, Collard-Wexler, and De Loecker, 2014; Gollin and Udry, 2019), and unobserved heterogeneity in technology (Gollin and Udry, 2019).

Second, on the policy side, even if one were able to fully correct for mismeasurement and quantify the reduction in productivity due to misallocation, the specific sources of misallocation cannot be identified from aggregate comparisons.[1] This leaves policymakers with limited information about what levers can be used to reduce misallocation and increase prosperity (Syverson, 2011). Yet, in low-income countries, where there are likely to be large firm-level frictions in the allocation of resources (Hsieh and Klenow, 2009; Bento and Restuccia, 2017), policies that reduce misallocation could prove to be a powerful tool to foster economic growth.

An unusual natural experiment in India allows us to make progress on both the measurement and policy fronts. Over the 2000s, India introduced the automatic approval of foreign direct investments up to 51% of domestic firms' equity, potentially reducing capital market frictions. The staggered introduction of the policy across industries allows us to implement a difference-in-differences framework to estimate the effects of foreign capital liberalization on the misallocation of capital across firms in a way that avoids some of the measurement challenges mentioned above.

We find that the liberalization reduced capital misallocation and reallocated capital towards the firms with the highest marginal returns on capital. We develop a method, based on the theoretical results of Baqaee and Farhi (2019), to translate our quasi-experimental microeconomic estimates into a lower bound measure of the effect of the policy on manufacturing productivity. Our proposed method allows us to use exogeneous variation to

1. To quantify the overall degree of misallocation, the literature usually compares outcomes such as the distribution of marginal revenue products across units of production after controlling for different characteristics and attributes the residual dispersion to misallocation. Since this method of quantifying misallocation typically does not show which characteristics causally affect the residual dispersion in marginal products, it provides little guidance for identifying the causes of misallocation.

generate estimates of the effect of changing misallocation on a measure of aggregate productivity under relatively weak identifying assumptions and importantly, without relying on cross-sectional variation in firm performance.

To measure the effects of the reform, we hand-collected data on industry-level liberalization episodes in 2001 and 2006 that allowed for the automatic approval of foreign investments. Combining this policy variation with a panel of large and medium-sized Indian firms, we investigate whether the reform reduced misallocation by testing whether the policy had differential effects depending on firms' ex ante marginal returns to capital. By exploiting within-industry variation in firms' MRPK, this empirical strategy requires milder identification assumptions than standard difference-in-differences estimators, as it allows us to control for the average effect of belonging to a deregulated industry. In our most stringent specifications, we can account for different unobserved shocks or differences in time trends at the industry level, as long as these shocks affect high and low MRPK firms in the same industry in the same way.

We find that high MRPK firms increase their physical capital by 60%, revenues by 18%, wage bills by 26%, and reduce their MRPK by 43% relative to low MRPK firms in response to the policy. In contrast, low MRPK firms are not affected. Since high MRPK firms initially have 140% higher MRPK, the micro-estimates imply that the policy reduces misallocation. Event study graphs confirm that these effects are not driven by differential pre-trends between high and low MPRK firms and provide visual evidence that the reduction in misallocation is not due to mean reversion.

Exploiting geographical variation in local access to credit prior to the reform, we also find that the liberalization's effects on misallocation are largest in areas where the local banking sector was less developed. This is consistent with the hypothesis that foreign investors can reduce misallocation by standing in for, and competing with, local credit markets.

Because the reduction in distortions on input prices should reduce marginal costs for affected firms, we then explore if firms pass these gains on to consumers in the form of lower prices. We exploit the fact that our panel of firm-level data provides detailed data on each firm's product-mix, as well as information about product-level prices, and find evidence of pass through on unit-prices by high MRPK firms in the treated industries.

The liberalization policy may have had broader effects than reducing firms' wedges on capital inputs. By relaxing financial constraints, the policy may also affect misallocation in other areas. If firms need to borrow to pay workers, relaxing financial constraints can also affect labor misallocation.[2] Motivated by this possibility, we examine the effect of the policy on labor misallocation. We again find that the reform had greater effects on firms with high MRPL, and that wage bills only increased for firms with above median

2. For more discussion of this mechanism, see Schoefer (2015) in the U.S. and Fonseca and Doornik (2019) in Brazil.

pre-treatment MRPL. For these firms, relative to low MRPL firms, wage bills increased by 32% and MRPL fell by 35%. Since high MRPL firms had 96% higher levels of MRPL prior to the treatment, labor misallocation fell along with capital misallocation following the reform

Finally, combining production function parameter estimates with reduced-form estimates of the policy effect, we generate a lower bound estimate of the aggregate effect of liberalization episodes on the manufacturing industry's Solow Residual of +6.5%. Using our quasi-experimental estimates to adjust for the biases arising from estimating misallocation with cross-sectional data is important. If we attributed *all* of the baseline variation in the marginal products of inputs to misallocation, we would estimate that the policy increased productivity by 156%. While our preferred lower bound estimate is not sensitive to the treatment of outliers, the latter estimate is highly sensitive. For example, dropping the top and bottom 15% of the marginal revenue product measures reduces the estimated policy effect to 9.9%. While trimming can help address measurement error, the degree to which researchers do so can result in a wide range of estimates.

This paper contributes to two main literatures, as we discuss below. First, we relate to the literature quantifying the importance of misallocation for aggregate outcomes (e.g. Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009; Bartelsman, Haltiwanger, and Scarpetta, 2013; Restuccia and Rogerson, 2013; Baqaee and Farhi, 2017), particularly in the context of developing countries (e.g. Banerjee and Moll, 2010; Guner, Ventura, and Xu, 2008; Collard-Wexler, Asker, and De Loecker, 2011; Kalemli-Ozcan and Sørensen, 2014).[3] Second, it contributes the literature on the effects of capital account liberalization (Buera, Kaboski, and Shin; 2011; Midrigan and Xu, 2014; Moll, 2014; Catherine, Chaney, Huang, Sraer, and Thesmar, 2018).

Regarding the misallocation literature, a great deal of research has focused on measuring the effect of all sources of misallocation on aggregate output by exploiting cross-sectional dispersion in marginal revenue products. The principal advantage of this "indirect approach" (Restuccia and Rogerson, 2017) is that it allows for the estimation of the cost of misallocation without identifying the underlying sources of the distortions, even if the sources are not observable to researchers. However, in this approach, model mis-specification and measurement error can inflate the estimates of misallocation. We make two contributions to this literature. First, since we exploit a liberalization episode that affected only certain industries, we can estimate the effect of deregulation on misallocation using weaker identification assumptions. Our difference-in-differences estimation only requires that measurement error or other unobserved attributes are uncorrelated with the policy to identify *changes* in input wedges. Second, our approach isolates the changes in distortions produced by a specific policy, foreign capital liberalization. This allows us to isolate the effect of access to the foreign equity market, holding constant ac-

3. A survey of this literature can be found in Restuccia and Rogerson (2017).

cess to the foreign debt market and other macroeconomic determinants that might affect the cost of capital.[4]

In terms of the literature on capital account liberalization, this paper relates most closely to a recent strand of this literature that has explored how increased foreign financial flows affect domestic firms' productivity and misallocation (Alfaro, Chanda, Kalemli-Ozcan, and Sayek, 2004; Gopinath, Kalemli-Özcan, Karabarbounis, and Villegas-Sanchez, 2017; Varela, 2017; Larrain and Stumpner; 2017; Saffie, Varela, and Yi, 2018).[5] We add to this literature in several ways. First, while much of the previous literature exploits country-level variation in access to foreign investment, this paper exploits variation across industries over time *within the same country.* This allows us to hold institutional differences constant, as such differences may be important in cross-country settings. Second, since the Indian deregulation only affected foreign investment in equity, it allows us to cleanly isolate the effect of foreign investment in *equity* on misallocation holding fixed access to foreign *debt*.[6]

Third, we estimate the direction of the effect of de-regulating foreign investment on misallocation. In the context of low-income countries, where formal credit markets are limited and informal credit markets are a poor substitute (Townsend, 1994; Udry, 1994; Banerjee, Duflo, Glennerster, and Kinnan, 2015), credit constraints are likely to be large (Banerjee, Duflo, and Munshi, 2003; Banerjee and Duflo, 2014). Indeed, Anne Krueger, who was deputy managing director of the IMF during the time of the reforms we study, wrote that in India, "banks are considered to be very high cost and inefficiently run" and that, "enabling [Indian banks] to allocate credit to the most productive users, rather than by government allocation, would make a considerable contribution to the Indian economy's growth potential" (Krueger et al., 2002). Thus, foreign investment may play a crucial role in reducing misallocation if foreign investors have better screening technologies, or are not bound by historical, political, regulatory or institutional domestic constraints (e.g. Banerjee and Munshi, 2004; Cole, 2009; Cole, 2009).

Yet, judging by prior findings in the literature, the effect of opening-up to foreign

4. In the context of India, several recent papers have estimated specific characteristics of the Indian economy that might explain the high degree of misallocation observed in the country: the role of property rights and contract enforcement (Bloom, Eifert, Mahajan, McKenzie, and Roberts, 2013; Boehm and Oberfield, 2018); land regulation (Duranton, Ghani, Goswani, and Kerr, 2017); industrial licensing (Chari, 2011; Alfaro and Chari, 2015); privatization (Dinc and Gupta, 2011 Gupta, 2005); reservation laws (Garcia-Santana and Pijoan-Mas, 2014); highway infrastructure (Ghani, Goswami, and Kerr, 2016); and electricity shortages (Allcott, Collard-Wexler, and Connell, 2016).

5. Varela (2017) shows that financial liberalization can increase productivity, while Saffie, Varela, and Yi (2018) find that financial liberalization also accelerates the reallocation of resources across sectors, promoting the development of service/high-income sectors. On the other hand, Gopinath, Kalemli-Özcan, Karabarbounis, and Villegas-Sanchez (2017) find that better access to capital markets can amplify misallocation.

6. In contrast, Varela (2017) studies the deregulation of capital controls in Hungary, in a context where foreign capital was already integrated and was not affected by the policy. Gopinath, Kalemli-Özcan, Karabarbounis, and Villegas-Sanchez (2017) exploit the drop in the interest rate for Southern European countries following the adoption of the Euro, which did not directly change the equity market.

capital on misallocation is a priori unclear. For example, foreign investors may also be worse at processing and monitoring soft information, particularly in low-income countries (Detragiache, Tressel, and Gupta, 2008).[7] Therefore, a final contribution of this paper is showing that foreign capital liberalization policies *do* reduce misallocation, suggesting that these policies could be a powerful tool for low-income countries to increase aggregate productivity.

The remainder of the paper is organized as follows. Section 2 provides a brief conceptual framework for understanding misallocation. Section 3 describes the data and the context of the policy change. Section 4 discusses our main empirical strategy, while Section 5 reports our estimates of the average effect of the foreign capital liberalization policy and its heterogeneous effects on firms with high and low capital constraints. Section 6 replicates the analysis for firms that appear to have high and low labor constraints to test whether the policy also reduced labor misallocation, and Section 7 reports estimates of the foreign capital liberalization policies' aggregate effects on the Solow Residual. Section 8 concludes.

2 Conceptual Framework

This section sketches a simple conceptual framework in partial equilibrium that illustrates how our regression results can shed light on changes in misallocation. We follow standard practice in the literature and model misallocation via wedges on the prices of inputs. Intuitively, the wedges can be thought of as explicit taxes or implicit taxes which implement a given (potentially inefficient) allocation in the decentralized Arrow-Debreu-McKenzie economy. Thus, the price paid by a firm i for an input x is $(1+\tau_i^x)p^x$, where $x \in \{K, L, M\}$ and K, L, and M denote capital, labor, and materials, respectively. The price of input x is p^x, and τ_i^x is the additional wedge a firm pays for the input over the market price. The wedge τ_i^x can be negative indicating that a firm is subsidized, or positive, indicating that the firm pays a tax. A single-product firm's profit function is

$$\pi_i = p_i f_i(K_i, L_i, M_i) - \sum_{x \in \{K,L,M\}} (1+\tau_i^x)p^x x_i$$

where $f_i(K_i, L_i, M_i)$ is the firm's production function, which exhibits diminishing marginal returns in each input. Since we allow full flexibility on the size of the wedges on the inputs, we can assume price-taking for output without loss of generality.[8]

7. In the context of foreign banks' behavior in low-income countries, several studies have found that foreign banks lend essentially to large domestic firms, potentially increasing credit constraints for local firms (e.g. Mian (2006) for Pakistan, Gormley (2010) for India, or Detragiache, Tressel, and Gupta (2008) for a cross-section of countries).

