Misallocation and Capital Market Integration: Evidence From India*

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Abstract

We show that foreign capital liberalization reduces capital misallocation and increases aggregate productivity in India. The staggered liberalization of access to foreign capital across disaggregated industries allows us to identify changes in firms' input wedges, overcoming major challenges in the measurement of the effects of changing misallocation. For domestic firms with initially high marginal revenue products of capital (MRPK), liberalization increases revenues by 25%, physical capital by 57%, wage bills by 27%, and reduces MRPK by 35% relative to low MRPK firms. There are no effects on low MRPK firms. The effects of liberalization are largest in areas with less developed local banking sectors, indicating that foreign capital partially substitutes for an efficient banking sector. Finally, we develop a novel method to use natural experiments to bound the effect of changes in misallocation on treated industries' aggregate productivity. Treated industries' Solow residual increases by 4-17%.

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1 Introduction

The misallocation of resources across competing uses is a leading explanation for economic disparities across countries. However, identifying policies that can affect misallocation and quantifying their aggregate effects remains a major challenge. There are at least two reasons for this.

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 and can contaminate estimates of differences in allocative efficiency across countries or over time.[1,2]

On the policy side, even if one were able to fully correct for mismeasurement and quantify the effect of changes in misallocation on aggregate productivity, the specific sources of misallocation are difficult to identify from aggregate comparisons.[3] This leaves policymakers with limited information about what levers to pull to reduce misallocation. In low-income countries, where there are likely to be large firm-level frictions in the allocation of resources, understanding which policies reduce misallocation would provide policymakers with powerful tools to foster economic growth.

An unusual natural experiment in India allows us to make progress on both the measurement front and the policy front, providing some of the first evidence on a policy tool that can be used to reduce misallocation. Over the 2000s, India introduced the automatic approval of foreign direct investments up to 51% of domestic firms' equity, potentially reducing capital market frictions. Using the staggered introduction of the policy across industries, we implement a difference-

1. Upward bias can come, for example, from measurement error (Bils, Klenow, and Ruane, 2018; Rotemberg and White, 2017; Gollin and Udry, 2019), model misspecification (Haltiwanger, Kulick, and Syverson, 2018; Nishida, Petrin, Rotemberg, and White, 2017), volatility of productivity paired with the costly adjustment of inputs (Asker, Collard-Wexler, and De Loecker, 2014; Gollin and Udry, 2019), unobserved heterogeneity in technology (Gollin and Udry, 2019), and informational frictions and uncertainty (David, Hopenhayn, and Venkateswaran, 2016; David and Venkateswaran, 2019).

2. In a striking example, Bils, Klenow, and Ruane (2018) show that when cross-sectional comparisons do not correct for measurement error, misallocation appears to be *greater* in the United States than in India in the 2000s.

3. 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 observable 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 is mostly silent on what policies would be required to reduce misallocation in low-income countries. An important exception is David and Venkateswaran (2019), which makes progress on distinguishing various sources of dispersion.

in-differences framework to estimate the effects of this foreign capital liberalization on the misallocation of capital across firms. In the absence of a natural experiment, the measurement of changes in misallocation would be contaminated by measurement error and other (unobserved) shocks, as described above. However, in this setting, the natural experiment allows us to isolate changes in inputs and the observed marginal revenue product of capital due to the policy, controlling for many sources of unobserved heterogeneity that would otherwise lead to mismeasurement.

A priori, the effect of opening-up to foreign capital on allocative efficiency is unclear. On the one hand, in low-income countries, where formal credit markets are limited, opening up to foreign capital markets might reduce funding constraints if foreign investors have better screening technologies or are not bound by domestic historical, political, or regulatory constraints.[4,5] On the other hand, foreign investors may also be worse at processing and monitoring soft information, particularly in low-income countries, worsening the allocation of capital.[6]

We find that the liberalization of foreign capital reduces capital misallocation by increasing capital for firms with the highest marginal revenue returns to capital prior to the reform. We then develop a method, based on the theoretical results of Petrin and Levinsohn (2012) and Baqaee and Farhi (2019), to translate our quasi-experimental microeconomic estimates into lower and upper bound measures of the effect of the policy on the treated industries' Solow residual (a proxy for these industries' aggregate productivity). Our proposed method uses exogenous variation to generate a lower bound for the aggregate effect of changing misallocation under relatively weak identifying assumptions, without relying on cross-sectional dispersion in marginal revenue products.

To measure the effects of the reform, we collected data on industry-level liberalization episodes in 2001 and 2006. Combining this policy variation with a panel of large and medium-sized Indian firms over the period 1995–2015, we investigate

4. See Townsend (1994), Udry (1994), Banerjee, Duflo, Glennerster, and Kinnan (2015), Banerjee, Duflo, and Munshi (2003), Banerjee and Duflo (2014), Banerjee and Munshi (2004), Burgess and Pande (2005), and Cole (2009) for examples of domestic frictions in financing.

5. Indeed, Anne Krueger, 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).

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

whether the reform reduced misallocation by testing whether the policy had differential effects depending on firms' ex-ante marginal revenue products of capital (henceforth "MRPK"). By exploiting within-industry variation in firms' MRPK dispersion, 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. Thus, determining whether the policy reduced misallocation does not require deregulation to be random, nor for firms to have similar levels of pre-reform covariates, or even for treated and untreated industries to be on the same trends prior to the reforms. It only requires that high MRPK firms are not growing relatively more quickly than low MRPK firms within treated industries prior to the reform, an assumption that we provide visual evidence for using event studies. In our most stringent specifications, we can account for any unobserved shocks or differences in time trends at the disaggregated industry, state, and size quartile levels.

We find that, in response to the policy, high MRPK firms in deregulated industries increase their physical capital by 57%, revenues by 25%, wage bills by 27%, and reduce their MRPK by 35% relative to low MRPK firms. In contrast, low MRPK firms are not affected. Since high MRPK firms had more than 170% higher MRPK than low MRPK firms, 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 MRPK firms within treated industries relative to untreated industries. They also provide evidence that the reduction in misallocation is not due to mean reversion.

To better understand the mechanism underlying these results, we exploit geographic variation in local access to credit prior to the reform. We find that the effects of liberalization 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.

We next explore the effect of the reform on firms' products, including product portfolio, prices, and quantities. This is made possible by a rare feature of our firm-level data set: detailed data on each firm's product-mix, product-level output, and prices. Since reductions in distortions on input prices should reduce marginal costs for affected firms, firms may pass some of these gains onto consumers via lower prices. Depending on the degree of pass-through, the change in the price could be greater than or less than the change in the marginal cost. We find that the reform differentially reduced prices for high MRPK firms in treated industries by

15% but had no significant effect on the prices of low MRPK firms. Additionally, treated, high MRPK firms increase the number of products in their portfolio, in part by introducing more new products.

The liberalization policy may have had broader effects than reducing firms' wedges on capital inputs. If firms need to borrow to pay workers, relaxing financial constraints can also affect labor misallocation.[7] Motivated by this possibility, we examine the effect of the policy on labor misallocation. Analogous to our approach for capital, we estimate the policy's differential effect on firms with high marginal revenue products of labor (henceforth, "MRPL"). We find wage bills only increased for firms with high MRPL. For these firms, relative to low MRPL firms, wage bills increased by 29%, and MRPL fell by 32%. Since high MRPL firms had at least two times higher levels of MRPL prior to the treatment in treated industries, labor misallocation also fell.

Finally, combining production function parameter estimates with reduced-form estimates of the policy effect, we generate bounds on the effect of the liberalization on the treated industries' Solow residual. As a lower bound, the treated industries' Solow residual increased by 4%. Accounting for the cumulative effects of the policy over time raises this number to 7%. Even at a lower bound, the policy had economically meaningful aggregate effects. In contrast, if we infer baseline wedges from the pre-treatment cross-sectional data, the upper bound effect is 17%.

The paper is organized as follows. The remainder of the introduction discusses the related literature. Section 2 provides a brief conceptual framework for understanding misallocation and introduces the expression we will use for aggregation. Section 3 describes the data and the context of the policy change. Section 4 discusses our reduced-form empirical strategy. 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 MRPK. It also replicates the analysis for firms that have high and low MRPL to test whether the policy also reduced labor misallocation. Section 6 describes the aggregation strategy and reports estimated bounds on the foreign capital liberalization policies' effect on the Solow residual for treated industries. Section 7 concludes.

Related Literature. This paper contributes to two main literatures. First, it contributes to the literature quantifying the importance of misallocation for aggregate outcomes (e.g. Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009;

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

Bartelsman, Haltiwanger, and Scarpetta, 2013; Restuccia and Rogerson, 2013; Bento and Restuccia, 2017; Baqaee and Farhi, 2020; David and Venkateswaran, 2019; Sraer and Thesmar, 2020), particularly in the context of developing countries (e.g. Guner, Ventura, and Xu, 2008; Banerjee and Moll, 2010; Collard-Wexler, Asker, and De Loecker, 2011; Kalemli-Ozcan and Sørensen, 2014).[8] Second, it contributes to the literature on the effects of capital account liberalization, financial frictions, and misallocation (Buera, Kaboski, and Shin; 2011; Midrigan and Xu, 2014; Moll, 2014; Hombert and Matray, 2016; Bai, Carvalho, and Phillips, 2018; Delatte, Matray, and Pinardon Touati, 2019).

Regarding the misallocation literature, much of the previous work 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 overall 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 misspecification and measurement error can inflate estimates of misallocation and bias estimates of the effects of changing misallocation.

We make three 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 strategy only requires that measurement error or other unobserved attributes are uncorrelated with the policy change to identify *changes* in input wedges. Second, our approach isolates the changes in distortions produced by a specific policy, foreign capital liberalization.[9] This allows us to isolate the effect of access to the foreign equity market, holding constant access to the foreign debt market and other macroeconomic determinants that might affect the cost of capital differentially for different firms. Third, we show how our natural experiment estimates can be used to compute aggregate effects of reducing misallocation that are less vulnerable to inflation due to measurement error or model

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

9. In the context of India, several recent papers have studied 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 et al., 2013); land regulation (Duranton, Ghani, Goswani, and Kerr, 2017); industrial licensing (Chari, 2011; Alfaro and Chari, 2015); privatization (Gupta, 2005; Dinc and Gupta, 2011); reservation laws (Garcia-Santana and Pijoan-Mas, 2014; Martin, Nataraj, and Harrison, 2017; Boehm, Dhingra, and Morrow, 2019; Rotemberg, 2019); highway infrastructure (Ghani, Goswami, and Kerr, 2016); roads (Asher and Novosad, 2020); electricity shortages (Allcott, Collard-Wexler, and Connell, 2016), and labor regulation (Amirapu and Gechter, 2019).

mis-specification. In so doing, we develop a method that can be applied in many other contexts by researchers studying misallocation.

By developing a general method that exploits a natural experiment to identify changes in misallocation and quantify their effects on aggregate productivity, we also relate to Sraer and Thesmar (2020). Sraer and Thesmar (2020) develop a sufficient statistics approach that uses estimates from natural experiments to calculate the counterfactual effects of scaling-up a policy to the entire economy. This is fundamentally different from the object we bound — the aggregate effect of the policy that was actually enacted — which can be bounded with relatively few assumptions about firms' production functions and interactions.

In terms of 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, sectoral misallocation, and welfare (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, 2020; Xu, 2020; Méndez-Chacón and Van Patten, 2020; Li and Su, 2020; Liu, Wei, and Zhou, 2020).[10] We add to this literature in two 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 the institutional setting constant, which is important since institutional differences affect cross-country comparisons. 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.*[11]

10. Varela (2017) shows that financial liberalization can increase productivity, while Saffie, Varela, and Yi (2020) 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.

11. 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) study the effect of the drop in the interest rate for Southern European countries following the adoption of the Euro, which did not directly change the equity market.

2 Conceptual Framework

2.1 Misallocation and Reduced-Form Predictions

We follow standard practice in the literature and model misallocation as wedges on the prices of inputs. Intuitively, the wedges can be thought of as explicit or implicit taxes that implement a given (potentially inefficient) allocation in the decentralized Arrow-Debreu-McKenzie economy. Thus, the allocative price paid by a firm i for an input x is $(1+\tilde{\tau}_i^x)p^x$, where $x \in \{K, L, M\}$ and K, L, and M denote capital, labor, and materials, respectively. The observed price of input x is p^x, and $\tilde{\tau}_i^x$ is the additional wedge a firm pays for the input over the observed price. The wedge $\tilde{\tau}_i^x$ can be negative, indicating that a firm is subsidized, or positive, indicating that the firm pays a tax relative to the observed price. A single-product firm's profit function is

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

where $f_i(K_i, L_i, M_i)$ is the firm's production function, which exhibits diminishing marginal returns in each input.

