

MISALLOCATION AND CAPITAL MARKET INTEGRATION: EVIDENCE FROM INDIA

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We show that foreign capital liberalization reduces capital misallocation and increases aggregate productivity for affected industries 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. Liberalization increases capital overall. For domestic firms with initially high marginal revenue products of capital (MRPK), liberalization increases revenues by 23%, physical capital by 53%, wage bills by 28%, and reduces MRPK by 33% relative to low MRPK firms. The effects of liberalization are largest in areas with less developed local banking sectors, indicating that inefficiencies in that sector may cause misallocation. Finally, we propose an assumption under which a novel method exploiting natural experiments can be used to bound the effect of changes in misallocation on treated industries' aggregate productivity. These industries' Solow residual increases by 3–16%.

KEYWORDS: Misallocation, foreign capital liberalization, aggregate productivity, economic development, India.

1. INTRODUCTION

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

On the measurement side, it is common to attribute all—or much of—the cross-sectional dispersion in the observed marginal returns to firms' inputs to misallocation. This creates upward bias in measures of misallocation and can contaminate estimates of differences in allocative efficiency across countries or over time.¹

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¹Upward bias can come, for example, from measurement error (Bils, Klenow, and Ruane (2021), Rotemberg and White (2017), Gollin and Udry (2021)), 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 (2021)), unobserved heterogeneity

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. In low-income countries, where there are likely to be large firm-level frictions in the allocation of resources, understanding which policies reduce domestic misallocation could provide policymakers with powerful tools to foster economic growth.

An unusual natural experiment in India allows us to make progress on both the measurement and the policy fronts, providing some of the first evidence on a policy tool that can be used to reduce misallocation in affected sectors.³ Over the 2000s, India introduced the automatic approval of foreign direct investments up to at least 51% of domestic firms' equity, potentially increasing overall access to capital while reducing capital market frictions.

Such a policy can affect aggregate output for two reasons. First, by increasing the overall amount of capital available, it allows the *average* firm to grow. Second, it can change the *distribution* of capital across firms, which, in the presence of heterogeneous firms, will affect the degree of capital misallocation in treated industries and therefore, those industries' aggregate productivity. If the increased capital allows firms with high marginal returns to capital ("MRPK") to grow relatively faster, the misallocation of capital will decrease as the within-industry dispersion in MRPK declines.

Using the staggered introduction of the policy across industries, we implement a difference-in-differences framework to estimate the effects of this foreign capital liberalization on both total capital and the distribution of capital across firms. In the absence of a natural experiment, the measurement of changes in misallocation would be contaminated by measurement error and other (unobserved) shocks. However, in this setting, the natural experiment allows us to isolate changes in inputs and the change in the marginal revenue product of capital due to the policy, allowing us to cleanly estimate how misallocation changes due to the policy. The inclusion of various controls, such as firm, year, and 5-digit industry-year fixed effects, accounts for many sources of unobserved heterogeneity that could otherwise bias measurement.

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

in technology (Gollin and Udry (2021)), and informational frictions and uncertainty (David, Hopenhayn, and Venkateswaran (2016), David and Venkateswaran (2019)).

²To quantify the overall degree of misallocation, the literature usually compares outcomes such as the distribution of marginal revenue products across units of production after controlling for different observable characteristics and attributes the residual dispersion to misallocation. Since this method of quantifying misallocation typically does not show which characteristics causally affect the residual dispersion in marginal products, it is mostly silent on what policies would be required to reduce misallocation in low-income countries.

³For simplicity, throughout this paper we use the shorthands of "reducing misallocation" and "changing misallocation" to refer to changes in the misallocation of inputs among formal sector firms within manufacturing industries that were treated by the policy. We cannot speak to the global effects of the policy on misallocation, as this would require us to be able to observe the universe of firms and FDI worldwide.

⁴In the context of foreign banks' behavior in low-income countries, several studies have found that foreign banks mainly lend to large domestic firms, thereby potentially increasing credit constraints for local firms (e.g., Detragiache, Tresselt, and Gupta (2008)).

We find that the liberalization of foreign capital increases total capital in treated industries, while also reducing capital misallocation within those industries by increasing capital for the firms with the highest marginal revenue returns to capital prior to the reform. This implies that at least some of the dispersion in observed marginal revenue products of capital in India is due to misallocation rather than noise. We then develop a method, based on the theoretical results of [Petrin and Levinsohn \(2012\)](#) and [Baqae and Farhi \(2019\)](#), to translate our microeconomic estimates into more and less conservative measures of the effect of the policy on the treated industries' Solow residual (a proxy for these industries' aggregate productivity). Our proposed method uses exogenous variation to generate a lower bound for the aggregate effect of changing misallocation on treated industries under relatively weak identifying assumptions.

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

Estimating whether the policy reduced misallocation does not require deregulation to be random, nor for firms to have similar levels of pre-reform covariates. It only requires that, in the absence of the reform, high MRPK firms would not have grown relatively more quickly than low MRPK firms in treated versus untreated industries.

While fundamentally untestable, we provide three pieces of evidence to support this assumption. First, we graphically show similar trends across various outcomes pre-reform, including in the difference in the marginal revenue return to capital between high and low MRPK firms. Second, we show that our coefficients of interest are robust to including high-dimensional fixed effects to account for as many unobserved shocks as possible. In the most stringent specifications, we account for time-varying differences by industry, state, and pre-treatment size quartile. Third, we show that a large number of industry characteristics do not predict which industries were deregulated.

We find that, in response to the policy, high MRPK firms in deregulated industries increase their physical capital by 53%, revenues by 23%, wage bills by 28%, and reduce their MRPK by 33%, relative to low MRPK firms. In contrast, low MRPK firms are not affected. Since high MRPK firms had more than 160% higher MRPK than low MRPK firms, the micro-estimates imply that the policy reduced dispersion in MRPK.

To better understand the mechanism underlying these results, we exploit geographic variation in local access to credit prior to the reform. We find that the effects of liberalization on misallocation are largest in areas where the local banking sector was less developed. This is consistent with the hypothesis that the misallocation of capital in India is at least partially driven by inefficiencies in the domestic banking sector.

We next explore the effect of the reform on firms' products, including product portfolio, prices, and quantities. This is made possible by a rare feature of our firm-level data set: detailed data on each firm's product-mix, product-level output, and prices. Since a reduction in distortions on input prices should reduce marginal costs for affected firms, firms may pass some of these gains onto consumers via lower prices. Depending on the degree of pass-through, the change in the price could be greater than or less than the change in the marginal cost. We find that the reform differentially reduced prices for high MRPK firms in treated industries by 12% but had no significant effect on the prices of low MRPK firms. Additionally, high MRPK firms in treated industries increase the number of products in their portfolio.

The liberalization policy may have had broader effects than reducing firms' wedges on capital inputs. If firms need to borrow to pay workers, relaxing financial constraints can

also affect labor misallocation.⁵ Motivated by this possibility, we examine the effect of the policy on labor misallocation in treated industries. Analogously to our approach for capital, we estimate the policy's differential effect on firms with high marginal revenue products of labor (henceforth, "MRPL"). We find wage bills only increased for firms with high MRPL. For these firms, relative to low MRPL firms, wage bills increased by 24%, and MRPL fell by 28%. Since high MRPL firms had at least two times higher levels of MRPL prior to the treatment in treated industries, dispersion in MRPL also fell.

Finally, combining production function parameter estimates with reduced-form estimates of the policy effect, we generate estimates of the effect of the liberalization on the treated industries' Solow residual. At a lower bound, the treated industries' Solow residual increased by 3%. Accounting for the cumulative effects of the policy over time raises this number to 6%. Even at a lower bound, the policy had economically meaningful aggregate effects. In contrast, if we infer baseline wedges from the pre-treatment cross-sectional data (a more conventional approach), the effect is 16%.

The paper is organized as follows. The remainder of the Introduction discusses the related literature. Section 2 provides a brief conceptual framework for understanding misallocation, introduces the expression we will use for aggregation, and derives testable predictions for how inputs will change if misallocation in treated industries falls. Section 3 describes the data and the context of the policy change. Section 4 discusses our reduced-form empirical strategy. Section 5 reports our estimates of the average effect of the foreign capital liberalization policy and its heterogeneous effects on firms with high and low MRPK. It also replicates the analysis for firms that have high and low MRPL to test whether the policy also reduced labor misallocation. Section 6 describes the aggregation strategy and reports estimated lower bounds on the foreign capital liberalization policies' effect on the Solow residual for treated industries. Section 7 concludes.

Related Literature. This paper contributes to two main literatures. First, it contributes to the literature quantifying the importance of misallocation for aggregate outcomes (e.g., Lagos (2006), and Rogerson (2008), Hsieh and Klenow (2009), Bartelsman, Haltiwanger, and Scarpetta (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)).⁶ Second, it contributes to the literature on the effects of financial frictions and misallocation (e.g., Buera, Kaboski, and Shin (2011), Midrigan and Xu (2014), Moll (2014), Hombert and Matray (2016), Kehrig and Vincent (2019)).

Regarding the misallocation literature, much of the previous work has focused on measuring the effect of all sources of misallocation on aggregate output by exploiting cross-sectional dispersion in marginal revenue products. The principal advantage of this "indirect approach" (Restuccia and Rogerson (2013)) 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 in treated industries using milder identification assumptions than the

⁵For more discussion of this mechanism, see Fonseca and Van Doornik (2022) in Brazil.

⁶A survey of this literature can be found in Restuccia and Rogerson (2013).

literature that depends on cross-sectional or cross-country variation. Our difference-in-differences strategy only requires that measurement error or other unobserved attributes are uncorrelated with the policy change to identify *changes* in input wedges. Second, our approach isolates the changes in distortions produced by a specific policy, foreign capital equity liberalization.⁷ This allows us to isolate the effect of access to the foreign equity market, holding constant other macroeconomic determinants that might affect the cost of capital differentially for different firms. Third, we show how our natural experiment estimates can be used to compute aggregate effects of reducing misallocation that are less vulnerable to inflation due to measurement error or model misspecification. In so doing, we develop a method that can be applied in other contexts by researchers studying misallocation.

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

In terms of capital account liberalization, this paper relates most closely to a recent strand of this literature that has explored how increased foreign financial flows affect domestic firms' productivity, sectoral misallocation, and welfare (e.g., [Gopinath, Kalemli-Özcan, Karabarbounis, and Villegas-Sanchez \(2017\)](#), [Varela \(2017\)](#), [McCaig, Pavcnik, and Wong \(2021\)](#), [Xu \(2022\)](#)). We add to this literature in two ways. First, while much of the previous literature exploits country-level variation in access to foreign investment, this paper exploits variation across industries over time *within the same country*. This allows us to hold the institutional setting constant, which is important since institutional differences affect cross-country comparisons. Second, since the Indian deregulation only affected foreign investment in equity, it allows us to cleanly isolate the effect of liberalizing foreign investment in *equity* on misallocation, holding fixed access to foreign *debt*.

Our results contrast with those of [Gopinath et al. \(2017\)](#), who showed that an overall increase in capital due to increased access to foreign debt increased misallocation in Spain. However, these findings are not mutually exclusive. First, it is unclear whether expansions in equity and debt will have similar effects. Equity encourages investors to invest in firms with high upside potential, while debt may encourage investment in “safe” firms with high levels of collateral. Second, the baseline levels of misallocation and financial development are different in India from those in an OECD country, and this context is likely to be important for determining the size and direction of the effects of increased financial integration (e.g., [Varela \(2017\)](#)).