8. Wedges on outputs, like markups or output taxes, can equivalently be represented as input wedges under price-taking. By varying input-wedges, we can make a price-taking firm i produce any desired

A price-taking profit-maximizing firm will consume an input x_i until that input's marginal revenue returns are equal to the cost

$$p_i \frac{\partial f_i(K_i, L_i, M_i)}{\partial x_i} = (1 + \tau_i^x) p^x$$

For each input, the marginal revenue product of that input, $\partial f_i(K_i, L_i, M_i) p_i / \partial x_i$, is proportional to the wedge τ_i^x. Therefore, firms with higher capital wedges will have higher marginal revenue products on capital.

A decrease in capital misallocation occurs when the wedges τ_i^k decline for firms whose wedges are high relative to other firms. A decline in the wedges of firms with relatively high initial τ_i^k will have several effects. The most direct effect is that, since τ_i^k falls, the measured MRPK should also fall for these firms. Second, firms with high wedges will increase their capital use. Finally, the increase in capital will increase the marginal revenue products of the other inputs, which incentivizes firms to also increase their demand for labor or materials. As a result of higher input use, these firms produce more and earn higher revenues. Thus, if the policy reduces misallocation by reducing the wedges of firms with high τ_i^k, we should expect to find that the policy increases capital, labor, and sales for these firms and decreases MRPK. Moreover, these effects should be differentially stronger in previously capital constrained (high MRPK) firms relative to less constrained firms.

3 Data and Policy Change

In this section, we describe the context of the financial liberalization policies in India and the data used in this paper.

3.1 Indian Foreign Investment Liberalization

Following its independence, India became a closed, socialist economy, and most sectors were heavily regulated.[9] However, in 1991, India experienced a severe balance of payments crisis, and in June 1991, a new government was elected. Under pressure from the IMF, World Bank, and the Asian Development Bank, which offered funding, this government engaged in a series of structural reforms. These reforms in turn led India to become more open and free-market oriented. In addition to initiating foreign capital reforms in this period, India also liberalized trade (e.g. Topalova and Khandelwal, 2011;

feasible allocation $q_i = f_i(K_i, L_i, M_i)$, including the allocations that result from output taxes and monopolistic behavior. In other words, cost-minimization implies that $p_i \partial f_i / \partial x_i = \mu_i (1 + \tau_i^x) p^x$, where μ_i is the gross markup (or equivalently, the output tax).

9. See Panagariya (2008) for a thorough review of the Indian growth experience and government policies.

Goldberg, Khandelwal, Pavcnik, and Topalova, 2010), and dismantled extensive licensing requirements (e.g. Aghion, Burgess, Redding, and Zilibotti, 2008; Chari, 2011).

Before 1991, most industries were regulated by the Foreign Exchange Regulation Act (1973), which required every instance of foreign investment to be individually approved by the government and foreign ownership rates were restricted to below 40% in most industries. With the establishment of the initial liberalization reforms in 1991, foreign investment up to 51% of equity in several industries was automatically approved.[10] In the following years, different industries liberalized at different times and the cap for automatically approving foreign investment was increased. We study the effects of financial liberalization episodes that ocurred after 2000, after the main period of reform in the 1990s. This is both due to data availability, as described below, and to avoid conflating the effects of financial liberalization reforms with other ongoing reforms.

To study the effects of foreign investment liberalization, we hand-collected data on the timing of disaggregated industry-level policy changes from different editions of the *Handbook of Industrial Policy and Statistics*. We match this data to industries at the 5-digit NIC level. An industry is coded as having been treated if a policy change occurred that allowed automatic approval for investments up to at least 51% of capital (though, in some cases, the maximum is higher). We then merge this data at the industry-level with the firm-level dataset described below.

3.2 Firm and Product-Level Data

Our firm-level data comes from the Prowess database compiled by the Centre for Monitoring the Indian Economy (CMIE). Unlike the Annual Survey of Industries (ASI), which is the other main source of information used to study dynamics in the Indian manufacturing sector, Prowess is a firm-level panel dataset.[11] The data is therefore particularly well suited for examination of how firms adjust over time in reaction to policy changes. The dataset contains information from the income statements and balance sheets of companies comprising more than 70% of the economic activity in the organized industrial sector of India and 75% of all corporate taxes collected by the Government of India. It is thus representative of large and medium-sized Indian firms. We retrieve yearly information about sales, capital stock, consumption of raw materials and energy, compensation of employees, and ownership group for each firm.

To estimate the effect of the reform on prices, we take advantage of one rare feature in firm-level datasets that is available in Prowess: the dataset reports both total product sales and total quantity sold at the firm-product level, allowing us to compute unit-price and quantities. This peculiar feature is due to the fact that Indian firms are required by

10. This policy is described by Topalova (2007), Sivadasan (2009) and Chari and Gupta (2008).

11. The ASI is a plant-level repeated cross-section and does not include information on whether plants are owned by the same firm.

the 1956 Companies Act to disclose product-level information on capacities, production, and sales in their annual reports. A detailed discussion of the data can be found in Goldberg, Khandelwal, Pavcnik, and Topalova (2010). The definition of a product is based on Prowess's internal product classification, which is in turn based on India's national industrial classification (NIC) and contains 1,400 distinct products. Using this information, we can calculate the unit-quantity price of products, which we define as total sales over total quantity. This allows us to also construct a separate panel of product-level output and prices from 1995-2015.[12]

3.3 Local Financial Development Data

India is a federal country with a banking market that is largely regulated at the state-level, creating important disparities in the degree of the development of the local credit market across states (Vig, 2013). To take advantage of this geographic variation, we hand-collected data at the state-level from each of the pre-reform years (1995-2000) on the offices, deposits, and credits of all scheduled commercial banks from the Reserve Bank of India.

Over the study period, the administrative organization of districts and states in India changed several times due to the foundation of new states (e.g. Jharkhand was carved out of Bihar in November 2000) or the bifurcation of existing districts within a state. We keep the administrative organization of states fixed as of 1999. This is straightforward since the vast majority of cases where a new state is created are because that state was carved out of only one existing state. Our state-level measures encompass 25 out of 26 Indian states and four out of seven union territories. Altogether, this data covers 91.5% of net domestic product and 99% of credit.

3.4 Combined Data Sets

To arrive at our final data for analysis, we merge the firm-level and product-level panel datasets with the industry-level policy data. We further merge the firm-level data with the state-level financial development data. We then make three restrictions to these samples, which we describe below.

As is common in the literature estimating production functions, we restrict our analysis to manufacturing firms. We further restrict the sample to observations from the period between 1995 and 2015. Restricting the sample to 1995-2015 has two advantages. First, focusing on this later period avoids potential bias from other liberalization reforms during

12. One limitation of this dataset is that firms choose which type of units to report, and units are not standardized across firms or within-firms over time. Thus, when we want to analyze the effects of policy changes on prices/output and there is not enough information to reconcile changes in unit types within a firm-product over time, we are forced to drop the set of observations associated with a firm-product. As a result, we omit 5,077 firm-product-year observations.

the early-1990s, the main Indian liberalization period. While liberalization occurred for 47% of manufacturing firms in the data, by restricting our sample to observations after 1995, we only exploit policy variation for the 9% of manufacturing firms who experienced foreign capital liberalization in the 2000s. Second, although Prowess, technically starts in 1988, its coverage in the first few years is limited and grows substantially over time. In 1988, Prowess only included 1,057 firms total, but it had grown to 7,061 firms by the beginning of our study period in 1995. In contrast, from 1995 onward, during our study period, the coverage of the database is more stable, with similar numbers of firms observed across subsequent years (7,526 firms observed in 1996, 7,286 in 1997, and 7,717 in 1998).[13] Appendix Table A1 provides a list of the different industries in the manufacturing sector affected by the deregulation during this restricted period. As the table shows, among manufacturing firms, the only liberalization episodes occurred in 2001 and 2006.

Finally, we restrict the sample to the set of firms for whom we can compute marginal revenue products of capital and labor (MRPK and MRPL) prior to the earliest policy change in 2001. These pre-policy change measures are needed to estimate the effects of the policy on misallocation. Thus, we restrict the sample to firms observed before 2001 with non-missing, positive data on both assets and sales.[14] These three restrictions leave us with 63,950 observations.

Table 1 documents summary statistics for the final firm-level sample used in our analysis. As the table shows, classifying firms based on the owner's name, we find that the typical firm in our analysis is a privately-owned domestic firm (57%), while 5% of firms are private, foreign-owned firms, and 4% are state-owned. The table also shows that 9% of firms are in industries that experienced the policy change over the course of the sample.

4 Empirical Strategy

This section describes our main reduced-form strategy for measuring the effects of foreign financial liberalization on misallocation. As a first step in our analysis, in the first subsection, we detail how we classify firms as having high marginal revenue products of capital prior to the policy change. The second subsection documents the specifications we use to estimate the heterogeneous effects of the policy for high and low MRPK firms

13. This likely reflects the fact that the first wave of liberalizing reforms also standardized financial reporting in the mid-1990s.

14. This is the minimal requirement to calculate MRPK. As we document in the next subsection, we use two methods to estimate marginal revenue returns to capital. The least data intensive method exploits the fact that, under Cobb-Douglas production functions, deflated sales divided by deflated capital will be proportional to MRPK within an industry as long as capital intensity is the same for all firms in that industry.

and discusses the identifying assumptions.

4.1 Classifying Firms as High or Low MRPK

To estimate whether foreign investment liberalization reduces misallocation, we test if the reform had a differential effect on firms with high and low MPRK. For our main analyses, we use two methods to measure firms' MRPK.

As is standard in the production function estimation literature[15] and consistent with our conceptual framework, we assume that firms have Cobb-Douglas production functions, such that

$$Y_{ijt} = A_{ijt} K_{ijt}^{\alpha_j} L_{ijt}^{\beta_j} M_{ijt}^{\kappa_j} \tag{1}$$

where i denotes a firm, j denotes a 2-digit industry, and t denotes a year. Y_{ijt}, K_{ijt}, L_{ijt}, and M_{ijt} are measures of output, assets, the wage bill, and materials, and A_{ijt} is the firm-specific unobserved productivity. We measure these parameters with deflated Ruppee amounts, so that Y_{ijt} is proxied with deflated sales.[16] As we observe sales rather than quantities, our production function estimates are in revenue terms.

Our first and primary method for estimating MRPK takes advantage of the fact that, under the revenue Cobb-Douglas production function, $MRPK = \frac{\partial Y_{it}}{\partial K_{it}} = \alpha_j \frac{Y_{it}}{K_{it}}$. Thus, $\frac{Y_{it}}{K_{it}}$ provides a within-industry measure of MRPK, under the assumption that all firms in a 2-digit industry share the same α_j. This is our preferred method because it imposes the fewest data requirements, and therefore, allows us to use the largest possible sample for estimation.

As an alternative, we also use the method of Levinsohn and Petrin (2003) (LP), using the GMM estimation proposed by Wooldridge (2009) to estimate the parameters of the revenue production function. The LP method assumes the same Cobb-Douglas production function as in equation (1) and estimates its parameters using a control function approach. Once we estimate the full parameters of these production functions, MRPK is given by the derivative of the production function with respect to K_{it}. This method requires observing L_{it} and M_{it} in addition to K_{it} and Y_{it}. Using the LP method, we also estimate TFPR, which is equivalent to $\tilde{p}_{ijt} \times TFP$, where $\tilde{p}_{ijt}$ is the firm's deflated price. As the production function is in revenue terms, this is accomplished by estimating A_{ijt}. By estimating the effect of the reforms on TFPR, we will be able to determine if

15. Duranton, Ghani, Goswani, and Kerr (2017) describe the variety of methods used to estimate production functions and the revenue returns to capital and labor.

16. We use deflators for India made available by Allcott, Collard-Wexler, and O'Connell (2016) for the period 1995–2012, and we manually extended the price series to 2015. Revenue is deflated using three-digit commodity price deflators. The materials deflators are measures of the average output deflator of a given industry's suppliers using the 1993-4 input-output table. The capital deflator is obtained using an implied national deflator.

foreign capital liberalization affects within-firm productivity with the caveat that changes in prices that are not captured by industry-level deflators will also affect TFPR.

To determine whether firms had a high or low pre-reform MRPK, we average each firm's measures of MRPK over time from 1995 to 2000 (the last year prior to the first policy change). We then classify a firm as capital constrained (high MRPK) if it is above the 4-digit level industry median for the averaged measure. Since we have two measures of MRPK, this produces two measures of whether a firm is capital constrained or not.