A cost-minimizing firm will consume an input x_i until that input's marginal revenue returns $p_i \partial f_i(K_i, L_i, M_i)/\partial x_i$ are equal to the cost

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

where μ_i is the mark-up or output wedge.[12] Then, define the combined wedge $1+\tau_i^x = \mu_i(1+\tilde{\tau}_i^x)$. The marginal revenue product of input x is proportional to the (combined) wedge τ_i^x. Therefore, firms with higher combined input wedges τ_i^x (capital, labor or any other) will have higher marginal revenue products on this input (henceforth, "MRPX").

We now generate partial equilibrium predictions that we can use to test for a reduction in misallocation in the data. A decrease in the misallocation of input x occurs when the wedge τ_i^x declines for a firm whose wedge is high relative to other firms. A decline in the wedges of firms with relatively high initial τ_i^x will

12. Technically, if firm i has pricing power, then the marginal revenue product of an input x (MRPX) is better defined as $p_i \partial f_i(K_i, L_i, M_i)/\partial x_i + \partial p_i/\partial x_i f_i(K_i, L_i, M_i)$ rather than $p_i \frac{\partial f_i(K_i, L_i, M_i)}{\partial x_i}$. This is because a change in x both directly affects a firm's output and (if it has pricing power) its price. However, in the misallocation literature, MRPX typically refers to $p_i \frac{\partial f_i(K_i, L_i, M_i)}{\partial x_i}$ because it is dispersion in this value that causes misallocation. Thus, we use this definition of MRPX at the cost of abusing terminology.

have several effects. First, since τ_i^x falls, the measured MRPX should also fall for these firms. Second, firms with high wedges will increase their use of x. Finally, the increase in input x (say capital) will increase the marginal revenue products of the other inputs, which will incentivize firms to also increase their demand for these other inputs (e.g. labor). As a result of higher input use, these firms will produce more and earn higher revenues. Thus, if the policy reduces capital 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, and decreases MRPK for firms with ex-ante high values of MRPK.

2.2 Framework for Quantifying Effects on Solow Residual

To quantify the effects of reducing misallocation on treated industries' aggregate productivity, following much of the literature, we proxy for changes in aggregate productivity with changes in the Solow residual, which measures the net output growth minus the net input growth. Thus, denoting the Solow residual for a sector of interest I as $Solow_I$,

$$\Delta Solow_I = \Delta Net\, Output_I - \Delta Net\, Input_I. \tag{1}$$

Net output growth is the change in the treated firms' output net the outputs re-used as inputs by treated firms. Net input growth is the change in the inputs used by treated firms net of the inputs that are produced by treated firms. Let net output of good i be $c_i = y_i - \sum_{j\in I} y_{ji}$, where y_i is the output of firm i and y_{ji} are the inputs used by firm j of the output of i. The change in the treated firms' net output is defined as $\Delta C_I = \sum_{i\in I} p_i \Delta c_i$. This is the total change in net quantities valued using fixed prices. The Solow residual in discrete time is then

$$\Delta Solow_I = \Delta \log C_I - \sum_{j\notin I} \frac{\sum_{i\in I} p_j y_{ij}}{\sum_{i\in I} p_i c_i} \Delta \log \sum_{i\in I} y_{ij}. \tag{2}$$

The summation $\sum_{j\notin I}$ sums over firms that supply intermediate goods to firms in the treated industries but are not themselves treated, while the summation $\sum_{i\in I}$ sums over firms in the treated industries. Thus, $\Delta \log C_I$ measures the change in output due to the policy (differencing out outputs that are re-used as inputs), while the latter term in equation (2) subtracts out changes in inputs purchased from outside the treated industries. Intuitively, as shown in equation (1), the Solow residual measures the change in output valued using current market prices and differences out the growth in inputs valued using those same prices. Thus, in an accounting sense, it controls for input growth due to the policy.

In general, as demonstrated by Petrin and Levinsohn (2012) and Baqaee and Farhi (2019), a first order approximation of the change in the Solow residual of the set of treated firms in I over time is given by:

$$\Delta Solow_{I,t} \approx \sum_{i \in I} \lambda_i \Delta \log A_i + \sum_{\substack{i \in I \\ x \in \{K,L,M\}}} \lambda_i \alpha_i^x \frac{\tau_i^x}{1+\tau_i^x} \Delta \log x_i \tag{3}$$

where λ_i is the ratio of firm i's sales to treated industry I's net output, $\Delta \log A_i$ is the change in total factor productivity (TFPQ), α_i^x is the output elasticity with respect to x, τ_i^x is the level of firm-specific input wedges prior to the policy change, and $\Delta \log x_i$ is the change in the log input x consumed by firm i. This expression allows us to convert firm-level effects, which are in different units depending on the goods being produced, into aggregate effects. A derivation of this expression is provided in Appendix A. We show that this expression does not require any assumptions about returns to scale, cross-good aggregation, or the shape of input-output networks. As we will explain in Section 6, equation (3) will allow us to exploit our reduced-form estimates to put bounds on the aggregate effect of the policy change on the treated industries' Solow residual.

3 Data and Policy Change

3.1 Indian Foreign Investment Liberalization

Following its independence, India became a closed, socialist economy, and most sectors were heavily regulated.[13] 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, the World Bank, and the Asian Development Bank, which offered funding, the Indian government engaged in a series of structural reforms. These reforms led India to become more open and market-oriented. In addition to initiating foreign capital reforms in more than one-third of the manufacturing sector in this period, India also liberalized trade (e.g. Topalova and Khandelwal, 2011; Goldberg, Khandelwal, Pavcnik, and Topalova, 2010) and dismantled extensive licensing requirements (e.g. Aghion, Burgess, Redding, and Zilibotti, 2008; Chari, 2011).

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

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% for each firm in most industries. With the establishment of the initial liberalization reform in 1991, foreign investment up to 51% of equity in certain industries became automatically approved.[14] In the following years, different industries liberalized at different times, with each liberalization increasing the cap on foreign investment and allowing for automatic approval.

We study the effects of financial liberalization episodes that occurred after 2000, well 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 the financial liberalization reforms with other ongoing reforms. To study the effects of foreign investment liberalization, we 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) and includes all publicly traded firms, as well as a large number of private firms. 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.[15] The data is therefore particularly well-suited for examining 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 (measured as tangible, physical assets), consumption of raw materials and

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

15. The ASI is collected at the plant-level and does not include information on whether plants are owned by the same firm, making it impossible to detect changes in misallocation across firms due to opening or closing establishments.

energy, and compensation of employees 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 prices and quantities. This unusual feature is due to the fact that Indian firms are required by the 1956 Companies Act to disclose product-level information on capacities, production, and sales in their annual reports.[16] 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-level price for each product, which we define as total unit sales over total unit quantity. This allows us to also construct a separate panel of product-level output and prices from 1995-2015.[17]

3.3 Local Financial Development Data

To examine whether financial liberalization's effects depend on local financial development, we also collect state-level banking 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 (e.g. Burgess and Pande, 2005; Vig, 2013). To take advantage of this geographic variation, we collected data at the state-level from each of the pre-reform years (1995–2000) on the 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 formation 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 an existing state. Our state-level measures encompass 25 out of 26 Indian states and 4 out of 7 union territories. Altogether, this data covers 91.5% of net domestic product and 99% of credit.

16. A detailed discussion of the data can be found in Goldberg, Khandelwal, Pavcnik, and Topalova (2010).

17. One limitation of this dataset is that firms choose which type of units to report, and units are not always 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 drop the set of observations associated with a firm-product. We omit 2% of observations.

3.4 Final Combined Datasets

To arrive at our final datasets for analysis, we merge the firm-level and product-level panel data with the industry-level policy data and state-level financial development data.

As is common in the literature, 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 the early-1990s, the main Indian liberalization period. While 45% of manufacturing firms in the data are in industries that liberalized at some point, by restricting our sample to observations after 1995, we only exploit policy variation from the 10% of manufacturing firms who experienced foreign capital liberalizations in the 2000s. Second, although Prowess technically starts in 1988, its coverage in the first few years was limited and grew substantially over time. In 1988, Prowess only included 735 manufacturing firms total, but it had grown to 3,652 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 (3,664 firms observed in 1996, 3,470 in 1997, and 3,614 in 1998).[18]

Additionally, to allow for a longer pre-policy period over which to calculate MRPK and classify MRPK as high or low, as described below, we drop a very small number of observations that experienced a liberalization in 1998. This amounts to 104 total firm-year observations (roughly 4–5 per year) or 0.26% of the sample. Appendix Table A1 provides a list of the different industries in the manufacturing sector affected by the deregulation during the remaining sample. As the table shows, after dropping the 1998 liberalization, the only remaining 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-treatment 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.[19] These restrictions leave us with 5,013 distinct firms, across 343 distinct

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

19. This is the minimal requirement to calculate MRPK. As we document in the next subsection, we exploit the fact that, under Cobb-Douglas production functions, sales divided by

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

			Percentile		
	Obs.	Mean	10	50	90
Treated during Study Period (%)	68,690	10	0	0	100
Foreign (%)	68,690	4	0	0	0
State Owned (%)	68,690	4	0	0	0
Firm Age	68,690	26	8	21	52
Gross Fixed Assets (Deflated)	67,339	23	0	3	38
Sales/Revenues (Deflated)	64,808	61	1	11	113
Wages	65,912	3	0	1	7
MRPK (Revenue/K)	63,210	8	1	3	13

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. The 10th, 50th, and 90th percentiles are given by the final three columns.

5-digit industries, for a total of 68,690 observations.

Table 1 documents summary statistics for the final firm-level sample used in our analysis. As the table shows, the typical firm in our analysis is a domestic firm, while 4% of firms are foreign-owned firms, and 4% are state-owned. The table also shows that 10% of firms are in industries that experienced a policy change between 1995 and 2015.

4 Empirical Strategy

4.1 Measurement: MRPK and TFPQ

To determine whether foreign investment liberalization reduces misallocation, we follow the predictions in our conceptual framework and test if the reform has a differential effect on firms with high and low MRPK. Below, we describe the method used to measure firms' MRPK.

As is standard in the production function estimation literature,[20] we assume that firms have Cobb-Douglas revenue production functions:

$$Revenue_{ijt} = TFPR_{ijt} K_{ijt}^{\alpha_j^k} L_{ijt}^{\alpha_j^l} M_{ijt}^{\alpha_j^m} \tag{4}$$

where i denotes a firm, j denotes an industry, and t denotes a year. $Revenue_{ijt}$,

capital will be proportional to MRPK within an industry, as long as α_j^k is the same for all firms in industry j.

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

K_{ijt}, L_{ijt}, and M_{ijt} are measures of sales, capital, the wage bill, and materials, and $TFPR_{ijt}$ is the firm-specific unobserved revenue productivity. Throughout this paper, capital is measured as the total value of tangible, physical assets.

To estimate MRPK, we take advantage of the fact that, under the revenue Cobb-Douglas production function, $MRPK = \frac{\partial Revenue_{it}}{\partial K_{it}} = \alpha_j^k \frac{Revenue_{it}}{K_{it}}$. Thus, $\frac{Revenue_{it}}{K_{it}}$ provides a within-industry measure of MRPK, under the assumption that all firms in an industry share the same α_j^k. To determine whether firms had a high or low MRPK prior to the reform, we average each firm's measures of MRPK over 1995–2000 (the last year prior to the first policy change). We then classify a firm as high MRPK if its average MRPK is above the 4-digit industry-level median.

In addition to measuring MRPK, we also create a measure of TFPQ as a proxy for firm-level productivity. We implement the Levinsohn and Petrin (2003) method (henceforth "LP"), using the GMM estimation proposed by Wooldridge (2009), to estimate the parameters of revenue production functions at the 2-digit industry-level.[21,22] The LP method estimates the parameters of the production function using a control function approach, where materials are assumed to be increasing in a firm's unobserved productivity conditional on capital. This identifying assumption does not require that capital or labor are not misallocated — the key sources of misallocation that we study in this paper — but does assume away misallocation of materials. For the production function estimation, we measure inputs and revenues with deflated Ruppee amounts, so that $Revenue_{ijt}$ is proxied with deflated sales.[23] The revenue production function allows us to calculate revenue total factor productivity, TFPR. Using the product data, which measures unit prices, we calculate $\log TFPQ = \log TFPR - \log \tilde{p}$, where $\tilde{p}$ is the sales share weighted average of the prices of a firm's products. By estimating the effect of the reform on TFPQ, we can examine whether foreign capital liberalization affects within-firm productivity as well as misallocation. The sample size for which TFPQ is available is much smaller (46,765 firm-year observations), as calculating

21. In principle, we could use the quantity data to directly estimate quantity production functions, but in practice, relying on this data greatly reduces the sample size available for estimation.

22. One concern is that multi-product firms produce goods in multiple industries, leading to bias when we estimate production function parameters at the industry-level. We use the firm-level industry identifiers provided by Prowess to assign firms to industries (Prowess provides a single industry value for each firm), and this issue is partially mitigated by the fact that subsidiaries of large conglomerates in different industries appear as different observations in the data.

