Misallocation and Capital Market Integration: Evidence From India*

Natalie Bau† Adrien Matray‡

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 increased revenues by 19%, physical capital by 59%, wage bills by 29%, and reduced MPRK by 41% relative to low MRPK firms. There were no effects on low MRPK firms. The effects of liberalization are largest in areas with less developed local banking sectors, indicating that the reform may substitute for an efficient banking sector. Finally, we develop a method to use natural experiments to estimate the lower bound effect of changes in misallocation on manufacturing productivity. The liberalization episode increased manufacturing's Solow residual by at least 5.5%.

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†UCLA, NBER, and CEPR. (email: nbau@ucla.edu)

‡Princeton. (email: amatray@princeton.edu)

1 Introduction

The misallocation of resources across firms may have a meaningful effect on aggregate productivity, particularly in low-income countries (e.g. Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009; Bento and Restuccia, 2017). Yet, despite the potential importance of misallocation for explaining economic disparities, quantifying its aggregate effects and identifying the best policy tools to reduce it are complicated by two challenges.

First, on the measurement side, it is common to attribute all — or much of — the cross-sectional dispersion in the observed marginal returns to firms' inputs to misallocation, which creates upward bias in measures of misallocation.[1] These measurement challenges are in turn likely to inflate estimates of the aggregate gains from reducing misallocation.

Second, 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.[2] This leaves policymakers with limited information about what levers to pull to reduce misallocation (Syverson, 2011). 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 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. To quantify the overall degree of misallocation, the literature usually compares outcomes such as the distribution of marginal revenue products across units of production after controlling for different characteristics and attributes the residual dispersion to misallocation. Since this method of quantifying misallocation typically does not show which characteristics causally affect the residual dispersion in marginal products, it 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 foreign capital liberalization on the misallocation of capital across firms. This setting allows us to isolate changes in the observed marginal revenue product of capital due to the policy from changes due to measurement error or other shocks.

We find that the liberalization reduced capital misallocation by increasing capital for firms with the highest marginal 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 a lower bound measure of the effect of the policy on the Solow residual, a proxy for aggregate manufacturing productivity. Our proposed method uses exogeneous variation to generate estimates of the aggregate effect of changing misallocation under relatively weak identifying assumptions and importantly, 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, we investigate 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, this empirical strategy requires milder identification assumptions for determining whether misallocation decreased 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 only requires that industry-level shocks, which may be correlated with the policy change, affect high and low MRPK firms in the same industry in the same way. 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 high MRPK firms in deregulated industries increase their physical capital by 59%, revenues by 19%, wage bills by 29%, and reduce their MRPK by 41% relative to low MRPK firms in response to the policy. In contrast, low MRPK firms are not affected. Since high MRPK firms initially have more than 150% higher MRPK, the micro-estimates imply that the policy reduces misallocation. Event study graphs confirm that these effects are not driven by differential pre-trends between high and low MPRK firms within treated industries relative to untreated industries and provide visual evidence that the reduction in misallocation is not due to mean reversion.

Exploiting geographic variation in local access to credit prior to the reform, we also 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 prices, exploiting a rare feature of our firm-level dataset: the fact that our panel provides detailed data on each firm's product-mix, as well as information about product-level 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 reduced prices in high MRPK firms in treated industries by 6% but had no effect on the prices of low MRPK firms.

The liberalization policy may have broader effects than reducing firms' wedges on capital inputs. By relaxing financial constraints, the policy may also affect the misallocation of other inputs. If firms need to borrow to pay workers, relaxing financial constraints can also affect labor misallocation.[3] 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 again find that the reform had greater effects on firms with high MRPL and that wage bills only increased for firms with above median pre-treatment MRPL. For these firms, relative to low MRPL firms, wage bills increased by 33%, and MRPL fell by 36%. Since high MRPL firms had more than 100% higher levels of MRPL prior to the treatment, labor misallocation fell along with capital misallocation following the reform.

Finally, combining production function parameter estimates with reduced-form estimates of the policy effect, we generate a lower bound estimate of the aggregate effect of liberalization episodes on the manufacturing sector's Solow residual of +5.5%. Using our quasi-experimental estimates to adjust for the biases arising from estimating misallocation with cross-sectional data is important. If we attributed *all* of the baseline variation in the marginal products of inputs to misallocation, we would estimate that the policy increased productivity by 92.6%. Moreover, this cross sectional estimate is highly sensitive to the treatment of outliers: winsorizing the top and bottom 15% of the marginal revenue product

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

measures reduces the estimated policy effect to 7.1%. Thus, under this approach, the degree to which researchers winsorize can result in a wide range of estimates. In contrast, our preferred lower bound estimate is not sensitive to the treatment of outliers.

This paper contributes to two main literatures, as we discuss below. First, it contributes to the literature quantifying the importance of misallocation for aggregate outcomes (e.g. Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009; Bartelsman, Haltiwanger, and Scarpetta, 2013; Restuccia and Rogerson, 2013; Baqaee and Farhi, 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; Oberfield, 2013; Kalemli-Ozcan and Sørensen, 2014).[4] Second, it contributes to literature on the effects of capital account liberalization and financial frictions (Buera, Kaboski, and Shin; 2011; Midrigan and Xu, 2014; Moll, 2014; Bai, Carvalho, and Phillips, 2018; Catherine et al., 2018).

Regarding the misallocation literature, much of the literature 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 estimation only requires that measurement error or other unobserved attributes are uncorrelated with the policy to identify *changes* in input wedges. Second, our approach isolates the changes in distortions produced by a specific policy, foreign capital liberalization. This allows us to isolate the effect of access to the foreign equity market, holding constant access to the foreign debt market and other macroeconomic determinants that might affect the cost of capital.[5] Third, relative to methodologies that rely

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

5. In the context of India, several recent papers have estimated specific characteristics of the Indian economy that might explain the high degree of misallocation observed in the country: the role of property rights and contract enforcement (Bloom et al., 2013; Boehm and Oberfield, 2018); land regulation (Duranton, Ghani, Goswani, and Kerr, 2017); industrial licensing (Chari,

on cross-sectional variation to identify wedges, our estimates of the aggregate effects of changing misallocation are less vulnerable to inflation due to measurement error.

By exploiting 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 the literature on capital account liberalization, this paper relates most closely to a recent strand of this literature that has explored how increased foreign financial flows affect domestic firms' productivity and sectoral misallocation (Alfaro, Chanda, Kalemli-Ozcan, and Sayek, 2004; Gopinath, Kalemli-Özcan, Karabarbounis, and Villegas-Sanchez, 2017; Varela, 2017; Larrain and Stumpner; 2017; Saffie, Varela, and Yi, 2018, Xu, 2020).[6] We add to this literature in several ways. First, while much of the previous literature exploits country-level variation in access to foreign investment, this paper exploits variation across industries over time *within the same country*. This allows us to hold the institutional setting constant, which is important since institutional differences are likely to 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*.[7]

Lastly, we estimate the direction of the effect of deregulating foreign investment on misallocation. Judging by prior findings in the literature, the effect of

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).