Before turning to our main econometric specifications, we report the baseline levels of misallocation in the Indian manufacturing sector based on the cross-sectional dispersion of MRPK. However, we caution that dispersion in the cross-sectional distribution of MRPK is likely to be upwardly biased by measurement error or mis-specified production functions. Figure 1 reports the distribution of log(MRPK) as measured using the LP methodology during 2000.[17] Based on this measure, there appears to be substantial misallocation. A firm at the 90th percentile has a log(MRPK) 22 times greater than that of a firm at the 10th percentile, implying an almost improbably high degree of misallocation.

4.2 Econometric Specification

Firm-level Outcomes

We are interested in measuring the effect of financial liberalization on misallocation. If liberalization reduces misallocation, it will have heterogeneous effects on firms within the same industry. More specifically, if firms that are more capital constrained ex-ante experience reductions in their capital frictions, they will invest differentially more in response to the reform.

Thus, to asses the effect of liberalization on the reallocation of resources within industries, our main regression equation is the following:

$$y_{ijt} = \beta_1 \; Reform_{jt} + \beta_2 Reform_{jt} \times I_i^{High\, MRPK} + \mathbf{\Gamma X_{it}} + \alpha_i + \delta_t + \epsilon_{ijt} \tag{2}$$

where i denotes a firm, j denotes an industry, t denotes a year, and y_{ijt} is the outcome variable of interest, consisting of the logs of physical capital, wage bill, sales, TFPR and MRPK. $Reform_{jt}$ is an indicator variable equal to one if foreign investment has been liberalized in industry j and $\mathbf{X_{it}}$ is a collection of firm age fixed effects. $I_i^{High\, MRPK}$ is an indicator variable equal to 1 if a firm had a high pre-treatment MRPK according to our measures defined in Section 4.1. Because our treatment of interest occurred at the industry level, we two-way cluster our standard errors both at the 4–digit industry

17. Our primary measure (Y/K) only allows us to compare MRPK within-industries, as opposed to across industries. Thus, measures of MRPK produced by the Y/K method cannot be used to obtain a cross-sectional measure of misallocation.

and year levels to account for any serial correlation that might bias our standard errors downward.[18]

The coefficient of interest is β_2 and captures the differential effect of the reform on ex-ante capital constrained firms relative to unconstrained firms. $\beta_2 > 0$ implies that the dependent variable increases differentially for capital constrained firms in industries that have opened up to foreign capital relative to industries that have not opened up.

Firm (α_i) and year (δ_t) fixed effects respectively account for several important sources of variation in firms' outcomes that would otherwise bias the estimates. Firm fixed effects absorb all unobserved time-invariant heterogeneity across firms and remove biases that could occur if, for example, more productive industries are more likely to be liberalized or if more productive firms are more likely to enter liberalized industries. Time fixed effects absorb any macro-economic fluctuations or country-wide reforms that may be correlated with the deregulation episodes.

Because we are interested in *within* industry reallocation, we can control directly for the average effect of being in a deregulated industry with the variable $Reform_{jt}$. This implies that any industry-level time trends or industry-wide specific shocks that differentially affect deregulated and non-deregulated industries are accounted for. In fact, in our most conservative specification, we control non-parametrically for industry-level unobserved shocks/time trends by including 5-digit industry-by-year fixed effects. In this specification, even if the Indian government liberalized industries that were growing more quickly earlier, β_2 would not be biased as long as high MPRK firms were not growing relatively more within these industries. The identifying assumptions for equation (2) are therefore milder than in the classic difference-in-differences framework, which would require that the liberalization policy was uncorrelated with industry-level time trends.

Our estimates could still be biased if high MPRK firms in treated industries would have grown at a different rate than high MPRK firms in untreated industries in the absence of the policy. This might occur if the Indian government targeted the policy toward industries where misallocation was already decreasing, although it is not clear why this would be the case. We can test for this source of bias directly by estimating and plotting the year-by-year relative treatment effect for high MRPK firms in event study graphs. If the outcomes of high MPRK firms were indeed changing faster in treated industries relative to in untreated industries prior to the policy change, we should see an effect of belonging to an industry that would be deregulated in the future on high MPRK firms prior to the policy change. The yearly differential effects of the policy are obtained

18. Our treatment variable is coded at the 5-digit industry-level, but we cluster at the 4–digit level to account for possible correlations in treatment statuses across more closely related industries.

by estimating the following equation:

$$
\begin{aligned}
y_{ijt} = \sum_g \beta_{1,g}\ Reform_{jt} \times I_{it}^g + \sum_g \beta_{2,g} Reform_{jt} \times I_i^{High\, MRPK} \times I_{it}^g \\
+ \mathbf{\Gamma X_{it}} + \alpha_i + \delta_t + \epsilon_{ijt},
\end{aligned} \tag{3}
$$

where an industry's policy change is normalized to take place in period 0, and $\sum_g$ is a summation over the years that firms were observed before and after the policy event. I_{it}^g is an indicator variable equal to 1 if in year t a firm was observed g years after the policy event. Then, our event study graphs plot the set of coefficients $\beta_{2,g}$, which estimate the relative effect of being in a treated industry on a high MPRK firm for each year.

Finally, we also estimate the effect of the reform on the average firm in an industry using a classic difference-in-differences strategy of the following form:

$$
y_{ijt} = \beta_1\ Reform_{jt} + \mathbf{\Gamma X_{it}} + \alpha_i + \delta_t + \epsilon_{ijt} \tag{4}
$$

In this case, the coefficient of interest, β_1, measures the effect of being in an industry that has liberalized, relative to other industries, and is identified only by comparing changes in outcomes for the liberalized firms between the pre and post-periods to the changes for non-liberalized firms.

Product-level Outcomes

To assess the heterogeneous effects of the policy on unit prices and quantities at the product-firm-year level, we estimate:

$$
y_{ipjt} = \beta_1\ Reform_{jt} + \beta_2 Reform_{jt} \times I_i^{High\, MRPK} + \mathbf{\Gamma X_{it}} + \alpha_{ip} + \delta_t + \epsilon_{ipjt} \tag{5}
$$

with the additional subscript p denoting a product, and the fixed effect α_{ip} denoting a firm-by-product fixed effect. The remaining notation and terms are unchanged, with β_2 now capturing the differential effect of the reform on log unit prices and log quantity produced for high MRPK firms, while β_1 identifies the effect for low MRPK firms. We also estimate the effect for the average firm, as in equation (4), by dropping the interaction term.

The inclusion of firm-by-product fixed effects (α_{ip}) means that we estimate the effect of the reform within firm-products and account for unobserved time-invariant differences across products. The fixed effects also account for the fact that the definition of a unit is different across firms or products and for the potential deletion and addition of products by firms over the study period.

5 Results

5.1 Average Effects for Firm-Level Outcomes

Table 2 reports the effect of the reform on the average firm (equation (4)). The estimates indicate that the liberalization policy had moderate, positive effects on the average firm's development. For the average firm, revenues increased by 22% (column 1), and capital increased by 29% (column 2), both significant at the 1% level. The point estimate for total wage bill is positive but not statistically significant, while the marginal revenue product of capital (MRPK) decreases by an insignificant 18%. The reform, however, did not change the average firm's TFPR. However, we caution that this identification strategy could underestimate gains in firm-level productivity, since TFPR is equal to TFP multipled by the deflated price. If prices fell in response to the policy, TFPR could fall or remain unchanged even if TFP increased.

5.2 Differential Effects by MRPK for Firm-Level Outcomes

Baseline Specification

Table 3 reports the estimates of the heterogeneous effects of the policy from equation (2), our main estimating equation. Panel A uses our primary method for classifying whether firms are capital constrained, while Panel B reports the results using the LP method. Since both methods produce economically large and statistically highly significant effects, in our discussion of the results, we focus on the case where credit constrained firms are identified using the Y/K method to simplify exposition.

Following the liberalization, capital constrained firms (high MRPK firms) generate relatively greater revenues by 18% (column 1). These higher revenues are made possible by the fact that capital constrained firms invest more, with their physical capital relatively increasing by 60% (column 2).[19] Higher investment does not crowd-out labor. Capital constrained firms also experience a relative increase in their wage bills by 26%,[20] suggesting that there are important complementarities between capital and labor in India.[21] We will explore whether the reform also reduced labor misallocation in Section

19. If our classification of high and low MRPK firms is affected by measurement error, firms with large negative measurement error in their capital will be classified as high MRPK. Then, if the policy change led firms to improve their reporting, perhaps to attract foreign investment, high MRPK firms would appear to increase their capital due to the policy. This is unlikely to be the case. First, we would then expect sales to *decrease* for high MRPK firms following the policy change, while the opposite is the case. Second, we will show the same pattern of effects for output, as well as capital and revenues. Output is measured separately from sales and capital at the product-level, and its idiosyncratic measurement error should be independent.

20. Unfortunately, Prowess only reports the total wage bill rather than the number of employees. Thus, we cannot determine whether the increase in wage bill is due to greater labor productivity enhanced by capital or to more employees being hired.

21. The existence of these complementarities is consistent with evidence in Fonseca and Doornik (2019)

6. Additionally, among the ex ante capital constrained firms, the policy also reduced MRPK by 43%. Given that, prior to the reform, high MRPK firms had a MRPK 3.8 times greater than low MRPK firms, the reform led to an important decline in the dispersion of MRPK. Taken together, our effects imply that the liberalization of foreign capital substantially reduced misallocation.

While the reforms changed the allocation of inputs across the firms, we again find no evidence that they affected within-firm productivity, as proxied by TFPR (column 5). These findings seem to suggest that the liberalization of foreign capital mainly led to efficiency gains due to the reallocation of inputs within industries rather than an acceleration of productivity growth within firms.

Next, to assess whether these results are driven by pre-trends, using our primary classification for high MRPK firms, we plot the event study graphs described by equation (3) for our key outcomes of interest. Figure 2 reports these results for the logs of assets, sales, the wage bill, and MRPK. Two facts are noteworthy.

First, for all of these outcomes, being treated by the policy had no differential effect on high MRPK firms before the policy was adopted, providing visual evidence that pre-trends were parallel. The lack of correlation between firm-outcomes and the reform prior to the year of deregulation also implies that our results are not driven by mean reversion. If that were the case, we should observe a decline in firm outcomes prior to the policy change.

Second, the effect of the liberalization on the different firm outcomes is progressive over time, consistent with the idea that the reallocation of resources (such as the adjustment of worker flows and adaptation of production tools) is likely to take time. In addition, some of the reallocation we observe might also come from competitive effects, where the relaxation of credit constraints allows firms with higher returns to capital to expand at the expense of the less efficient/ex-ante less constrained firms, potentially leading to important economic gains (Foster, Haltiwanger, and Syverson, 2008). We also expect this phenomenon to be progressive and only fully observable after some time has passed.

Importance of Local Banking Market

Our results so far show that opening-up to foreign capital allows capital constrained firms to invest more and grow faster. If foreign capital is acting as a substitute for a more efficient domestic banking sector, a natural implication is that firms located in areas with more developed local banking markets prior to the reform should benefit less from the reduction in credit constraints. We directly test this hypothesis by creating a variable *Local Credit Market Development*$_s$, defined as the log average over the pre-reform period of all bank credit in state s. We then interact this measure with all the

in a different developing country, Brazil.

single and cross-terms in equation (2). The variable is de–meaned to restore the baseline effect on $I^{High\,MRPK} \times Reform_j$. The coefficient of interest is the coefficient for the triple interaction $I^{High\,MRPK} \times Reform_{jt} \times Local\ Credit\ Market\ Development_s$, which captures the differential effect of the policy on capital constrained firms located in more developed local banking markets.

Table 4 reports the results. For revenues, capital, and wages, the interaction $I^{High\,MRPK} \times Reform_{jt} \times Local\ Credit\ Market\ Development_s$ is negative and significant at the 1% level. For *MPRK*, the triple interaction is positive and significant. Taken together, these results imply that capital wedges fell more following the reforms for high MRPK firms located in less financially developed states.

In addition to being statistically significant, the magnitudes of the heterogeneous effects are economically meaningful. If we focus on the change in the marginal revenue products of capital (column 4), ex ante high MRPK firms located in a state at the 75^{th} percentile of the bank credit distribution experienced a decrease in MRPK of 34% (-0.44+(0.08×-0.71)). In contrast, high MRPK firms located in a state at the 25^{th} percentile of the bank credit distribution experienced a decrease in MPRK of 51% (-0.44+(0.08×1.37)). Thus, the reduction at the 25^{th} percentile is 50% larger than the one at the 75^{th} percentile.