23. We use deflators for India made available by Allcott, Collard-Wexler, and Connell (2016) for the period 1995–2012, and we 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.

this measure requires data on all firm inputs, as well as price data. Thus, we view our within-firm level productivity results as more exploratory than our main misallocation results.

4.2 Main Specification: Heterogeneous Effects

To measure the effect of liberalization on the allocation of resources across firms within industries, we estimate the following equation:

$$Outcome_{ijt} = \beta_1 \; Reform_{jt} + \beta_2 Reform_{jt} \times I_i^{High\,MRPK} + \mathbf{\Gamma X_{it}} + \theta_i + \delta_t + \epsilon_{ijt} \tag{5}$$

where i denotes a firm, j denotes an industry, t denotes a year, and $outcome_{ijt}$ is the outcome variable of interest, consisting of the logs of physical capital, the total wage bill, sales, and MRPK. $Reform_{jt}$ is an indicator variable equal to one if foreign investment has been liberalized in industry j, and $I_i^{High\,MRPK}$ is an indicator variable equal to 1 if a firm has a high pre-reform MRPK according to our measure defined in Section 4.1. $\mathbf{X_{it}}$ consists of firm age and firm pre-treatment size-by-year fixed effects,[24] so that β_1 and β_2 are identified by comparing two firms with the same age and within the same size bin. In a robustness check, we show that our main results are robust to including a more parsimonious set of controls. θ_i and δ_t are firm and year fixed effects respectively. δ_t controls for aggregate fluctuations, while θ_i removes time invariant unobserved firm-level heterogeneity, which may bias estimates of the MRPK dispersion.[25] Standard errors are two-way clustered at the 4-digit industry and year level to account for any serial correlation that might bias our standard errors downward.[26]

The coefficient of interest is β_2, which captures the differential effect of the reform on ex-ante high MRPK firms relative to low MRPK firms. $\beta_2 > 0$ implies that the dependent variable increases for high MRPK firms relative to low MRPK firms in industries that have opened up to foreign capital relative to industries that have not. β_1 measures changes in low MRPK firms' outcomes, and $\beta_1 + \beta_2$

24. Firm size is defined as fixed effects for the within 2-digit industry quartiles of firms' average pre-treatment capital.

25. As previously discussed, cross-sectional measures of MRPK are likely to be inflated by measurement error. Indeed, if we calculated the level of capital misallocation using cross-sectional data, a standard approach would be to use an estimate of the variance of MRPK as a proxy for the dispersion of the wedges. This estimate would sum over both the variance of the wedges and the variance of measurement error, leading to inflated estimates of the dispersion of the wedges.

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

measures total changes in high MRPK firms' outcomes.

4.3 Identification

Below, we discuss the extent to which our empirical strategy is vulnerable to three potential sources of bias: (1) non-random assignment of treatment status across firms, (2) the endogeneity of foreign equity flows, and (3) measurement error in MRPK. We also emphasize that our test does not require that foreign investors directly identify and invest in high MRPK firms for the liberalization policies to reduce misallocation.

Selection of treated firms. One natural concern is that firms in industries that are liberalized are different from firms in industries that are not. As long as these differences are time-invariant, this selection is fully accounted for by firm fixed effects (θ_i). Similarly, firm fixed effects account for any time invariant differences, observed or unobserved, between high and low MRPK firms. Thus, our specification does *not* require that the reform was randomly allocated, nor does it require that firms must have the same pre-treatment characteristics.

A classic difference-in-differences set-up requires that treated firms would have had the same time trends as untreated firms in the absence of the reform. However, because we exploit differences *within* deregulated industries to estimate β_2, our key parameter for evaluating the change in misallocation, our identification assumption for β_2 is milder than the classic differences-in-differences assumption. We can still identify β_2 if treated and untreated industries have different industry-level time trends, as the latter are controlled for by the variable $Reform_{jt}$.[27] Thus, even if the Indian government liberalized industries that were growing more quickly earlier, β_2 would not be biased as long as high MRPK firms were not growing relatively more quickly than low MRPK firms within these industries.

While the assumptions needed to identify β_2 are milder than the standard difference-in-differences assumptions, when we turn to the aggregation exercise in Section 6, we will use our estimates of both β_1 and β_2. In contrast to identifying solely β_2, identifying β_1 requires that time trends are parallel between treated and untreated industries. We provide support for this assumption in two ways. First, we visually assess whether there are parallel pre-trends between treated and untreated industries in an event study figure. Second, we show that our

27. Our most stringent specifications account for time-varying differences across industries non-parametrically by including 5-digit industry-by-year fixed effects.

estimates of both β_1 and β_2 are insensitive to the inclusion of additional controls for differential time trends at the firm and industry-level.

Endogeneity of foreign equity flows. While it is likely that within an industry, foreign capital is targeted towards specific firms, we do not use observed variation in foreign capital in our regressions. Instead, we exploit an exogenous shifter to the amount of foreign capital an industry can receive. Therefore, to be unbiased, β_1 and β_2 do *not* require that foreign capital is allocated randomly across firms in treated industries. As long as the differential time trends assumptions discussed above are not violated, our approach delivers valid estimates of the effect of liberalizing industry-level access to foreign capital.

Measurement error in MRPK. Measurement error should have little effect on our estimates if it is either firm-specific and time-invariant or time-variant but common across firms in a given year. Firm fixed effects and year fixed effects account for systematic measurement error at the firm and year level.

On the left side of the equation, as is well-known in the econometrics literature, classical measurement error (i.e. error independent of the latent true variable) in the outcome variable will not bias the point estimates. On the right side, idiosyncratic measurement error in MRPK may bias our estimate of β_2 if it leads to error in the coding of $I_i^{High\ MRPK}$. This measurement error would lead some firms that are actually high MRPK to be coded as low MRPK, while some low MRPK firms will be coded as high MRPK. As long as the true effect of the policy is to reduce MRPK more for ex-ante high MRPK firms, misclassification will lead to attenuation bias. Since β_2 captures the change in high MRPK firms' capital wedges, this would lead us to *underestimate* the change in these firms' wedges due to the policy. However, non-classical measurement error could still bias our results in the other direction. We return to this issue in Section 5, when we show that our reduced-form estimates are not sensitive to winzorizing extreme values.

Investors allocate FDI in response to characteristics besides MRPK. Our test of the effect of the policy on misallocation does not require that foreign investors knowingly invest more in high MRPK firms or even that foreign investment specifically increases for high MRPK firms. Indeed, we do not take a stance on whether the relative increase in capital investment in ex-ante high MRPK firms is directly or indirectly driven by foreign investment. It could be, for example, that foreign investment frees up domestic capital to flow to smaller, high MRPK

Table 2: Average Effect of the Foreign Capital Liberalization

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
$Reform_{jt}$	0.110	0.305***	0.171*	-0.181*
	(0.088)	(0.102)	(0.093)	(0.092)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	62,924	65,393	63,999	61,342

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 or before year t and zero otherwise. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. In column 4, MRPK is computed using $Revenue/K$ as a proxy for the marginal revenue product of capital. Standard errors are two-way clustered at the 4-digit industry and year level. *, **, and *** denote 10, 5, and 1% statistical significance respectively.

firms. Regardless of whether foreign investors can identify and directly target high MRPK firms or not, foreign capital liberalization policies reduce misallocation if they lead to a relative increase in capital for high MRPK firms.

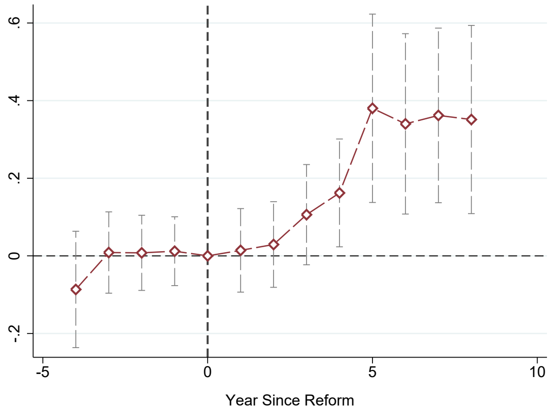
5 Results

5.1 Average Effects

We start by estimating the effect of the reform on the average firm by removing the interaction term $Reform_{jt} \times I_i^{High\ MRPK}$ from equation (5). Table 2 reports the results. The estimates indicate that the liberalization policy has positive effects on firm investments. For the average firm, capital increases by 31% (column 2). The point estimates for the total wage bill and revenues are also positive, albeit not significant at the 5%-level, while average MRPK declines by a marginally significant 18% (column 4).

Figure 1 plots the event study graph for the average effects on capital by showing the estimated yearly effect of belonging to a treated industry before and after the reform, including the same controls as in Table 2. Here, 0 is normalized to be the year before a reform. Consistent with the absence of differential pre-trends, we see that there is no effect of belonging to a treated industry before the reform took place.

Figure 1: Event Study Graph for the Average Effect of Foreign Capital Liberalization on Physical Capital



This figure reports the event study graph for the average effect of the liberalization on firms' physical capital. The dependent variable is in logs. The reform is normalized to take place in year 1. Each dot is the coefficient on the interaction between being observed t years after the reform and being in a treated industry. The confidence interval is at the 95% level.

5.2 Differential Effects by Ex-ante MRPK

Baseline specification. Table 3 reports the estimates of the heterogeneous effects of the policy from equation (5), our main estimating equation. Following the liberalization, high MRPK firms generate higher revenues by 25% (column 1), made possible by the fact that these firms invest more, with their physical capital increasing by 57% (column 2).

Higher investment does not crowd-out labor. High MRPK firms also experience a relative increase in their wage bills by 27%, suggesting that there may be important complementarities between capital and labor. We will explore specifically whether the reform also reduced labor misallocation in Section 5.5. Among the ex-ante high MRPK firms, the policy reduced MRPK by 35%. Given that, prior to the reform, high MRPK firms had a MRPK more than twice as high as low MRPK firms, the reform led to an important decline in the dispersion of MRPK (and a substantial reduction in misallocation).[28]

We use the same empirical strategy to examine whether the composition of capital changed heterogeneously as a result of the reform. Appendix Table A2

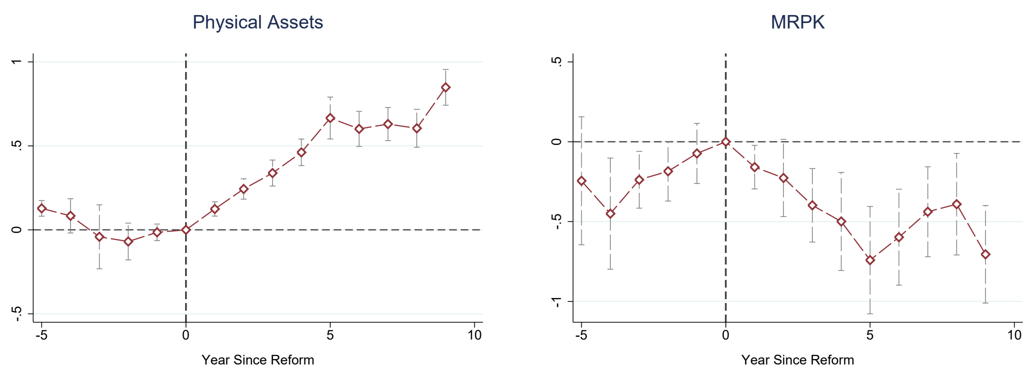
28. This finding might be surprising given the results in Bollard, Klenow, and Sharma (2013), who find that most of economic growth in this period in India could be attributed to within firm changes in productivity. However, Nishida, Petrin, Rotemberg, and White (2017) show that this conclusion depends on the form of the production function and might underestimate the contribution of reallocation to growth.

Table 3: Heterogeneous Effects of Foreign Capital Liberalization by Firms' Ex-ante MRPK

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
$Reform_{jt} \times I_i^{High\ MRPK}$	0.245***	0.565***	0.265***	-0.353***
	(0.071)	(0.063)	(0.058)	(0.101)
$Reform_{jt}$	-0.030	-0.009	0.022	0.021
	(0.115)	(0.077)	(0.095)	(0.113)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size $\times$Year	✓	✓	✓	✓
Observations	62,924	65,393	63,999	61,342

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 4-digit industry median. MRPK is estimated with the $Revenue/K$ method. Size$\times$Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are two-way clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

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 relative to those with low pre-treatment MRPK in treated industries. The reform is normalized to take place in year 1. Each dot is the coefficient on the interaction between being observed t years after the reform and being a high MRPK firm in a treated industry. All dependent variables are in logs. The confidence intervals are at the 95% level.

reports the results when the outcome variables are the share of a firm's capital in each category. These results show that following the reform, for high MRPK firms, 4 percentage points more of firms' capital was in the form of plants and equipment. There are no effects for low MRPK firms.