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

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

opening-up to foreign capital on misallocation is a priori unclear. On the one hand, in the context of low-income countries, where formal credit markets are limited and informal credit markets are a poor substitute (Townsend, 1994; Udry, 1994; Banerjee, Duflo, Glennerster, and Kinnan, 2015), credit constraints are likely to be large (Banerjee, Duflo, and Munshi, 2003; Banerjee and Duflo, 2014). Indeed, Anne Krueger, deputy managing director of the IMF during the time of the reform we study, wrote that in India, "banks are considered to be very high cost and inefficiently run" and that, "enabling [Indian banks] to allocate credit to the most productive users, rather than by government allocation, would make a considerable contribution to the Indian economy's growth potential" (Krueger et al., 2002). Thus, foreign investment could play a crucial role in reducing misallocation if foreign investors have better screening technologies or are not bound by historical, political, regulatory or institutional domestic constraints (e.g. Banerjee and Munshi, 2004; Burgess and Pande, 2005; Cole, 2009). On the other hand, foreign investors may also be worse at processing and monitoring soft information, particularly in low-income countries (Detragiache, Tressel, and Gupta, 2008).[8] Therefore, a final contribution of this paper is showing that foreign capital liberalization policies *do* reduce misallocation, suggesting that these policies could be a powerful tool for low-income countries to increase aggregate productivity.

The remainder of the paper is organized as follows. Section 2 provides a brief conceptual framework for understanding misallocation 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 marginal revenue products of capital/sales-capital ratios. It also replicates the analysis for firms that have high and low marginal revenue products of labor to test whether the policy also reduced labor misallocation. Section 6 describes the aggregation strategy and reports lower bound estimates of the foreign capital liberalization policies' aggregate effects on the Solow residual. Finally, Section 7 concludes.

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

2 Conceptual Framework

Our conceptual framework section proceeds in two parts. In the first subsection, we sketch a simple framework in general equilibrium that illustrates how our reduced-form results can shed light on changes in misallocation. In the second subsection, we introduce the expression that we will use to quantify the aggregate effects of changes in misallocation.

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 taxes or implicit taxes which implement a given (potentially inefficient) allocation in the decentralized Arrow-Debreu-McKenzie economy. Thus, the price paid by a firm i for an input x is $(1+\tilde{\tau}_i^x)p^x$, where $x \in \{K, L, M\}$ and K, L, and M denote capital, labor, and materials, respectively. The price of input x is p^x, and $\tilde{\tau}_i^x$ is the additional wedge a firm pays for the input over the market 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. 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.[9] 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

9. 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.

input (henceforth, "MPRX").

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 have several effects. The most direct effect is that, 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 or materials). 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 the Solow Residual

To quantify the aggregate effect of reducing misallocation on manufacturing productivity, following much of the literature, we proxy for changes in aggregate manufacturing productivity with changes in the Solow residual, which measures net output growth minus net input growth. Net output growth is the change in the sector's output net the outputs re-used as inputs by firms in the sector. Net input growth is the change in the inputs used by the sector net inputs that are produced by firms in the sector. 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 industry's 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, $\Delta Solow_I$ (output growth net of input growth) in discrete time is

$$\Delta Solow_I = \Delta \log C_I - \sum_{j \notin I} \frac{\sum_{i \in I} p_j y_{ij}}{\sum_{i \in I} p_i c_i} \Delta \log \sum_{i \in I} y_{ij}. \tag{1}$$

The summation $\sum_{j \notin I}$ sums over firms that supply intermediate goods to the manufacturing sector but are not themselves in manufacturing, while the summation $\sum_{i \in I}$ sums over firms in the manufacturing sector. 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 (1) subtracts out changes in inputs purchased from outside the manufacturing sector. Intuitively, 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 industry I over time is given by:

$$\Delta Solow_{I,t} \approx \sum_{i \in I} \lambda_i \Delta \log A_i + \sum_{\substack{i \in I \\ x \in \{K,L,M\}}} \lambda_i \; \alpha_i^x \; \tau_i^x \; \Delta \log x_i, \tag{2}$$

where α_i^x is the output elasticity of i with respect to input x, λ_i is each producer's sales as a share of manufacturing's net output, and $\Delta \log A_i$ is the firm-specific change in total factor productivity. 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 (2) will allow us to exploit our reduced-form estimates to bound the aggregate effect of the policy change on the manufacturing Solow residual.

3 Data and Policy Change

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

3.1 Indian Foreign Investment Liberalization

Following its independence, India became a closed, socialist economy, and most sectors were heavily regulated.[10] 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 this period, India also liberalized

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

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).

Before 1991, most industries were regulated by the Foreign Exchange Regulation Act (1973), which required every instance of foreign investment to be individually approved by the government, and foreign ownership rates were restricted to below 40% in most industries. With the establishment of the initial liberalization reform in 1991, foreign investment up to 51% of equity in certain industries became automatically approved.[11] In the following years, different industries liberalized at different times, with each liberalization increasing the cap on foreign investment and allowing for automatic approval. Based on our discussions with civil servants in charge of implementing financial liberalizations, the choice of which industries to liberalize may have been driven by the lack of clear foreign competitors that could enter the country via the FDI route and quickly wipe out local competition.[12] Thus, a cross-sectional comparison of treated and untreated industries would likely be biased by selection. As we discuss further in Section 4, the panel aspect of our data allows us to to account for any static, cross-sectional differences between industries.

We study the effects of financial liberalization episodes that occurred after 2000, after the main period of reform in the 1990s. This is both due to data availability, as described below, and to avoid conflating the effects of 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,

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

12. This would explain, for instance, why even within 3-digit industries, some industries were liberalized such as "Manufacture of rubber tyres and tubes for cycles and cycle-ricks" but not the manufacture of rubber more broadly.

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.[13] The data is therefore particularly well-suited for examination of how firms adjust over time in reaction to policy changes. The dataset contains information from the income statements and balance sheets of companies comprising more than 70% of the economic activity in the organized industrial sector of India and 75% of all corporate taxes collected by the Government of India. It is thus representative of large and medium-sized Indian firms. We retrieve yearly information about sales, capital stock (measured as physical assets), consumption of raw materials and 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. A detailed discussion of the data can be found in Goldberg, Khandelwal, Pavcnik, and Topalova (2010). The definition of a product is based on Prowess's internal product classification, which is in turn based on India's national industrial classification (NIC) and contains 1,400 distinct products. Using this information, we can calculate the unit-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.[14]

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

13. 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.

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

(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 four out of seven union territories. Altogether, this data covers 91.5% of net domestic product and 99% of credit.

3.4 Combined Data Sets

To arrive at our final 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 estimating production functions, we restrict our analysis to manufacturing firms. We further restrict the sample to observations from the period between 1995 and 2015. Restricting the sample to 1995-2015 has two advantages. First, focusing on this later period avoids potential bias from other liberalization reforms during the early-1990s, the main Indian liberalization period. While liberalization occurred for 45% of manufacturing firms in the data, by restricting our sample to observations after 1995, we only exploit policy variation for the 9% of manufacturing firms who experienced foreign capital liberalization in the 2000s. Second, although Prowess technically starts in 1988, its coverage in the first few years is limited and grows substantially over time. In 1988, Prowess only included 1,057 firms total, but it had grown to 7,061 firms by the beginning of our study period in 1995. In contrast, from 1995 onward, during our study period, the coverage of the database is more stable, with similar numbers of firms observed across subsequent years (7,526 firms observed in 1996, 7,286 in 1997, and 7,717 in 1998).[15]

Additionally, to allow for a longer pre-policy period over which to calculate

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

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 0.16% 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-policy change measures are needed to estimate the effects of the policy on misallocation. Thus, we restrict the sample to firms observed before 2001 with non-missing, positive data on both assets and sales.[16] These restrictions leave us with 4,926 distinct firms, across 340 distinct 5-digit industries, for a total of 63,950 observations.