The fact that the effects of the policy were smaller in states where credit constraints were a priori lower further confirms that opening up to foreign capital relaxed credit constraints and allowed previously constrained firms to invest more.

5.3 Product-level Outcomes: Quantities and Prices

Prices

We next turn to the effect of the reforms on prices. Opening-up to foreign capital can reduce prices for two reasons. If liberalization reduced the wedges on capital for high MRPK firms, as described in the conceptual framework, these firms' marginal costs would fall. Lower marginal costs may be passed on to consumers in the form of lower prices. In addition, by allowing credit constrained firms to invest more and expand, the reform could also increase competition in the product market, leading firms to reduce their mark-ups and cut their prices.

To examine whether this is the case, we estimate equation (5) with log unit price as the outcome variable. Columns 1–3 of Table 5 report the results. On average, the reform reduces prices by 9% (column 1). When we disaggregate this average effect, we find that the reduction is concentrated among the high MRPK firms (column 2) according to the Y/K classification of high MRPK. The alternative classification of MRPK yields a similar pattern, although the estimate of the differential effect is smaller.

The decrease in prices we observe for capital constrained firms following the liberalization may partially explain the fact that the policy change had little effect on our measures of revenue productivity (TFPR). Even if the policy did increase firm-level productivity (TFP), if prices also fell, this may not be reflected by increases in TFPR. The decline in prices also implies that the liberalization benefited consumers on two dimensions. Greater quantities were produced and sold at lower prices.

Quantities

We also test whether the increase in revenues caused by the reform was accompanied by an increase in output. To do so, we estimate equation (5) with log units produced as the outcome. The last three columns of Table 5 report the results, with column 4 reporting the average effects and columns 5 and 6 reporting heterogeneous effects using the Y/K and LP definitions, respectively. On average, output increases by 23%, with larger effects on capital constrained firms. Among this group, quantity produced increases by 13% more relative to low MPRK firms, which also experience an increase in quantity produced by 14%. Since our observations are at the product-firm-year level, and the specification controls for firm-by-product fixed effects, the effect of the liberalization on quantities is estimated exclusively on the intensive margin. That is, we show firms produce more units of the same products. Regressions on the number of products yield small and statistically insignificant positive point estimates.[22] Thus, we conclude that the reform allowed credit constrained firms to produce more of their existing goods, rather than leading the firms to offer new products.

5.4 Robustness of Firm-level Results

In this subsection, we provide several tests for potential remaining sources of bias for our estimates of the effects of the foreign capital liberalization policies, as well as other robustness tests. These are (i) controlling for differential time trends among industries, (ii) controlling for other Indian reforms that may have coincided with financial liberalization, (iii) accounting for differential attrition rates by firms, (iv) showing the results are robust to using alternative methods for estimating MRPK and classifying high MRPK firms, and (v) controlling for cross-industry spillovers.

Differential Time Trends

Differential time trends pose a threat to our estimation strategy if they are correlated with the deregulation policies that we study. It is worth emphasizing that the identification assumption for our main misallocation results in Table 3 is already milder than in

22. These results are available upon request.

standard difference-in-differences settings. This is because the key coefficient of interest in equation (2) is β_2, the coefficient on $Reform_{jt} \times I_i^{High\,MRPK}$. The estimation of β_2 exploits variation in the *within*-industry evolution of capital constrained firms' outcomes relative to unconstrained firms. Thus, the key identifying assumption is that, in the absence of the deregulation, the within-industry gap between constrained and unconstrained firms would have evolved in the same way in deregulated and non-deregulated industries, an assumption for which Figure 2 provides graphical evidence.

We next show our estimates of β_2 are robust to adopting a more conservative specification. We include 5-digit industry-by-year fixed effects in equation (2) to control for any time-varying unobserved characteristics at the most disaggregated industry level possible, including differential time-trends. These more stringent fixed effects ensure that the coefficient of $Reform \times\ I^{High\,MRPK}$ is identified by comparing firms within the same narrowly defined industry in the same year.[23] Note that, in this case, because the reform varies at the 5-digit industry level, the baseline effect of the reform is no longer identified, since it is collinear with the fixed effects, but the differential effect on the high MRPK firms is. Appendix Table A2 reports the results for this specification and shows that the differential effect of the policy on high MRPK firms remains quantitatively similar.

While estimating β_2 tests whether the policy affects misallocation, as we will see in Section 7, estimating β_1 (the coefficient on $Reform_{jt}$) is a necessary step to identify the aggregate effect of the policy changes. Therefore, the stronger assumption that industries are on parallel time trends is needed to compute the aggregate effects of misallocation.

To show that even estimates of β_1 are unlikely to be driven by differences in industry trends, we include 2-digit industry-by-year fixed effects in equation (2). Including these fixed effects ensures that the average effect of the reform is identified by comparing firms with similar levels of MRPK across different 5-digit industries that belong to the same 2-digit industry-year.[24] This strategy effectively accounts for any unobserved time-varying, sector-level shocks, such as aggregate trade shocks and differences in input costs at the 2–digit industry level. By definition, it also controls for sector-level time trends. We report the results in Appendix Table A3. Across all the different firm outcomes, the point estimates are similar to our baseline specification in Table 3, suggesting that β_1 is also not biased by differential time trends.

Controlling for Trade Liberalization

In addition to a liberalization of its capital account, India also experienced a massive reduction in its trade tariffs in the 1990s. This raised firms' productivity by increasing competition in the industries in which they operate and allowed them to access a broader set of inputs at a cheaper price (Topalova and Khandelwal, 2011; Goldberg, Khandelwal,

23. At the 5-digit level, there are 303 distinct industries in manufacturing.
24. There are 23 distinct 2-digit industries and 303 distinct 5-digit industries.

Pavcnik, and Topalova, 2010; De Loecker, Goldberg, Khandelwal, and Pavcnik, 2016). If trade liberalization occurred in similar industries to the foreign financial liberalization and its effects took time to appear, this could bias our results.

Our specification with industry-year fixed effects already partially accounts for this potential bias, since the trade liberalization occurred at the industry-level. However, it's possible that trade tariff liberalization had a differential effect on capital constrained and unconstrained firms. For instance, this would occur if opening up to trade allowed the more efficient (more capital constrained) firms to export more and thereby grow. To account for this, we take measures of input and output tariffs from 1995-2001 from Goldberg, Khandelwal, Pavcnik, and Topalova (2010).[25] To create time-variant tarriff measures for our entire study period, we regress these measures on a linear time trend whose coefficient is allowed to vary at the 5-digit industry-level and then predict a firm's yearly input and output tariff levels. We then include both these predicted levels and their interaction with $I_i^{High\,MRPK}$ as controls in our regression.

Appendix Table A4 reports the results when we control for the output tariffs only (the odd columns) or both the output and input tariffs (the even columns). Across the different specifications, the effect of the international capital market liberalization on capital constrained firms remains virtually unchanged, and there is some suggestive evidence that input tariffs also increase misallocation.

Firm Entry and Exit

To examine if our results could be affected by differential attrition between treated and untreated industries, we re-estimate equation (2) using a balanced panel of firms who appear in both 1995 and 2015. Appendix Table A5 reports the results from this exercise. While the balanced samples are substantially smaller for both classifications, the same pattern as before is evident. In both cases, physical capital for high MRPK firms increase by approximately 50% relative to low MRPK firms, and the results are also similar for other outcomes. Thus, differential attrition is unlikely to explain our main findings.

Using the industry-level variation in the policy over time, we also directly test whether the policy affected firm exit and entry. If the policy had no effect on attrition, attrition should not bias our results. We identify entry in the data using the year of incorporation. True exit is not explicitly recorded in Prowess, since a firm may simply exit the panel because it decides to stop reporting its information to CMIE. Nonetheless, we can use whether a firm is observed exiting the dataset (and not reappearing later) as a proxy for exit. To estimate the average effect of the policy on exit and entry we then create counts of the number of firms in a 5-digit industry by year cell that exited or entered. To

25. To create input tariff measures, Goldberg, Khandelwal, Pavcnik, and Topalova (2010) take the weighted sum of the percent tariffs on each input used to produce a product (based on the Indian input-output table).

estimate the differential effect on high and low MRPK firms, we create these counts for industry-year-MPRK category cells.

Appendix Table A6 reports our results. At least in the context of Prowess, we find little evidence that the policy affected entry and exit.[26] While column 2 does show that the policy had a statistically significant effect on exit for low MRPK firms under the Y/K classification, this effect is small in magnitude (.06 more firms per year) and does not replicate for the LP classification. Altogether, Appendix Table A6 provides further evidence that neither differential attrition nor firm exit and entry themselves are driving the estimated effect of the policy on misallocation.

Alternative Measures of MRPK

We next verify if our results are robust to a two additional methods for estimating MRPK. First, we re-estimate our industry-level production functions as value-added production functions following the methodology of Ackerberg, Caves, and Frazer (2015). Based on these production functions, we recalculate MRPK and re-assign firms to the high MRPK category. Appendix Table A7 reports our results using this alternative classification. Despite a greatly reduced sample size, we again see evidence that the policy increases capital among capital constrained firms and reduces misallocation.

Second, since production function estimation methodologies are designed to estimate quantity production functions, we also take advantage of the fact that Prowess has price and quantity data to use Levinsohn-Petrin to estimate the quantity production functions and then recalculate MRPK.[27] Appendix Table A8 reports our results using this classification. While the sample size is again significantly smaller than for our main specifications, both qualitatively and quantitatively, the patterns are again very similar.

Spillovers

Cross-industry spillovers through input-output linkages across treated and non-treated industries could bias our estimates downward if they lead the policy to affect the outcomes of firms in non-liberalized industries. To assess this possibility, we directly estimate the spillover effects of the financial liberalization.

As in Acemoglu, Akcigit, and Kerr (2016), we separately measure the intensity of spillover effects of liberalization through the input-output matrix on upstream and downstream industries, using entries of the Leontief inverse matrices as weights:

26. This is not necessarily surprising since Prowess only includes large and medium-sized firms, for which exit and entry rates are likely to be relatively low. Indeed, in the average 5-digit industry, there are only 0.84 exit events a year, and in the average industry, there are only 0.033 entry events. In more than 50% of industry-years, there are zero exits. In 95% of industry-years, there are zero entrances.

27. For multi-product firms, we create single price by taking the sales-share weighted average of their prices. Then, quantity is given by the sales divided by this single price.

$$Upstream_{k,t} = \sum_{l} \left(Input\%_{l \to k}^{2000} - \mathbf{1}_{l=k} \right) \times Reform_{l,t},$$

and

$$Downstream_{k,t} = \sum_{l} \left(Output\%_{k \to l}^{2000} - \mathbf{1}_{l=k} \right) \times Reform_{l,t},$$

where k and l represents industries at input-output table level, $\mathbf{1}_{l=k}$ is an indicator function for $l = k$, and the summation is over all industries, including industry k itself. The notation $Input\%_{l \to k}$ represents the elements of the input-output matrix $\mathbf{A} = [a_{ij}]$, where $a_{ij} \equiv \frac{Sales_{j \to i}}{Sales_i}$ measures the total sales of inputs from industry j to industry i, as a share of the total inputs of industry i. The notation $Output\%_{k \to l}$ denotes the input-output matrix $\hat{\mathbf{A}} = [\hat{a}_{ij}]$, where $\hat{a}_{ij} \equiv \frac{Sales_{i \to j}}{Sales_i} = a_{ji} \frac{Sales_j}{Sales_i}$ measures the total sales of outputs from industry i to industry j, as a share of the total sales of industry i. We use the input-output matrices in 2000 since it is the last pre-treatment year and subtract the direct policy effects by controlling directly for the policy change in industry k in the regression.[28] We then directly control for these spillover measures in our main regression equations, and also allow spillovers to be differential for high MRPK firms.

Appendix Table A9 reports the results for the average effect of the policy. In Appendix Table A9, we find no evidence of average spillover effects through the production network. Additionally, the positive average effects of the reform are robust to the inclusion of the controls for spillovers. Appendix Table A10 reports the estimates of the heterogeneous effects of the policy, controlling for spillovers. The estimates are again very similar to those that don't account for spillovers.