Pre-trends. To assess whether these heterogeneous effects are driven by pre-trends, we produce event study graphs. We create indicator variables for being observed five years before a reform, four years before, and so on and interact these with being in a treated industry and being a high MRPK firm in a treated industry. We include the same additional controls as in Table 3. Figure 2 reports the relative effects by year of being a high MRPK firm in a treated industry for the two key outcomes – the logs of capital and MRPK. Appendix Figure A1 reports the graphs for revenues and wages. Two facts are noteworthy.

First, for both of the main outcomes, being treated by the policy did not have a strong differential effect on high MRPK firms before the policy was adopted, providing visual evidence that pre-trends are not driving the results. To the extent there is a pre-trend for MRPK, it is in the wrong direction, indicating that MRPK was *increasing* for high MRPK firms prior to the policy change. The lack of correlation between high MRPK firms' outcomes and the reform prior to the year of deregulation also provides some preliminary evidence that the results are not driven by mean reversion, an alternative explanation that we explore in more detail in Section 5.4.

Second, the effect of the liberalization is progressive over time, consistent with the idea that changes in the allocation of resources (such as the adjustment of worker flows and adaptation of production tools) are likely slow-moving, particularly in India (e.g. Topalova, 2010). In addition, some of the changes in allocative efficiency, might also come from competitive effects, which also happen progressively through time.

TFPQ. Turning to our measure of within-firm productivity, column 1 of Table 4 reports the average effect of the policy on TFPQ. While the reform changed the allocation of inputs across the firms, we cannot reject a zero effect on within-firm productivity. Though imprecise, the point estimate is consistent with a positive average effect on TFPQ. Similarly, when we interact the reform with the indicator variable for high MRPK (column 2), we do not find any statistically significant differential effect. However, to the extent that the effects of the policy on TFPQ are positive, when we estimate the effects of reducing misallocation on aggregate productivity in Section 6, we may be underestimating the aggregate productivity gains from the policy.

Table 4: Effect of Foreign Capital Liberalization on TFPQ

Dependent Variable	**TFPQ**	**TFPQ**
	(1)	(2)
$Reform_{jt}$	0.241	0.220
	(0.180)	(0.146)
$Reform_{jt} \times I_i^{High\ MRPK}$		0.035
		(0.063)
Fixed Effects		
Firm	✓	✓
Firm Age	✓	✓
Size ×Year	✓	✓
Observations	46,765	46,765

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 4-digit industry median. MRPK is estimated with the $Revenue/K$ method. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. TFPQ is measured by estimating revenue production functions using the methodology of Levinsohn and Petrin (2003) and subtracting log average price from log TFPR. Standard errors are two-way clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Importance of the local banking market. Our results so far show that opening-up to foreign capital allows high MRPK 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 reform. We directly test this hypothesis by creating a variable $Financial\ Development_s$, defined as the log average over 1995–2000 of all bank credit in state s. We then interact this measure with all the single and cross-terms in equation (5). The variable is demeaned to restore the baseline effect on $Reform_{jt} \times I_i^{High\ MRPK}$. The coefficient of interest is the coefficient for the triple interaction $Reform_{jt} \times I_i^{High\ MRPK} \times Financial\ Development_s$, which captures the differential effect of the policy on high MRPK firms located in more developed local banking markets.

Table 5 reports the results.[29] For capital and wages, the interaction $I_i^{High\ MRPK} \times Reform_{jt} \times Financial\ Development_s$ is negative and significant at the 1% level. For MRPK, the triple interaction is positive and significant. Taken together, these results imply that capital wedges fell more following the reform for high MRPK firms located in less financially developed states.

In addition to being statistically significant, the magnitudes of the hetero-

29. The sample sizes are somewhat reduced from Table 3 since state information is not available for all firms.

Table 5: Heterogeneity by Local Financial Development

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
$Reform_{jt} \times {I_{jt}}^{High\ MRPK} \times Financial\ Development_s$	-0.0752	-0.258***	-0.176***	0.184***
	(0.0703)	(0.0819)	(0.0592)	(0.0378)
$Reform_{jt} \times {I_{jt}}^{High\ MRPK}$	0.207**	0.546***	0.232***	-0.378***
	(0.0793)	(0.0823)	(0.0575)	(0.115)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	57,435	59,788	58,480	56,005

All dependent variables are in logs. $Reform_{jt}$ is an indicator variable equal to one if the industry has 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 4-digit industry median. MRPK is calculated using the $Revenue/K$ method. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Local financial development is proxied using the log average amount of bank credit in the state in the pre–treatment period. All double and single interactions of the triple-differences specification are included in the regressions. Standard errors are two-way clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

geneous effects are economically meaningful. If we focus on the change in the marginal revenue products of capital (column 4), ex-ante high MRPK firms whose state is at the 25^{th} percentile of the bank credit distribution experience a decrease in MRPK of 51% $(-0.38 + (0.18 \times -0.71))$. In contrast, high MRPK firms whose state is at the 75^{th} percentile of the bank credit distribution experience a decrease in MRPK of 13% $(-0.38+(0.18\times 1.37))$. Thus, the reduction at the 25^{th} percentile is roughly four times larger than the one at the 75^{th} percentile.

The fact that the effects of the policy are smaller in states where credit constraints were a priori lower further suggests that opening up to foreign capital relaxed credit constraints and allowed previously constrained firms to invest more. Moroever, it provides evidence that under-developed domestic banking markets are an important source of misallocation in India (consistent with Krueger et al., 2002) and that foreign capital can act as a substitute.

5.3 Product Outcomes

We next estimate the effects of the policy on product-level outcomes, including prices and output. Opening-up to foreign capital can reduce prices for two reasons. If liberalization reduced the wedges on capital for high MRPK firms, these firms'

Table 6: Effect of Foreign Capital Liberalization on Product Outcomes

Dependent Variable	Price		Output		Log(# Products)	Pr(Addition)	Pr(Deletion)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Reform_{jt}$	-0.181***	-0.075	0.222***	0.033	0.007	-0.057**	0.011
	(0.033)	(0.049)	(0.060)	(0.094)	(0.021)	(0.023)	(0.030)
$Reform_{jt} \times {I_i}^{High\ MRPK}$		-0.154**		0.273**	0.022*	0.083***	-0.074*
		(0.066)		(0.122)	(0.011)	(0.028)	(0.041)
Fixed Effects							
Firm	✓	✓	✓	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓	✓	✓	✓
Firm × Product	✓	✓	✓	✓	—	—	—
Observations	108,046	108,046	109,059	109,059	34,863	34,863	34,863

In columns 1-4, each observation is at the firm-product-year level. In columns 5-7, each observation is at the firm-year level. Firms are classified as high MRPK if their average MRPK in the pre-treatment period from 1995–2000 is above the 4-digit industry median. MRPK is calculated using the $Revenue/K$ method. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are two-way clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

marginal costs would fall. Lower marginal costs may be passed on to consumers in the form of lower prices. In addition, by allowing high MRPK 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.

Using product-level data on prices and output, we use the same identification strategy as before but now control for product-firm fixed effects. With these fixed effects, the regressions are identified by changes in prices or output for a given product produced by a firm. Thus, the results are not biased by the addition or the deletion of products. Columns 1–2 of Table 6 report the results. On average, the reform reduces prices by 18% (column 1). Column 2 shows that the reduction is mainly driven by high MRPK firms, who reduce their prices (in total) by more than 20%.

We also test whether the increase in revenues caused by the reform is accompanied by a product-level increase in output. An increase in output for high MRPK firms does not need to occur mechanically in the data, since the results we have shown previously are for firm-level sales. Separately reported unit-level sales and prices are used to calculate output. Columns 3–4 of Table 6 report the effect of the reform on product-level output, which increases by 22% on average. The average effect masks considerable heterogeneity: high MRPK firms increased output by 27% relative to low MRPK firms, while low MRPK firms' output does not change.

In the last three columns of Table 6, we examine whether the policy affected the product portfolio of treated firms. Column 5 indicates that the number of products offered increased for high MRPK firms but not low MRPK firms. Low MRPK firms were less likely to add new products (column 6) but not more likely to delete products (column 7). High MRPK firms, on the other hand, were relatively more likely to offer new products and (marginally significantly) less likely to delete products. Altogether, these results are consistent with the initially high MRPK firms expanding into new areas, crowding out expansions by low MRPK firms.

5.4 Robustness of Firm-level Results

In this subsection, we report a variety of robustness tests. We show that our results are not driven by mean reversion and that they are robust to the inclusion of alternative sets of controls, accounting for other Indian policies and cross-industry spillovers, and winsorizing variables to reduce measurement error.

Mean Reversion. We provide several additional pieces of evidence that our results are not driven by mean reversion. First, in Appendix Figure A2, we plot the event study graphs using only variation from the later, 2006 reform. Since high and low MRPK status are assigned using data from 1995–2000, if mean reversion is driving the results, we would expect to see the effects appear before 2006 (normalized to be year 1 in the graph). Instead, the timing of the effects lines up with the timing of the reform.

Second, in Appendix Table A3 we show that the results are robust to assigning high MRPK status using a shorter pre-treatment period (1995-1997 in Panel A and 1995-1998 in Panel B) or using only variation from the 2006 reform (Panel C). In all three cases, the years directly before the reform are not used to assign high MRPK status, so these results should be less affected by any mean reversion. In all three panels, we see that the estimates are similar to the baseline results in Table 3.

Differential industry-level time-varying shocks. We further explore whether β_2 is robust to differential time trends by controlling for 5-digit industry-year fixed effects in equation (5). This non-parametrically accounts for 5-digit industry-level unobserved, time-varying shocks and only exploits *within*-industry changes in firms' outcomes. $Reform_{jt}$ is therefore subsumed by the fixed effects. Appendix Table A4 reports the results and shows that the estimates of β_2 are very similar.

Because the estimation of the coefficient on $Reform_{jt}$ will be important when we compute the aggregate effect of the policy, we also show in Appendix Table A5 that the point estimates for $Reform_{jt}$ and $Reform_{jt} \times I_i^{High\ MRPK}$ are robust to the inclusion of 2-digit industry-year fixed effects.[30] These fixed effects force the coefficients to be estimated by solely comparing firms in the same 2-digit industry, in the same year, which 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.

Accounting for state-year fixed effects. To account for the possibility that some Indian states are more exposed to the reform due to their industrial composition and may have instituted policies affecting misallocation or were affected by shocks concurrent with the reform, we flexibly control for state-level, time-varying, unobserved shocks. In Appendix Table A6, we include state-year fixed effects in our main specifications. The estimates are therefore identified by comparing firms in the same state and the same year. The inclusion of these controls has little effect on the magnitude of our estimates.

Controlling for reservation laws. Starting in 1967, the government implemented a policy of reserving certain products for exclusive manufacture by small-scale industry (SSI) firms in order to boost their development. By the end of 1978, more than 800 products had been reserved. In 1996, it was more than a thousand. After the wave of deregulation in the early 1990s, the Indian government decided to remove most of these protective laws, and from 1997 to 2008, the government dereserved almost all products. The consensus is that dereservation led to more entry, higher output, and greater efficiency for deregulated industries.[31]

Because part of the dereservation happened during our sample period, we check that our results are robust to accounting for this deregulation. To do so, we use the list of deregulated industries in ASICC from Boehm, Dhingra, and Morrow (2019) and create a crosswalk between ASICC and our definition of industry (NIC 2008) by using the ASI 2008–2009.[32]

To assess whether dereservation could be driving our results, we perform two

30. There are 23 distinct 2-digit industries.

31. See Garcia-Santana and Pijoan-Mas (2014), Martin, Nataraj, and Harrison (2017), Boehm, Dhingra, and Morrow (2019), and Rotemberg (2019) for a detailed description of the laws and their consequences.

32. We would like to thank the authors for generously sharing their data with us. For each establishment in the ASI, the data reports both the NIC code of the establishment and the list of all the products sold at the ASICC level. We compute a one to one mapping by assigning to each NIC the ASICC with the highest share of products sold.

tests, both reported in Appendix Table A7. In the odd columns, we exclude all 5-digit NIC industries that contained a product that was affected by a dereservation reform after 2000 (the year before our first episode of liberalization). Because this cuts our sample by more than half, in even columns, we create an indicator variable $Dereservation_{jt}$ that is equal to one after industry j has been dereserved and control for it and its interaction with $I_i^{High\,MRPK}$. In both cases, the pattern of the point estimates is largely unchanged.

Controlling for trade liberalization. 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 operated and allowed them to access a broader set of inputs at a lower price (Topalova and Khandelwal, 2011; Goldberg, Khandelwal, Pavcnik, and Topalova, 2010; De Loecker, Goldberg, Khandelwal, and Pavcnik, 2016). If trade liberalization occurred in similar industries to the foreign financial liberalization, 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 is possible that trade liberalization had a differential effect on high and low MRPK firms. To account for this, we compute input and output tariffs from 1995-2010 — the period for which tariff data is available — following Goldberg, Khandelwal, Pavcnik, and Topalova (2010) and assume tariffs remained constant for the period 2010-2015.[33] Input tariff measures are obtained by computing the weighted sum of the percent tariffs on each input used to produce a product based on the Indian input-output table. We then include both the tariff measure and its interaction with $I_i^{High\,MRPK}$ as controls in our main regression specification.