Table 1 documents summary statistics for the final firm-level sample used in our analysis. As the table shows, classifying firms based on the owner's name, we find that the typical firm in our analysis is a privately-owned domestic firm (57%), while 5% of firms are private, foreign-owned firms, and 4% are state-owned. The table also shows that 9% of firms are in industries that experienced the policy change between 1995 and 2015.

4 Empirical Strategy

4.1 Measurement: MRPK and TFPQ

To estimate 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 MPRK. Below, we describe the method used to measure firms' MRPK.

As is standard in the production function estimation literature,[17] we assume that firms have Cobb-Douglas revenue production functions:

$$Revenue_{ijt} = A_{ijt} K_{ijt}^{\alpha_j^k} L_{ijt}^{\alpha_j^l} M_{ijt}^{\alpha_j^m}, \tag{3}$$

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

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

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

			Percentile		
	Obs.	Mean	10	50	90
Treated During Study Period (%)	66,654	9	0	0	0
Private, Domestic (%)	66,654	57	0	100	100
Private, Foreign (%)	66,654	5	0	0	0
State Owned (%)	66,654	4	0	0	0
Firm Age	66,654	26	8	21	52
Gross Fixed Assets (Deflated)	63,950	23	0	3	37
Sales/Revenues (Deflated)	62,784	58	1	11	107
Salaries (Deflated)	49,090	3	0	1	6
Income	64,155	68	1	10	115

This table reports summary statistics for the manufacturing firms appearing in the CMIE Prowess dataset from 1995 to 2015. An observation is at the firm-year level. Firms' capital, income, salaries, and revenues are measured in millions of USD. The 10th, 50th, and 90th percentiles are given by the final three columns.

where i denotes a firm, j denotes an industry, and t denotes a year. $Revenue_{ijt}$, K_{ijt}, L_{ijt}, and M_{ijt} are measures of sales, capital, the wage bill, and materials, and A_{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 it is above the 4-digit industry-level median for the averaged measure.

In addition to measuring MRPK, we also create a measure of TFPQ as a proxy for firm-level productivity. To do so, we use the method of Levinsohn and Petrin (2003) (henceforth "LP"), using the GMM estimation proposed by Wooldridge (2009), to estimate the parameters of revenue production functions at the 2-digit industry-level.[18] 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.[19] This iden-

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

19. One concern in our setting is that multi-product firms produce goods in multiple industries,

tifying 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 Y_{ijt} is proxied with deflated sales.[20] The revenue production function allows us to calculate revenue total factor productivity, TFPR. Using the product data, which measures 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. However, we note that the sample size for which TFPQ is available is much smaller (27,583 firm-year observations), as calculating this measure requires data on all firm inputs, as well as frequently missing price data. Thus, we view our within-firm level productivity results as more exploratory than our main misallocation results.

4.2 Econometric Specification

4.2.1 Main Specification: Heterogeneous Effects

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

$$outcome_{ijt} = \beta_1 \ Reform_{jt} + \beta_2 Reform_{jt} \times I_i^{High\ MRPK} + \mathbf{\Gamma X_{it}} + \theta_i + \delta_t + \epsilon_{ijt} \quad (4)$$

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

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.

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

size-by-year fixed effects,[21] so that β_1 and β_2 are identified by comparing two firms within the same size bin. 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 of MRPK dispersion.[22] 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.[23]

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 $\beta_1 + \beta_2$ measures total changes in high MRPK firms' outcomes.

4.2.2 Identification.

Below, we discuss to what extent 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 clarify 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 reformed. 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, to be valid, our specification does *not* require that the reform was randomly allocated, nor does it require that firms must be the same in terms of their static characteristics.

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

22. As previously discussed, cross-sectional measures of MPRK 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. In contrast, estimates of the *change in wedges* are less likely to be inflated by measurement error in MRPK, as we discuss below.

23. 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.

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 estimate 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 $Reform_{jt}$.[24] Thus, even if the Indian government liberalized industries that were growing more quickly earlier, β_2 would not be biased as long as high MPRK firms were not growing relatively more quickly than low MPRK 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 estimating solely β_2, estimating β_1 requires that time trends are parallel between treated and untreated industries. We will provide support for this in two ways. First, we will visually assess whether there are parallel pre-trends between treated and untreated industries in an event study figure. Second, we show that our 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 FDI an industry can receive. 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 has little effect on our estimates if it is either firm-specific and time-invariant, time-variant but common across firms in a given year, or classical (i.e. independent of the latent true variable). 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 litera-

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

ture, classical measurement error 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 MPRK, while some low MPRK firms will be coded as high MPRK. 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. We return to this issue in Section 5, when we show that our reduced-form estimates are not sensitive to winzorizing extreme values.

Allocation of FDI to firms in response to other characteristics. 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 driven by foreign investment. It could be, for example, that foreign investment frees up domestic capital to flow to smaller, high MRPK 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 (4). Table 2 reports the results. The estimates indicate that the liberalization policy had positive effects on the average firm's capital. For the average firm, capital increased by 28% (column 2). The point estimates for the total wage bill and revenues are also positive, albeit not significant. Figure 1 plots the event study graph for the average effects on capital. That is, it plots the estimated yearly effect of belonging to a treated industry up to five years before the reform and up to ten years afterwards, including the same controls as in Table 2. If there are no differential pre-trends,

Table 2: Average Effect of the Foreign Capital Liberalization

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
$Reform_{jt}$	0.10	0.28**	0.14	-0.17
	(0.08)	(0.11)	(0.11)	(0.11)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	61,494	63,832	48,177	61,236

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, MPRK is computed using Y/K as a proxy for the marginal revenue product of capital. Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10, 5, and 1% statistical significance respectively.

we should see that there is no effect of belonging to a treated industry before the reform took place, and this is indeed the case.

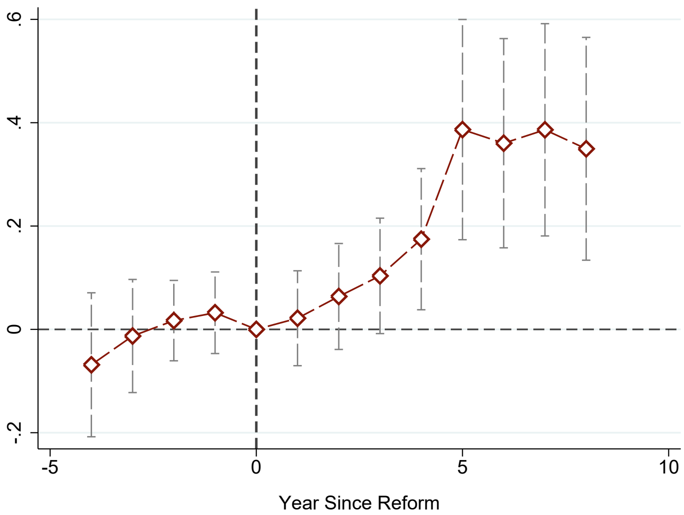
5.2 Differential Effects by Ex Ante MRPK

Baseline specification. Table 3 reports the estimates of the heterogeneous effects of the policy from equation (4), our main estimating equation. Following the liberalization, high MRPK firms generate higher revenues by 19% (column 1), made possible by the fact that these firms invest more, with their physical capital increasing by 59% (column 2).