More broadly, our paper relates to the literature that studies misallocation in developing countries, which has particularly focused on agriculture. This literature has studied the possibility that missing markets (land, insurance, credit) may give rise to misallocation,

⁷In the context of India, several recent papers have studied specific characteristics of the Indian economy that might explain the high degree of misallocation observed in the country: the role of property rights and contract enforcement ([Bloom, Eifert, Mahajan, McKenzie, and Roberts \(2013\)](#)); land regulation ([Duranton, Ghani, Goswami, Kerr, and Kerr \(2015\)](#)); industrial licensing ([Chari \(2011\)](#)); privatization (e.g., [Dinc and Gupta \(2011\)](#)); reservation laws ([Garcia-Santana and Pijoan-Mas \(2014\)](#), [Martin, Nataraj, and Harrison \(2017\)](#), [Boehm, Dhingra, and Morrow \(2022\)](#), [Rotemberg \(2019\)](#)); electricity shortages ([Allcott, Collard-Wexler, and Connell \(2016\)](#)), and labor regulation ([Amirapu and Gechter \(2020\)](#)).

which in turn has implications for the distributional impact of productivity and technology shocks (Gollin and Udry (2021)), risk sharing (Townsend (1994)), the impact of microcredit (Kaboski and Townsend (2011)), property rights (e.g., Adamopoulos, Brandt, Leight, and Restuccia (2022)), and the farm size distribution (Restuccia, Yang, and Zhu (2008), Adamopoulos and Restuccia (2014), Restuccia and Santaaulalia-Llopis (2017)). This literature was among the first to use panel data to improve upon cross-sectional analyses of input and output dispersion, which may be contaminated by unobserved heterogeneity (e.g., Udry (1996), LaFave and Thomas (2016), Gollin and Udry (2021)).

2. THEORETICAL FRAMEWORK

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

Relationship Between Wedges and Marginal Revenue Products. 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,⁸ and the combined wedge is $1 + \tau_i^x = \mu_i (1 + \tilde{\tau}_i^x)$. The marginal revenue product of input x is proportional to the (combined) wedge τ_i^x . Therefore, firms with higher combined input wedges τ_i^x (capital, labor, or any other) will have higher marginal revenue products on this input (henceforth, “MRPX”).

Misallocation and the Solow Residual. To quantify the effects of reducing misallocation on treated industries' aggregate productivity and develop predictions about when reductions in misallocation will occur, we proxy for changes in aggregate productivity with

⁸Technically, 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 directly affects both 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.

changes in the Solow residual. The Solow residual measures the net output growth minus the net input growth. Thus, denoting the Solow residual for a sector of interest I as $Solow_I$,

$$\Delta Solow_I = \Delta Net\ Output_I - \Delta Net\ Input_I. \quad (1)$$

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

$$\Delta Solow_I = \Delta \log C_I - \sum_{s \notin I} \frac{\sum_{i \in I} p_j y_{is}}{\sum_{i \in I} p_i c_i} \Delta \log \sum_{i \in I} y_{is}. \quad (2)$$

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

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

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

where λ_i is the ratio of firm i 's sales to treated industry I 's net output, $\Delta \log A_i$ is the change in total factor productivity (TFPQ), α_i^x is the output elasticity with respect to x , τ_i^x is the level of firm-specific input wedges prior to the policy change, and $\Delta \log x_i$ is the change in the log input x consumed by firm i , which itself is endogenous to A_i . A derivation of this expression is provided in Appendix A of the Supplemental Material ([Bau and Matray \(2023\)](#)). We show that this expression does not require any assumptions about returns to scale, cross-good aggregation, the joint lognormality of TFPR and TFP, or the shape of input-output networks.⁹

Detecting Reductions in Misallocation Within Industry I From Changes in Inputs. Equation (3) tightly guides our empirical strategy. Declines in misallocation within industry I are cases where the allocation of inputs changes such that the Solow residual increases.

⁹We provide more details on the benefits of measuring misallocation without the lognormality assumption using a simple example economy in [Bau and Matray \(2022\)](#).

From equation (3), we see that the Solow residual (and therefore output) will increase if the amount of x_i used by firms with relatively higher wedges for x (high values of $\frac{\tau_i^x}{1+\tau_i^x}$) increases.

This relationship shows that misallocation in I can fall (and the Solow residual for I can increase) even if the overall amount of capital in treated industries changes. Increases in total capital will reduce misallocation as long as ex ante high wedge firms grow faster than low wedge firms. That is, misallocation in I can decrease even if the expansion of high wedge firms is not made at the expense of low wedge firms.¹⁰ With panel firm-level data, we can identify ex ante high wedge firms (firms with ex ante high MRPX) and precisely test whether input usage increases for these firms.

While our approach to testing for declines in misallocation requires firm-level panel data, it requires weaker assumptions than approaches that rely on changes in industry-level dispersion measures (e.g., changes in the industry-level variance of log TFPR). Changes in the variance of log TFPR are only sufficient statistics for changes in misallocation if TFPR and TFPO are jointly log normal, production is constant returns, and aggregate output is produced by a CES aggregator (Hsieh and Klenow (2009)). Equation (3) does not make any of these assumptions.

We will return to equation (3) when we convert firm-level effects, which are in different units depending on the goods being produced, into aggregate effects. As we will explain in Section 6, equation (3) will allow us to exploit our reduced-form estimates to put a lower bound on the aggregate effect of the policy change on the treated industries' Solow residual.

3. DATA AND POLICY CHANGE

3.1. Foreign Investment Liberalization

Following its independence, India became a closed, socialist economy, and most sectors were heavily regulated.¹¹ However, in 1991, India experienced a severe balance of payments crisis, and in June 1991, a new government was elected. Under pressure from the IMF, the World Bank, and the Asian Development Bank, which offered funding, the Indian government engaged in a series of structural reforms. These reforms led India to become more open and market-oriented. In addition to initiating foreign capital reforms in more than one-third of the manufacturing sector in this period, India also liberalized trade (e.g., Topalova and Khandelwal (2011), Goldberg, Khandelwal, Pavcnik, and Topalova (2010)) and dismantled extensive licensing requirements.

Before 1991, most industries were regulated by the Foreign Exchange Regulation Act (1973), which required every instance of foreign investment to be individually approved by the government, and foreign ownership rates were restricted to below 40% for each firm in most industries. With the establishment of the initial liberalization reform in 1991, foreign investment up to 51% of equity in certain industries became automatically approved.¹² In the following years, different industries liberalized at different times, with each liberalization increasing the cap on foreign investment and allowing for automatic

¹⁰Note that increasing firms' capital inputs does not itself mechanically increase the Solow residual in equation (3) for two reasons. First, for a fixed amount of total capital in the economy, increasing capital for a low MPRK firm reduces capital for a high MPRK firm, reducing the Solow residual. Second, if a firm faces a negative wedge (it is subsidized), $\frac{\tau_i^x}{1+\tau_i^x}$ will be negative, and the positive change in inputs will be multiplied by a negative value.

¹¹See Panagariya (2008) for a thorough review of the Indian growth experience and government policies.

¹²This policy is described by Topalova (2007) and Sivadasan (2009).

approval. FDI can take a variety of forms in India, from joint partnerships with technological transfers to the direct purchase of equity in Indian firms (with little additional training or technological investment). The latter form has been much more common following the reforms (Beena, Bhandari, Bhaumik, Gokarn, and Tandon (2004)).

We study the effects of financial liberalization episodes that occurred after 2000, well after the main period of reform in the early 1990s. This is due both to data availability and to avoid conflating the effects of the financial liberalization reforms with other ongoing reforms. To obtain information on these reforms, 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 these 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 and/or increased the cap on investments to at least 51% of capital (though, in some cases, the maximum is higher). We then merge these data at the industry level with the firm-level data set described below.

Timing and Choice of Industries. In the late 1980s, India faced a major economic crisis and was, for the most part, a socialized economy. This context helps explain why the initial wave of reform in 1991 was so controversial. To limit political push-back, the government only reformed specific industries at this time (Singh (2005)). However, by the early 2000s, there was a broad consensus across both major political parties on the merits of promoting FDI. This consensus faced a different challenge: the overall decline of FDI entering India. This decline pushed the government to promote reforms in industries that had not yet been deregulated as part of a broader effort to show the international community that India welcomed foreign investment.

The government's motives are reflected in quotations from India's contemporaneous *Economic Surveys*. The 2001 reform was described as a “remedial measure” to address “widespread industrial slowdown” (India Ministry of Finance, Economic Division, Government (2002, p. 163)). Similarly, the larger 2006 reform appears to have been part of a “comprehensive review of FDI policy,” which was undertaken to “consolidate the liberalization already effected and further rationalize the FDI policy governing various activities” (India Ministry of Finance, Economic Division, Government (2006, p. 154)).

While the *Economic Surveys* offer some insight into the timing of the reforms, they offer less insight into why specific industries were chosen. Across countries, there is evidence that political motivations affect the choice of industries that receive foreign capital liberalization (Pandya (2014)). While we are not aware of any qualitative or quantitative evidence that the reforms we study were affected by political concerns, Chari and Gupta (2008) showed that politics played a role in the controversial reform of 1991, during which more concentrated industries and those with more state-owned firms were less likely to liberalize.¹³ If similar political motives also dictated the choice of industries liberalized in 2001 and 2006, this may affect our estimates. We return to this concern in Section 4.3, where we evaluate whether political and economic variables predict the reforms.

3.2. Firm- and Product-Level Data

Our firm-level data come from the Prowess database compiled by the Centre for Monitoring the Indian Economy (CMIE) and include all publicly traded firms, as well as a large

¹³More concentrated industries are thought to be better able to organize to lobby against reform. Similarly, industries with state-owned firms are less likely to be liberalized both because these firms are better connected and because the government receives their revenues.

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 data set. Therefore, the data are particularly well-suited for examining how firms adjust over time in reaction to policy changes. The data set 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.

Recalling equation (3), changes in the treated industries' Solow residual will be driven by changes in inputs to larger firms (those with a high value of λ_i). For reasonable changes in firms' inputs, changes in inputs to firms with a very small sales share (e.g., the informal sector) will have little effect on the Solow residual unless they release inputs to larger firms, and if those larger firms are in Prowess, we will still be able to detect these changes. Thus, while Prowess does not include very small firms or the informal sector, it contains the firms that are most relevant for studying the effect of changes in misallocation on the Solow residual. Furthermore, to address the concern that relying on Prowess may lead us to miss important effects on firms that are not included in the data, we also augment our analyses with data from the Annual Survey of Industries, which is representative of India's formal manufacturing sector and accounts for almost all the sales in the manufacturing sector (Ghani, Kerr, and Segura (2015)).

We retrieve yearly information about sales, capital stock (measured as tangible, physical assets), consumption of raw materials and energy, and compensation of employees for each firm. Unfortunately, Prowess does not contain information on number of employees. To estimate the effect of the reform on prices, we take advantage of one rare feature in firm-level data sets that is available in Prowess: the data set 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.¹⁴ 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 1400 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.¹⁵

3.3. Main Combined Data Sets

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

As is common in the literature, we restrict our analysis to manufacturing firms. We further restrict the sample to observations from the period between 1995 and 2015. Restricting the sample to 1995–2015 has two advantages. First, focusing on this later period avoids potential bias from other liberalization reforms during the early 1990s, the main Indian liberalization period. While 45% of manufacturing firms in the data are in industries that liberalized at some point, by restricting our sample to observations after 1995,

¹⁴ A detailed discussion of the data can be found in Goldberg et al. (2010).

¹⁵ One limitation of this data set is that firms choose which type of units to report, and units are not always standardized across firms or within firms over time. Thus, when we want to analyze the effects of policy changes on prices/output and there is not enough information to reconcile changes in unit types within a firm-product over time, we drop the set of observations associated with a firm-product. We omit 2% of observations.

we only exploit policy variation from the 10% of manufacturing firms who experienced foreign capital liberalizations in the 2000s. Second, although Prowess technically starts in 1988, its coverage in the first few years was limited and grew substantially over time. In 1988, Prowess only included 735 manufacturing firms total, but it had grown to 3652 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 (3664 firms observed in 1996, 3470 in 1997, and 3614 in 1998).¹⁶

Additionally, to allow for a longer pre-policy period over which to calculate MRPK and classify MRPK as high or low, as described below, we drop a very small number of firms that experienced a liberalization in 1998. This amounts to 104 total firm-year observations (roughly 4–5 per year) or 0.26% of the sample. Table A.I in the Supplemental Material provides a list of the different industries in the manufacturing sector affected by the deregulation during the study period. As the table shows, after dropping the 1998 liberalization, the only remaining liberalization episodes occurred in 2001 and 2006.