Appendix Table A8 reports the results when we control for the output tariffs only (the odd columns) or both the output and input tariffs (the even columns). For our key outcomes, capital and MRPK, the effect of the foreign capital liberalization on high MRPK firms remains virtually unchanged.

Winsorizing outliers. We directly test the extent to which our results might be driven by outliers by winsorizing the data at the 5% level. We identify outliers either across industries or within each 2-digit industry. We report the results in Appendix Table A9 and show that the point estimates are similar to those without a measurement error correction.

33. We would like to thank Johanes Boehm for generously sharing his tariff measure with us.

Firm entry and exit. To test whether differential attrition could affect our results, we directly test whether the policy affected firm exit and entry using industry-level variation in the policy over time. 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 and use the last year in the dataset as a proxy for exit.[34] 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-year cell that exited or entered. To estimate the differential effect on exit for high and low MRPK firms, we create these counts for industry-year-MRPK category cells. We cannot use the same strategy to test for differential entry, since, if a firm enters after 2000, we do not observe its MRPK during the pre-treatment period. Appendix Table A10 reports the results. We find little evidence that the policy affected entry and exit.[35]

Spillovers. Cross-industry spillovers through input-output linkages across treated and non-treated industries could bias our estimates if they lead the policy to affect the outcomes of firms in non-liberalized industries.

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

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

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 the 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 $\hat{a}_{ij} \equiv$

34. 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.

35. 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 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.

$\frac{Sales_{i\to j}}{Sales_i} = a_{ji}\frac{Sales_j}{Sales_i}$ 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.

Appendix Table A11 reports the results for the average effect of the policy, controlling for the upstream and downstream effects, and shows that they are unchanged. Appendix Table A12 reports the estimates of the heterogeneous effects of the policy, again controlling for spillovers. The estimates are again very similar to those that do not account for spillovers.

Minimal controls. We next show that our results are robust to including only firm and year fixed effects and removing all additional controls. Appendix Table A13 reports the results and shows that our key estimates are again quite similar.

Heterogeneous effects by firm risk. One potential explanation for our estimates is that firms with high ex-ante MRPK were relatively riskier for domestic lenders since they were more correlated with the Indian economy. Then, after the reform, foreign capital flowed to these firms (e.g. Liu, Wei, and Zhou, 2020). Indeed, our misallocation framework nests this possibility, since wedges may represent the constraints that kept foreign lenders from lending to risky firms. To test this hypothesis, in Appendix Table A14, we calculate firms' pre-treatment correlation between revenue growth and the Indian economy and control for this measure interacted with the reform. The reform did not have a differential effect on ex-ante riskier firms, and controlling for this relationship has little impact on our estimates.

5.5 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 capital stock) 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 that we test in this section.

Although labor is often modelled as a fully adjustable variable input across periods,[36] in reality, labor is likely to have a fixed-cost component due to wage

36. For example, Olley and Pakes (1996) model labor as a flexible, variable input, while modeling capital as a stock that requires adjustment.

Table 7: Effect of Foreign Capital Liberalization by Firms' Ex-ante MRPL

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPL**
	(1)	(2)	(3)	(4)
$Reform_{jt} \times I_i^{High\ MRPL}$	-0.037	0.230*	0.285***	-0.322***
	(0.093)	(0.116)	(0.076)	(0.070)
$Reform_{jt}$	0.139	0.116	0.014	0.125
	(0.099)	(0.068)	(0.096)	(0.089)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	46,064	46,030	46,064	46,064

All dependent variables are in logs. High MRPL firms are defined in an analoguous way to high MRPK firms using the $Revenue/L$ method. $Reform_{jt}$ is an indicator variable equal to one if the industry has liberalized access to the foreign capital market. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are two-way clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

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) and Fonseca and Doornik (2019) 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 reform. We classify high and low MRPL firms analogously to how we classify high and low MRPK firms and estimate the heterogeneous effects of the reform on high MRPL firms.

Table 7 reports the results. Following the reform, high MRPL firms relatively increase their total wage bill (column 3) by 29%. Among ex-ante high MRPL firms, MRPL decreases by 32% relative to low MRPL firms (column 4). By allowing high MRPL firms to grow faster and to expand employment, the deregulation appears to have reduced labor misallocation.

6 Aggregate Effects

While our reduced-form estimates show that misallocation fell, they do not tell us whether this had economically meaningful effects on output growth. To measure the policies' aggregate effects, we now estimate bounds for the effect of the reduction in misallocation on treated industries' Solow residual, a proxy for aggregate productivity, using equation (3). Equation (3) is re-stated below:

$$\Delta Solow_{I,t} \approx \sum_{i \in I} \lambda_i \Delta \log A_i + \sum_{\substack{i \in I \\ x \in \{K,L,M\}}} \lambda_i \alpha_i^x \frac{\tau_i^x}{1+\tau_i^x} \Delta \log x_i$$

where λ_i is the ratio of firm i's sales to treated industry I's net output, $\Delta \log A_i$ is the change in TFPQ, α_i^x is the output elasticity with respect to x, τ_i^x is the level of firm-specific input wedges prior to the policy change, and $\Delta \log x_i$ is the change in the log input x consumed by firm i.

6.1 Identification

Equation (3) shows that the Solow residual can increase for two reasons: (1) individual firms become more productive (within-firm productivity) or (2) inputs either increase for producers with positive wedges or decrease for producers with negative wedges (firm-level inputs). We discuss each part in turn.

Within-firm productivity. The contribution of the change in within-firm productivity to the Solow residual is given by $\sum_{i \in I} \lambda_i \Delta \log A_i$. Since we do not observe a significant effect of the policy on TFPQ in the difference-in-differences regressions (see Table 4), we set $\Delta \log A_i = 0$.[37]

Firm-level inputs. The contribution of changing firm-level inputs to the Solow residual is given by:

$$\Delta Solow_{I,t} = \sum_{\substack{i \in I \\ x \in \{K,L,M\}}} \lambda_i \alpha_i^x \frac{\tau_i^x}{1+\tau_i^x} \Delta \log x_i. \tag{6}$$

Note that an increase in inputs for some firms does not need to mechanically increase the Solow residual. The wedge τ_i^x can be negative for firms whose capital

37. To the extent that our estimated effect on TFPQ, while insignificant, is positive, by setting A_i to zero, we may underestimate the policy's effect on the Solow residual.

is subsidized, zero if there is no distortion, or positive for firms that face distortions in accessing capital. An increase in inputs if there is no misallocation will have no effect on the Solow residual, since, in that case, τ_i would be zero for all firms. Similarly, if the policy increases misallocation by increasing inputs x_i for firms with negative wedges, the contribution to the Solow residual would be negative even though inputs increase.

To estimate (6), observe that most components of this expression are readily identifiable in the data or given by our estimates from the natural experiment. The sales shares of net output λ_i can be computed using input-output data,[38] and α_i^x can be estimated using the production function estimation. Under the standard difference-in-differences assumption that untreated industries are unaffected by the policy, $\Delta \log x_i$ can be predicted from difference-in-differences regressions with heterogeneous effects where log usage of each input is the outcome variable.[39]

We next turn to identifying τ_i^x. Equation (6) highlights that errors in the estimation of τ_i^x, the level of firm-specific input wedges prior to the policy change, can bias the aggregate policy effects, as $\frac{\tau_i^x}{1+\tau_i^x}$ is multiplicative with $\Delta \log x_i$ and increasing in τ_i^x. If we use cross-sectional variation prior to the policy change to identify τ_i^x, measurement error could lead to greater dispersion in these values. Since we have shown in Section 5 that the reform has a positive effect on inputs for firms with relatively greater wedges, inflated values of $\frac{\tau_i^x}{1+\tau_i^x}$ would be multiplied by the positive predicted change in inputs.

To circumvent this potential bias, we generate lower and upper bound estimates of the policies' effect. We first note that if the policy strictly reduces misallocation, then the aggregate effects are strictly increasing in τ_i^x (as discussed above). Then, a lower bound for τ_i^x generates a lower bound aggregate effect, while an upper bound for τ_i^x generates an upper bound aggregate effect. To generate an upper bound, we can attribute *all* observed pre-treatment deviations from efficiency to wedges. As we will see, under the assumption that the policy strictly reduced misallocation, we can also generate a lower bound value for τ_i^x. While this assumption may not always be reasonable, our reduced-form empirical results, which show that the policy causally reduced MRPK and MRPL for firms that had ex-ante above median values of MRPK and MRPL, provide strong evidence in its favor.

38. To measure total sales by sector I not re-used by firms in I as inputs, we sum over treated firms' total sales in 2000 (the last pre-treatment year). We then use information from the Annual Survey of Industries to compute the share of output that is re-used by the treated industries as inputs and scale total sales by 1 minus this value. Finally, λ_i is calculated for a firm i by dividing a firm i's sales by this value.

39. The difference-in-differences assumption could be partially relaxed by modeling spillovers explicitly and estimating spillovers effects.

Identifying & estimating the lower bound of τ_i^x. By definition, the post-policy wedge for a firm is always 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 and i is suppressed to simplify notation. Under the assumption of no increase in misallocation after the policy (i.e. the policy does not subsidize firms), $\tau^x_{post} \geq 0$, which implies that $\min_{\tau^x_{post}\geq 0} \tau^x_{pre} = -\Delta\tau^x$. Thus, the minimum possible pre-treatment wedge is given by the scenario where, after the policy change, the industry is 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 misspecficiation as opposed to misallocation. So estimating $\Delta\tau^x$ gives us a lower bound estimate of τ^x_{pre}, and we can apply equation (6) to estimate a lower bound of the first order effects of the policy on the Solow residual.

Since the minimum values of the pre-reform wedges τ^x are given by the change in the wedges due to the policy, and since in our formula, wedges vary at the firm level, we can predict the minimum firm-specific wedges with a difference-in-differences regression with heterogeneous effects where the outcome variable is the marginal revenue product of input x. For example, in the case of τ_i^k, we estimate

$$\log MRPK_{ijt} = g(Reform_{jt}, \mathbf{C}_{it}) + \mathbf{\Gamma X_{it}} + \alpha_i + \delta_t + \epsilon_{ijt} \tag{7}$$

where $g(Reform_{jt}, \mathbf{C}_{it})$ is a flexible function of $Reform_{jt}$ and firm characteristics $\mathbf{C}_{it}$. Since we focus on within-industry changes in allocation, allowing the effect of $Reform_{jt}$ to depend on firm characteristics is important, as it allows our estimates of τ_i^k to vary within an industry j. As shown in Appendix B, if the policy completely eliminated misallocation, $\hat{g}(1, \mathbf{C_{it}})$ is an unbiased predictor of $\log(1+\tau_i^k)$. Then, τ_i^k can be estimated by computing $\widehat{\tau}_i^k = e^{\hat{g}_i(1,\mathbf{C}_{it})} - 1$. An analogous process can be used to estimate the wedges on labor. Appendix C discusses more generally the settings where this methodology can be applied to estimate lower bound aggregate effects with natural experiments.

As discussed in Section 4.2, estimating the change in wedges using a difference-in-differences specification is less sensitive to the issues that occur when cross-sectional data are used to estimate distortions. To the extent that firms' measurement error is time-invariant over the period of our experiment, it will be differentiated out by the firm fixed effects α_i. Time-varying macro-economic shocks or economy-wide changes in markups or the costs of inputs will be absorbed by year fixed effects, as well as any year fixed effects interacted with firm characteristics. Additionally, the effects of time-varying shocks to marginal revenue products, such as productivity shocks, even if they are not economy-wide, will *not* be attributed to the reform, as long as the standard difference-in-differences assumption holds, and they are uncorrelated with $Reform_{jt}$ conditional on the controls. Appendix Table A9 provides further evidence that these estimates are not highly sensitive to measurement error.

