Exporters, Credit Constraints, and Misallocation *

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Abstract

Exporters are typically large and productive firms, and many policies implicitly or explicitly encourage them to expand. In this paper I combine a natural experiment and a dynamic model to show that credit constraints are particularly binding for exporting firms, and to quantify the effects of policies that target them. I exploit a directed credit policy in India as a source of exogenous variation in credit supply and find that exporters respond strongly while similarly sized non-exporters do not. I build a model in which heterogeneous entrepreneurs produce subject to credit constraints and choose to export. The model highlights that the decision to export is driven both by an entrepreneur's productivity and by their access to credit. Which force dominates determines whether exporters are typically more or less constrained than non-exporters. I estimate the model by targeting the results of the natural experiment and find that productivity is the key driver of the decision to export. Overall, 37% of exporters in the model are constrained, compared to 8% of non-exporters, and reallocating inputs towards them would raise aggregate productivity. In counterfactual experiments, I show that directly relaxing the credit constraint of exporting firms increases aggregate productivity by 3.33%. However, subsidizing exporter employment worsens misallocation because the primary beneficiaries are relatively unproductive, unconstrained exporters.

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1 Introduction

Exporters are different: they use more capital and labor, sell more output, and are more productive than other firms.[1] Policies that target exporters, through subsidies or via favorable access to credit, are widespread, particularly in developing countries.[2] Furthermore, trade liberalization allows exporting firms to expand at the expense of non-exporters (Melitz 2003). In both cases, the result is a reallocation of capital and labor towards exporters, and the desirability of such a reallocation hinges on whether it raises or lowers aggregate productivity. In turn, the effect of this reallocation on aggregate productivity depends crucially on the presence of frictions that distort the size of exporters relative to non-exporters.

In this paper, I combine a natural experiment in India with a quantitative model to show that credit constraints are just such a friction. Empirically, I find that exporters respond to an exogenous increase in credit supply by borrowing more, hiring more workers, and selling more output. In contrast, similarly sized non-exporters do not respond to this shock. I use these results to estimate a dynamic model in which heterogeneous entrepreneurs produce output subject to credit constraints and decide to export. The estimated model implies that credit constraints bind for many exporters but few non-exporters, and that exporters are on average inefficiently small. Reallocating inputs towards them therefore has the potential to raise aggregate productivity. Finally, I use the estimated model to study the productivity effects of a range of policies that target exporters, distinguishing between their effects on misallocation between exporters and non-exporters, and their effects on misallocation within each of these sets of firms. I find that both dimensions of misallocation are quantitatively important in determining the success or failure of the policies I study. Below I discuss each aspect of the paper in detail.

I begin by using India's Priority Sector Lending (PSL) policy as a source of exogenous variation in firms' credit constraints. Banks were incentivized to lend to firms eligible for PSL, and a cutoff rule determined eligibility. Manufacturing firms with capital below 50 million rupees (roughly 1 million USD) were eligible, while firms with capital above this level were not. This policy allows me to explicitly compare the importance of constraints across exporters and non-exporters, precisely because it was not contingent on a firm's export status. Using a regression discontinuity design, I show that eligible exporters borrowed 33% more, hired 25% more workers, and sold 22% more output, while I find no effect of PSL on non-exporters. I further show that PSL did not cause any change in eligible firms' export choices on either the intensive or extensive margin. Instead, it allowed exporters to expand both their domestic and export sales symmetrically. This fact suggests that export sales *per se* are not uniquely distorted by credit constraints. Instead, the type of firm that chooses to export is particularly likely to find credit constraints binding.

Motivated by these findings, I build a model which connects credit constraints and exporting. Entrepreneurs differ in their productivity and in their fixed costs of exporting and accumulate

[1] See Bernard and Jensen (1999) for the US, Aw, Chung, and Roberts (2000) for Taiwan, and Clerides, Lach, and Tybout (1998) for Colombia, Mexico and Morocco.

[2] For examples, see Itskhoki and Moll (2019).

physical capital and liquid assets over time. They must pay for the labor they use before production takes place, and do so using either liquid assets or by borrowing using physical capital as collateral. Some entrepreneurs — those with relatively high productivity, but relatively low levels of liquid assets and physical capital — will hit a binding credit constraint, where they wish to hire more workers but cannot borrow to do so. Finally, entrepreneurs choose whether or not to export.

The model highlights two factors in the decision to export. More productive entrepreneurs find it more worthwhile to overcome the fixed cost of exporting because their sales abroad will be large. All else equal, more productive entrepreneurs are also more likely to be constrained, and so this force tends to make exporters more constrained. However, entrepreneurs with better access to credit — determined by their stocks of liquid assets and physical capital — are also more likely to export, because they are more able to expand to take advantage of the larger market they can access by exporting. This force tends to make exporters less constrained. Therefore, which of these two forces dominates is crucial in determining the relative importance of credit constraints for exporters.