Following other work with manufacturing data in India (e.g., Hsieh and Klenow (2009), Rotemberg and White (2017)), to reduce noise from mis-entry in the data, for our main analyses, we also drop observations where there are extreme negative year-to-year fluctuations in revenues (declines greater than 85%). This reduces our sample size by 8%, but as we will show, our main results are virtually unchanged when we do not make this restriction.

Finally, we restrict the sample to the set of firms for which we can compute marginal revenue products of capital and labor (MRPK and MRPL) prior to the earliest policy change in 2001. These pre-treatment measures are needed to estimate the effects of the policy on misallocation. Thus, we restrict the sample to firms observed before 2001 with non-missing, positive data on both assets and sales.¹⁷ These restrictions leave us with an unbalanced panel of 5013 distinct firms across 337 distinct 5-digit industries, for a total of 63,149 observations.

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

3.4. Supplementary Data Sources

While most of our analyses are conducted using Prowess, we also supplement it with a variety of additional data sources. We outline each briefly in this section.

We use two additional data sets to provide evidence that the policy had first-stage effects on access to foreign capital: (1) data on foreign loans (available after 2004) scraped from the Reserve Bank of India's website and (2) CapEx data on the timing and ownership of large capital projects in India. To ensure our results are robust to the inclusion of small establishments, we also evaluate the effects of the policy using the Annual Survey of Industries, a repeated survey of a representative cross-section of Indian manufacturing establishments conducted by the Indian government. Additionally, to evaluate whether the

¹⁶This likely reflects the fact that the first wave of liberalizing reforms also standardized financial reporting in the mid-1990s.

¹⁷This is the minimal requirement to calculate MRPK. As we document in the next section, 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 .

TABLE I
SUMMARY STATISTICS.

	Control			Treated		
	Mean	S.D.	Median	Mean	S.D.	Median
Foreign (%)	4.1	19.7	0.0	7.0	25.6	0.0
State Owned (%)	3.7	18.8	0.0	2.0	14.1	0.0
Firm Age	26.4	18.9	21.0	26.7	18.9	21.0
Gross Fixed Assets	25.5	132.4	3.5	18.2	49.9	3.1
Sales/Revenues	64.3	274.4	12.3	57.8	146.6	11.9
Wages	3.6	19.7	0.6	4.2	9.8	0.8
MRPK (<i>Revenue/K</i>)	7.5	89.4	3.1	9.3	71.6	3.2
Observations	56,861			6288		

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

policy had heterogeneous effects in states with less-developed banking systems, we collected data from the Reserve Bank of India at the state level for each of the pre-reform years (1995–2000) on the total credit of all scheduled commercial banks. Finally, for our robustness analyses, we supplement the Prowess data with data on Indian dereservation policies¹⁸ (the removal of protective laws for small-scale industries) and input and output tariff data,¹⁹ both at the 5-digit industry level.

4. EMPIRICAL STRATEGY

4.1. Measurement: MRPK and TFPQ

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

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

$$Revenue_{ijt} = TFPQ_{ijt} K_{ijt}^{\alpha_k} L_{ijt}^{\alpha_l} M_{ijt}^{\alpha_m}, \quad (4)$$

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 $TFPQ_{ijt}$ is the firm-specific unobserved revenue productivity. Throughout this paper, capital is measured as the total value of tangible, physical assets.

¹⁸Garcia-Santana and Pijoan-Mas (2014), Martin, Nataraj, and Harrison (2017), Boehm, Dhingra, and Morrow (2022), and Rotemberg (2019) described these laws and estimated their consequences. We thank Boehm, Dhingra, and Morrow for generously sharing the data with us.

¹⁹India experienced a massive reduction in its trade tariffs in the 1990s, as has been studied by Topalova and Khandelwal (2011) and Goldberg et al. (2010). We would like to thank Johannes Boehm for generously sharing his tariff measure with us.

²⁰Duranton et al. (2015) described a variety of methods used to estimate production functions and the revenue returns to capital and labor.

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

In addition to measuring MRPK, we also create a measure of TFPQ as a proxy for firm-level productivity. We implement the Levinsohn and Petrin (2003) method (henceforth “LP”), using the GMM estimation proposed by Wooldridge (2009), to estimate the parameters of revenue production functions at the 2-digit industry level.^{21,22} The LP method estimates the parameters of the production function using a control function approach, where materials are assumed to be increasing in a firm's unobserved productivity conditional on capital. This identifying assumption does not require that capital or labor are not misallocated—the key sources of misallocation that we study in this paper—but does assume away misallocation of materials. For the production function estimation, we measure inputs and revenues with deflated Rupee amounts, so that $Revenue_{ijt}$ is proxied with deflated sales.²³ The revenue production function allows us to calculate revenue total factor productivity, TFPR. Using the product data, which measure unit prices, we calculate $\log TFPQ = \log TFPR - \log \tilde{p}$, where \tilde{p} is the sales share weighted average of the prices of a firm's products.²⁴ By estimating the effect of the reform on TFPQ, we can examine whether foreign capital liberalization affects within-firm productivity as well as misallocation. The sample size for which TFPQ is available is much smaller (43,791 firm-year observations), as calculating this measure requires data on all firm inputs, as well as price data. Thus, we view our within-firm productivity results as more exploratory than our main misallocation results.

4.2. Main Specification: Heterogeneous Effects

To measure the effect of liberalization on changes in input usage among firms within industries, we estimate the following equation:

$$Outcome_{ijt} = \beta_1 Reform_{jt} + \beta_2 Reform_{jt} \times I_i^{HighMRPK} + \Gamma X_{it} + \theta_i + \delta_t + \epsilon_{ijt}, \quad (5)$$

where i denotes a firm, j denotes an industry, t denotes a year, and $Outcome_{ijt}$ is the outcome variable of interest, consisting of the logs of physical capital, the total wage bill,

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

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

²³We use deflators for India made available by Allcott, Collard-Wexler, and Connell (2016) for the period 1995–2012, and we extended the price series to 2015. Revenue is deflated using 3-digit commodity price deflators. The materials deflators are measures of the average output deflator of a given industry's suppliers using the 1993–1994 input-output table. The capital deflator is obtained using an implied national deflator.

²⁴TFPQ in the production function literature is conventionally measured in units of a given product outputted. Thus, this measure will not capture changes in the products a firm outputs (including improvements in product quality). Instead, TFPQ can be thought of as capturing process efficiency.

sales, and MRPK. $Reform_{jt}$ is an indicator variable equal to 1 if foreign investment has been liberalized in industry j , and $I_i^{HighMRPK}$ is an indicator variable equal to 1 if a firm has a high pre-reform MRPK according to our measure defined in Section 4.1. \mathbf{X}_{it} consists of firm age and firm pre-treatment size-by-year fixed effects,²⁵ so that β_1 and β_2 are identified by comparing two firms with the same age and within the same (pre-treatment) size bin. We include the pre-treatment firm size-by-year fixed effects to account for any differential trends by firm size that may bias our estimates. In a robustness check, we show that our main results are robust to including a more parsimonious set of controls. θ_i and δ_t are firm and year fixed effects, respectively. δ_t controls for aggregate fluctuations, while θ_i removes time-invariant unobserved firm-level heterogeneity, which may bias estimates of the MRPK dispersion.²⁶ 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.²⁷

The regression specification given by equation (5) is the empirical counterpart of equation (3). Equation (3) indicates that a change in the distribution of capital increases the Solow residual for treated industries when capital differentially increases for firms with larger ex ante wedges. Equation (5) precisely tests whether this is the case. 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. Thus, if $\beta_2 > 0$ when the outcome is capital, capital differentially increases for firms with ex ante high wedges, and within-industry capital misallocation falls. β_1 measures changes in low MRPK firms' outcomes, and $\beta_1 + \beta_2$ measures total changes in high MRPK firms' outcomes.

4.3. Identification

Below, we discuss the extent to which our empirical strategy is vulnerable to three potential sources of bias: (1) non-random assignment of treatment status across firms, (2) the endogeneity of foreign equity flows, and (3) measurement error in MRPK. We also emphasize that the policy can reduce misallocation within treated industries even if foreign investors do not directly identify and invest in high MRPK firms.

Selection of Treated Firms. One natural concern is that firms in industries that are liberalized are different from firms in industries that are not. As long as these differences are time-invariant, this selection is accounted for by firm fixed effects (θ_i). Similarly, firm fixed effects account for any time-invariant differences, observed or unobserved, between high and low MRPK firms. Thus, our specification does *not* require that the reform was randomly allocated, nor does it require that firms must have the same pre-treatment characteristics. Rather, β_2 will be unbiased as long as the difference in outcomes between ex

²⁵Firm size is defined as fixed effects for the within 2-digit industry quartiles of firms' average pre-treatment capital.

²⁶As previously discussed, cross-sectional measures of MRPK are likely to be inflated by measurement error. Indeed, if we calculated the level of capital misallocation using cross-sectional data, a standard approach would be to use an estimate of the variance of MRPK as a proxy for the dispersion of the wedges. This estimate would sum over both the variance of the wedges and the variance of measurement error, leading to inflated estimates of the dispersion of the wedges.

²⁷Our 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.

ante high and low MRPK firms in treated industries would have evolved the same way as in control industries in the absence of the reform. This assumption implies that industry-level trends in misallocation would have been the same in treated and untreated industries in the absence of the policy.

While, by definition, this assumption is untestable, we provide three pieces of supporting evidence. First, using event study figures, we will show parallel pre-trends (1) between treated and untreated industries and (2) in the difference between high MRPK and low MRPK firms' outcomes in treated versus untreated industries. Second, we will 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.

Third, we test whether we can predict which industries were selected for reform. While the validity of our design does not require that treatment and control units do not differ in levels, similarity in levels suggests that the common-trends assumption is more plausible. We run a series of regressions at the industry level, where we regress an indicator variable equal to 1 if an industry was reformed in 2001 or 2006 on economic and political economy measures from 2000 (the last pre-treatment year).²⁸ Table II reports the results.

TABLE II
ASSOCIATIONS BETWEEN INDUSTRY-LEVEL CHARACTERISTICS AND THE REFORM.

Dependent Variable	Reform = 1								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log (Variance in MRPK)	0.032 (0.027)								
Log (Num. Firms)		0.014 (0.025)							
Log (Avg. Firm Capital)			-0.033 (0.027)						
Share Export				0.116 (0.176)					
State-Owned Firms' Share of Total Sales					-0.006 (0.011)				
Herfindahl Index						-0.173 (0.123)			
Ihs(Industry-Level FDI)							-0.029 (0.041)		
FDI Growth, 2001–2005								-0.012 (0.015)	
Ihs(FDI 2005)-Ihs(FDI 2001)									0.064 (0.052)
Observations	274	274	274	274	274	274	274	169	188

Note: This table reports the association of different industry-level characteristics with liberalization. Each observation is a 5-digit industry. In columns 1–7, the outcome of interest is whether an industry was reformed during the study period. In columns 8–9, the outcome is whether an industry was reformed in 2006, and we exclude industries deregulated in 2001. This is because foreign equity data are only available in Prowess from 2001, and therefore, the growth rate is not defined for the 2001 reform. Industry-level FDI (column 7) is the level in 2001. “Ihs” in columns 7 and 9 corresponds to the inverse hyperbolic sine log transformation, defined as: $\log[X + (X^2 + 1)^{1/2}]$. Industries are weighted by size (measured as total capital in 2000, the last pre-treatment year).

²⁸To ensure we have enough observations to estimate the log variance of MRPK, we restrict the sample in Table II to industries with at least five firms in 2000. For consistency, we use the same sample in columns 1–7. Using the larger sample for the remaining columns does not affect the results.

Column 1 reports the association of industry-level log variance of MRPK with the reforms. Industries with an ex ante greater dispersion in their wedges are *not* more likely to be liberalized. Other variables, such as the number of firms (column 2) and the average firm size measured in capital in a given industry (column 3), also do not predict the deregulation. Column 4 shows that the share of sales from exporting in an industry also does not predict liberalization, despite research suggesting that liberalizing exporting industries will be more politically palatable, as it will not increase competition with domestic firms (Pandya (2014)). Columns 5 and 6 further show that the political economy variables that predicted the 1991 reforms (the sales share of state-owned firms and the Herfindahl Index) do not predict the subsequent reform. Finally, columns 7–9 show that pre-existing levels or trends in industry-level FDI do not predict the reforms.²⁹ Industries that opened up to foreign investors in 2001 and 2006 are similar to other industries across a wide array of covariates pre-reform.