6 Extension to Labor Misallocation

Our results so far show that opening up to foreign capital allowed firms not only to invest more (as seen by the increase in their stock of capital) but also to expand their wage bills. Reducing capital market frictions may simply increase the demand for labor because of the complementarity between capital and labor in the production function. However, it is also possible that the financial liberalization directly reduced labor misallocation, a hypothesis which we test in this section.

There is a natural link between capital market frictions and labor misallocation, though this link may at first be less intuitive than the link between capital market frictions and capital misallocation. Although labor is often modelled as a fully adjustable variable across periods,[29] in reality, labor is likely to have a fixed-cost component due

28. We use the input-output matrix for India from the World Input-Output database.

29. For example, Olley and Pakes (1996) model labor as a flexible, variable input, while modeling

to wage rigidity and hiring/firing costs. As a result, when there is a mismatch between the payments to labor and the generation of cash-flows, financial constraints may affect employment and labor (mis)allocation. Schoefer (2015), Chodorow-Reich (2014), Benmelech, Bergman, and Seru (2015), and Fonseca and Doornik (2019) all provide evidence in support of this channel.

To investigate if the reform reduces labor misallocation, we use the same estimation strategy as before but now compare the effects of the policy on firms with higher or lower marginal revenue products of labor (MRPL) prior to the policy change. We classify high and low MRPL firms analogously to how we classify high and low MRPK firms and estimate the heterogeneous effects of the policy on high MRPL firms.

Table 6 reports the results. We find some evidence that following the liberalization, labor constrained firms' revenue increased, although the effect is not statistically significant. These firms also invest 29% more in physical capital (column 2, significant at 10%) relative to low MRPL firms. Interestingly, the largest effect of the reform is on the firm total wage bill (column 3), with a relative increase of 32%, which confirms that our measure of MRPL indeed captures constraints on labor inputs. By allowing labor-constrained firms to grow faster and to expand employment, the deregulation appears to have led to a further reduction in misallocation. Among ex-ante labor constrained firms, MRPL decreased by approximately 35%.

7 Aggregate Effects

Having shown that the liberalization policies reduced misallocation, we now quantify the effect of this reduction on the manufacturing sector's productivity. In this section, we describe how to use our quasi-experimental policy variation to estimate a lower bound effect of the policy on the manufacturing industry's Solow Residual.

Framework for Approximating Changes in the Solow Residual. In general, as demonstrated by Baqaee and Farhi (2019), a first order approximation of the change in the Solow Residual of industry I over time is given by

$$\Delta Solow_{I,t} \approx \sum_{i \in I} \lambda_i \Delta \log A_i + \sum_{i \in I} \lambda_i (1 - \mu_i^{-1})(\Delta \log y_i - \Delta \log A_i) \tag{6}$$

where i indexes producers in industry I, λ_i is each producer's sales as a share of industry net-output, μ_i^{-1} is producer i's output wedge, $\Delta \log y_i$ is the log change in the quantity of goods produced by producer i, and $\Delta \log A_i$ is a Hicks-neutral productivity shifter to producer i. A derivation of this expression is provided in Appendix A.

capital as a stock that requires adjustment.

To apply this expression to our setting, we rewrite equation (6) as follows. First, since we are interested in the effect of the policy due to the change in misallocation and since we do not find that the policy has significant effects on productivity, we set $\Delta \log A_i = 0$. Second, we rewrite equation (6) in terms of firm-level capital, labor, and materials wedges and consider each firm's capital, labor, and materials as a "producer."[30] So, as in the conceptual framework, the wedge on firm i's input x is τ_i^x, and the price paid by the firm is $(1 + \tau_i^x)p^x$, while the marginal cost of producing x is p^x. The gross output wedge is given by: $\mu_i^x = 1 + \tau_i^x$.

Hence, equation (6) becomes:

$$\Delta Solow_{I,t} \approx \sum_{\substack{i \in I \\ x \in \{k,l,m\}}} \lambda_i \alpha_i^x \tau_i^x \Delta \log x_i \tag{7}$$

where the right-hand side sums over the firms and the three input types (capital, labor, and materials), and τ_x is the input wedge for each input type. To estimate the change in the Solow Residual of industry I caused by the policy, we estimate the right-side of equation (7).

We first recognize that most of the components of equation (7) are already observed in the data or given by our natural experiment estimates. To estimate λ_i, we note that the net-output of the manufacturing sector is simply the share of total industry sales that is not re-used as manufacturing inputs. This can be estimated for 2000 (the last pre-treatment year) by summing over manufacturing firms' total sales and using information from India's input-output table.[31] α_i^x is given by our production function estimates from the LP production function. We also note that $\Delta \log x_i$ is given by the firm-level effects of the policy on each input x. Therefore, we can estimate $\Delta \log x_i$ for capital, labor, and materials using our difference-in-differences strategy. For example, for capital, we estimate

$$\begin{aligned} \log(k_{ijt}) = &\beta_1 Reform_{jt} + \beta_2 Reform_{jt} \times I_i^{High\,MRPK} + \beta_3 Reform_{jt} \times I_i^{High\,MRPL} \\ &+ \beta_4 Reform_{jt} \times I_i^{High\,MRPM} + \mathbf{\Gamma X_{it}} + \alpha_i + \delta_t + \epsilon_{ijt} \end{aligned}$$

where $I_i^{High\,MRPM}$ is an indicator variable equal to 1 if the marginal revenue product of materials according to the LP production function estimation is above the industry-level median in the pre-treatment period. We allow for this additional heterogeneity in case the policy affected materials misallocation as well. All the remaining covariates are defined in the same way as in Section 4.2. Then, we estimate the change in $\log k_i$ due to the

30. While the framework of Baqaee and Farhi (2019) models wedges on output rather than inputs, their framework is general and input wedges can be thought of as a special case of this formulation. In particular, we can think of each input wedge for firm i coming from a fictitious middleman firm that buys the input without a wedge and then sells it with an output wedge to firm i.

31. We use the publically available input-output table from the World Input-Output Database.

policy, $\widehat{\log k_i}$, with

$$\widehat{\log k_i} = \widehat{\beta}_1 Reform_j + \widehat{\beta}_2 Reform_j \times I_i^{High\, MRPK} + \widehat{\beta}_3 Reform_j \times I_i^{High\, MRPL} \\ + \widehat{\beta}_4 Reform_j \times I_i^{High\, MRPM},$$

where $Reform_j$ is an indicator variable equal to 1 if an industry is liberalized between 1995 and 2015. We use an analogous strategy to estimate $\log l_i$ and $\log m_i$.

The coefficient estimates from these regressions are in Appendix Table A11, and we use these coefficients to estimate $\Delta \log x_i$. Now, the only unobserved term on the right-side of equation (6) is τ_x, the size of the wedges prior to the policy. As we discuss below, identifying these objects is more complicated.

Identifying the Lower Bound Effect of the Policy. Our identification strategy does not allow us to identify the level of wedges prior to the policy change. As we have discussed previously, identifying baseline wedges from cross-sectional variation in the marginal revenue products of capital, labor, and materials can be problematic.[32] If there is measurement error in inputs or the production functions are misspecified, attributing the dispersion of measured marginal revenue products to the dispersion in actual marginal revenue products (wedges) can greatly affect measures of misallocation. Instead of this approach, we focus on bounding the effects of the policy.

We see from equation (7) that the aggregate policy effect on the Solow Residual will always be increasing in the size of the pre-policy wedges. Therefore, we can bound the effects of the policy on the Solow Residual if we can bound the size of the pre-treatment wedges. To place a lower bound on the size of the pre-treatment wedge, we make two assumptions. Let $Reform_j$ be an indicator variable for whether there was a policy change in industry j.

Assumption 1 *The policy did not subsidize firms for which $Reform_j = 1$*

That is, after the policy, the marginal revenue product of capital for these firms did not become less than the price of capital. This is consistent with the fact that the average differences in the marginal revenue products of high and low MRPK and MRPL firms at baseline were much higher than the estimated effect of the policy on firms with high MRPK or MRPL.

Assumption 2 *The policy did not affect the wedges of firms for which $Reform_j = 0$*

32. For example, Rotemberg and White (2017) show that the estimated cross-sectional variation in TFPR, a common measure of misallocation, is extremely sensitive to standard data cleaning methods like winsorizing extreme values. Haltiwanger, Kulick, and Syverson (2018) also show that the estimated distribution of TFPR in cross-sectional data is sensitive to the econometrician's choice of specification. Estimates of MRPK, MRPM, and MRPL are likely to be vulnerable to the same issues.

The latter assumption is our standard difference-in-differences assumption. Together, these assumptions imply that the policy only reduced firms' wedges.

The post-policy wedge for a firm is given by $\tau^x_{post} = \tau^x_{pre} + \Delta\tau^x$, where $\Delta\tau^x$ is the change in τ^x due to the policy. Under the assumption that the policy does not subsidize firms, $\tau^x_{post} \geq 0$. Then, $\min_{\tau^x_{post} \geq 0} \tau^x_{pre} = -\Delta\tau^x$. Thus, the minimum possible pre-treatment wedge is given by the scenario where, after the policy change, the industry is Pareto-efficient, and there are no wedges left. In this case, any measured dispersion in marginal revenue products after the policy change is attributed to mismeasurement and mis-specficiation as opposed to misallocation. So, if we can estimate $\Delta\tau^x$, this gives us a lower bound estimate of τ^x_{pre}, and we can apply equation (7) to estimate the effects of the policy on the Solow Residual.

To see how to estimate τ^x_{pre} for each input x, let's focus on the case of capital inputs.[33] Denote $mrpk_i$ to be the true marginal revenue product of capital of firm i, which is never observed. The measured marginal product of capital is denoted $MRPK_i$. As the marginal product of capital is observed with measurement error, $\log(MRPK_{it}) = \log(mrpk_{it}) + \mu_i + \eta_t + \epsilon_{it}$, where ϵ_{it} is a firm-period idiosyncratic error, μ_i is a firm-specific, time-invariant shock, and η_t is a time-period specific shock. Denote T_j to be the time period of the reform in industry j. If a firm is in an industry that did not go through a reform ($Reform_j = 0$) or if the firm is in an industry that will be reformed but the reform has not taken place yet ($Reform_j = 1$ and $t < T_j$):

$$\log(mrpk_{ijt}) = \log(1 + \tau^k_{it}) + \log(p^k_t)$$

By Assumption 1, if the firm is in an industry that is reformed and the reform has taken place, $Reform_j = 1$ and $t > T_j$, then $\tau^k_{it} = 0$ and

$$\log(mrpk_{ijt}) = \log(p^k_t).$$

Hence, if $Reform_j = 0$ or $Reform_j = 1$ and $t < T_j$:

$$\log(MRPK_{ijt}) = \log(1 + \tau^k_{it}) + \log(p^k_t) + \mu_i + \eta_t + \epsilon_{it}$$

For firms where $Reform_j = 1$ and $t \geq T_j$

$$\log(MRPK_{ijt}) = \log(p^k_t) + \mu_i + \eta_t + \epsilon_{it}$$

Now, consider our difference-in-differences regression

$$\log MRPK_{ijt} = g_i(Reform_{jt}) + \mathbf{\Gamma X_{it}} + \alpha_i + \delta_t + \epsilon_{ijt} \tag{8}$$

33. The reasoning is identical for labor and materials.

where $g_i(Reform_{jt})$ is a flexible function of $Reform_{jt}$, so that the effect of the reform can depend on firms' or industries' attributes. Notice that in the difference-in-differences regression, the firm-specific shock will be accounted for by firm fixed effects, the year specific shock will be accounted for by year fixed effects, and $\log(p_t^k)$ is also subsumed by year fixed effects. Furthermore, by Assumption 2, any change in the wedges of these firms will be uncorrelated with $Reform_{jt}$. Our identification assumption is that ϵ_{it} is orthogonal to the reform. Therefore, for a firm i for which $Reform_j = 1$,

$$E(\log(1+\tau_i^k)|g(Reform_{jt})) = E(\hat{g}_i(1))$$

Thus, the difference-in-differences regression allows us to predict $\log(1+\tau_i^k)$ as a function of the policy change. Once the difference-in-differences regression has been used to estimate $\log\widehat{(1+\tau_i^k)}$, we simply compute $\exp(\log\widehat{(1+\tau_i^k)}) - 1 = \widehat{\tau}_i^k$ to obtain our estimate of τ^k. The process for estimating τ_i^l and τ_i^m is exactly the same.