In practice, since we observe larger effects on inputs and marginal revenue products for firms with ex-ante higher marginal revenue products, we specify $g(Reform_{jt}, \mathbf{C}_{it})$ to allow for heterogeneous effects for firms with above median pre-treatment values of MRPK and MRPL.[40] We use analogous regression specifications to estimate the change in inputs due to the policy. Appendix Table A15 reports the results of the regressions used to identify both the change in wedges and the change of inputs. Following the identifying assumption in the production function estimation used to identify TFPQ, we assume that materials are not misallocated ($\tau_i^m = 0$ for all i).[41]

Identifying & estimating the upper bound of τ_i^x. For our upper bound measure of τ_i^x, we attribute all the pre-treatment, cross-sectional observed deviations from efficiency in the data to misallocation. While we observe pre-treatment measures of MRPK directly, $MRPK = (1 + \tau_i^x)p_i^x$. Since p_i^x is not observable, observing MRPK alone does not directly identify the level of the wedges. Instead, to identify the levels of the wedges, we use the relationships $\tau_i^K = \alpha_i^K \frac{p_i y_i}{rK_i} - 1$ and $\tau_i^L = \alpha_i^L \frac{p_i y_i}{wL_i} - 1$, where r is the rental rate of capital and wL_i is the wage bill.[42] The wage bill wL_i and sales $p_i y_i$ are observable in the last pre-treatment year (2000) in Prowess, and as before, α_i^K and α_i^L are given by production function estimation. For the rental rate of capital r, we follow Hsieh and Klenow (2009)

40. For example, for the marginal revenue product of capital, we estimate

$$\begin{aligned}\log MRPK_{ijt} =& \beta_1 Reform_{jt} + \beta_2 Reform_{jt} \times I_i^{High\ MRPK} + \beta_3 Reform_{jt} \times I_i^{High\ MRPL} \\ &+ \mathbf{\Gamma X_{it}} + \alpha_i + \delta_t + \epsilon_{ijt}.\end{aligned}$$

We can then predict $\widehat{\tau}_i^k$ by computing:

$$\widehat{\log(1 + \tau_i^k)} = \hat{\beta}_1 Reform_j + \hat{\beta}_2 Reform_j \times I_i^{High\ MRPK} + \hat{\beta}_3 Reform_j \times I_i^{High\ MRPL},$$

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

41. In practice, relaxing this assumption and calculating wedges and changes in inputs for materials the same way we do for capital and labor has a small but positive effect on the estimated change in the Solow residual.

42. These relationships come from firms' cost-minimization problems. A cost-minimizing firm sets $\frac{rK(1+\tau_i^K)}{p_i y_i} = \alpha_i^K$ and $\frac{wL_i(1+\tau_i^L)}{p_i y_i} = \alpha_i^L$. An efficient firm sets $\frac{rK}{p_i y_i} = \alpha_i^K$ and $\frac{wL_i}{p_i y_i} = \alpha_i^L$, so the wedges are the taxes or subsidies that would make firms' decisions appear efficient.

and set $r = 10\%$. In practice, since the capital wedges are decreasing in r, the estimated aggregate effect will also be decreasing in r. Thus, our choice of a relatively low value of $r = 10\%$ for India is consistent with our goal of calculating an upper bound.

6.2 Results

Bounds on the first order approximation. Having estimated all the components of equation (3), we calculate that the lower bound increase in the treated industries' Solow residual is 3.7% (see row 1 of Table 8), and the upper bound is 16.9% (row 2).

Comparison with alternative estimates. For robustness, we next compare this lower bound estimate to several other estimates of the aggregate effect of the policy. In row 3, we combine the CES model of Hsieh and Klenow (2009), which translates changes in the log variance of TFPR into changes in aggregate TFP, with our reduced-form estimates.[43] This allows us to obtain a direct estimate of the aggregate effects of the policy rather than a first order approximation, at the cost of stronger assumptions about firm's production functions, demand, and the distribution of wedges. This model indicates that treated industries' aggregate TFP increased by 8.6%.

We next consider the cumulative effect of the reforms. The estimates of the reforms' effects over time in Figure 2 suggest that the effects on inputs and the wedges grew over time. Thus, using estimates from a standard difference-in-differences that assumes constant treatment effects over time may lead row 1 of Table 8 to underestimate the long-run effects of the policies. Since the effects plateau after 5 years in Figure 2, we re-calculate the lower bound approximation using the estimated policy effect five years after the reforms. This yields a larger estimate (6.7%, row 4).

Since the first order approximation may not be a good approximation if there are important higher order effects of the policy on the Solow residual, we also construct a non-linear approximation of the policies' effects on the Solow residual by estimating policy effects year-by-year and chaining the results. Appendix D

43. In the model of Hsieh and Klenow (2009), $\log TFP = -\sigma/2Var(\log TFPR_i)$, where σ is assumed to be 3, and $\log(TFPR_i) = \alpha_i^K \log MRPK_i + \alpha_i^L \log MRPL_i$. We can use our estimates of $\log MRPK_i$ and $\log MRPL_i$ in 2000 to directly predict $\log TFPR_i$ and then estimate its variance. To get the post-treatment $\log TFPR_i$, we can simply use the regression estimates in Appendix Table A15 to predict the firm-level change in $\log MRPK_i$ and $\log MRPL_i$ and add the predicted $\Delta \log TFPR_i$ to the pre-treatment value of $\log TFPR_i$.

Table 8: Effects of Foreign Capital Market Liberalization on the Solow Residual of Treated Industries

	Increase in Solow Residual
Lower Bound	3.7%
Upper Bound	16.9%
Hsieh-Klenow Model	8.6%
Lower Bound Allowing for Cumulative Effects	6.7%
Non-Linear Approximation	6.5%

This table reports the estimates of the effect of the foreign capital liberalizations in 2001 and 2006 on treated industries' Solow residual. Rows 1, 2, and 4 use a first order approximation (equation (3)). Row 3 uses a model in the style of Hsieh and Klenow (2009), and the last row uses a non-linear approximation, described in Appendix D. The estimates are generated using the Prowess dataset. Rows 1, 4, and 5 identify the wedges by assuming the policy eliminated misallocation. Rows 2 and 3 use cross-sectional data to identify the baseline wedges.

describes this process. Since the non-linear approximation requires estimating dynamic policy effects over five years, it should be compared to the cumulative estimate in row 4. The non-linear approximation (6.5%, reported in row 5) is quite close to the simpler, cumulative first order approximation.

7 Conclusion

Exploiting within-country, within-industry, and cross-time variation, we show that foreign capital liberalization reduced the misallocation of capital and labor in India. In doing so, this paper addresses two of the key challenges faced by the misallocation literature. First, it provides direct evidence that policymakers can change allocative efficiency and productivity. Second, it develops new tools that can be combined with estimates from natural experiments to measure the aggregate effects of policies.

The liberalization, which allowed for the automatic approval of foreign investments and raised caps on foreign equity in the 2000's, increased capital in the treated industries. However, the effect of the liberalization on the average firm masks important heterogeneity. The entirety of the liberalization's effect on firms' outcomes is driven by increased investment in firms that previously had high MRPK (high sales to capital ratios). These firms' MRPK fell, indicating that the policy reduced misallocation. Thus, foreign capital liberalization can be an important tool for low-income countries to reduce capital market frictions.

Variation from a natural experiment also allows us to estimate aggregate effects of reducing misallocation that – unlike cross-country or time series comparisons – are less sensitive to measurement and model misspecification error. Aggregating our reduced-form estimates, we find that the policy had economically meaningful effects, increasing treated industries' Solow residual by between 4% and 17%.

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Online Appendix

Appendix A: Derivation of Aggregation Formula

In this section, we derive equation (3), the formula used to approximate the change in the Solow residual due to the policy. We start by defining

$$y_i = A_i f_i\left(\{y_{ij}\}_j\right),$$

where y_i is the output of firm i, A_i is firm i's productivity, f_i is the production function, and y_{ij} is the amount of input j used by firm i. Then, the total derivative of y_i is

$$d\log y_i = \sum_j \frac{\partial \log f_i}{\partial \log y_{ij}} d\log y_{ij} + d\log A_i. \tag{8}$$

A firm i solves the cost minimization problem

$$\mathcal{C}_i(p, y_i) = \sum_j p_j y_{ij} + \gamma_i(y_i - A_i f_i\left(\{y_{ij}\}_j\right), \tag{9}$$

where p is the vector of prices, p_j is the price of a good produced by j, and γ_i is the Lagrange multiplier. From the first order conditions of equation (9)

$$p_j = \gamma_i A_i \frac{\partial f_i}{\partial y_{ij}}. \tag{10}$$

Then,

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

where μ_i is the output wedge of i (price over marginal cost), implying that $\gamma_i = \frac{p_i}{\mu_i}$. Substituting this relationship into equation (10) shows that $p_j = \frac{p_i}{\mu_i} A_i \frac{\partial f_i}{\partial y_{ij}}$. 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 (8)) 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

$$\frac{1}{\mu_i}(d\log y_i - d\log A_i) - \sum_{j\notin I} \frac{p_j y_{ij}}{p_i y_i} d\log y_{ij} = \sum_{j\in I} \frac{p_j y_{ij}}{p_i y_i} d\log y_{ij}. \tag{11}$$

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 nominal industry-level output to be $PC = \sum_{i\in I} p_i c_i$, where $c_i = y_i - \sum_{j\in I} y_{ji}$. Then

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

The change in industry-level net output is defined by

$$d\log C = \sum_i \frac{p_i c_i}{PC} d\log c_i,$$

where after substitution, we get

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

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

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

Using equation (11), with a little algebra, we can rewrite this as

$$dSolow_I = \sum_{i\in I} \lambda_i (1 - \frac{1}{\mu_i})(d\log y_i - d\log A_i) + \sum_{i\in I} \lambda_i d\log A_i, \tag{12}$$

where $\lambda_i = \frac{p_i y_i}{PC}$. Now, we transform equation (12) to use input wedges instead of output wedges, so that it matches equation (3). This allows us to rewrite equation (12) in terms of firm-level capital, labor, and materials wedges where each firm-input combination is a "producer."[44] The wedge on firm i's input x is τ_i^x, and the

44. While equation (12) models wedges on output rather than inputs, this framework is general and input wedges can be thought of as a special case of this formulation. In particular, we can

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 for producer (x,i) is given by: $\mu_i^x = 1+\tau_i^x$. Then, for a given firm i, $d\log y_i - d\log A_i = \sum_{x\in\{K,L,M\}} \alpha_i^x d\log x_i$, where α_i^x is the output elasticity with respect to input x. So, we can rewrite equation (12) as:

$$dSolow_{I,t} = \sum_{i\in I} \lambda_i d\log A_i + \sum_{\substack{i\in I \\ x\in\{K,L,M\}}} \lambda_i \alpha_i^x \big(1 - \frac{1}{1+\tau_i^x}\big) d\log x_i,$$

which in turn simplifies to

$$dSolow_{I,t} = \sum_{i\in I} \lambda_i d\log A_i + \sum_{\substack{i\in I \\ x\in\{K,L,M\}}} \lambda_i \alpha_i^x \frac{\tau_i^x}{1+\tau_i^x} d\log x_i.$$

To implement the first order approximation, for any variable x, we use discrete changes Δx instead of infinitesimal changes dx. Then the first order approximation is given by

$$\Delta Solow_{I,t} \approx \sum_{i\in I} \lambda_i \Delta\log A_i + \sum_{\substack{i\in I \\ x\in\{K,L,M\}}} \lambda_i \alpha_i^x \frac{\tau_i^x}{1+\tau_i^x}\ \Delta\log x_i.$$

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.

Appendix B: Estimating the Distribution of the Minimum Wedges

In this appendix, we show that the difference-in-differences regressions with heterogeneous effects can be used to estimate the minimum wedge prior to the policy under the assumptions outlined in the main text. We focus here on estimating τ^k_{pre}, where the i subscript is surpressed for notational simplicity. The reasoning is identical for labor and materials.

Denote $mrpk_i$ the true marginal revenue product of capital of firm i (which is never observed) and $MRPK_i$ the marginal revenue product of capital observed in the data with measurement error, such that we have:

$$\log(MRPK_{it}) = \log(mrpk_{it}) + \mu_i + \eta_t + \epsilon_{it}$$

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 a disaggregated industry j. If a firm is in an industry that does not go through a reform ($Reform_{jt} = 0 \forall t$) or if the firm is in an industry that will be reformed but the reform has not taken place yet ($t < T_j$):

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

Under the assumption the policy has eliminated misallocation, if the firm is in an industry that is reformed and the reform has taken place, $Reform_{jt} = 1$, then $\tau^k_{it} = 0$ and

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

Hence, if $Reform_{jt} = 0$:

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

For firms where $Reform_{jt} = 1$:

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

Denote $g(Reform_{jt}, \mathbf{C_{it}})$ to be a function of the time-varying, reform indicator variable and time-varying firm-level characteristics $\mathbf{C_{it}}$. Then, the difference-in-differences regression estimates

$$\log(MRPK_{ijt}) = g(Reform_{jt}, \mathbf{C}_{it}) + \mathbf{\Gamma X_{it}} + \alpha_i + \delta_t + \epsilon_{it}.$$

In this regression, firm fixed effects absorb μ_i, as well as any time invariant industry shocks, and time fixed effects absorb η_t and $\log(p_t^k)$. Idiosyncratic shocks ϵ_{it} are independent of $Reform_{jt}$. Thus, $\hat{g}_i(1, \mathbf{C}_{it})$ is an unbiased estimator of $E(\log(1+\tau_{it}))$ over the pre-period and can be used to predict the average value of $\log(1 + \tau_i^k)$ over the pre-period.