The lack of association between political economy variables and the reforms may be because the 2001 and 2006 reforms were much smaller and would have likely attracted less political pressure than the 1991 reform. The context of these reforms was also very different, with India now trying to attract FDI, rather than being forced by external pressures to liberalize. Finally, the lack of association between industry characteristics and the reform may also reflect the fact that the reforms in the 2000s have been characterized as “disorganized” (Singh (2005)).

Endogeneity of Foreign Equity Flows. While it is likely that, within an industry, foreign capital is targeted toward specific firms, we do not use observed variation in foreign capital in our regressions. Instead, we exploit an exogenous shifter to the amount of foreign capital an industry can receive. Therefore, to be unbiased, β_1 and β_2 do *not* require that foreign capital is allocated randomly across firms *within* treated industries. As long as the parallel trends assumption discussed above holds at the industry level, our approach delivers valid estimates of the effect of liberalizing industry-level access to foreign capital.

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

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

²⁹In columns 8 and 9, we examine whether changes in foreign equity among listed firms predict the reform. Since these data are only available from 2001 onward, our outcome is the 2006 reform in these columns. Since we exclude industries reformed in 2001, the sample size is smaller.

Investors Allocate FDI in Response to Characteristics Besides MRPK. Our test of the effect of the policy on within-industry 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. It could be, for example, that foreign investors invest more in large, well-established low MRPK firms. As low MRPK firms may already be saturated with capital (unlike high MRPK firms), this could reduce these firms' demand for capital from the domestic financial sector, freeing up resources that could then be redirected to smaller, high MRPK firms. In this case, greater access to capital for high MRPK firms would be a "by-product" of greater access to capital at the industry level.³⁰ Regardless of whether foreign investors can identify and directly target high MRPK firms or not, foreign capital liberalization policies reduce within-industry misallocation if they lead to a relative increase in capital for ex ante high MRPK firms. Nonetheless, in Section 5.1, we provide suggestive evidence on whether foreign investment targets high MRPK firms.

5. RESULTS

5.1. First Stage: Access to Foreign Capital

Before estimating equation (5), we provide industry-level evidence that foreign capital flows increased in treated industries. Additionally, using two different proxies for accessing foreign capital, we provide suggestive evidence that increased access to foreign capital was concentrated in ex ante high MRPK firms.

Industry-Level Evidence. First, we obtain evidence on industry-level FDI flows by exploiting the "Equity Composition" module of Prowess, which was collected from 2001 onward. This module, which focuses on publicly-listed firms, covers less than a quarter of firms in our sample. Therefore, these data cannot be analyzed at the firm level in our difference-in-differences framework. Instead, we aggregate the total FDI flows to listed firms at the industry-year level. In Figure 1, we report the overall amount of foreign equity when we bundle industries by year of FDI regulation and normalize the flows to their initial, 2001 levels. While we cannot observe if there is a trend-break for industries treated in 2001, since there is no pre-period, the figure suggests that the growth in FDI accelerates over the post-period (the solid line) relative to all the sectors whose status did not change over the period (the dotted line). The effect is even more striking for industries that liberalized in 2006 (the dashed line), as there is a clear trend-break after 2006.

Firm-Level Evidence. To analyze the reform's firm-level effects, we use two proxies for access to foreign capital. First, we take advantage of information on another form of foreign financing: foreign debt. Second, we observe whether a firm implements a large capital project that is at least partially financed by foreign investors.

Since 2004, all Indian firms must report any foreign loans to the Reserve Bank of India (RBI) in a database called "External Commercial Borrowings." We scraped these data from the RBI website and match all foreign loan information to Prowess using the name

³⁰It is impossible to test this directly because India does not have a credit registry. However, there is recent evidence that a similar reallocation of capital happened in Europe when the ECB introduced its program purchasing corporate bonds. Arce, Mayordomo, and Gimeno (2021) reported that large firms with access to the bond markets issued more bonds, leading to a drop in the demand for bank loans by bond issuers. This led banks to increase their supply of credit to smaller, non-bond issuing firms that were constrained prior to the ECB intervention.

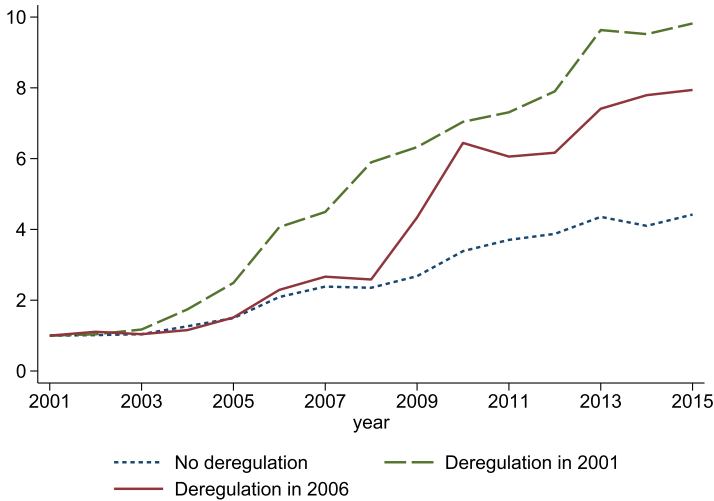


FIGURE 1.—Flow of foreign equities. *Note:* This figure plots the overall amount of foreign equity in Prowess for industries that have deregulated in 2001 (the red line), in 2006 (the green line), or whose regulation did not change during the period 1995–2015 (the blue line). The flows are normalized to 1 in 2001.

of the company. We then construct two outcome measures: (1) an indicator variable equal to 1 if a firm has ever accessed the foreign market, and (2) the inverse hyperbolic sine of the cumulative amount of foreign loans.³¹ Acknowledging the limitation that we only have two pre-treatment years and can only analyze the effects of the 2006 reform, we estimate our difference-in-differences with heterogeneous effects specification. Table III reports the results. The odd columns report the average effect of the reform, and the even columns report the heterogeneous effects by MRPK. The estimates indicate that ex ante high MRPK firms differentially increase any access of foreign debt by 6 percentage points and increase their total foreign debt by 96%.³²

For our second measure, we use another data set maintained by CMIE—CapEx—to further measure increases in foreign capital flows. CapEx compiles data on all projects that entail a capital expenditure of 10 million Rupees or more (roughly 135,000 USD) in India.³³ The data include a company identifier that can be matched to Prowess and an ownership variable indicating whether the project is foreign-owned or not. We emphasize that these data certainly undercount foreign capital flows, since a project is unlikely to be marked as foreign-owned if it is minority foreign-funded, since not all FDI will result in capital projects, and since the data do not include smaller capital projects. We report the same specifications as above with an indicator variable equal to 1 if a firm has any foreign-owned project (columns 5–6) and the inverse hyperbolic sine of cumulative

³¹The inverse hyperbolic sine transformation of the log function (e.g., [Burbidge, Magee, and Robb \(1988\)](#), [MacKinnon and Magee \(1990\)](#)) is defined as: $\log[X + (X^2 + 1)^{1/2}]$. Except for very small values of X , the *ihs* is approximately equal to $\log(2X)$ or $\log(2) + \log(X)$ and can be interpreted in exactly the same way as a standard logarithmic dependent variable. However, unlike a log transformation, the inverse hyperbolic sine is defined at zero and is not overly sensitive to jumps around zero, unlike the more classic $\log(x + 1)$ transformation.

³² Note that this increase in foreign debt does not imply anything about the substitutability or complementarity of debt and equity since we do not observe total debt. Increases in foreign debt can occur even if firms hold their total debt fixed or reduce it.

³³ Examples of projects include the creation, expansion, or renovation and modernization of factories or retail establishments.

TABLE III
EFFECT OF THE REFORM ON PROXIES FOR FOREIGN DIRECT INVESTMENT.

Dependent Variable	Any Foreign Debt		Ihs(Foreign Debt)		Any Foreign Project		Ihs(Foreign Spending)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Reform_{jt}$	0.030 (0.019)	-0.005 (0.013)	0.451 (0.345)	-0.078 (0.213)	0.004 (0.003)	0.0002 (0.003)	0.029 (0.022)	0.001 (0.020)
$Reform_{jt} \times I_i^{HighMRPK}$		0.064 (0.025)		0.963 (0.326)		0.007 (0.002)		0.050 (0.018)
<i>Fixed Effects</i>								
Firm	✓	✓	✓	✓	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓	✓	✓	✓	✓
Size \times Year	✓	✓	✓	✓	✓	✓	✓	✓
Observations	61,156	61,156	61,156	61,156	61,156	61,156	61,156	61,156

Note: This table reports the effect of the reform on access to foreign debt and the cost of capital projects financed by foreign investors. In columns 1–4, we use the universe of foreign loans since 2004 tracked by the RBI to study if firms issue any foreign debt (columns 1–2) or to estimate the total change in foreign debt (columns 3–4). Due to the large number of zeros, we use the inverse hyperbolic sine transformation (denoted $Ihs(x)$) in columns 3–4 and 7–8. In columns 5–8, we use the data set CapEx, compiled by CMIE, which contains data on all large capital projects in India. The outcomes are an indicator variable for whether a firm is associated with a foreign-owned project (columns 5–6), and the Ihs of the total spending on foreign-owned projects associated with the firm (columns 7–8). Standard errors are two-way clustered at the 4-digit industry and year level.

spending on foreign projects (columns 7–8) as our outcome variables. We find similar patterns to the ones for foreign debt. Ex ante high MRPK firms are more likely to have any foreign project post-treatment and have larger spending on foreign-owned projects. While certainly not conclusive, these results are consistent with ex ante high MRPK firms receiving more foreign capital as a result of the reforms.

5.2. Average Effects

We estimate the effect of the reform on the average firm's financial outcomes by removing the interaction term $Reform_{jt} \times I_i^{HighMRPK}$ from equation (5). Table IV reports the results. The estimates indicate that the liberalization policy has positive effects on firm investments overall. For the average firm, capital increases by 32% (column 2), while average MRPK declines by 18.7% (column 4), both significant at the 5% level. Based on levels in 2000, the increase in capital in treated industries is equivalent to an average firm-level increase of approximately 3.7 million USD in the post-treatment period. The point estimates for the total wage bill and revenues are also positive, albeit not significant at conventional levels (the wage bill is borderline significant with $p = 0.12$). Figure 2 plots the event study graph for the average effects on capital, reporting the yearly effect of belonging to a treated industry before and after the reform, including the same controls as in Table IV. Here, 0 is normalized to be the year before a reform. Consistent with the absence of differential pre-trends, we see that there is no effect of belonging to a treated industry before the reform took place. Additionally, the point estimates suggest that the full effects of the reform are not felt until 5 years after its initiation.

These results suggest that FDI liberalization increased output in India by allowing firms to grow on average. However, when firms have heterogeneous MRPKs, the positive effect of increasing capital on treated industries' aggregate output can be amplified or attenuated depending on which types of firms (those with high or low MRPK) benefit more from this increase in capital. Thus, we next examine how the increase in capital was distributed.

TABLE IV
AVERAGE EFFECT OF THE FOREIGN CAPITAL LIBERALIZATION.

Dependent Variable	Revenues	Capital	Wages	MRPK
	(1)	(2)	(3)	(4)
$Reform_{jt}$	0.108 (0.094)	0.318 (0.118)	0.159 (0.098)	-0.187 (0.088)
<i>Fixed Effects</i>				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size × Year	✓	✓	✓	✓
Observations	58,391	60,096	59,162	57,017

Note: All dependent variables are in logs. $Reform_{jt}$ is an indicator variable equal to 1 if the industry had liberalized access to the international capital market in or before year t and zero otherwise. Size × Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. In column 4, MRPK is computed using $Revenue/K$ as a proxy for the marginal revenue product of capital. Standard errors are two-way clustered at the 4-digit industry and year level.