Regression Equation and Results. In practice, to more fully capture the effects of the policy change, we specify g_i to allow for heterogeneous effects by firms' pre-treatment characteristics. Thus, for the marginal revenue product of capital we estimate

$$\begin{aligned}\log MRPK_{ijt} =& \beta_1 Reform_{jt} + \beta_2 Reform_{jt} \times I_i^{High\,MRPK} + \beta_3 Reform_{jt} \times I_i^{High\,MRM} \\ &+ \beta_4 Reform_{jt} \times I_i^{High\,MRPL} + \mathbf{\Gamma X_{it}} + \alpha_i + \delta_t + \epsilon_{ijt} \end{aligned} \quad (9)$$

Our regression results for capital, along with labor and materials are analogous and reported in Appendix Table A11. Then

$$\begin{aligned}\log\widehat{(1+\tau_i^k)} =& \hat{\beta}_1 Reform_j + \hat{\beta}_2 Reform_j \times I_i^{High\,MRPK} + \hat{\beta}_3 Reform_j \times I_i^{High\,MRM} \\ &+ \hat{\beta}_4 Reform_j \times I_i^{High\,MRPL} \end{aligned} \quad (10)$$

where $Reform_j$ is again an indicator variable equal to 1 if a firm is in an industry that liberalized between 1995 and 2015.

Now that we have estimated all the components of equation (7), we can calculate the effect of the policy on the Solow Residual and find that the lower bound effect is a 6.5% increase.

Following Hsieh and Klenow (2009), it is common in the misallocation literature to estimate differences in misallocation by using differences in the cross-sectional or time series dispersion in marginal revenue products. This approach has recently been criticized by Haltiwanger, Kulick, and Syverson (2018), Rotemberg and White (2017), and Asker, Collard-Wexler, and De Loecker (2014) for inflating the effects of misallocation. For comparison, we also estimate the effects of the policies on the Solow Residual if we attributed all of the dispersion in MRPK, MRPL, and MRPM to misallocation. If we attribute *all* the dispersion within a 5-digit manufacturing industry to misallocation, we estimate that the policy would increase the Solow Residual by 159%. However, this large effect is driven by outliers. If we only attribute within-5 digit industry dispersion to misallocation and winsorize the top and bottom 15% of deviations, we find that the policy increased the Solow Residual by 10%. The fact that winsorizing has a meaningful effect on the estimates is consistent with the findings of Rotemberg and White (2017), who show that winsorizing has large effects on the degree of measured misallocation in cross-sectional data from the U.S. and India. Indeed, Hsieh and Klenow (2009) trim the 1% tails of plant productivity and distortions when they estimate the degree of misallocation in India. Given the range of estimates produced by different choices about the treatment of outliers (from a 10% to 159% increase in the Solow Residual), it appears that approaches that use cross-sectional variation to identify wedges will be highly sensitive to arbitrary choices of where to winsorize or trim data.

8 Conclusion

This paper addresses two key challenges in a growing literature on misallocation. First, we develop new tools for measuring the aggregate effects of reducing misallocation, which do not rely on observed cross-sectional variation in the marginal revenue products of inputs. Second, we provide evidence on important levers that policy-makers can use to reduce misallocation, particularly in low-income countries, where the costs of misallocation are likely to be great.

Exploiting within-country, within-industry and cross-time variation, we show that foreign capital liberalization reduced the misallocation of capital and labor in India. The liberalization that automatically approved foreign investments in the 2000's increased capital in the treated industries. However, the effects of the liberalization on the average firm mask important heterogeneity in the policy effect. The entirety of the liberalization's effect on firms' outcomes is driven by increased investment in firms that previously had high marginal revenue products of capital. Thus, the policy change reduced the marginal revenue returns to capital for these firms, reducing misallocation. These results suggest that foreign capital liberalization may be an important tool for low-income countries to reduce capital market frictions.

Aggregating our reduced-form estimates, we also find that the policy increased the manufacturing's industry's Solow Residual by at least 6.5%. In contrast, if we assumed all the dispersion in the marginal revenue products of inputs was due to misallocation, we would estimate the policy increased the Solow Residual by 159%. Our methodology, which is less sensitive to measurement error or outliers, can be applied to other settings where there is an exogeneous shock to firms' input wedges. Thus, our results provide evidence that quasi-experimental variation can improve the measurement of the effects of reducing misallocation.

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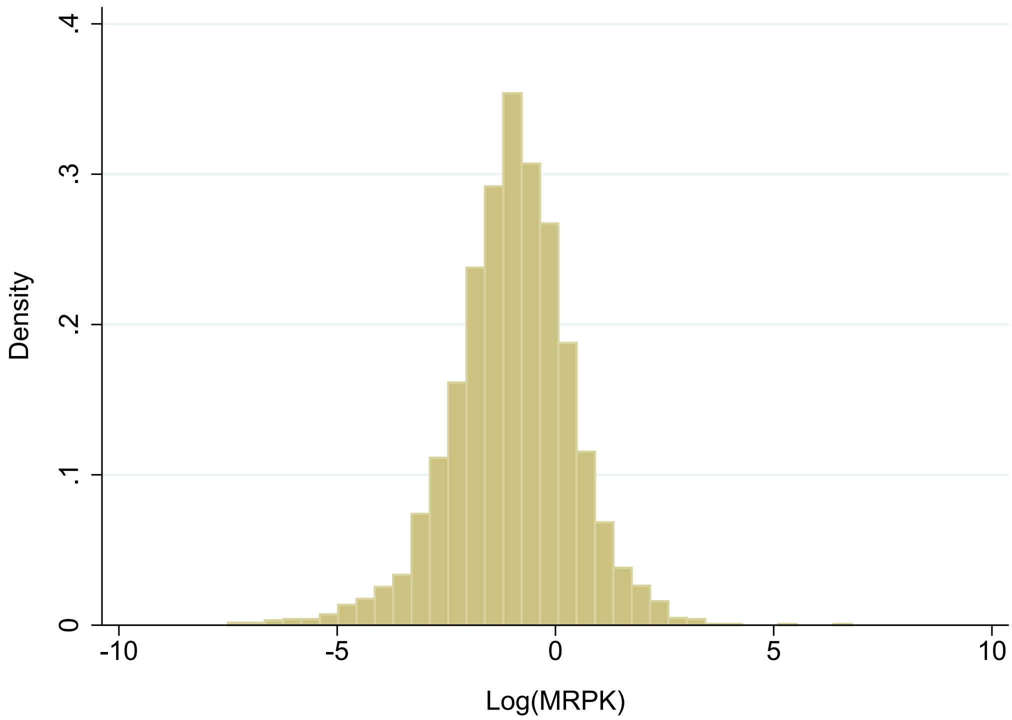
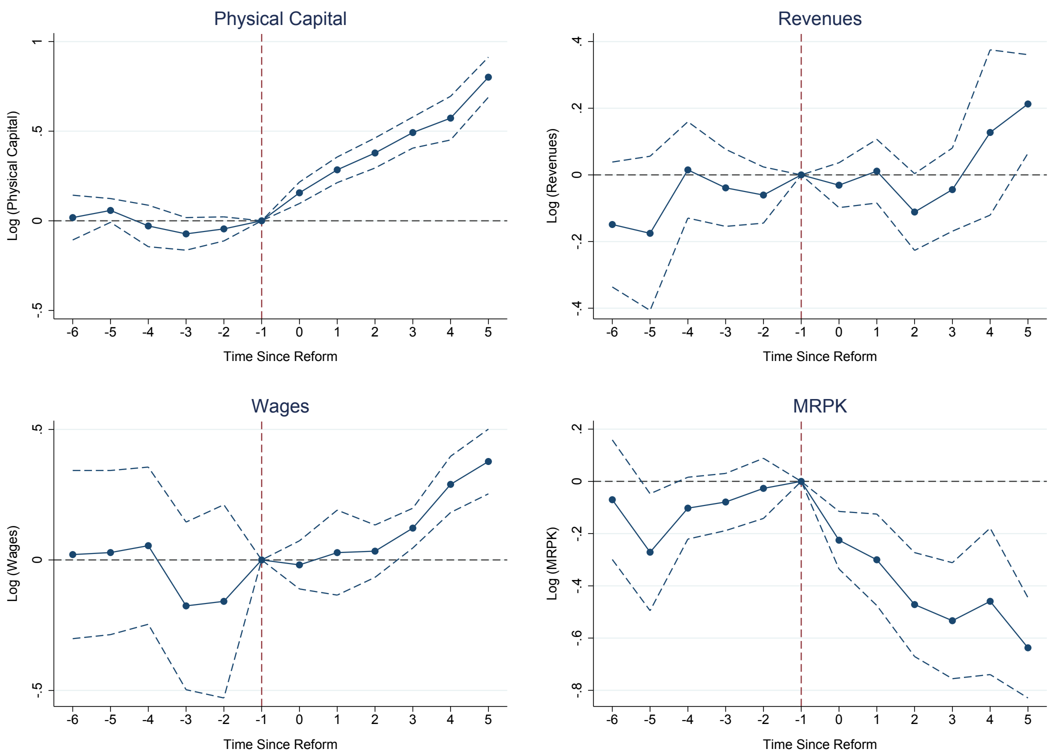
Figures

Figure 1: Distribution of Log(MRPK) in 2000



This figure displays the distribution of log(MRPK) for manufacturing firms in the Prowess data in 2000, the year before the first deregulation episode in 2001. MRPK is computed from revenue production functions estimated with the methodology of Levinsohn and Petrin (2003).

Figure 2: Event Study Graphs for the Relative Effect of Foreign Capital Liberalization on High MRPK Firms



This figure reports event study graphs for the relative effects of the liberalization on firms with high pre-treatment MRPK. MRPK is calculated using Y/K as a within-industry proxy for MRPK.

Tables

Table 1: Summary Statistics for Manufacturing Firms in the Prowess Data

	Obs.	Mean	p10	p50	p90
Treated During Study Period (%)	66,654	9	0	0	0
Private, Domestic (%)	66,654	57	0	100	100
Private, Foreign (%)	66,654	5	0	0	0
State Owned (%)	66,654	4	0	0	0
Firm Age	66,654	26	8	21	52
Gross Fixed Assets (Deflated)	63,950	23	0	3	37
Sales/Revenues (Deflated)	62,784	58	1	11	107
Salaries (Deflated)	49,090	3	0	1	6
Income	64,155	68	1	10	115

This table reports summary statistics for the manufacturing firms appearing in the CMIE Prowess dataset from 1995 to 2015. An observation is at the firm-year level. Firms' capital, income, salaries, and revenues are measured in millions of USD.

Table 2: Average Effect of the Foreign Capital Liberalization

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**	**TFPR**
	(1)	(2)	(3)	(4)	(5)
$Reform_{jt}$	0.22***	0.29***	0.14	-0.18	-0.08
	(0.07)	(0.10)	(0.11)	(0.11)	(0.06)
Observations	62,636	63,704	48,983	61,081	44,888
Firm FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓

This table reports difference-in-differences estimates of the effect of the foreign capital liberalization (equation (4)) over the period 1995–2015. All dependent variables are in logs. $Reform_{jt}$ is an indicator variable equal to one if the industry had liberalized access to the international capital market in year t and zero otherwise. In Column (4), MPRK is computed using Y/K as a proxy for the marginal revenue product of capital. In Column (5) TFPR is computed by estimating the production function using the method of Levinsohn and Petrin (2003). Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10, 5, and 1% statistical significance respectively.