Appendix C: Identifying Lower Bound Wedges and Inputs for Aggregation in Other Settings

In this appendix, we discuss how researchers can apply the aggregation methodology in Section 6 to estimates from a natural experiment or an experiment in a different setting. As described in Section 6, to apply the lower bound methodology, the researcher must make two key assumptions: (1) the reform only reduced misallocation, and (2) spillovers from the unit of treatment (e.g. industries, geographic entities) to untreated units are either nonexistent or can be measured using observable characteristics like input-output linkages or with the experimental design. In our context, the second assumption is the standard difference-in-differences assumption.

We consider two categories of settings where researchers may want to apply the aggregation methodology: (1) reductions in misallocation due to changes in the distribution of inputs *within* the treated group and (2) reductions in misallocation due to changes in inputs used by the treated group.

Changes in allocation within the treated group. The natural experiment studied in this paper falls into this category. In this case, there is a treatment at the unit j level, which can potentially refer to an industry or a geographic region but could also refer to the whole treated group of firms. If the researcher believes that the treatment reduced misallocation by reducing wedges for firms with high wedges in unit j and/or increasing wedges for firms with low wedges, she can apply a similar estimation strategy to the one used in Section 6 to estimate firm-level changes in inputs and wedges. To study cross-industry or cross-geography changes in misallocation, as opposed to the cross-firm changes in the same industry, as we do, the researcher can allow g to depend on industry or geographic unit-level characteristics as well as, or in place of, firm-level characteristics.

Changes in inputs for the treated group versus the control. In some cases, the design of a policy or an experiment may allow the researcher to assume that the policy/treatment reduced misallocation even if the researcher is not interested in changes in allocation within the treated group. For example, if a policy that improves access to inputs is targeted toward firms with ex-ante higher input wedges and increases input use for these firms, it may be reasonable for the researcher to assume the policy reduced misallocation. In this case, the researcher may not need to estimate heterogeneous treatment effects. The researcher could use the firm-level average treatment effect of the policy as an estimate of the wedges and changes in inputs *for the treated group.*

Appendix D: Non-Linear Approximation

This appendix describes how we calculate a non-linear approximation of the policies' effects on the treated industries' Solow residual. Following Baqaee and Farhi (2019), we note that a non-linear approximation of the effect of the policies on the Solow residual – given the shocks realized in the economy – is a Reimann sum over the first order approximations of the policies' effects each year. Then, the non-linear approximation of the culmulative effect at time T is

$$\Delta Solow_{I,T} \approx \sum_{t \leq T} \sum_{i \in I} \lambda_{it} \Delta \log A_{it} + \sum_{t \leq T} \sum_{\substack{i \in I \\ x \in \{K,L,M\}}} \lambda_{it} \; \alpha_i^x \; \frac{\tau_i^x}{1+\tau_i^x} \; \Delta \log x_{it}, \quad (13)$$

where t indexes a year, and the summation begins in the year of the first policy change. As before, since we did not find the policy had a significant effect on TFPQ (see Table 4), we set $\Delta \log A_{it} = 0$, causing the first term of equation (13) to drop out. We calculate λ_{it} exactly as we did in Section 6, except that we now calculate a separate value for each year, instead of only using the Prowess data from 2000. Similarly, the output elasticities are still given by the production function estimates.

To arrive at a time-varying estimate of the policies' effects on inputs, we use more flexible regressions specifications. For capital, we estimate

$$\begin{aligned} \log K_{ijtd} = & \sum_{s=1}^{5} \beta_{1,s} I_{jt}^{s \geq d} + \beta_{2,s} I_{jt}^{s \geq d} \times I_i^{High\, MRPK} + \beta_{3,s} I_{jt}^{s \geq d} \times I_i^{High\, MRPL} \\ & + \mathbf{\Gamma X_{it}} + \alpha_i + \delta_t + \epsilon_{ijt}, \end{aligned} \quad (14)$$

where d indexes the number of years since a reform occurred in industry j, and $I_{jt}^{s \geq d}$ in an indicator variable equal to 1 if it has been more than s years since a reform occurred in industry j. Therefore, $\beta_{1,s}$ captures the change in capital that occurs due to the reform between $s-1$ years after the reform and s years after the reform, and $\beta_{2,s}$ and $\beta_{3,s}$ allow these changes to be heterogeneous for high MRPK and MRPL firms. We allow effects to vary up to 5 years after the policies took place since the effects of the policies appear to plateau after five years (see Figures 1 and 2). Then, to estimate the firm-level change in capital due to the policy in year t, we calculate

$$\widehat{\log K_{ijtd}} = \sum_{s=1}^{5} \hat{\beta}_{1,s} I_{jt}^{d=s} + \hat{\beta}_{2,s} I_{jt}^{d=s} \times I_i^{High\, MRPK} + \hat{\beta}_{3,s} I_{jt}^{d=s} \times I_i^{High\, MRPL},$$

where $I_{jt}^{d=s}$ is an indicator variable equal to 1 if it is s years after an event in industry j and time t. We use an analogous approach to estimate the change in labor by year.

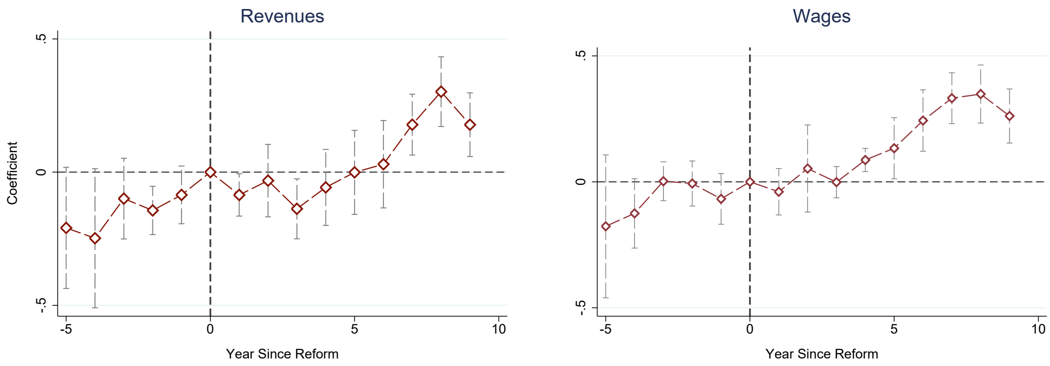
To estimate the baseline wedges in each year, we replace the outcome variable in equation (14) with $\log MRPK_{ijt}$ and $\log MRPL_{ijt}$. Then, under the lower bound identifying assumption that the policy eliminated misallocation, the wedge at time t is the sum of the estimated changes in the wedges that occurred between t and T. For the wedge on capital, after estimating equation (14) with $\log MRPK$ as the outcome variable, this is given by

$$\log \widehat{MRP}K_{ijtd} = \sum_{n=t}^{T} \sum_{s=1}^{5} \hat{\beta}_{1,s} I_{jn}^{d=s} + \hat{\beta}_{2,s} I_{jn}^{d=s} \times I_i^{High\, MRPK} + \hat{\beta}_{3,s} I_{jn}^{d=s} \times I_i^{High\, MRPL}.$$

The method for identifying the time-varying wedges for labor is analogous.

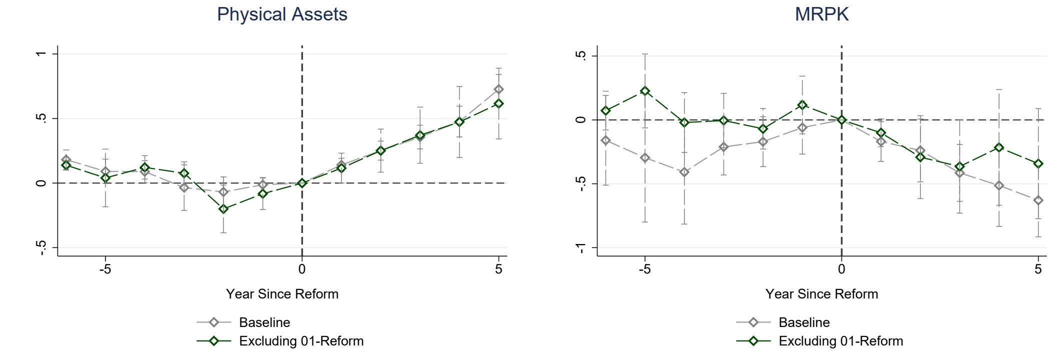
Appendix Figures

Figure A1: Event Study Graphs for the Relative Effect of Foreign Capital Liberalization on High MRPK Firms for Revenues and Wages



This figure reports event study graphs for the relative effects of the liberalization on firms with high pre-treatment MRPK relative to those with low pre-treatment MRPK. Year 0 is normalized to be the year before a reform. All dependent variables are in logs. The confidence intervals are at the 95% level.

Figure A2: Event Study Graphs Excluding the 2001 Reform



This figure reports event study graphs for the relative effects of the liberalization on firms with high pre-treatment MRPK relative to those with low pre-treatment MRPK excluding variation from the 2001 reform, so that year 1 is normalized to be 2006 for the green lines. All dependent variables are in logs. Year 0 is normalized to be the year before a reform. The confidence intervals are at the 95% level.

Appendix Tables

Table A1: List of Industries Affected by the 2001 and 2006 Reforms

NIC 5-Digit Industry Classification	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: Composition of Change in Capital

Dependent Variable	**Land**	**Plants and Equipment**	**Infrastructure**	**Other**
	(1)	(2)	(3)	(4)
$Reform_{jt} \times I_i^{High\ MRPK}$	-0.026***	0.035***	-0.002	-0.007
	(0.007)	(0.009)	(0.003)	(0.010)
$Reform_{jt}$	0.004	0.002	-0.002	-0.004
	(0.012)	(0.009)	(0.004)	(0.008)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	64,406	64,406	64,406	64,406

This table reports estimates of the heterogeneous effects of foreign capital liberalization reforms on high and low MRPK firms in the Prowess dataset (equation (5)). All dependent variables are the share of capital in a category. Firms are observed between 1995 and 2015. Firms are classified as high MRPK if their average MRPK in the pre-treatment period from 1995-2000 is above the 4-digit industry median. MRPK is calculated as $Revenue/K$. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are two-way clustered at the 4-digit industry and year level. *, **, and *** denote 10, 5, and 1% statistical significance respectively.

Table A3: Heterogeneous Effects of Foreign Capital Liberalization and Mean Reversion

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
	Panel A: 1995-1997 Pre-treatment Period			
$Reform_{jt} \times I_i^{High\ MRPK}$	0.123**	0.408***	0.030	-0.290***
	(0.048)	(0.088)	(0.054)	(0.066)
$Reform_{jt}$	0.102	0.157	0.213*	-0.038
	(0.092)	(0.105)	(0.112)	(0.095)
Observations	49,791	51,788	50,660	48,581
	Panel B: 1995-1998 Pre-treatment Period			
$Reform_{jt} \times I_i^{High\ MRPK}$	0.148**	0.451***	0.187***	-0.341***
	(0.066)	(0.076)	(0.058)	(0.084)
$Reform_{jt}$	0.059	0.086	0.102	0.008
	(0.102)	(0.101)	(0.099)	(0.115)
Observations	54,475	56,647	55,414	53,173
	Panel C: Only 2006 Reform			
$Reform_{jt} \times I_i^{High\ MRPK}$	0.159	0.462***	0.211**	-0.352**
	(0.117)	(0.074)	(0.081)	(0.170)
$Reform_{jt}$	0.001	-0.079	-0.028	0.168
	(0.195)	(0.105)	(0.116)	(0.147)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	62,924	65,393	63,999	61,342

This table provides evidence that the results in Table 3 are not driven by mean reversion. Firms are classified as high MRPK if their average MRPK in a pre-treatment period is above the 4-digit industry median. In Panel A, the pre-treatment period is defined as 1995-1997. In Panel B, it is 1995-1998. In Panel C, the pre-treatment period is 1995-2000, but $Reform_{jt}$ is only coded as 1 for industries that have been treated by the 2006 reform. In Panel C, the regressions control separately for being treated by the 2001 reform and its interaction with high MRPK. MRPK is calculated as $Revenue/K$. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are two-way clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table A4: Robustness: 5-Digit Industry-by-Year Fixed Effects

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
$Reform_{jt} \times {I_i}^{High\ MRPK}$	0.397***	0.682***	0.381***	-0.306***
	(0.062)	(0.063)	(0.059)	(0.097)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
5-digit Industry × Year	✓	✓	✓	✓
Observations	62,924	65,393	63,999	61,342

This table reports estimates of the heterogeneous effects of the liberalization reforms on high MRPK firms in the Prowess data set (equation (5)). All dependent variables are in logs. Firms are observed between 1995 and 2015. Firms are classified as high MRPK if their average MRPK in the pre-treatment period from 1995-2000 is above the 4-digit industry median. MRPK is calculated as $Revenue/K$. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are two-way clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table A5: Robustness: Inclusion of 2-Digit Industry-by-Year FE

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
$Reform_{jt} \times {I_i}^{High\ MRPK}$	0.261***	0.583***	0.275***	-0.340***
	(0.064)	(0.057)	(0.053)	(0.097)
$Reform_{jt}$	-0.141	-0.141	-0.099	0.051
	(0.124)	(0.111)	(0.076)	(0.123)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size $\times$Year	✓	✓	✓	✓
2-digit Industry $\times$Year	✓	✓	✓	✓
Observations	62,924	65,393	63,999	61,342

This table reports estimates of the heterogeneous effects of foreign capital liberalization reforms on high and low MRPK firms in the Prowess dataset (equation (5)). All dependent variables are in logs. Firms are observed between 1995 and 2015. Firms are classified as high MRPK if their average MRPK in the pre-treatment period from 1995-2000 is above the 4-digit industry median. MRPK is calculated as $Revenue/K$. Size$\times$Year are quartile fixed effects for firm's average pre-treatment capital interacted with year fixed effects. Standard errors are two-way clustered at the 4-digit industry and year level. *, **, and *** denote 10, 5, and 1% statistical significance respectively.