5.3. Differential Effects by Ex Ante MRPK

Baseline Specification. Table V reports the estimates of the heterogeneous effects of the policy from equation (5), our main estimating equation. Following the liberalization, high MRPK firms generate higher revenues by 23% (column 1) relative to low MRPK firms (approx. 8.5 million USD). This is made possible by the fact that these firms invest more, with their physical capital differentially increasing by 53% (column 2) or 5.6 million USD. Higher investment does not crowd out labor. High MRPK firms also experience a relative increase in their wage bills by 28% (column 3) or 0.9 million USD, suggesting that there may be important complementarities between capital and labor. We will further

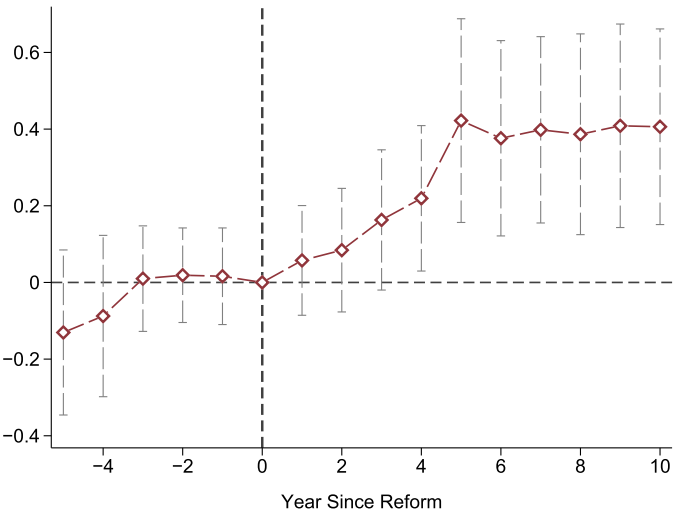


FIGURE 2.—Event study graph for the average effect of foreign capital liberalization on physical capital. Note: This figure reports the event study graph for the average effect of the liberalization on firms' physical capital. The dependent variable is in logs. The reform is normalized to take place in year 1. Each dot is the coefficient on the interaction between being observed t years after the reform and being in a treated industry. The confidence interval is at the 95% level.

TABLE V

HETEROGENEOUS EFFECTS OF FOREIGN CAPITAL LIBERALIZATION BY FIRMS' EX ANTE MRPK.

Dependent Variable	Revenues (1)	Capital (2)	Wages (3)	MRPK (4)
$Reform_{jt} \times I_i^{High\ MRPK}$	0.226 (0.076)	0.527 (0.075)	0.280 (0.058)	-0.326 (0.107)
$Reform_{jt}$	-0.024 (0.125)	0.017 (0.089)	-0.002 (0.097)	0.004 (0.102)
<i>Fixed Effects</i>				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size \times Year	✓	✓	✓	✓
Observations	58,391	60,096	59,162	57,017

Note: All dependent variables are in logs. Firms are classified as high MRPK if their average MRPK in the pre-treatment period from 1995–2000 is above the 4-digit industry median. MRPK is estimated with the *Revenue/K* method. Size \times Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are two-way clustered at the 4-digit industry and year level.

explore whether the reform also reduced labor misallocation in Section 5.5. Among the ex ante high MRPK firms, the policy relatively reduced MRPK by 33% (column 4). Given that, prior to the reform, high MRPK firms had a MRPK more than twice as high as low MRPK firms, this implies that the reform shrank but did not fully eliminate the gap between high and low MRPK firms' MRPK. That is, the reform led to a decline in the dispersion of MRPK. Altogether, given the faster increase in capital for ex ante high wedge firms relative to low wedge firms, misallocation within treated industries appears to decline due to the policy.

These results also indicate that in this setting, where total capital in treated industries is increasing, the lognormality assumption required to measure changes in misallocation with changes in the variance of log TFPR is violated. Given that the estimates show that wedges decline for high MRPK firms by 32% after the reform, with no effects on low MRPK firms, even if the assumption of lognormality of TFPR held before the policy change, it would be violated afterwards.

The fact that we do not see a symmetric effect where low MRPK firms revert to the mean also helps allay potential concerns about mean reversion. To further ensure our results are not driven by mean reversion, in Table A.II, we show that the results are robust to assigning high MRPK status using a shorter pre-treatment period (1995–1997 in Panel A and 1995–1998 in Panel B) or using only variation from the 2006 reform (Panel C). In all three cases, the years directly before the reform are not used to assign high MRPK status, so these results should be less affected by any mean reversion. In all three panels, we see that the estimates are similar to the baseline results in Table V.

Another potential concern arises because a recent literature has shown that difference-in-differences estimates from treatments where different units are treated at different points in time can be biased. In our context, such a bias would be driven by the comparison of firms that were treated in 2006 to both the never treated (during the study period) and to those treated in 2001 (e.g., Goodman-Bacon (2021), de Chaisemartin and D'Haultfoeuille (2022)). We do not expect this bias to be large since the vast majority of our observations are not treated during the study period (around 90%). Nonetheless, we show that our results are quantitatively similar when we only compare each treated group

to the never treated group. Appendix B of the Supplemental Material describes this test in more detail, and Table A.III reports the results. Table A.II also provides evidence that the results are robust to this type of bias, since it separately estimates the effects of the 2006 reform relative to firms that were not treated during the study period.

Finally, we use the same empirical strategy as in Table V to examine whether the composition of capital changed heterogeneously as a result of the reform. Table A.IV reports the results when the outcome variables are the share of a firm's capital in each category. Following the reform, high MRPK firms relatively increased the share of their capital in plants and equipment by 4 percentage points. There are no effects for low MRPK firms.

Pre-Trends & Time-Varying Shocks. To assess whether the results in Table V are driven by pre-trends, we produce event study graphs. We create indicator variables for being observed five years before a reform, four years before, and so on and interact these with being in a treated industry and being a high MRPK firm in a treated industry. We include the same controls as in Table V. Figure 3 reports the relative effects by year of being a high MRPK firm in a treated industry for the logs of physical assets, MRPK, wages, and sales. The key outcome for assessing whether misallocation declined is capital.

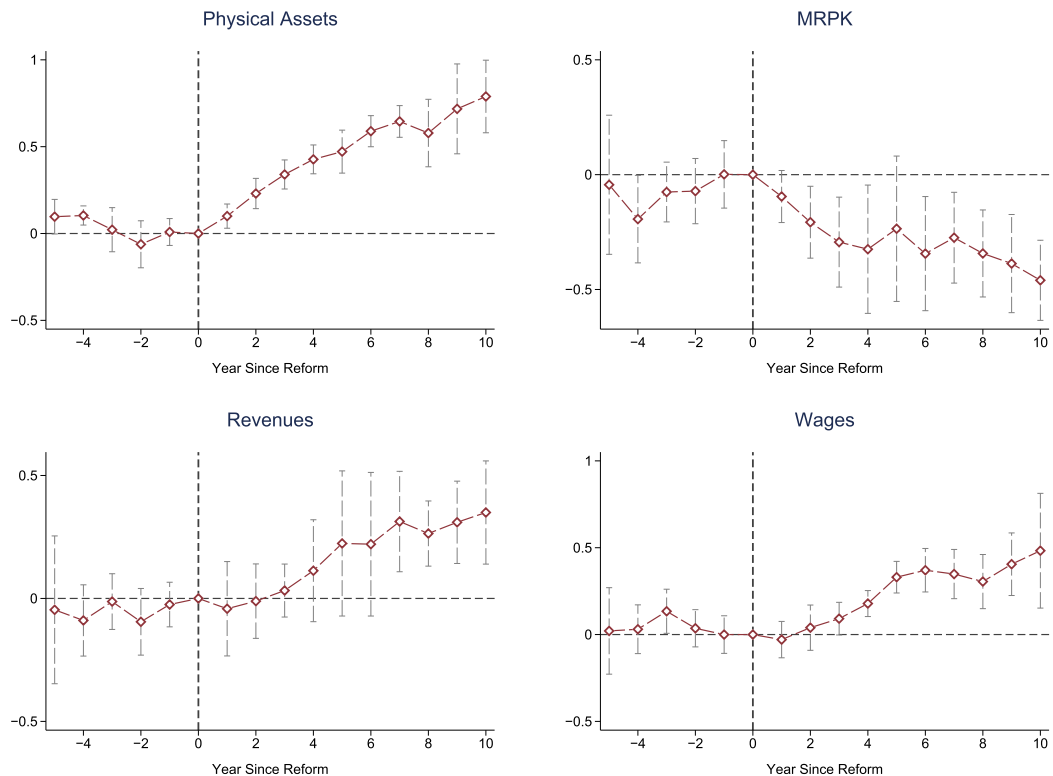


FIGURE 3.—Event study graphs for the relative effect of foreign capital liberalization on high MRPK firms. *Note:* This figure reports event study graphs for the relative effects of the liberalization on firms with high pre-treatment MRPK relative to those with low pre-treatment MRPK in treated industries relative to untreated industries. The reform is normalized to take place in year 1. Each dot is the coefficient on the interaction between being observed t years after the reform and being a high MRPK firm in a treated industry. All dependent variables are in logs. The confidence intervals are at the 95% level.

Two facts are noteworthy. First, for all the outcomes, including the main outcome of capital, being treated by the policy did not have a strong differential effect on high MRPK firms before the policy was adopted, providing visual evidence that pre-trends are not driving the results.

Second, the effect of the liberalization is progressive over time, consistent with the idea that changes in inputs (such as the adjustment of worker flows and adaptation of production tools) are likely slow-moving, particularly in India (e.g., Topalova (2010)). In addition, some of the changes in allocative efficiency might also come from competitive effects, which also happen progressively over time. The patterns in the graphs suggest that the full effects of the reform take at least five years to materialize. Thus, while the relative effects on ex ante high MRPK firms averaged over the post-treatment period for capital and MRPK are +53% and -33%, respectively, effects of this size take 3–4 years to materialize, and by 10 years after the policy change, the relative effects on capital and MRPK are +79% and -46%.

To provide further evidence in favor of our identification strategy, we also plot event study graphs separately for high and low MRPK firms for each of the four outcomes. Figure 4 reports the results. Consistent with our previous estimates, the reform has no

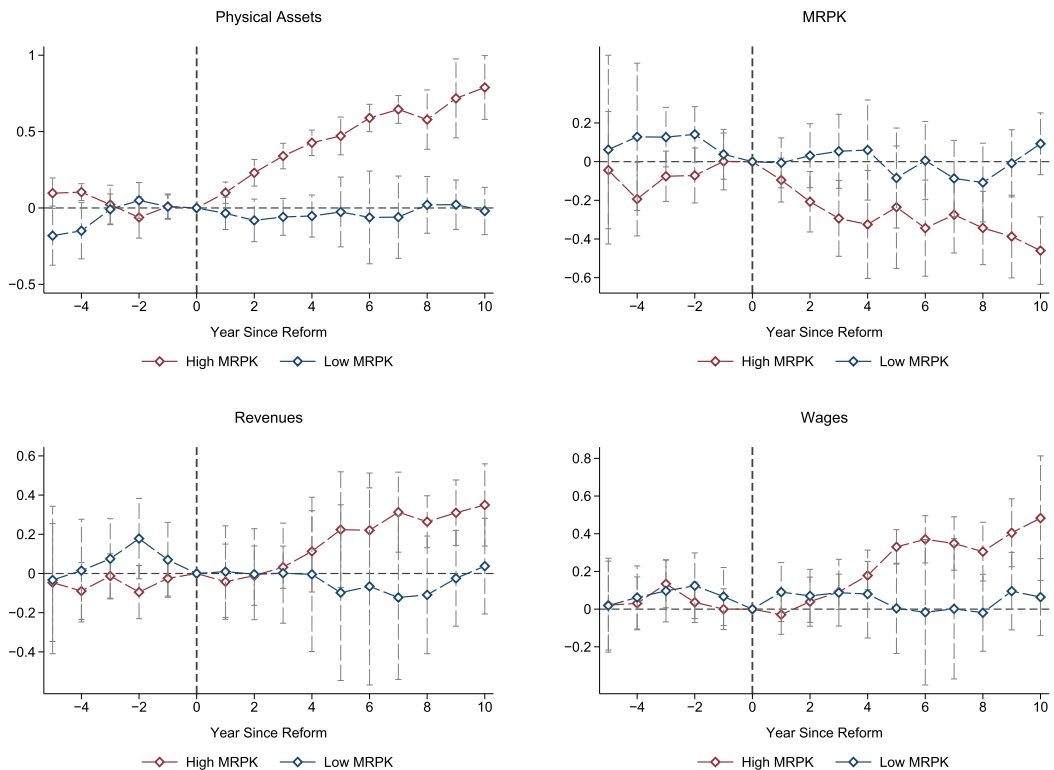


FIGURE 4.—Separate event studies for high and low MRPK firms. *Note:* This figure reports the effect of FDI deregulation for high and low MRPK firms separately for physical assets, MRPK, revenues, and the wage bill. The dependent variables are in logs. The reform is normalized to take place in year 1. Each dot is the coefficient on the interaction between being observed t years after the reform and being in a treated industry. The confidence interval is at the 95% level.

effect on low MRPK firms across outcomes, while high MRPK firms' outcomes change sharply following the reform.