Table 3: Effect of Foreign Capital Liberalization by Firms' Ex-Ante Capital Constraints

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**	**TFPR**
	(1)	(2)	(3)	(4)	(5)
			Panel A: Y/K Classification		
$Reform_{jt} \times I_i^{High\ MRPK}$	0.18***	0.60***	0.26**	-0.43***	-0.07
	(0.05)	(0.07)	(0.11)	(0.08)	(0.06)
$Reform_{jt}$	0.12	-0.04	-0.01	0.07	-0.04
	(0.08)	(0.09)	(0.09)	(0.12)	(0.08)
Observations	62,636	63,704	48,983	61,081	44,888
Firm FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓
			Panel B: LP Classification		
$Reform_{jt} \times I_i^{High\ MRPK}$	0.23**	0.46***	0.31**	-0.56***	-0.13
	(0.09)	(0.15)	(0.11)	(0.11)	(0.08)
$Reform_{jt}$	0.12	0.08	-0.01	0.19	-0.00
	(0.08)	(0.07)	(0.09)	(0.12)	(0.09)
Observations	50,070	50,478	41,035	38,613	38,613
Firm FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓

This table reports estimates of the foreign capital liberalization on high and low pre-treatment MRPK firms (equation (2)) over the period 1995–2015. All dependent variables are in logs. Firms are classified as high MRPK if their average MRPK in the pre-treatment period from 1995-2000 is above the industry median. In Panel A, MRPK is estimated with the Y/K method. In Panel B, it is estimated by estimating the production function using the methodology of Levinsohn and Petrin (2003). Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table 4: Heterogenous Effect of Foreign Capital Liberalization: Local Financial Development

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
	Panel A: Y/K Classification			
$Reform_{jt} \times I_i^{High\,MRPK}$	0.17***	0.60***	0.26**	-0.44***
	(0.00)	(0.00)	(0.03)	(0.00)
$Reform_{jt} \times I_i^{High\,MRPK}$	-0.15**	-0.27***	-0.16***	0.08*
$\times$*Local Credit Market Development*	(0.02)	(0.00)	(0.00)	(0.05)
Observations	57,636	58,733	45,161	56,183
Double and Single Interactions	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Age FE	✓	✓	✓	✓
	Panel B: LP Classification			
$Reform_{jt} \times I_i^{High\,MRPK}$	0.23**	0.46**	0.32**	-0.52***
	(0.01)	(0.02)	(0.02)	(0.00)
$Reform_{jt} \times I_i^{High\,MRPK}$	-0.15	-0.33***	-0.19**	0.12*
$\times$*Local Credit Market Development*	(0.10)	(0.00)	(0.01)	(0.06)
Observations	57,636	58,733	45,161	56,183
Double and Single Interactions	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Age FE	✓	✓	✓	✓

This table reports estimates of the foreign capital liberalization on high and low pre-treatment MRPK firms by ex ante state-level financial development over the period 1995–2015. All dependent variables are in logs. *Reform* is an indicator variable equal to one if the industry liberalized access to the international capital market. Firms are classified as high MRPK if their average MRPK in the pre–treatment period from 1995-2000 is above the industry median according to the Y/K method. Local credit market development is proxied using the amount of bank credit in the state in the pre–treatment period. Double and single interactions consistent of the relevant controls for the triple-differences specification. Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table 5: Effect of Foreign Capital Liberalization on Prices and Product Outputs

Dependent Variable	**Log Unit Price**			**Log Output**		
	(1)	(2)	(3)	(4)	(5)	(6)
$Reform_{jt}$	-0.09*	-0.03	-0.06	0.23***	0.14**	0.07
	(0.05)	(0.04)	(0.04)	(0.08)	(0.06)	(0.07)
$Reform_{jt} \times I_i^{High\,MRPK}$ (Y/K)		-0.09**			0.13*	
		(0.04)			(0.08)	
$Reform_{jt} \times I_i^{High\,MRPK}$ (LP)			-0.03***			0.27*
			(0.00)			(0.16)
Observations	149,867	149,867	124,212	151,113	151,113	125,244
Firm–Product FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓	✓

This table reports estimates of the effect of the liberalizations of access to foreign investors on unit prices and product output (equations (5)) for the period 1995–2015. Each observation is at the firm-product-year level. Firms are classified as high MRPK if their average MRPK in the pre-treatment period from 1995-2000 is above the industry median. In columns 2 and 5, MRPK is approximated as Y/K. In columns 3 and 6, it is calculated by estimating the production function using Levinsohn and Petrin (2003) methods. Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively

Table 6: Effect of Foreign Capital Liberalization by Firms' Ex-Ante MRPL

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPL**
	(1)	(2)	(3)	(4)
	Panel A: Y/L Classification			
$Reform_{jt} \times I_i^{High\ MRPL}$	0.15	0.29*	0.32***	-0.35***
	(0.11)	(0.15)	(0.08)	(0.09)
$Reform_{jt}$	0.17***	0.19***	-0.00	0.15
	(0.05)	(0.05)	(0.10)	(0.10)
Observations	52,097	52,616	42,705	41,797
Firm FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Age FE	✓	✓	✓	✓
	Panel B: LP Classification			
$Reform_{jt} \times I_i^{High\ MRPL}$	0.13	0.25*	0.31***	-0.35***
	(0.11)	(0.14)	(0.09)	(0.11)
$Reform_{jt}$	0.18***	0.21***	-0.00	0.14
	(0.05)	(0.05)	(0.11)	(0.13)
Observations	50,121	50,524	41,068	38,657
Firm FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Age FE	✓	✓	✓	✓

This table reports estimates of the international capital market liberalization reforms' effects on high and low pre-treatment MRPL firms (analoguous to equation (2), except substituting the high MRPL classification for high MRPK) over the period 1995–2015. All dependent variables are in logs. *Reform* is an indicator variable equal to one if the industry has liberalized access to the foreign capital market. Firms are classified as high MRPL if their average MRPL in the pre-treatment period from 1995-2000 is above the industry median. In Panel A, MRPL is approximated as Y/L. In Panel B, it is calculated by estimating the production function using Levinsohn and Petrin (2003) (LP). Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table 7: Effects of Foreign Capital Market Liberalization on the Solow Residual of Manufacturing

	Increase in Solow Residual
Lower Bound	6.5%
Attributing All Cross-Sectional Variation	159.3%
Measurement Error Correction (Top and Bottom 15%)	10.1%

This table reports the estimates of the effect of the foreign capital liberalizations in 2001 and 2006 on the manufacturing industry's Solow Residual. The estimates are generated using the Prowess data set. The first row gives the lower bound estimate, which assumes that the policy eliminated misallocation. The second row attributes all of the baseline variation in the marginal revenue products of inputs to misallocation. The third row does the same after trimming the top and bottom 15% of the marginal revenue products.

Appendix A: Derivation of Aggregation Formula

In this section, we derive equation (6), the formula used to approximate the change in the solow residual due to the policy. We start by defining

$$y_i = A_i f(y_{ij}),$$

where y_i is the output of firm i, A_i is firm i's productivity, f is the production function, and y_{ij} is a vector of inputs to firm i, where j denotes the firm that sold the input. Then, the total derivative of y_i is

$$d\log y_i = \sum_j \frac{\partial \log f_i}{\partial \log y_{ij}} d\log y_{ij} + d\log A_i. \tag{11}$$

A firm i solves the constrained cost minimization problem

$$\mathcal{C}_i(p, y_i) = \sum_{p_j y_{ij}} + \gamma_i(y_i - A_i f_i(y_i)), \tag{12}$$

where p is the vector of prices, p_j is the price of a good produced by j, and γ_i is the Lagrangian multiplier. From the first order conditions of equation (12)

$$p_j = \gamma_i A_i \frac{\partial f_i}{\partial y_{ij}}. \tag{13}$$

Then,

$$\mu_i = \frac{p_i}{\partial \mathcal{C}/\partial y_i} = \frac{p_i}{\gamma_i},$$

where μ_i is the mark-up of i, implying that $\gamma_i = \frac{p_i}{\mu_i}$. Substituting this relationship into (13) shows that $p_j = \frac{p_i}{\mu_i} A_i \frac{\partial f_i}{\partial y_i}$. Then

$$\begin{aligned} \frac{p_j y_{ij}}{p_i y_i} &= \frac{A_i y_{ij}}{\mu_i y_i} \frac{\partial f_i}{\partial y_{ij}} \\ &= \frac{\partial \log f_i}{\partial \log y_{ij}} \frac{1}{\mu_i}, \end{aligned}$$

which can be rewritten as $\mu_i \frac{p_j y_{ij}}{p_i y_i} = \frac{\partial \log f_i}{\partial \log y_{ij}}$. Then, substituting this into the total derivative (equation (11)) produces

$$d\log y_i = d\log A_i + \mu_i \sum_j \frac{p_j y_{ij}}{p_i y_i} d\log y_i.$$

Note that this implies that

$$\frac{1}{\mu_i}(d\log y_i - d\log A_i) - \sum_{j \notin I} \frac{p_j y_{ij}}{p_i y_i} d\log y_{ij} = \sum_{j \in I} \frac{p_j y_{ij}}{p_i y_i} d\log y_{ij}. \tag{14}$$

Now that we have these expressions, we can turn to deriving our object of interest. We define firm-level net output to be c_i and total industry-level output to be $PC = \sum_{i \in I} p_i c_i$, where $c_i = y_i - \sum_{j\in I} y_{ij}$. Then

$$d\log c_i = \frac{y_i}{c_i} d\log y_i - \sum_{j\in I} \frac{y_{ij}}{c_i} d\log y_{ij}$$

and the change in industry-level net output is given by

$$d\log C = \sum_i \frac{p_i c_i}{PC} d\log c_i = \sum_i \Big(\frac{p_i y_i}{PC} d\log y_i - \sum_{j\in I} \frac{p_i y_{ij}}{PC} d\log y_{ij}\Big).$$

Then, the change in the Solow residual for I is approximated by

$$\Delta Solow_I \approx d\log C - \sum_{i\in I} \sum_{j\notin I} \frac{p_j y_{ij}}{p_i y_i} \frac{p_i y_i}{PC} d\log y_i.$$

Using equation (14), with a little algebra, we can rewrite this as

$$\Delta Solow_I \approx \sum_{i\in I} \lambda_i (1 - \frac{1}{\mu_i})(d\log y_i - d\log A_i) + \sum_{i\in I} \lambda_i d\log A_i,$$

where $\lambda_i = \frac{p_i y_i}{PC}$.

Appendix Tables

Table A1: List of Industries that Changed Foreign Investment Polices Between 1995 and 2015

(1) **NIC 5-Digit Industry Classification**	(2) **Reform Year**
Manufacture of 'ayurvedic' or 'unani' pharmaceutical preparation	2001
Manufacture of allopathic pharmaceutical preparations	2001
Manufacture of medical impregnated wadding, gauze, bandages, dressings, surgical gut string etc.	2001
Manufacture of homoeopathic or biochemic pharmaceutical preparations	2001
Manufacture of other pharmaceutical and botanical products n.e.c. like hina powder etc.	2001
Manufacture of rubber tyres and tubes n.e.c.	2006
Manufacture of essential oils; modification by chemical processes of oils and fats (e.g. by oxidation, polymerization etc.)	2006
Manufacture of various other chemical products	2006
Manufacture of rubber tyres and tubes for cycles and cycle-rickshaws	2006
Manufacture of distilled, potable, alcoholic beverages such as whisky, brandy, gin, 'mixed drinks' etc.	2006
Coffee curing, roasting, grinding blending etc. and manufacturing of coffee products	2006
Retreading of tyres; replacing or rebuilding of tread on used pneumatic tyres	2006
Manufacture of chemical elements and compounds doped for use in electronics	2006
Manufacture of country liquor	2006
Manufacture of matches	2006
Manufacture of rubber plates, sheets, strips, rods, tubes, pipes, hoses and profile -shapes etc.	2006
Distilling, rectifying and blending of spirits	2006
Manufacture of bidi	2006
Manufacture of catechu(katha) and chewing lime	2006
Stemming and redrying of tobacco	2006
Manufacture of other rubber products n.e.c.	2006
Manufacture of rubber contraceptives	2006
Manufacture of other tobacco products including chewing tobacco n.e.c.	2006
Manufacture of pan masala and related products.	2006

This table lists 5-digit NIC industries that changed to automatic foreign investment approval for investments up to (at least) 51% of a firm's capital and the year that the policy reform took place.