Higher investment does not crowd-out labor. High MRPK firms also experience a relative increase in their wage bills by 29%, suggesting that there may be important complementarities between capital and labor in India. We will explore whether the reform also reduced labor misallocation in Section 5.5. Among the ex ante high MRPK firms, the policy also reduced MRPK by 41%. Given that, prior to the reform, high MRPK firms had a MRPK 3.8 times greater than low MRPK firms, the reform led to an important decline in the dispersion of MRPK. Taken together, our effects imply that the liberalization of foreign capital substantially reduced misallocation.[25]

25. This finding my be surprising given the results in Bollard, Klenow, and Sharma (2013), who find that most of economic growth in the earlier period in India could be attributed to within firm changes in productivity and not reallocation on inputs. However, Nishida, Petrin,

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 confidence interval is at the 90% level.

We use the same empirical strategy to examine whether the composition of capital changed heterogeneously as a result of the reform. Appendix Table A2 reports the results, and 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. Next, to assess whether these results are driven by pre-trends, we estimate 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 logs of capital, sales, the wage bill, and MRPK. Two facts are noteworthy.

First, for all of these outcomes, being treated by the policy had no differential effect on high MRPK firms before the policy was adopted, providing visual evi-

Rotemberg, and White (2017) show that this conclusion depends on the form of the production function, which might underestimate the contribution of reallocation to aggregate growth.

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

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
$Reform_{jt} \times I_i^{High\,MRPK}$	0.19***	0.59***	0.29***	-0.41***
	(0.06)	(0.07)	(0.11)	(0.08)
$Reform_{jt}$	-0.01	-0.04	-0.02	0.06
	(0.10)	(0.09)	(0.09)	(0.12)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	61,494	63,832	48,177	61,236

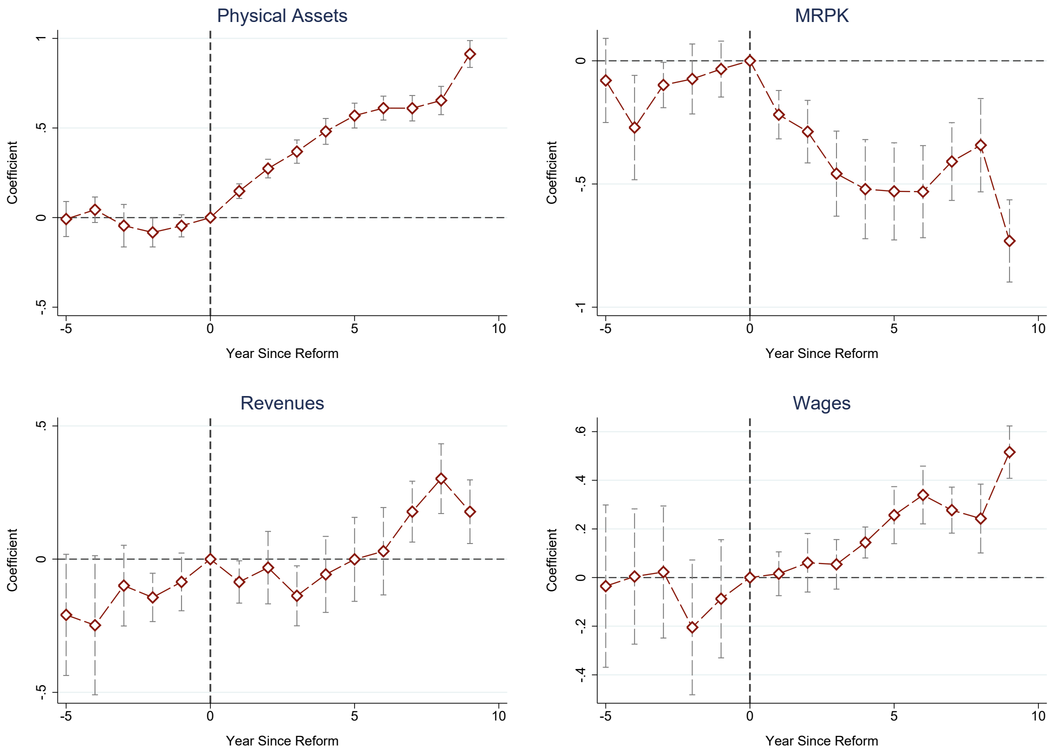
All dependent variables are in logs. Firms are classified as high MRPK if their average MRPK in the pre-treatment period from 1995-2000 is above the 4-digit industry median. MRPK is estimated with the Y/K method. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

dence that pre-trends were parallel. The lack of correlation between high MRPK firms' outcomes and the reform prior to the year of deregulation also implies that our results are not driven by mean reversion. If that was the case, we should observe a decline in MRPK prior to the policy change.

Second, the effect of the liberalization on the different firm outcomes is progressive over time, consistent with the idea that 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 allocation we observe might also come from competitive effects, where foreign capital liberalization allows firms with higher returns to capital to expand at the expense of ex ante low MRPK firms. We also expect this phenomenon to be progressive and only fully observable after some time has passed.

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 whether a firm in high or low MRPK (column 2), we again fail to find

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. All dependent variables are in logs. The confidence intervals are at the 90% level.

any effect.

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 reduction in credit constraints. 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 (4). The variable is de-meaned to restore the baseline effect on $I_i^{High\ MRPK} \times Reform_{jt}$. The coefficient of interest is the coefficient for the triple interaction $I_i^{High\ MRPK} \times Reform_{jt} \times Financial$

Table 4: Effect of Foreign Capital Liberalization on TFPQ

Dependent Variable	**TFPQ**	**TFPQ**
	(1)	(2)
$Reform_{jt}$	0.23	0.29
	(0.24)	(0.38)
$Reform_{jt} \times I_i^{High\ MRPK}$		-0.10
		(0.44)
Fixed Effects		
Firm	✓	✓
Firm Age	✓	✓
Size ×Year	✓	✓
Observations	27,583	27,583

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 Y/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 twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

$Development_s$, which captures the differential effect of the policy on high MRPK firms located in more developed local banking markets.

Table 5 reports the results. For revenues, 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 MPRK, 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 heterogeneous effects are economically meaningful. If we focus on the change in the marginal revenue products of capital (column 4), ex ante high MRPK firms located in a state at the 25^{th} percentile of the bank credit distribution experienced a decrease in MRPK of 49% $(-0.42 + (0.10 \times -0.71))$. In contrast, high MRPK firms located in a state at the 75^{th} percentile of the bank credit distribution experienced a decrease in MPRK of 28% $(-0.42 + (0.10 \times 1.37))$. Thus, the reduction at the 25^{th} percentile is nearly 50% larger than the one at the 75^{th} percentile.

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

Table 5: Heterogeneity by Local Financial Development

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
$Reform_{jt} \times I_i^{High\ MRPK} \times Financial\ Development_s$	-0.18***	-0.28***	-0.16***	0.10**
	(0.06)	(0.08)	(0.05)	(0.04)
$Reform_{jt} \times I_i^{High\ MRPK}$	0.20***	0.60***	0.29**	-0.42***
	(0.07)	(0.09)	(0.11)	(0.10)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	56,575	58,851	44,413	56,328

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 Y/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 twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

5.3 Product Outcomes

We next turn to the effect of the reform on 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' 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 (an insignificant) 4% (column 1). Column 2 shows that the reduction is driven by high MRPK firms, who reduce their prices by 6%.