Finally, to further rule out bias from other time-varying shocks, we augment our baseline specification with different sets of industry or state-by-year fixed effects to capture various unobserved time-varying shocks. In Panel A of Table A.V, we report the results using 2-digit industry-by-year fixed effects.³⁴ Here, the coefficients are identified by comparing firms in the same 2-digit industry and year; this 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. We find that the coefficients of both $Reform_{jt}$ and $Reform_{jt} \times I_i^{HighMRPK}$ are unaffected. In Panel B, we report the results when we control for 5-digit-by-year fixed effects. In this case, $Reform_{jt}$ is absorbed, and we only exploit within industry-year variation. The coefficient for the interaction variable $Reform_{jt} \times I_i^{HighMRPK}$ remains similar. In Panel C, we include state-by-year fixed effects to account for the possibility that some Indian states are more exposed to the reform due to their industrial composition and may have instituted policies affecting misallocation or were affected by shocks concurrent with the reform. Our point estimates remain quantitatively similar.

Additional Robustness. Here, we further explore whether the results in Table V are robust to accounting for other Indian policies, alternative approaches to measurement error, spillovers, and differential attrition.

Controlling for dereservation laws. To assess whether dereservation policies could be driving our main results, we perform two tests, both reported in Table A.VI. 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).³⁵ Because this cuts our sample by more than half, in even columns, we create an indicator variable $Dereservation_{jt}$ that is equal to 1 after industry j has been dereserved and control for it and its interaction with $I_i^{HighMRPK}$. In both cases, the pattern of the point estimates is largely unchanged.

Controlling for trade liberalization. Our specification with industry-year fixed effects already partially accounts for potential bias from tariff reductions, since the trade liberalization occurred at the industry level. However, it is possible that trade liberalization had a differential effect on high and low MRPK firms. To account for this, we include both the input and output tariff measures and their interaction with $I_i^{HighMRPK}$ as controls in our main regression specification. Table A.VII reports the results when we control for the output tariffs only (the odd columns) or both the output and input tariffs (the even columns). The effect of the foreign capital liberalization on high MRPK firms remains virtually unchanged.

Winsorizing outliers. We directly test the extent to which our results might be driven by outliers by winsorizing the data at the 5% level. We identify outliers either across indus-

³⁴There are 23 distinct 2-digit industries.

³⁵To develop our dereservation measure, we use the list of deregulated industries in ASICC from Boehm, Dhingra, and Morrow (2022) and create a crosswalk between ASICC and our definition of industry (NIC 2008) by using the ASI 2008–2009. For each establishment in the ASI, the data report 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.

tries or within each 2-digit industry. We report the results in Table A.VIII and show that the point estimates are similar to those without a measurement error correction.

Firm entry and exit. To test whether differential attrition could affect our results, we directly test whether the policy affected firm exit and entry using industry-level variation in the policy over time. If the policy had no effect on attrition, attrition should not bias our results. We identify entry in the data using the year of incorporation and use the last year in the data set as a proxy for exit.³⁶ To estimate the average effect of the policy on exit and entry, we then create counts of the number of firms in a 5-digit industry-year cell that exited or entered. To estimate the differential effect on exit for high and low MRPK firms, we create these counts for industry-year-MRPK category cells. We cannot use the same strategy to test for differential entry, since, if a firm enters after 2000, we do not observe its MRPK during the pre-treatment period. Table A.IX reports the results. There is little evidence that the policy affected entry and exit.³⁷

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:

$$Upstream_{k,t} = \sum_l (Input\%_{l \rightarrow k}^{2000} - \mathbf{1}_{l=k}) \times Reform_{l,t}$$

and

$$Downstream_{k,t} = \sum_l (Output\%_{k \rightarrow l}^{2000} - \mathbf{1}_{l=k}) \times Reform_{l,t},$$

where k and l represent 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 $Input\%_{l \rightarrow k}$ represents the elements of the input-output matrix $\mathbf{A} = [a_{ij}]$, where $a_{ij} \equiv \frac{Sales_{j \rightarrow i}}{Sales_i}$ measures the total sales of inputs from industry j to industry i , as a share of the total inputs of industry i . The notation $Output\%_{k \rightarrow l}$ denotes the input-output matrix $\hat{\mathbf{A}} = [\hat{a}_{ij}]$, where $\hat{a}_{ij} \equiv \frac{Sales_{i \rightarrow j}}{Sales_i} = a_{ji} \frac{Sales_j}{Sales_i}$ 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.

Table A.X reports the average and heterogeneous effects, controlling for the upstream and downstream effects, and shows that they are unchanged.

Minimal controls and unfiltered data. We next show that our results are robust both to including only firm and year fixed effects as controls (removing all additional controls) and to retaining the full set of observations rather than dropping observations if firms contract their year-to-year revenues by more than 85%. Table A.XI reports the results

³⁶True 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.

³⁷This 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.

with the minimal controls, and Table A.XII reports the results with the unfiltered sample. The estimates are again very similar.

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

Alternative cut-off. In Table A.XIV, we report our baseline specification when we split ex ante MRPK along the mean instead of the median (odd columns) or when we split the data into ex ante MRPK terciles instead of using the median (even columns), and find similar results. The use of the tercile split allows us to test whether the effect is monotonic with the degree of ex ante misallocation in a nonparametric way, as the coefficient on each interaction $Reform_{jt} \times I_i^{High\ MRPK-Tercile=x}$ gives the *marginal* effect for tercile x relative to the first tercile. For all outcomes, the effect shows a relative increase over each tercile, implying that firms that are “medium” constrained (second tercile) respond more than firms that are less constrained (first tercile) but not as much as firms that are “very” constrained (third tercile).

Representativeness. While Prowess provides us with firm-level panel data, it is not fully representative of the formal manufacturing sector. To ensure that our results hold in a data set that captures almost all the sales in the manufacturing sector at the time of the reforms, we turn to the ASI, a representative sample of the universe of formal manufacturing establishments. Since the ASI is not a panel for smaller establishments, we focus on industry-level difference-in-differences regressions and weight industries by their ex ante (2000) size in terms of capital since some industries include only a few firms. Furthermore, we control for 2-digit industry-year fixed effects to help account for differential time trends across industries. The firm-level results in Table V imply that the policy caused the variance of MRPK to fall in treated industries. Table VI uses 4-digit industry-level differences-in-differences regressions to first confirm that this is the case in Prowess (Panel A) and then evaluate whether it is the case in the ASI (Panel B).³⁸ Panel A shows that the policy increased industry-level capital by 28% (column 1), which is very close to the firm-level estimate, and reduced the dispersion of MRPK by 40–70%.³⁹ This result is robust to the inclusion of a control for the log number of firms in an industry-year (to account for cases where variance is estimated over a small number of firm-year observations). In Panel B, we replicate the same regressions using the ASI. The results are similar. Total capital increases even more, by 62%. The variance of MRPK falls by a similar amount to the Prowess estimates ($\approx 80\%$). Thus, the results in Prowess appear to be representative of patterns in the formal sector.

³⁸Since industry-level aggregates will be more sensitive to firm-level measurement error, we winsorize the top and bottom 5% of capital and revenues measures within each industry-year before constructing these aggregates.

³⁹We focus on 4-digit level industries for consistency with the ASI, where multiple changes in the industry coding system make assigning consistent 5-digit level industries impossible.

TABLE VI
INDUSTRY-LEVEL CAPITAL AND VARIANCE OF MRPK IN PROWESS AND THE ASI.

Dependent Variable	Total Capital	Variance(MRPK)	
	(1)	(2)	(3)
<i>Panel A: Prowess</i>			
Share of (4-digit) Industry Treated	0.275 (0.145)	−0.401 (0.168)	−0.684 (0.320)
<i>Fixed Effects</i>			
Industry (4-digit)	✓	✓	✓
Industry (2-digit) × Year	✓	✓	✓
<i>Controls</i>			
Nb firms	—	—	✓
Observations	2278	1789	1789
<i>Panel B: ASI</i>			
Share of (4-digit) Industry Treated	0.616 (0.270)	−0.783 (0.327)	−0.771 (0.321)
<i>Fixed Effects</i>			
Industry (4-digit)	✓	✓	✓
Industry (2-digit) × Year	✓	✓	✓
<i>Controls</i>			
Nb firms	—	—	✓
Observations	2184	2120	2120

Note: This table reports the effects of the reform on log total 4-digit industry-level capital and the 4-digit industry-level log variance of MRPK in Prowess (Panel A) and the ASI (Panel B). ‘Nb firms’ refers to a control for the log number of firms in an industry-year. Standard errors are two-way clustered at the 4-digit industry and year level.

TFPQ. Motivated by the literature documenting a relationship between FDI and within-firm productivity due to technological transfers (e.g., Keller and Yeaple (2009), Gorodnichenko, Svejnar, and Terrell (2014)), Table VII estimates the effects of the reform on TFPQ. While the reform changed the allocation of inputs across the firms, we cannot reject a zero effect on within-firm average productivity (column 1). Though imprecise, the point estimate is consistent with a meaningful positive effect (+16%). Similarly, when we interact the reform with the indicator variable for high MRPK (column 2), we do not find any statistically significant differential effect. However, to the extent that the effects of the policy on TFPQ are positive, when we estimate the effects of reducing misallocation on the Solow residual in Section 6, we may underestimate the total gains from the policy.

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 liberalization is acting as a substitute for a more efficient domestic banking sector, a natural implication is that capital will increase less among ex ante high MRPK firms in areas with more developed local banking markets prior to the reform. We directly test this hypothesis by creating a variable *Financial Development_s*, defined as the log average over 1995–2000 of all bank credit in state *s*. We then interact this measure with all the single and cross-terms in equation (5). The variable is de-meanned to restore the baseline effect on $Reform_{jt} \times I_i^{HighMRPK}$. The coefficient of interest is the coefficient for the triple interac-

TABLE VII
EFFECT OF FOREIGN CAPITAL LIBERALIZATION ON TFPO.

Dependent Variable	TFPO (1)	TFPO (2)
$Reform_{jt}$	0.157 (0.166)	0.106 (0.143)
$Reform_{jt} \times I_i^{HighMRPK}$		0.084 (0.074)
<i>Fixed Effects</i>		
Firm	✓	✓
Firm Age	✓	✓
Size \times Year	✓	✓
Observations	43,791	43,791

Note: All dependent variables are in logs. Firms are classified as high MRPK if their average MRPK in the pre-treatment period from 1995 to 2000 is above the 4-digit industry median. MRPK is estimated with the *Revenue/K* method. Size \times Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. TFPO is measured by estimating revenue production functions using the methodology of Levinsohn and Petrin (2003) and subtracting log average price from log TFP. Standard errors are two-way clustered at the 4-digit industry and year level.

tion $Reform_{jt} \times I_i^{HighMRPK} \times Financial\ Development_s$, which captures the differential effect of the policy on high MRPK firms located in more developed local banking markets.