Table A2: Heterogeneous Effects of Foreign Capital Liberalization: 5-Digit Industry-by-Year Fixed Effects

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
	Panel A: Y/K Classification			
$Reform_{jt} \times I_i^{High\ MRPK}$	0.32***	0.74***	0.43***	-0.40***
	(0.05)	(0.08)	(0.11)	(0.10)
Observations	62,439	62,116	47,339	59,462
Firm FE	✓	✓	✓	✓
5-Digit Industry–Year FE	✓	✓	✓	✓
Age FE	✓	✓	✓	✓
	Panel B: LP Classification			
$Reform_{jt} \times I_i^{High\ MRPK}$	0.40***	0.63***	0.44***	-0.54***
	(0.09)	(0.14)	(0.10)	(0.10)
Observations	49,322	48,932	39,428	37,005
Firm FE	✓	✓	✓	✓
5-Digit Industry–Year FE	✓	✓	✓	✓
Age FE	✓	✓	✓	✓

This table reports estimates of the heterogeneous effects of the liberalization reforms access to foreign investors on high MRPK firms in the Prowess data set (equation (2)). Firms are observed between 1995 and 2015. All regressions include firm fixed effects, survey year fixed effects, firm age fixed effects, and 5-digit industry by year fixed effects. Firms are classified as constrained if their average MRPK in the pre-treatment period from 1995-2000 is above the industry median. In Panel A, MRPK is approximated as Y/K. In Panel B, it is calculated by estimating the production function using LP. Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table A3: Robustness of Heterogeneous Effects of Foreign Capital Liberalization to Inclusion of 2-Digit Industry by Year FE

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
		Panel A: Y/K Classification		
$Reform_{jt} \times I_i^{High\,MRPK}$	0.20***	0.61***	0.29**	-0.40***
	(0.05)	(0.08)	(0.11)	(0.09)
$Reform_{jt}$	0.01	-0.13	-0.11	0.13
	(0.11)	(0.14)	(0.11)	(0.13)
Observations	64,009	63,697	48,968	61,061
Firm FE	✓	✓	✓	✓
2-Digit Industry–Year FE	✓	✓	✓	✓
Age FE	✓	✓	✓	✓
		Panel B: LP Classification		
$Reform_{jt} \times I_i^{High\,MRPK}$	0.27**	0.48***	0.33***	-0.55***
	(0.09)	(0.15)	(0.12)	(0.11)
$Reform_{jt}$	-0.03	-0.03	-0.14	0.14
	(0.12)	(0.18)	(0.09)	(0.12)
Observations	50,857	50,454	41,006	38,595
Firm FE	✓	✓	✓	✓
2-Digit Industry–Year FE	✓	✓	✓	✓
Age FE	✓	✓	✓	✓

This table reports estimates of the heterogeneous effects of foreign capital liberalization reforms on high and low MRPK firms in the Prowess data set (equation (2)). Firms are observed between 1995 and 2015. All regressions include firm fixed effects, survey year fixed effects, firm age fixed effects, and 2-digit industry by year fixed effects. Firms are classified as constrained if their average MRPK in the pre-treatment period from 1995-2000 is above the industry median. In Panel A, MRPK is approximated as Y/K. In Panel B, it is calculated by estimating the production function using LP. Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10, 5, and 1% statistical significance respectively.

Table A4: Effect of Foreign Capital Liberalization, Controlling for Tariffs

Dependent Variable	**Revenues**		**Capital**		**Wages**		**MRPK**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Reform_{jt} \times I_i^{High\ MRPK}$	0.15**	0.15**	0.55***	0.52***	0.20	0.20*	-0.39***	-0.36**
	(0.06)	(0.07)	(0.10)	(0.10)	(0.12)	(0.11)	(0.10)	(0.13)
Product Tariff	0.21	0.27	0.18	-0.09	0.27	0.16	-0.19	0.02
	(0.21)	(0.25)	(0.22)	(0.24)	(0.19)	(0.16)	(0.18)	(0.26)
Product Tariff $\times I_i^{High\ MRPK}$	-0.14	-0.28	-0.20	0.54	-0.26*	-0.00	0.16	-0.50
	(0.15)	(0.16)	(0.25)	(0.33)	(0.14)	(0.15)	(0.28)	(0.37)
Input Tariff		-0.35		1.46		0.58		-1.10
		(0.95)		(1.05)		(0.57)		(0.92)
Input Tariff $\times I_i^{High\ MRPK}$		0.69		-3.77***		-1.31**		3.36**
		(0.64)		(1.17)		(0.59)		(1.21)
Observations	64,022	64,022	63,704	63,704	48,983	48,983	61,081	61,081
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓	✓	✓	✓

This table reports estimates of the foreign capital liberalization on high and low pre-treatment MRPK firms (equation (2)) over the period 1995–2015, controlling for the effects of tariff policies and allowing those tariff policies to have differential effects by high and low MRPK. All dependent variables are in logs. *Reform* is a dummy equal to one if the industry has liberalized access to international capital market. Firms are classified as high MRPK if their average MRPK in the pre–treatment period from 1995-2000 is above the industry median. Tariff data from 1995-2001 are obtained from Goldberg, Khandelwal, Pavcnik, and Topalova (2010), and then regressed on a linear time trend whose coefficient is allowed to vary at the 5-digit industry level to obtain a time series of predicted tariff values from 1995 to 2015. Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table A5: Robustness of Heterogeneous Effects of Foreign Capital Liberalization to Using a Balanced Panel of Firms

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
	Panel A: Y/K Classification			
$Reform_{jt} \times I_i^{High\ MRPK}$	0.25*	0.47***	0.04	-0.24**
	(0.14)	(0.05)	(0.09)	(0.10)
$Reform_{jt}$	0.03	0.04	0.08	-0.10
	(0.15)	(0.12)	(0.09)	(0.12)
Observations	29,975	29,640	23,601	29,131
Firm FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Age FE	✓	✓	✓	✓
	Panel B: LP Classification			
$Reform_{jt} \times I_i^{High\ MRPK}$	0.26*	0.36**	0.21	-0.31***
	(0.15)	(0.17)	(0.12)	(0.10)
$Reform_{jt}$	0.05	0.13	-0.02	0.03
	(0.12)	(0.08)	(0.09)	(0.12)
Observations	25,624	25,338	20,452	19,642
Firm FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Age FE	✓	✓	✓	✓

This table reports estimates of the heterogeneous effects of foreign capital liberalization on capital constrained and unconstrained firms in a balanced panel of firms that appear in both 1995 and 2015 from the Prowess data set (equation (2)). Firms are observed between 1995 and 2015. All regressions include firm fixed effects, survey year fixed effects, and firm age fixed effects. Firms are classified as constrained if their average MRPK in the pre-treatment period from 1995-2000 is above the industry median. In Panel A, MRPK is approximated as Y/K. In Panel B, it is calculated by estimating the production function using the LP method. Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table A6: Effects of Foreign Capital Liberalization on Firm Exit and Entry

Dependent Variable	**Number of Exits**			**Number of Entrants**		
	(1)	(2)	(3)	(4)	(5)	(6)
$Reform_{jt}$	0.16	0.06**	0.04	-0.01***	-0.01	-0.01
	(0.34)	(0.03)	(0.06)	(0.00)	(0.01)	(0.01)
$Reform_{jt} \times I_i^{High\,MRPK}$ (Y/K)		-0.03			-0.00	
		(0.03)			(0.00)	
$Reform_{jt} \times I_i^{High\,MRPK}$ (LP)			-0.00			0.00
			(0.02)			(0.00)
Observations	8,190	12,411	11,025	8,190	12,411	11,025
Industry Fixed Effects	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓
High MPRK Control	–	✓	✓	–	✓	✓

This table estimates the effect of the foreign capital liberalization on firm exit and entry in the Prowess data. In columns (1) and (5), an observation is a 5-digit industry-year. In the remaining columns, it is a 5-digit industry-year-MRPK category cell. A firm is counted as exiting in a year if it is not observed in the data in that year and does not re-enter the data in a later year. A firm is counted as entering in a year if that is the year of the firm's incorporation. Standard errors are twoway clustered at the 4-digit industry and year level.

Table A7: Robustness of Heterogeneous Effects of Foreign Capital Liberalization to ACF Classification

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
		Panel: ACF Classification		
$Reform_{jt} \times I_i^{High\ MRPK}$	0.19	0.65***	0.37***	-0.52***
	(0.16)	(0.16)	(0.09)	(0.16)
$Reform_{jt}$	0.27*	0.07	0.13	0.08
	(0.14)	(0.13)	(0.11)	(0.22)
Observations	18,378	18,613	16,286	12,356
Firm FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Age FE	✓	✓	✓	✓

This table reports estimates of the foreign capital liberalization on high and low pre-treatment MRPK firms (equation (2)) over the period 1995–2015. All dependent variables are in logs. *Reform* is a dummy equal to one if the industry has liberalized access to international capital market. Firms are classified as high MRPK if their average MRPK in the pre-treatment period from 1995-2000 is above the industry median. MRPK is calculated by estimating the production function using ACF. Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table A8: Robustness of Heterogeneous Effects of Foreign Capital Liberalization to Estimates From Quantity Production Functions

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
		Panel: LP Classification		
$Reform_{jt} \times I_i^{High\ MRPK}$	0.21**	0.45***	0.15*	-0.49***
	(0.09)	(0.12)	(0.08)	(0.10)
$Reform_{jt}$	0.18	0.20*	0.15	0.18**
	(0.11)	(0.11)	(0.11)	(0.07)
Observations	32,339	32,557	26,257	19,605
Firm FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Age FE	✓	✓	✓	✓

This table reports estimates of the foreign capital liberalization on high and low pre-treatment MRPK firms (equation (2)) over the period 1995–2015. All dependent variables are in logs. *Reform* is a dummy equal to one if the industry has liberalized access to international capital market. Firms are classified as high MRPK if their average MRPK in the pre-treatment period from 1995-2000 is above the industry median. MRPK is calculated by estimating the production function using quantities data using Levinsohn-Petrin. Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table A9: Average Effect of Foreign Capital Market Liberalization and Cross-Industry Spillover Effects

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
$Reform_{jt}$	0.23***	0.25***	0.11	-1.08
	(0.06)	(0.09)	(0.09)	(0.94)
Upstream	-0.38	-0.12	-0.30	-0.22
	(0.35)	(0.25)	(0.24)	(0.21)
Downstream	0.24	0.05	0.40	0.17
	(0.23)	(0.13)	(0.27)	(0.15)
Observations	54,081	54,905	40,234	52,633
Firm Age FE	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

This table reports difference-in-differences estimates of the effect of the foreign capital liberalization in the Prowess data set, taking into account cross-industry spillover effects. *Upstream* measures the composite reform shock from upstream industries, and *Downstream* measures the composite reform shock from downstream industries. Firms are observed between 1995 and 2015. Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table A10: Heterogeneous Effects of Foreign Capital Liberalization: Spillovers

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
$Reform_{jt} \times I_i^{High\ MRPK}$	0.18***	0.60***	0.26**	-0.44***
	(0.05)	(0.07)	(0.11)	(0.08)
$Reform_{jt}$	0.11	-0.05	-0.02	0.06
	(0.08)	(0.09)	(0.08)	(0.13)
Upstream	-0.12	0.14	-0.00	-0.25*
	(0.22)	(0.16)	(0.16)	(0.13)
Downstream	0.33	0.09	0.25	0.26
	(0.29)	(0.19)	(0.30)	(0.17)
Observations	51,541	51,244	37,598	49,026
Firm FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Age FE	✓	✓	✓	✓

This table reports estimates of the heterogeneous effects of foreign capital liberalization on capital constrained and unconstrained firms in a balanced panel of firms that appear in both 1995 and 2015 from the Prowess data set (equation (2)). Firms are observed between 1995 and 2015. All regressions include firm fixed effects, survey year fixed effects, and firm age fixed effects. Firms are classified as constrained if their average MRPK in the pre-treatment period from 1995-2000 is above the industry median. MRPK is approximated as Y/K. Standard errors are double clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table A11: Regression Estimates Used to Estimate the Effect of the Policy on the Manufacturing Solow Residual

Dependent Variable	**Log MRPK**	**Log MRPL**	**Log MRPM**	**Log Assets**	**Log Salaries**	**Log Materials**
	(1)	(2)	(3)	(4)	(5)	(6)
$Reform_{jt}$	0.30*	0.30*	0.18***	0.03	-0.12	-0.09
	(0.16)	(0.18)	(0.06)	(0.08)	(0.11)	(0.14)
$Reform_{jt} \times I_i^{High\ MRPK}$	-0.56***	-0.21***	0.01	0.47***	0.31***	0.05
	(0.09)	(0.05)	(0.10)	(0.14)	(0.09)	(0.08)
$Reform_{jt} \times I_i^{High\ MRPL}$	-0.14*	-0.35***	-0.12***	0.22*	0.30***	0.22*
	(0.08)	(0.09)	(0.03)	(0.13)	(0.11)	(0.12)
$Reform_{jt} \times I_i^{High\ MRPM}$	-0.07	-0.09	-0.23***	-0.10	-0.07	0.05
	(0.10)	(0.07)	(0.04)	(0.10)	(0.10)	(0.10)
Number of observations	38,284	38,284	38,284	50,030	40,683	48,443
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

This table reports the difference-in-differences estimates used to estimate the policy's effects on the manufacturing Solow Residual. Firms are observed between 1995 and 2015. All regressions include firm fixed effects, survey year fixed effects, and firm age fixed effects. Firms are classified as $High\ MRPK$ if their average MRPK in the pre-treatment period from 1995-2000 is above the industry median, where MRPK is calculated using the LP production function estimation method. $High\ MRPL$ and $High\ MRPM$ are defined analogously for materials. Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.