We also test whether the increase in revenues caused by the reform was accompanied by a product-level increase in output. An increase in output for high

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)
$Reform_{jt}$	-0.04	0.01	0.12	-0.01	-0.10***	-0.11***	-0.10***
	(0.05)	(0.05)	(0.07)	(0.05)	(0.03)	(0.02)	(0.02)
$Reform_{jt} \times I_i^{High\ MRPK}$		-0.07**		0.19**	0.10***	0.18***	0.05
		(0.03)		(0.08)	(0.03)	(0.02)	(0.05)
Fixed Effects							
Firm	✓	✓	✓	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓	✓	✓	✓
Firm × Product	✓	✓	✓	✓	—	—	—
Observations	97,382	97,382	97,382	97,382	31,412	31,412	31,412

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. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

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 output. On average, output (insignificantly) increases by 12%. The averge effect masks considerable heterogeneity: high MRPK firms experienced a significant increase of 18%.

In the last three columns of Table 6, we examine whether the policy affected product offerings. Column 5 indicates that the number of products offered by low MRPK firms fell by 10% but was unaffected for high MRPK firms. This is driven by the fact that low MRPK firms were less likely to add new products (column 6) rather than less likely to delete products (column 7). High MPRK firms, on the other hand, were more likely to offer new 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

Differential industry-level time trends. We further explore whether β_2 is robust to differential time trends by controlling for 5-digit industry-year fixed effects in equation (4). This non-parametrically accounts for 5-digit industry-level time trends and only exploits *within*-industry changes in firms' outcomes.

$Reform_{jt}$ is therefore subsumed by the fixed effects. Appendix Table A3 reports the results, and shows that the estimates of β_2 are virtually unchanged.

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 A4 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.[26] 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 Indian states that are more exposed to the reform due to their industrial composition 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 A5, 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 between 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.[27]

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.[28]

26. There are 23 distinct 2-digit industries.

27. 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.

28. We would like to thank the authors for generously sharing their data with us. For each

To assess whether dereservation could be driving our results, we perform two tests, both reported in Appendix Table A6. 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, our main point estimates are virtually 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 operate and allowed them to access a broader set of inputs at a cheaper 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 and its effects took time to appear, this could bias our results.

Our specification with industry-year fixed effects already partially accounts for this potential bias, since the trade liberalization occurred at the industry-level. However, it's possible that trade 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.[29] 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 A7 reports the results when we control for the output tariffs only (the odd columns) or both the output and input tariffs (the even columns). Across the different specifications, the effect of the 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

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.

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

either across industries or within each 2-digit industry. We report the results in Appendix Table A8 and show that the point estimates are similar to those without a measurement error correction.

Firm entry and exit. To examine if our results could be affected by differential attrition between treated and untreated industries, we re-estimate equation (4) using a balanced panel of firms who appear in both 1995 and 2015. Appendix Table A9 reports the results from this exercise. While the balanced samples are substantially smaller for both classifications, the same pattern as before is evident.

Using the industry-level variation in the policy over time, we also directly test whether the policy affected firm exit and entry. If the policy had no effect on attrition, attrition should not bias our results. We identify entry in the data using the year of incorporation and use the last year in the dataset as a proxy for exit.[30] 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-MPRK 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.[31]

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

30. 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.

31. 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.

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

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 $\hat{a}_{ij} \equiv \frac{Sales_{i \to j}}{Sales_i} = a_{ji} \frac{Sales_j}{Sales_i}$ 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.[32] We then directly control for these spillover measures in our main regression equations.

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

Only Basic Controls. Finally, we 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 show that our estimates are not affected.

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 stock of capital) but also to expand their wage bills. Reducing capital market frictions may simply increase the demand for labor because of the complementarity between capital and labor in the production function. However, it is also possible that the financial liberalization directly reduced labor misallocation, a hypothesis which we test in this section.

Although labor is often modelled as a fully adjustable variable input across periods,[33] in reality, labor is likely to have a fixed-cost component due to wage rigidity and hiring/firing costs. As a result, when there is a mismatch between the

32. We use the input-output matrix for India from the World Input-Output database (Timmer et al., 2015).

33. 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)
$Reform_{jt} \times I_i^{High\ MRPL}$	0.18	0.33**	0.33***	-0.34***
	(0.11)	(0.15)	(0.09)	(0.09)
$Reform_{jt}$	0.00	0.17***	-0.01	0.14
	(0.07)	(0.05)	(0.11)	(0.10)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	51,159	52,672	42,053	41,239

All dependent variables are in logs. High MRPL firms are defined in an analoguous way as high MRPK firms using the Y/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 twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

payments to labor and the generation of cash-flows, financial constraints may affect employment and labor (mis)allocation. Schoefer (2015), Chodorow-Reich (2014) 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 increased their total wage bill (column 3) by 33%. Among ex ante high MRPL firms, MRPL decreased by 34% relative to low MRPL firms (column 4). By allowing high MRPL firms to grow faster and to expand employment, the deregulation appears to have led to a reduction in labor misallocation.

6 Aggregate Effects

Having shown that the liberalization policies reduced misallocation, we now quantify the effect of this reduction on the Solow residual, a proxy for the manufacturing sector's aggregate productivity, using equation (2).

6.1 Identification

The manufacturing sector's Solow residual will increase for two reasons: (1) individual firms become more productive (higher "technical efficiency") or (2) inputs either increase for producers with positive wedges or decrease for producers with negative wedges (higher "allocative efficiency"). These forces are captured by the two parts of equation (2). 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 $\Delta \log A_i$. Since we does not observe a significant effect of the policy on our measures of $\log A_i$ in the difference-in-differences regressions (see Table 4), we set $\Delta \log A_i = 0$.[34]

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

$$\Delta Solow_{I,t} = \sum_{\substack{i \in I \\ x \in \{K,L,M\}}} \lambda_i \ \alpha_i^x \ \tau_i^x \ \Delta \log x_i \tag{5}$$

Note that in the absence of misallocation, the policy cannot affect the Solow residual through a change in inputs. No misallocation before the policy change implies that the wedge τ_i would be 0 for all firms i, and the equation would also be equal to 0. Intuitively, when the allocation of inputs is already optimal, changing the allocation cannot increase the Solow residual. Increasing inputs for some firms also does not need to mechanically increase the Solow residual. If the policy increased misallocation by increasing inputs x_i for firms with negative wedges, the contribution to the Solow residual would be negative even though inputs increased.

Most components of this expression are readily observed in the data or given by our natural experiment estimates. λ_i is the share of firm i's sales in the total

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

sector sales that is not re-used as manufacturing inputs.[35] Under the assumption that firms have Cobb-Douglas production functions, α_i^x is obtained from the LP production function estimates. Finally, $\Delta \log x_i$ can be predicted using the coefficients from difference-in-differences regressions with heterogeneous effects where log usage of each input is the outcome variable, as explained in detail in Section 6.2. That is, after running the regression for each input, we can predict a *firm-specific* change in that input using the heterogeneous effects of the policy variation. Thus, it is straightforward to identify all of the components of equation (5) except τ_i^x.

Equation (5) highlights that errors in the estimation of τ_i^x, the level of firm-specific input wedges prior to the policy change, can greatly bias the aggregate policy effects, as τ_i^x is multiplicative with $\Delta \log x_i$. If we were to use cross-sectional variation in the marginal revenue products of capital, labor, and materials prior to the policy change as measures of τ_i^x, measurement error would lead to greater dispersion in these values. Since we have shown in Section 5 that the reform has a positive effect on capital and labor for firms with relatively greater wedges, inflated wedges would be multiplied by the positive predicted change in inputs. Thus, attributing all the dispersion of *measured* marginal revenue products to wedges would over-estimate the effects of reducing misallocation on aggregate productivity.