Table VIII reports the results.⁴⁰ For capital and wages, the interaction $I_i^{HighMRPK} \times Reform_{jt} \times Financial\ Development_s$ is negative and significant at the 1% level. For MRPK, the triple interaction is positive and significant. Taken together, these results imply that capital increased 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 whose state is at the 25th percentile of the bank credit distribution experience a decrease in MRPK of 45% ($-0.349 + (0.138 \times -0.71)$). In contrast, high MRPK firms whose state is at the 75th percentile of the bank credit distribution experience a decrease in MRPK of 16% ($-0.349 + (0.138 \times 1.37)$). Thus, the reduction at the 25th percentile is roughly three times larger than the one at the 75th percentile.

The fact that the effects of the policy are smaller in states where credit constraints were a priori lower helps confirm that opening up to foreign capital relaxed credit constraints and allowed previously constrained firms to invest more. However, we note that these results do not imply that foreign investors necessarily targeted high MRPK firms. Areas with less available credit ex ante may be more misallocated if certain firms have preferential access to that credit, and when FDI liberalization increases aggregate credit, this could free domestic credit to be reallocated to the ex ante high MRPK firms.

These results also provide further evidence that our main results are not driven by differences in the relative trends between high and low MRPK firms in treated versus untreated industries. For differential trends to explain these results, they would have to vary

⁴⁰The sample sizes are somewhat reduced from Table V since state information is not available for all firms.

TABLE VIII
HETEROGENEITY BY LOCAL FINANCIAL DEVELOPMENT.

Dependent Variable	Revenues (1)	Capital (2)	Wages (3)	MRPK (4)
$Reform_{jt} \times I_i^{HighMRPK} \times Financial\ Development_s$	-0.095 (0.078)	-0.233 (0.084)	-0.189 (0.060)	0.138 (0.047)
$Reform_i \times I_i^{HighMRPK}$	0.203 (0.079)	0.527 (0.092)	0.259 (0.064)	-0.349 (0.121)
<i>Fixed Effects</i>				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size \times Year	✓	✓	✓	✓
Observations	53,109	54,692	53,852	51,873

Note: All dependent variables are in logs. $Reform_{jt}$ is an indicator variable equal to 1 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 to 2000 is above the 4-digit industry median. MRPK is calculated using the *Revenue/K* method. Size \times Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Local financial development is proxied using the log average amount of bank credit in the state in the pre-treatment period. All double and single interactions of the triple-differences specification are included in the regressions. Standard errors are two-way clustered at the 4-digit industry and year level.

systematically with states' financial development. These results also suggest that underdeveloped domestic banking markets are an important source of misallocation in India (consistent with Krueger et al. (2002)) and that foreign capital liberalization can act as an alternative for developing the banking sector.⁴¹ Consistent with our interpretation, capital is ex ante less efficiently allocated in less financially developed states. Figure 5 shows that the average dispersion in MRPK before the reform is higher in states with lower domestic financial development.

5.4. Product Outcomes

We next estimate the effects of the policy on product-level outcomes, including prices and output. Opening up to foreign capital can reduce prices for two reasons. If liberalization reduced the wedges on capital for high MRPK firms, these firms' 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 IX report the results. On average, the reform reduces prices by 17% (column 1). Column 2 shows that the reduction is mainly driven by high MRPK firms, who reduce their prices (in total) by 21% ($= -0.084 - 0.123$).

⁴¹Anne Krueger, deputy managing director of the IMF during the time of the reforms we study, wrote that in India, "banks are considered to be very high cost and inefficiently run" and that, "enabling [Indian banks] to allocate credit to the most productive users, rather than by government allocation, would make a considerable contribution to the Indian economy's growth potential" (Krueger et al. (2002)).

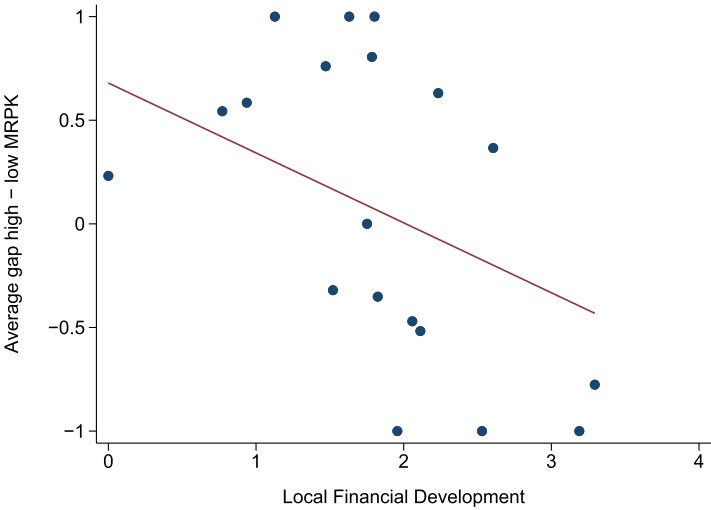


FIGURE 5.—Ex ante MRPK dispersion by state-level financial development. *Note:* This figure plots a binscatter plot of a measure of the pre-treatment dispersion of MRPK by state-level financial development. For each state-industry bin, we compute the average MRPK for high and low MRPK firms and take the difference. We then express these differences as a fraction of the total distance (namely, we compute: $[(MRPK_{high} - MRPK_{low}) / (MRPK_{high} + MRPK_{low})]$) and plot this measure against state-level financial development.

TABLE IX
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)
$Reform_{jt}$	-0.171 (0.031)	-0.084 (0.053)	0.268 (0.058)	0.097 (0.087)	0.004 (0.025)	-0.075 (0.016)	-0.005 (0.035)
$Reform_{jt} \times I_i^{High\ MRPK}$		-0.123 (0.065)		0.242 (0.125)	0.034 (0.014)	0.097 (0.020)	-0.051 (0.048)
<i>Fixed Effects</i>							
Firm	✓	✓	✓	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓	✓	✓	✓
Size × Year	✓	✓	✓	✓	✓	✓	✓
Firm × Product	✓	✓	✓	✓	—	—	—
Observations	103,035	103,035	103,974	103,974	32,660	32,660	32,660

Note: 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 to 2000 is above the 4-digit industry median. MRPK is calculated using the *Revenue/K* method. Size × Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are two-way clustered at the 4-digit industry and year level.

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

high MRPK firms increased output by 24% relative to low MRPK firms, while we cannot reject a zero effect on low MRPK firms' output.

In the last three columns of Table IX, we examine whether the policy affected the product portfolio of treated firms. Column 5 indicates that the number of products offered relatively increased for high MRPK firms (by 3%) but was unchanged for low MRPK firms. Low MRPK firms were 8 percentage points less likely to add new products (column 6) but not more likely to delete products (column 7). Relative to low MRPK firms, high MRPK firms were almost 10 percentage points 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.

5.5. Extension to Labor Misallocation

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

Although labor is often modeled as a fully adjustable variable input across periods, 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 payments to labor and the generation of cash-flows, financial constraints may affect employment and labor (mis)allocation. Fonseca and Van Doornik (2022), Schoefer (2021), and Fonseca and Matray (2022) provided evidence in support of this channel.

To investigate if the reform reduced labor misallocation within treated industries, 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 X reports the results. Following the reform, high MRPL firms relatively increase their total wage bill (column 3) by 24% (about 0.5 million USD). Among ex ante high MRPL firms, MRPL decreases by 28% relative to low MRPL firms (column 4). This closes about 20% of the gap in MRPL between ex ante high and low MRPL firms. By allowing high MRPL firms to grow faster and to expand employment, the deregulation appears to have reduced labor misallocation within treated industries. Table A.XV reports the estimates for the industry-level variance of MRPL in Prowess and the ASI. In both data sets, the variance of MRPL falls in response to the policy, though the estimates in the ASI are imprecise.

6. AGGREGATE EFFECTS

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

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

TABLE X
EFFECT OF FOREIGN CAPITAL LIBERALIZATION BY FIRMS' EX ANTE MRPL.

Dependent Variable	Revenues (1)	Capital (2)	Wages (3)	MRPL (4)
$Reform_{jt} \times I_i^{High MRPL}$	-0.039 (0.086)	0.198 (0.119)	0.237 (0.069)	-0.276 (0.061)
$Reform_{jt}$	0.120 (0.105)	0.121 (0.072)	0.024 (0.104)	0.096 (0.096)
<i>Fixed Effects</i>				
Firm	✓	✓	✓	✓
Firm Age	✓	✓	✓	✓
Size \times Year	✓	✓	✓	✓
Observations	43,407	43,384	43,407	43,407

Note: All dependent variables are in logs. High MRPL firms are defined in an analogous way to high MRPK firms using the Revenue/L method. $Reform_{jt}$ is an indicator variable equal to 1 if the industry has liberalized access to the foreign capital market. Size \times Year are quartile fixed effects for firms' average pre-treatment capital interacted with year fixed effects. Standard errors are two-way clustered at the 4-digit industry and year level.

where λ_i is the ratio of firm i 's sales to treated industry I 's net output, $\Delta \log A_i$ is the change in TFPQ, α_i^x is the output elasticity with respect to x , τ_i^x is the level of firm-specific input wedges prior to the policy change, and $\Delta \log x_i$ is the change in the log input x consumed by firm i . We note that this equation can be used to capture *changes* in the Solow residual due to the policy under relatively weak assumptions but cannot be used to calculate the counterfactual effects of alternative policies or to measure the effect of eliminating all misallocation in the Indian economy.

6.1. Identification

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

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

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

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

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

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

To estimate equation (6), observe that most components of this expression are readily identifiable in the data or given by our estimates from the natural experiment. The sales shares of net output λ_i can be computed using input-output data,⁴² and under the assumption that α_i^x is constant within an industry, the industry-level output elasticity α_j^x can be estimated using the production function estimation. Under the standard difference-in-differences assumption that untreated industries are unaffected by the policy, $\Delta \log x_i$ can be predicted from difference-in-differences regressions with heterogeneous effects where log usage of each input is the outcome variable. We note that this assumption rules out spillovers from treated to untreated industries due to general equilibrium effects.⁴³

The final terms that need to be identified are the initial wedges τ_i^x . We provide two alternative estimates: the first is a less conservative, conventional approach that attributes cross-sectional variation in sales to wage bills and sales to capital costs ratios within each industry to wedges. The second approach provides a lower bound for the effect of the reform under an assumption that we discuss below.

The “Conventional” Approach to Estimating τ_i^x . To start with, we identify τ_i^x by attributing all pre-treatment, cross-sectional deviations of expenditure shares relative to output elasticities in the data to misallocation. In other words, we use the relationships $\tau_i^K = \alpha_j^K \frac{p_i y_i}{r K_i} - 1$ and $\tau_i^L = \alpha_j^L \frac{p_i y_i}{w L_i} - 1$, where r is the rental rate of capital and $w L_i$ is the wage bill.⁴⁴ The wage bill $w L_i$ and sales $p_i y_i$ are observable in the last pre-treatment year (2000) in Prowess, and α_j^K and α_j^L are given by production function estimation. For the rental rate of capital r , we follow Hsieh and Klenow (2009) and set $r = 10\%$. In practice, since the capital wedges are decreasing in r , the estimated aggregate effect will also be decreasing in r . Thus, our choice of a relatively low value of $r = 10\%$ for India is consistent with our goal of calculating a less conservative measure (compared to the lower bound below).

A More Conservative, Lower Bound Approach to Estimating τ_i^x . Unfortunately, measurement error in expenditure shares may affect the estimates above. Since the effects of the reform appear to be very asymmetric, raising inputs for firms with high τ_i^x and not affecting inputs for those with low τ_i^x , measurement error in the estimates of τ_i^x can inflate the aggregate effect of the reforms. This is because larger estimated wedges will be mul-

⁴²To measure total sales by sector I not re-used by firms in I as inputs, we sum over treated firms' total sales in 2000 (the last pre-treatment year). We then use information from the Annual Survey of Industries to compute the share of output that is re-used by the treated industries as inputs and scale total sales by 1 minus this value. Finally, λ_i is calculated for a firm i by dividing a firm i 's sales by this value.

⁴³The difference-in-differences assumption could be partially relaxed by modeling spillovers explicitly and estimating spillovers effects.

⁴⁴These relationships come from firms' cost-minimization problems. A cost-minimizing firm sets $\frac{rK(1+\tau_i^K)}{p_i y_i} = \alpha_j^K$ and $\frac{wL_i(1+\tau_i^L)}{p_i y_i} = \alpha_j^L$.

multiplied by the positive change in inputs.⁴⁵ For this reason, we also consider an alternative, more conservative, approach.