Table A6: Robustness: Inclusion of State-by-Year FE

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
$Reform_{jt} \times I_i^{High\ MRPK}$	0.231***	0.521***	0.241***	-0.329***
	(0.082)	(0.064)	(0.065)	(0.105)
$Reform_{jt}$	-0.030	0.031	0.042	-0.016
	(0.123)	(0.077)	(0.093)	(0.114)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size $\times$Year	✓	✓	✓	✓
State $\times$ Year	✓	✓	✓	✓
Observations	62,871	65,340	63,961	61,290

This table reports estimates of the heterogeneous effects of foreign capital liberalization reforms on high and low MRPK firms in the Prowess dataset (equation (5)). All dependent variables are in logs. Firms are observed between 1995 and 2015. Firms are classified as high MRPK if their average MRPK in the pre-treatment period from 1995-2000 is above the 4-digit industry median. Size$\times$Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. MRPK is calculated as $Revenue/K$. Standard errors are two-way clustered at the 4-digit industry and year level. *, **, and *** denote 10, 5, and 1% statistical significance respectively.

Table A7: Robustness: Accounting for Dereservation

Dependent Variable	**Revenues**		**Capital**		**Wages**		**MRPK**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Reform_{jt} \times {I_i}^{High\ MRPK}$	0.287***	0.247***	0.680***	0.516***	0.329***	0.266***	-0.438***	-0.303**
	(0.066)	(0.071)	(0.034)	(0.109)	(0.062)	(0.058)	(0.076)	(0.131)
$Reform_{jt}$	0.050	-0.029	0.017	0.010	0.105	0.030	0.066	0.005
	(0.096)	(0.113)	(0.073)	(0.060)	(0.102)	(0.094)	(0.125)	(0.110)
Fixed Effects								
Firm	✓	✓	✓	✓	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓	✓	✓	✓	✓
Dereservation Controls	—	✓	—	✓	—	✓	—	✓
Observations	28,804	62,924	30,062	65,393	29,405	63,999	27,984	61,342
Sample	Restricted	All	Restricted	All	Restricted	All	Restricted	All

This table reports estimates of the heterogeneous effects of foreign capital liberalization reforms on high and low MRPK firms in the Prowess dataset (equation (5)), accounting for dereservation policies. Firms are observed between 1995 and 2015. In odd columns, we restrict the sample to firms in industries not affected by a dereservation policy after 2000 (i.e. a change in regulation specific to small and medium size firms). Data on dereservation events come from Boehm, Dhingra, and Morrow (2019). In even columns, we include the whole sample but interact $I_i^{High\ MRPK}$ with an indicator variable $Dereservation_{jt}$ that is equal to 1 after the industry has been dereserved. 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 approximated as $Revenue/K$. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are two-way clustered at the 4-digit industry and year level. *, **, and *** denote 10, 5, and 1% statistical significance respectively.

Table A8: Effect of Foreign Capital Liberalization, Controlling for Tariffs

Dependent Variable	**Revenues**		**Capital**		**Wages**		**MRPK**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Reform_{jt} \times I_i^{High\ MRPK}$	0.231***	0.134	0.543***	0.512***	0.260***	0.262***	-0.345***	-0.365**
	(0.071)	(0.096)	(0.069)	(0.051)	(0.057)	(0.064)	(0.095)	(0.132)
$Reform_{jt}$	0.025	0.172	0.122	0.135	0.123	0.168	-0.069	0.053
	(0.121)	(0.233)	(0.091)	(0.145)	(0.101)	(0.144)	(0.113)	(0.119)
Tariff Controls								
Output Tariffs	✓	✓	✓	✓	✓	✓	✓	✓
Input Tariffs	—	✓	—	✓	—	✓	—	✓
Fixed Effects								
Firm	✓	✓	✓	✓	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓	✓	✓	✓	✓
Observations	58,322	46,083	60,730	48,217	59,456	47,255	56,921	45,229

This table reports estimates of the heterogeneous effects of foreign capital liberalization on high and low pre-treatment MRPK firms (equation (5)) 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. $Reform_{jt}$ is an indicator variable 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 4-digit industry median. Tariff data from 1995-2010 are constructed following Goldberg, Khandelwal, Pavcnik, and Topalova (2010), and tariff levels are coded at the 2010 level from 2010-2015. Output tariff controls are the average tarriff on an industry and its interaction with $I_i^{High\ MRPK}$. Input tariff controls are the average tariff on the inputs used by an industry and its interaction with $I_i^{High\ MRPK}$. Standard errors are two-way clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table A9: Results after Winsorizing the Data

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	Panel A: Winsorized 5% Across Industries			
	(1)	(2)	(3)	(4)
$Reform_{jt} \times I_i^{High\ MRPK}$	0.172***	0.549***	0.206***	-0.373***
	(0.061)	(0.085)	(0.044)	(0.074)
$Reform_{jt}$	-0.019	-0.041	0.019	0.015
	(0.086)	(0.065)	(0.076)	(0.103)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	62,924	65,393	63,999	61,342
	Panel B: Winzorized 5% Within Industries			
	(1)	(2)	(3)	(4)
$Reform_{jt} \times I_i^{High\ MRPK}$	0.176***	0.550***	0.199***	-0.393***
	(0.058)	(0.084)	(0.046)	(0.069)
$Reform_{jt}$	-0.027	-0.041	0.038	0.022
	(0.083)	(0.066)	(0.079)	(0.107)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	62,924	65,393	63,999	61,342

This table reports estimates of the heterogeneous effects of foreign capital liberalization on capital constrained and unconstrained firms after winsorizing the top and bottom 5% of the sample for each outcome. In Panel A, the sample is winsorized *across* industries, while in Panel B, the sample is winsorized *within* 2-digit industries. All dependent variables are in logs. Firms are observed between 1995 and 2015. Firms are classified as high MRPK if their average MRPK in the pre-treatment period from 1995-2000 is above the 4-digit industry median. MRPK is calculated as $Revenue/K$. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are two-way clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table A10: Effects of Foreign Capital Liberalization on Firm Exit and Entry

Dependent Variable	**Number of Exits**		**Number of Entrants**
	(1)	(2)	(3)
$Reform_{jt}$	0.058	0.039	-0.029
	(0.105)	(0.055)	(0.028)
$Reform_{jt} \times I_i{}^{High\ MRPK}$		-0.027	
		(0.021)	
Fixed Effects			
5-Digit Industry	✓	✓	✓
Year	✓	✓	✓
Observations	6,668	11,833	6,668

This table estimates the effect of the foreign capital liberalization on firm exit and entry in the Prowess data. In columns 1 and 3, an observation is a 5-digit industry-year cell. In column 2, 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. Firms are classified as high MRPK if their average MRPK in the pre-treatment period from 1995-2000 is above the 4-digit industry median. In column 2, MRPK is calculated as $Revenue/K$. Standard errors are two-way clustered at the 4-digit industry and year level.

Table A11: Average Effect of Foreign Capital Market Liberalization, Accounting for Cross-Industry Spillover Effects

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
$Reform_{jt}$	0.109	0.295***	0.165*	-0.172*
	(0.088)	(0.099)	(0.090)	(0.089)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	62,924	65,393	63,999	61,342

This table reports difference-in-differences estimates of the effect of the foreign capital liberalization in the Prowess data, taking into account cross-industry spillover effects. All dependent variables are in logs. The regressions include controls for $Upstream_{jt}$, which measures the composite reform shock to an industry from upstream industries, and $Downstream_{jt}$, which measures the composite reform shock from downstream industries. Firms are observed between 1995 and 2015. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are two-way clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table A12: Heterogeneous Effects of Foreign Capital Liberalization, Accounting for Spillovers

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
$Reform_{jt} \times I_i^{High\ MRPK}$	0.245***	0.562***	0.263***	-0.350***
	(0.073)	(0.064)	(0.058)	(0.100)
$Reform_{jt}$	-0.031	-0.017	0.017	0.029
	(0.116)	(0.074)	(0.091)	(0.110)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	62,924	65,393	63,999	61,342

This table reports estimates of the heterogeneous effects of foreign capital liberalization on high and low MRPK firms, controlling for spillovers through the input-output matrix. All dependent variables are in logs. Firms are observed between 1995 and 2015. The regressions include controls for $Upstream_{jt}$, which measures the composite reform shock to an industry from upstream industries, and $Downstream_{jt}$, which measures the composite reform shock from downstream industries. Firms are classified as high MRPK if their average MRPK in the pre-treatment period from 1995-2000 is above the 4-digit industry median. MRPK is calculated as $Revenue/K$. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are two-way clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table A13: Robustness to More Parsimonious Controls

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
$Reform_{jt} \times {I_i}^{High\ MRPK}$	0.080	0.544***	0.152***	-0.490***
	(0.055)	(0.055)	(0.049)	(0.101)
$Reform_{jt}$	0.071	0.000	0.101	0.110
	(0.113)	(0.081)	(0.102)	(0.105)
Fixed Effects				
Firm	✓	✓	✓	✓
Year	✓	✓	✓	✓
Observations	64,808	67,339	65,912	63,210

This table reports estimates of the effect of foreign capital liberalization on high and low pre-treatment MRPK firms (equation (5)) 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 4-digit industry median. MRPK is estimated with the $Revenue/K$ method. Standard errors are two-way clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table A14: Heterogeneous Effects of Foreign Capital Liberalization, Accounting for Firm-Risk

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
$Reform_{jt} \times {I_i}^{High\ MRPK}$	0.241**	0.512***	0.223***	-0.263**
	(0.101)	(0.090)	(0.077)	(0.108)
$Reform_{jt}$ × $Revenue\ Beta_i$	-0.004	0.008	0.006	-0.019
	(0.014)	(0.021)	(0.014)	(0.015)
$Reform_{jt}$	0.013	0.052	0.089	-0.024
	(0.117)	(0.080)	(0.076)	(0.128)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	54,451	56,550	55,397	53,070

This table reports estimates of the heterogeneous effects of foreign capital liberalization on high and low MRPK firms, controlling for the firm's pre-treatment risk and its intearction with the reform. All dependent variables are in logs. Firms are observed between 1995 and 2015. $Revenue\ Beta_i$ is computed as the correlation between firm revenue growth up to 2000 and the average revenue growth in the economy. Firms are classified as high MRPK if their average MRPK in the pre-treatment period from 1995-2000 is above the 4-digit industry median. MRPK is calculated as $Revenue/K$. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are two-way clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table A15: Regression Estimates Used to Estimate the Effect of the Policy on the Solow Residual

Dependent Variable	**Capital**	**Wages**	**MRPK**	**MRPL**
	(1)	(2)	(3)	(4)
$Reform_{jt} \times I_i^{High\ MRPK}$	0.540***	0.242***	-0.366***	-0.129
	(0.081)	(0.061)	(0.108)	(0.080)
$Reform_{jt} \times I_i^{High\ MRPL}$	0.323***	0.114*	-0.246***	-0.330***
	(0.107)	(0.060)	(0.059)	(0.069)
$Reform_{jt}$	-0.129***	0.008	0.144	0.201*
	(0.046)	(0.109)	(0.112)	(0.115)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size $\times$Year	✓	✓	✓	✓
Observations	59,802	58,898	56,557	46,064

This table reports the difference-in-differences estimates used to estimate the policy's effects on the manufacturing Solow residual. All dependent variables are in logs. Firms are observed between 1995 and 2015. $I_i^{High\ MRPK}$ is coded as 1 if a firm's average MRPK in the pre-treatment period from 1995-2000 is above the 4-digit industry median, where MRPK is calculated using sales over physical assets. $I_i^{High\ MRPL}$ is defined analogously for labor. Size$\times$Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are two-way clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.