We circumvent this challenge by estimating a lower bound measure of equation (5), as described below. In particular, we note that if the policy strictly reduces misallocation, then the aggregate effects of the policy are strictly increasing in τ_i^x. Thus, if we can identify lower bound values of τ_i^x, we can estimate the lower bound effect on the Solow residual. While the assumption that a policy strictly reduces misallocation 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 favor of this assumption.

Identifying the Lower Bound of τ_i^x. By definition, the post-policy wedge for a firm is always given by: $\tau_{post}^x = \tau_{pre}^x + \Delta\tau^x$, where $\Delta\tau^x$ is the change in τ^x due

35. To measure total sales by sector I not re-used by firms in I as inputs, we sum over the universe of Indian manufacturing firms' total sales in 2000 (the last pre-treatment year) in the Annual Survey of Industries. We then use information from India's input-output table, drawn from the World Input-Output Database (Timmer et al., 2015), to compute the share of output that is re-used by the manufacturing sector 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.

to the policy and i is suppressed to simplify notation.

To derive a lower bound, we then make two assumptions. First, we assume the policy does not increase misallocation.[36] Second, we assume that the policy had no spillover effects on the wedges and inputs of firms that were not directly treated by the policy. This is the standard difference-in-differences assumption.[37] This assumption allows for general equilibrium effects within treated industries but rules out general equilibrium effects to untreated industries. In this sense, we capture the aggregate effects of the partial equilibrium changes to inputs and wedges in treated industries that our reduced-form identification strategy can causally estimate.

Under the assumption that the policy does not subsidize firms, $\tau^x_{post} \geq 0$. Then, $\min_{\tau^x_{post} \geq 0} \tau^x_{pre} = -\Delta\tau^x$. Thus, the minimum possible pre-treatment wedge is given by the scenario where, after the policy change, the industry is Pareto-efficient, and there are no wedges left. In this case, any measured dispersion in marginal revenue products after the policy change is attributed to mismeasurement and misspecficiation as opposed to misallocation. So, if we can estimate $\Delta\tau^x$, this gives us a lower bound estimate of τ^x_{pre}, and we can apply equation (2) 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^k_i, we estimate

$$\log MRPK_{ijt} = g_i(Reform_{jt}) + \mathbf{\Gamma X_{it}} + \alpha_i + \delta_t + \epsilon_{ijt} \tag{6}$$

where $g_i(Reform_{jt})$ is a flexible function of $Reform_{jt}$, so that the effect of the reform can depend on firms' or industries' attributes. 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^k to vary within an industry j. As shown in Appendix B, if the policy completely eliminated misallo-

36. This is consistent with the fact that the average differences in the marginal revenue products of high and low MRPK and MRPL firms at baseline were much higher than the estimated effect of the policy on firms with high MRPK or MRPL.

37. This assumption could be partially relaxed by modeling spillovers explicitly and estimating spillovers effects. It also allows us to clarify the difference between our aggregation exercise and the exercise in Sraer and Thesmar (2020). Our goal is to estimate the aggregate effect of the *existing* policy, not to estimate a counterfactual world where the policy would be extended to additional industries.

cation, $\hat{g}_i(1)$ is an unbiased predictor of $\log(1+\tau^k)$. Then, τ^k can be estimated by computing $\widehat{\tau}_i^k = e^{\hat{g}_i(1)} - 1$. An analogous process can be used to estimate the wedges on labor.

As discussed in Section 4.2, estimating the change in wedges using this difference-in-differences specification is less sensitive to the issues that occur when cross-sectional data is 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. 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 firm and year fixed effects. Appendix Table A8 provides further evidence that these estimates are less sensitive to measurement error, as winsorizing the data has little effect on the point estimates.

6.2 Estimation of Wedges and the Change in Inputs

Since we are interested in how an industry-level policy affected misallocation within an industry, to estimate the aggregate effect of the reform, we need to estimate how the allocation of resources changed across firms within the same industry. In other words, we need to estimate different wedges and changes in inputs for different firms in the same industry. To do so, following our reduced-form strategy, we estimate difference-in-differences regressions with heterogeneous effects, allowing the effects of being in a treated industry to depend on firm-level characteristics.[38] In Appendix C, we discuss how estimation for the aggregation exercise could be implemented in other settings.

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_i to allow for heterogeneous effects by firms' pre-treatment marginal revenue products.

38. So far, much of the literature has focused on estimating the industry-level effects of policies on the variance of measures of distortions. However, mapping these variances to aggregate productivity growth requires important functional form and distributional assumptions (for example, see Hsieh and Klenow (2009)). Focusing on firm-level effects, combined with the general aggregation formula given by equation (2), allows for a more non-parametric approach.

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

	Increase in Solow Residual
Lower Bound	5.5%
Attributing All Cross-Sectional Variation	92.6%
Measurement Error Correction (Top and Bottom 15%)	7.1%

This table reports the estimates of the effect of the foreign capital liberalizations in 2001 and 2006 on the manufacturing sector's Solow residual using a first order approximation (equation (2)). The estimates are generated using the Prowess data set. The first row gives the lower bound estimate, which assumes that the policy eliminated misallocation. The second row attributes all of the baseline within-5 digit industry variation in the marginal revenue products of inputs to misallocation. The third row does the same after winsorizing the top and bottom 15% of the marginal revenue product measures within industries.

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:

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

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

Our regression specifications for MRPL and $\widehat{\tau}_i^l$ are analogous, and the results are reported in Appendix Table A14. Appendix Table A14 also reports the results of the similar regressions used to estimate $\widehat{\Delta \log K_i}$ and $\widehat{\Delta \log L_i}$. Following the identifying assumption in the production function estimation, we assume that materials are not misallocated ($\tau_i^m = 0$ for all i).[39]

6.3 Results

Lower Bound Estimate. Now that we have estimated all the components of equation (2), we can calculate the lower bound effect of the policy on the Solow

39. In practice, relaxing this assumption and calculating wedges and changes in inputs for materials the same way we do for capital and labor has almost no effect on the estimated change in the Solow residual.

residual. We estimate that the policy increased the Solow residual by at least 5.5% (see row 1 of Table 8). To evaluate the magnitude of this effect, we can compare it to estimates of the gains from reallocation in Indian manufacturing from Nishida, Petrin, Rotemberg, and White (2017). Nishida, Petrin, Rotemberg, and White (2017) estimate the yearly increase in aggregate productivity in Indian manufacturing due to reallocation from 2000-2010. From 2006 (the earliest year following both liberalizations) to 2010, there are gains of approximately 35%. Thus, our estimated lower bound effect accounts for roughly 16% of the increase in manufacturing productivity over this period.

Comparison with Alternative Estimates. We next compare this lower bound estimate to estimates of the aggregate effect using alternative measures of the baseline wedges. It is common in the misallocation literature to estimate levels of distortions by using cross-sectional dispersion in marginal revenue products. This approach has recently been criticized by Haltiwanger, Kulick, and Syverson (2018), Rotemberg and White (2017), and Asker, Collard-Wexler, and De Loecker (2014) for inflating the effects of misallocation. For comparison to our lower bound approach, we use equation (2) to estimate the effects of the policies on the Solow residual if we computed the baseline wedges by attributing all of the dispersion in MRPK, MRPL, and MRPM to misallocation. If we attribute *all* the dispersion within a 5-digit manufacturing industry to misallocation, we estimate that the policy would increase the Solow residual by 92.6% (Table 8, row 2). However, this large effect is driven by outliers. If we additionally winsorize the top and bottom 15% of deviations, we find that the policy increased the Solow residual by 7.1% (Table 8, row 3). The fact that winsorizing has a meaningful effect on the estimates is consistent with the findings of Rotemberg and White (2017), who show that winsorizing has large effects on the degree of measured misallocation in cross-sectional data from the U.S. and India. Given the range of estimates produced by different choices about the treatment of outliers, it appears that approaches that use cross-sectional variation to identify wedges will be highly sensitive to arbitrary choices of where to winsorize or trim data.