We begin by imposing Assumption 1.

ASSUMPTION 1: Denote the pre- and post-policy wedge by $\tau_i^{x,\text{pre}}$ and $\tau_i^{x,\text{post}}$. Then

$$0 \leq \tau_i^{x,\text{post}} \leq \tau_i^{x,\text{pre}} \quad \text{and} \quad \Delta \log x_i \geq 0, \quad \text{whenever } \tau_i^{x,\text{pre}} \geq 0,$$

and

$$\tau_i^{x,\text{pre}} \leq \tau_i^{x,\text{post}} \leq 0 \quad \text{and} \quad \Delta \log x_i \leq 0, \quad \text{whenever } \tau_i^{x,\text{pre}} < 0.$$

In words, the policy (weakly) shrank the wedges toward zero (i.e., wedges declined in magnitude and firms that were ex ante taxed did not become subsidized or vice versa) and inputs always (weakly) increased for firms with ex ante positive wedges and (weakly) decreased for firms with ex ante negative wedges. We can bound the aggregate effect under this assumption.

This assumption may not always be reasonable, but our reduced-form empirical results provide evidence that it likely holds in our context. First, the policy causally reduced MRPK (Table V) and MRPL (Table X) for firms that had ex ante above median values of MRPK and MRPL (and did *not* reduce MRPK and MRPL for firms with ex ante below median values), consistent with (weakly) shrinking wedges toward zero. Second, while the magnitudes are consistent with a large decline in the MRPK/MRPL for ex ante high wedge firms, the decline is much smaller than the pre-existing average gap in these firms' MRPK (160%) or MRPL (130%), suggesting that ex ante positive wedges did not become negative or vice versa. Third, our regression results (Table V) suggest that capital increased for the higher wedge firms (consistent with $\Delta \log x_i \geq 0$ if $\tau_i^{x,\text{pre}} \geq 0$) and changed little for lower wedge firms (also consistent with $\Delta \log x_i \leq 0$ if $\tau_i^{x,\text{pre}} < 0$).

PROPOSITION 1: Under Assumption 1,

$$\sum_{i,x} \lambda_i \alpha_i \frac{\tau_i^{x,\text{pre}}}{1 + \tau_i^{x,\text{pre}}} \Delta \log x_i \geq - \sum_{i,x} \lambda_i \alpha_i \frac{\Delta \tau_i^x}{1 - \Delta \tau_i^x} \Delta \log x_i, \quad (7)$$

where $\Delta \tau_i^x$ is the change in the wedge due to the policy.

Under Assumption 1, Proposition 1 directly follows from the fact that if $\tau_i^{x,\text{pre}} \geq 0$, $\tau_i^{x,\text{pre}} = \tau_i^{x,\text{post}} - \Delta \tau_i^x \geq -\Delta \tau_i^x$, and if $\tau_i^{x,\text{pre}} < 0$, $\tau_i^{x,\text{pre}} = \tau_i^{x,\text{post}} - \Delta \tau_i^x \leq -\Delta \tau_i^x$. Since the left-hand side of equation (7) is the same as equation (6), the right-hand side gives a lower bound for the first-order effect of the policy on the Solow residual in terms of changes in the wedges.

⁴⁵ More formally, for the case where $\Delta \log x_i = 0$ whenever $\tau_i^i < 0$ (consistent with the estimates in Tables V and Table X),

$$\Delta \text{Solow}_{i,t} = \sum_{i,x} \lambda_i \alpha_i^x \frac{\tau_i^x}{1 + \tau_i^x} \Delta \log x_i \mathbf{1}(\tau_i^x > 0) = \sum_{i,x} \max \left\{ \lambda_i \alpha_i^x \frac{\tau_i^x}{1 + \tau_i^x} \Delta \log x_i, 0 \right\}.$$

Since maximum is a convex function, increased variance in estimated τ_i^i , caused by measurement error, will increase the aggregate effect size.

In other words, for a firm with a positive wedge ex ante, the minimum possible pre-treatment wedge is given by the scenario where, after the policy change, the wedge is zero. In this case, for ex ante high wedge firms, any measured dispersion in marginal revenue products after the policy change is attributed to mismeasurement and misspecification as opposed to misallocation. The intuition is symmetric for a firm with a negative wedge ex ante.

Applying Proposition 1, we can estimate a lower bound effect of the policy if we can estimate the change in the wedges due to the policy. We can estimate the changes in the 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 $\Delta\tau_i^k$, we estimate

$$\log MRPK_{ijt} = g(Reform_{jt}, C_{it}) + \Gamma X_{it} + \theta_i + \delta_t + \epsilon_{ijt}, \quad (8)$$

where $g(Reform_{jt}, C_{it})$ is a flexible function of $Reform_{jt}$ and firm characteristics C_{it} . Since we focus on within-industry changes in allocation, allowing the effect of $Reform_{jt}$ to depend on firm characteristics is important, as it allows our estimates of $\Delta\tau_i^k$ to vary within an industry j . Then, $\Delta\tau_i^k$ can be estimated by computing $\widehat{\Delta\tau_i^k} = e^{\hat{g}_i(1, C_{it})} - 1$. An analogous process can be used to estimate the wedges on labor.

As discussed in Section 4.3, estimating the change in wedges using a difference-in-differences specification is less sensitive to the issues that occur when cross-sectional data are used to estimate distortions. To the extent that firms' measurement error is time-invariant over the period of our experiment, it will be absorbed by the firm fixed effects α_i . Time-varying macro-economic shocks or economy-wide changes in markups or the costs of inputs will be absorbed by year fixed effects, as well as year fixed effects interacted with firm characteristics. Additionally, the effects of time-varying shocks to marginal revenue products, such as productivity shocks, even if they are not economy-wide, will *not* be attributed to the reform, as long as the standard difference-in-differences assumption holds and they are uncorrelated with $Reform_{jt}$ conditional on the controls.

In practice, since we observe larger effects on inputs and marginal revenue products for firms with ex ante higher marginal revenue products, we specify $g(Reform_{jt}, C_{it})$ to allow for heterogeneous effects for firms with above median pre-treatment values of MRPK and MRPL.⁴⁶ We use analogous regression specifications to estimate the change in inputs due to the policy. Table A.XVI reports the results of the regressions used to identify both the change in wedges and the change of inputs. Following the identifying assumption in the production function estimation used to identify TFPQ, we assume that materials are not misallocated ($\tau_i^m = 0$ for all i). Although we find evidence that capital and labor are misallocated, materials have a flexible variable cost and are less likely to be affected by financing frictions.

⁴⁶For example, for the marginal revenue product of capital, we estimate

$$\begin{aligned} \log MRPK_{ijt} = & \beta_1 Reform_{jt} + \beta_2 Reform_{jt} \times I_i^{HighMRPK} + \beta_3 Reform_{jt} \times I_i^{HighMRPL} \\ & + \Gamma X_{it} + \theta_i + \delta_t + \epsilon_{ijt}. \end{aligned}$$

We can then predict $\widehat{\Delta\tau_i^k}$ by computing

$$\log(1 + \widehat{\Delta\tau_i^k}) = \hat{\beta}_1 Reform_j + \hat{\beta}_2 Reform_j \times I_i^{HighMRPK} + \hat{\beta}_3 Reform_j \times I_i^{HighMRPL},$$

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

TABLE XI
EFFECTS OF FOREIGN CAPITAL MARKET LIBERALIZATION ON THE SOLOW
RESIDUAL OF TREATED INDUSTRIES.

	Increase in Solow Residual
Lower Bound	3.4%
Conventional Approach	16.3%
Lower Bound Allowing for Cumulative Effects	6.2%
Non-Linear Approximation	6.0%

Note: This table reports the estimates of the effect of the foreign capital liberalizations in 2001 and 2006 on treated industries' Solow residual. Rows 1, 2, and 3 use a first-order approximation (equation (3)). The last row uses a non-linear approximation, described in Appendix C of the Supplemental Material. The estimates are generated using the Prowess data set. Rows 1, 3, and 4 identify the wedges following the lower bound approach discussed above. Row 2 uses cross-sectional data to identify the baseline wedges.

6.2. Results

Lower Bound From the First-Order Approximation. Having estimated all the components of equation (3), we calculate that the lower bound increase in the treated industries' Solow residual is 3.4% (see row 1 of Table XI).⁴⁷ As expected, the less conservative, conventional estimate, which uses estimates of the τ_i^x 's that are sensitive to measurement and model misspecification error, is substantially larger at 16.3% (row 2).

Comparison With Dynamic Estimates. We next consider the cumulative effect of the reforms. The estimates of the reforms' effects over time in Figure 3 suggest that the effects on inputs and the wedges grew over time. Thus, using estimates from a standard difference-in-differences that assumes constant treatment effects over time may lead row 1 of Table XI to underestimate the long-run effects of the policies. Since the effects plateau after 5 years in Figure 3, we recalculate the lower bound approximation using the estimated policy effect five years after the reforms. This yields a larger estimate (6.2%, row 3).

Since the first-order approximation may not be a good approximation if there are important higher order effects of the policy on the Solow residual, we also construct a non-linear, lower bound approximation of the policies' effects on the Solow residual under Assumption 1 by estimating policy effects year-by-year and chaining the results. Appendix C describes this process. Since the non-linear approximation requires estimating dynamic policy effects over five years, it should be compared to the cumulative estimate in row 4. The non-linear approximation (6.0%, reported in row 4) is quite close to the simpler, cumulative first-order approximation.

Discussion of Magnitudes. Our ability to benchmark the size of our estimates is limited by the sparsity of the literature on misallocation and foreign capital liberalization. However, Bollard, Klenow, and Sharma (2013) and Sivadasan (2009) did both estimate the effects of the 1991 FDI liberalizations. While these liberalizations occurred during a macroeconomic crisis, complicating the interpretation of their estimates, both papers found at least some evidence that FDI liberalization led to large increases in aggregate

⁴⁷ We can also relax the assumption that there is no materials misallocation and estimate the change in the Solow residual, incorporating the changes in materials usage and wedges analogously to how we incorporate capital and labor. Doing so results in a small increase in the lower bound effect on the Solow residual to 3.7%.

productivity in affected sectors (on the order of or even larger than the range of potential aggregate effects we report). While both papers attribute the majority of these gains to within-firm productivity growth rather than reduced misallocation, their decompositions may systematically underestimate reallocation's contribution to productivity growth (Nishida et al. (2017)). More broadly, Topalova (2005) found that FDI openness in India has a large negative association with district-level poverty. Thus, our estimates of the aggregate productivity effects of FDI liberalization appear to be in line with the existing literature.

7. CONCLUSION

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

The liberalization, which allowed for the automatic approval of foreign investments and raised caps on foreign equity in the 2000s, increased capital in treated industries. However, the effect of the liberalization on the average firm masks important heterogeneity. The entirety of the liberalization's effect on firms' outcomes is driven by increased investment in firms that previously had high MRPK (high sales to capital ratios). These firms' capital increased, indicating that the policy reduced misallocation within treated industries. Thus, foreign capital liberalization can be an important tool for low-income countries to reduce capital market frictions. Our pattern of results is consistent with foreign capital flows directly targeting ex ante high MRPK firms. That said, it is also possible that the policy reduced misallocation through other mechanisms, such as by increasing aggregate funding in the industry.

Variation from a natural experiment also allows us to estimate the aggregate effects of reducing misallocation on the treated set of industries that—unlike cross-country or time series comparisons—are less sensitive to measurement and model misspecification error. Aggregating our reduced-form estimates, we find that the policy had economically meaningful effects, increasing the treated industries' Solow residual by 3–16%.

The large effects of foreign capital liberalization that we identify also point to questions for future research. In this paper, we have focused on the relationship between these policies and the distribution of capital. This focus led us to exploit firm-level data and concentrate on the formal sector, which dominates manufacturing sector sales. However, important questions remain regarding the aggregate employment and distributional consequences of these reforms (Ray (2010)). As much of manufacturing sector employment is in the informal sector, such research will need to account for the effects of the policy on small and informal firms (Hsieh and Klenow (2014)).

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