7 Conclusion

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

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

Aggregating our reduced-form estimates, we also find that the policy increased the manufacturing's sector's Solow residual by at least 5.5%. In contrast, if we assumed all the dispersion in the marginal revenue products of inputs was due to misallocation, we would estimate the policy increased the Solow residual by 92.6%. Our methodology, which is less sensitive to measurement error or outliers, can be applied to other settings where there is an exogeneous shock to firms' input wedges. Thus, our results provide evidence that quasi-experimental variation can improve the measurement of the aggregate effects of reducing misallocation.

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Online Appendix

Appendix A: Derivation of Aggregation Formula

In this section, we derive equation (2), the formula used to approximate the change in the Solow residual due to the policy. We start by defining

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

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

$$d \log y_i = \sum_j \frac{\partial \log f_i}{\partial \log y_{ij}} d \log y_{ij} + d \log A_i. \tag{7}$$

A firm i solves the constrained cost minimization problem

$$\mathcal{C}_i(p, y_i) = \sum_j p_j y_{ij} + \gamma_i (y_i - A_i f_i(y_i)), \tag{8}$$

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 (8)

$$p_j = \gamma_i A_i \frac{\partial f_i}{\partial y_{ij}}. \tag{9}$$

Then,

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

where μ_i is the mark-up of i, implying that $\gamma_i = \frac{p_i}{\mu_i}$. Substituting this relationship into (9) shows that $p_j = \frac{p_i}{\mu_i} A_i \frac{\partial f_i}{\partial y_i}$. Then

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

which can be rewritten as $\mu_i \frac{p_j y_{ij}}{p_i y_i} = \frac{\partial \log f_i}{\partial \log y_{ij}}$. Then, substituting this into the total derivative (equation (7)) 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

$$\frac{1}{\mu_i}(d\log y_i - d\log A_i) - \sum_{j\notin I}\frac{p_j y_{ij}}{p_i y_i} d\log y_{ij} = \sum_{j\in I}\frac{p_j y_{ij}}{p_i y_i} d\log y_{ij}. \tag{10}$$

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

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

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

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

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

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

Using equation (10), with a little algebra, we can rewrite this as

$$\Delta Solow_I \approx \sum_{i\in I}\lambda_i(1 - \frac{1}{\mu_i})(d\log y_i - d\log A_i) + \sum_{i\in I}\lambda_i d\log A_i, \tag{11}$$

where $\lambda_i = \frac{p_i y_i}{PC}$. Now, we transform equation (11) to use input wedges instead of output wedges, so that it matches equation (2). First, we rewrite the output wedges (μ_i) as input wedges, consistent with the theoretical framework in Section 2. This allows us to rewrite equation (11) in terms of firm-level capital, labor, and materials wedges where each firm-input combination is a "producer."[40] The wedge on firm i's input x is τ_i^x, and the price paid by the firm is $(1+\tau_i^x)p^x$, while the marginal cost of producing x is p^x. The gross output wedge for producer (x, i) is given by: $\mu_i^x = 1+\tau_i^x$. Second, we define α_i^x to be the output elasticity of input i with respect to input x. Then, for a given firm i, $d\log y_i - d\log A_i = \sum_{x\in\{k,l,m\}}\alpha_i^x$. So, we can rewrite equation (11) as:

40. While equation (11) 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 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.

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

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 two 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 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 ($Reform_j = 0$) or if the firm is in an industry that will be reformed but the reform has not taken place yet ($Reform_j = 1$ and $t < T_j$):

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

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

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

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

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

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

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

Denote $g_i(Reform_{jt})$ to be a firm-specific function of the reform indicator variable, which can be written as a linear interaction between a vector of firm-level characteristics $\mathbf{X_i}$ and the indicator variable $Reform_{jt}$. Then, the difference-in-differences regression estimates

$$\log(MRPK_{ijt}) = g_i(Reform_{jt}) + \alpha_i + \delta_t + \epsilon_{it}.$$

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, $\hat{g}_i(1)$ 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: Applications of the Aggregation Method to 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 across the unit of treatment (e.g. industries, geographic entities) are either nonexistent or can be measured using observable characteristics like input-output linkages or with the experimental design.

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.

Reallocation 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_i to depend on industry or geographic unit-level characteristics as well as, or in place of, firm-level characteristics.

Changes in Inputs to the Treated Group. In some cases, the design of a policy or an experiment may allow the researcher to assume the policy/treatment reduced misallocation even if the researcher is not interested in reallocation 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 Tables

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

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

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

Table A2: Composition of Change in Capital

Dependent Variable	**Land**	**Plants and Equipment**	**Infrastructure**	**Other**
	(1)	(2)	(3)	(4)
$Reform_{jt} \times I_i^{High\ MRPK}$	-0.03***	0.04***	-0.00	-0.01
	(0.01)	(0.01)	(0.00)	(0.01)
$Reform_{jt}$	0.00	0.00	-0.00	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	64,396	64,396	64,396	64,396

This table reports estimates of the heterogeneous effects of foreign capital liberalization reforms on high and low MRPK firms in the Prowess data set (equation (4)). 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 Y/K. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10, 5, and 1% statistical significance respectively.

Table A3: Robustness: 5-Digit Industry-by-Year Fixed Effects

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
$Reform_{jt} \times I_i^{High\ MRPK}$	0.35***	0.74***	0.46***	-0.38***
	(0.05)	(0.08)	(0.11)	(0.09)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
5-Digit Industry × Year	✓	✓	✓	✓
Observations	61,494	63,832	48,177	61,236

This table reports estimates of the heterogeneous effects of the liberalization reforms on high MRPK firms in the Prowess data set (equation (4)). 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 Y/K. Size×Year are quartile fixed effects for firm's average pre-treatment capital interacted with year fixed effects. Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table A4: Robustness: Inclusion of 2-Digit Industry-by-Year FE

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
$Reform_{jt} \times I_i^{High\ MRPK}$	0.22***	0.60***	0.32***	-0.38***
	(0.06)	(0.08)	(0.11)	(0.09)
$Reform_{jt}$	-0.05	-0.14	-0.13	0.13
	(0.12)	(0.14)	(0.11)	(0.13)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size×Year	✓	✓	✓	✓
2-Digit Industry × Year	✓	✓	✓	✓
Observations	61,494	63,832	48,177	61,236

This table reports estimates of the heterogeneous effects of foreign capital liberalization reforms on high and low MRPK firms in the Prowess data set (equation (4)). 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 Y/K. Size×Year are quartile fixed effects for firm's average pre-treatment capital interacted with year fixed effects. Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10, 5, and 1% statistical significance respectively.

Table A5: Robustness: Inclusion of State-by-Year FE

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
$Reform_i \times I_{jt}^{High\ MRPK}$	0.18**	0.55***	0.25**	-0.39***
	(0.08)	(0.07)	(0.11)	(0.08)
$Reform_{jt}$	-0.01	-0.00	-0.01	0.03
	(0.11)	(0.09)	(0.09)	(0.12)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
State × Year	✓	✓	✓	✓
Observations	61,494	63,832	48,177	61,236

This table reports estimates of the heterogeneous effects of foreign capital liberalization reforms on high and low MRPK firms in the Prowess data set (equation (4)). 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×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. MRPK is calculated as Y/K. *, **, and *** denote 10, 5, and 1% statistical significance respectively.

Table A6: Robustness: Accounting for Dereservation

Dependent Variable	**Revenues**		**Capital**		**Wages**		**MRPK**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Reform_{jt} \times I_i^{High\ MRPK}$	0.23***	0.20***	0.67***	0.56***	0.44***	0.28**	-0.46***	-0.37***
	(0.06)	(0.06)	(0.05)	(0.10)	(0.10)	(0.11)	(0.05)	(0.10)
$Reform_{jt}$	0.03	-0.01	0.01	-0.03	-0.01	-0.01	0.05	0.05
	(0.10)	(0.10)	(0.09)	(0.08)	(0.11)	(0.09)	(0.13)	(0.11)
Sample	Restricted	All	Restricted	All	Restricted	All	Restricted	All
Fixed Effects								
Firm	✓	✓	✓	✓	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓	✓	✓	✓	✓
Observations	27,689	61,494	28,815	63,832	21,703	48,177	27,563	61,236

This table reports estimates of the heterogeneous effects of foreign capital liberalization reforms on high and low MRPK firms in the Prowess dataset (equation (4)), 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 Y/K. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10, 5, and 1% statistical significance respectively.

Table A7: Effect of Foreign Capital Liberalization, Controlling for Tariffs

Dependent Variable	**Revenues**		**Capital**		**Wages**		**MRPK**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Reform_{jt} \times I_i{}^{High\ MRPK}$	0.18***	0.10	0.58***	0.54***	0.27**	0.22***	-0.41***	-0.39***
	(0.07)	(0.07)	(0.07)	(0.08)	(0.11)	(0.07)	(0.07)	(0.13)
$Reform_{jt}$	0.06	0.21	0.12	0.13	0.19*	0.16	-0.04	0.09
	(0.11)	(0.21)	(0.11)	(0.18)	(0.11)	(0.13)	(0.13)	(0.12)
Tariff Controls								
Output Tariffs	✓	✓	✓	✓	✓	✓	✓	✓
Input Tariffs	−−	✓	–	✓	–	✓	–	✓
Fixed Effects								
Firm	✓	✓	✓	✓	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓	✓	✓	✓	✓
Observations	57,004	45,104	59,314	47,235	47,432	45,250	56,780	44,973

This table reports estimates of the foreign capital liberalization on high and low pre-treatment MRPK firms (equation (4)) 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 twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table A8: Results after Winsorizing Data

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	Panel A: Winsorized 5% Across Industries			
	(1)	(2)	(3)	(4)
$Reform_{jt} \times I_i^{High\ MRPK}$	0.13**	0.57***	0.21**	-0.40***
	(0.05)	(0.08)	(0.09)	(0.05)
$Reform_{jt}$	-0.00	-0.06	0.01	0.04
	(0.08)	(0.08)	(0.07)	(0.12)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	61,494	63,832	48,177	61,236
	Panel B: Winzorized 5% Within Industries			
	(1)	(2)	(3)	(4)
$Reform_{jt} \times I_i^{High\ MRPK}$	0.13**	0.57***	0.21**	-0.42***
	(0.05)	(0.08)	(0.10)	(0.05)
$Reform_{jt}$	-0.01	-0.05	0.02	0.04
	(0.08)	(0.08)	(0.07)	(0.12)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	61,494	63,832	48,177	61,236

This table reports estimates of the heterogeneous effects of foreign capital liberalization on capital constrained and unconstrained firms after winsorizing 5% of the sample. 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 Y/K. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.

Table A9: Robustness of Heterogeneous Effects of Foreign Capital Liberalization to Using a Balanced Panel of Firms

Dependent Variable	**Revenues**	**Capital**	**Wages**	**MRPK**
	(1)	(2)	(3)	(4)
$Reform_{jt} \times I_i^{High\ MRPK}$	0.16	0.37***	0.05	-0.24**
	(0.12)	(0.06)	(0.09)	(0.11)
$Reform_{jt}$	-0.05	0.09	0.06	-0.10
	(0.13)	(0.09)	(0.09)	(0.12)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	28,302	28,662	22,536	28,235

This table reports estimates of the heterogeneous effects of foreign capital liberalization on capital constrained and unconstrained firms in a balanced panel of firms that appear in both 1995 and 2015 from the Prowess data set (equation (4)). 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 Y/K. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. *, **, 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)
$Reform_{jt}$	0.10	0.07	-0.03
	(0.13)	(0.07)	(0.03)
$Reform_{jt} \times I_i^{High\ MRPK}$		-0.04	
		(0.03)	
Fixed Effects			
5-Digit Industry	✓	✓	✓
Year	✓	✓	✓
Observations	6,568	11,714	6,568

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 columns 2, MRPK is calculated as Y/K. Standard errors are twoway 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)
$Reform_{jt}$	0.10	0.28**	0.14	-0.16
	(0.08)	(0.10)	(0.11)	(0.11)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	61,494	63,832	48,177	61,236

This table reports difference-in-differences estimates of the effect of the foreign capital liberalization in the Prowess data set, taking into account cross-industry spillover effects. All dependent variables are in logs. $Upstream_{jt}$ measures the composite reform shock from upstream industries, and $Downstream_{jt}$ 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 twoway 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)
$Reform_{jt} \times I_i^{High\ MRPK}$	0.19***	0.59***	0.29***	-0.41***
	(0.06)	(0.07)	(0.11)	(0.08)
$Reform_{jt}$	-0.01	-0.05	-0.02	0.06
	(0.10)	(0.09)	(0.09)	(0.12)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	61,494	63,832	48,177	61,236

This table reports estimates of the heterogeneous effects of foreign capital liberalization on capital constrained and unconstrained firms, controlling for spillovers through the input-output matrix. All dependent variables are in logs. Firms are observed between 1995 and 2015. $Upstream_{jt}$ measures the composite reform shock from upstream industries, and $Downstream_{jt}$ 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 Y/K. Size×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are twoway 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)
$Reform_{jt} \times I_{jt}^{High\ MRPK}$	0.04	0.59***	0.13	-0.55***
	(0.04)	(0.06)	(0.08)	(0.08)
$Reform_{jt}$	0.09	-0.03	0.06	0.14
	(0.09)	(0.09)	(0.11)	(0.11)
Fixed Effects				
Firm	✓	✓	✓	✓
Year	✓	✓	✓	✓
Observations	61,494	63,832	48,177	61,236

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

Table A14: Regression Estimates Used to Estimate the Effect of the Policy on the Manufacturing Solow Residual

Dependent Variable	**Capital**	**Wages**	**MRPK**	**MRPL**
	(1)	(2)	(3)	(4)
$Reform_{jt} \times I_i^{High\ MRPK}$	0.574***	0.268**	-0.447***	-0.198***
	(0.138)	(0.106)	(0.113)	(0.057)
$Reform_{jt} \times I_i^{High\ MRPL}$	0.361**	0.346***	-0.167***	-0.355***
	(0.137)	(0.092)	(0.053)	(0.083)
$Reform_{jt}$	-0.163***	-0.162	0.125	0.259*
	(0.056)	(0.095)	(0.126)	(0.127)
Fixed Effects				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size ×Year	✓	✓	✓	✓
Observations	61,494	63,832	48,177	61,236

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×Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are twoway clustered at the 4-digit industry and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance respectively.