I estimate the model by targeting the results of the natural experiment. The natural experiment disciplines the two forces mentioned above: whether the decision to export is mainly driven by productivity or by access to credit. Since in the natural experiment exporters responded strongly to a change in their credit constraints, the estimation infers that many exporters are constrained, and therefore that productivity is the main driver of the decision to export. The estimated model implies that 37% of exporting firms are at a binding credit constraint, compared to only 8% of non-exporters. Exporters are inefficiently small and have high marginal products of capital and labor; for example, the marginal revenue product of labor is roughly 9% higher among exporters. This difference in marginal products implies that reallocating inputs towards exporters has the potential to raise aggregate productivity.

I use the estimated model to study two policies that encourage exporters to expand. The first policy directly relaxes the credit constraint of exporters, while the second subsidizes their employment. While these policies cause comparable amounts of reallocation towards exporters, I show that they have sharply different consequences for aggregate productivity. The credit policy allows constrained exporters, who have relatively high marginal products, to expand, thus lowering misallocation. In the long run, this policy raises aggregate productivity by 3.33%. In contrast, the employment subsidy primarily benefits unconstrained exporters with low marginal products because these are the firms most able to expand in response to the subsidy. Thus it worsens misallocation and lowers aggregate productivity. My results highlight that subsidies struggle to undo the misallocation created by credit constraints, even when targeted towards a group of firms (exporters) in which such constraints are prevalent. To be effective, subsidies must encourage the most productive firms to expand; but, almost by definition, constrained firms cannot do so. On the other hand, directly tackling the source of the distortion yields substantial gains.

Finally, I consider a third intervention that implicitly targets exporters: lowering trade costs. As with the employment subsidy above, I find that heterogeneity within the set of exporters largely offsets any productivity gains from reallocation towards exporters on average. I contrast my results with those obtained from a model in which misallocation is the result of exogenous wedges, as in Hsieh and Klenow (2009). I show that such a model substantially overstates the productivity enhancing effects of reductions in trade costs.

This paper relates to four broad literatures. First, I contribute to an empirical literature that measures the firm-level effects of credit constraints. Focusing on exporters, Amiti and Weinstein (2011) and Paravisini et al. (2015) show that shocks to bank health are transmitted to export sales, while Zia (2008) studies the removal of subsidized export credit in Pakistan. Relative to these papers, I study a policy that affected both exporters and similarly sized non-exporters, allowing me to compare its effects across these two groups. My paper is also connected to a literature that analyzes the effects of the Priority Sector Lending policy (Banerjee and Duflo 2014; Kapoor, Ranjan, and Raychaudhuri 2017). Particularly relevant is Rotemberg (2019). That paper studies the same policy, and develops an empirical methodology to estimate its indirect effects via general equilibrium. My focus is instead on heterogeneity in the policy's direct effects, in particular across exporting and non-exporting firms. I show that this heterogeneity is informative about the determinants of the decision to export in a model in which entrepreneurs differ in their productivity and assets, and use it to estimate the model's key parameters.

Second, this paper is related to a literature that incorporates financial frictions into models of international trade. Manova (2012) and Leibovici (2021) show that the pattern of aggregate trade flows across countries and sectors is consistent with models in which financial frictions inhibit trade. Chaney (2016) links financial constraints to exchange rate fluctuations. Kohn, Leibovici, and Szkup (2014) study how these frictions affect the dynamics of new exporters and Brooks and Dovis (2020) show that conclusions about how they interact with the gains from trade are sensitive to exactly how credit constraints are modeled. These papers point to a variety of ways credit constraints might interact with exporting; by distorting the extensive or intensive margins, or simply by limiting overall firm size. The results of my natural experiment support a model in which export sales *per se* are not uniquely constrained. Rather, credit constraints limit the ability of some firms to expand overall, and these constrained firms are likely to be exporters.

Third, I contribute to a literature that studies the gains from trade in the presence of misallocation (Berthou et al. 2019; Bai, Jin, and Lu 2019). In these papers, misallocation results from exogenous distortions in input markets, whereas in my model, misallocation is endogenously generated by credit constraints, as well as adjustment costs in physical capital. I show that this distinction matters. In my model, the firms most able to expand in response to falling trade costs are unconstrained exporters with relatively low marginal products. As a result, misallocation within the set of exporting firms rises, limiting the overall gains from trade. I show that this force vanishes when misallocation is the result of exogenous wedges, because, conditional on export status, a reduction in trade costs affects all firms symmetrically. Hence, my results highlight the importance of explicitly modeling the source of misallocation for understanding how it will interact with a given policy change.

Fourth, a large literature in macroeconomics links financial constraints and misallocation (Buera, Kaboski, and Shin 2011; Midrigan and Xu 2014; Moll 2014). I show empirically that such constraints distort the decisions of a particularly productive group of firms — exporters. Moreover, in linking a natural experiment to a model of financial constraints and misallocation, my paper is related to Kaboski and Townsend (2011) and Buera, Kaboski, and Shin (2021). While they study microfinance interventions that affect poor households and very small firms, I show that similar constraints are relevant for much larger firms.

The remainder of the paper proceeds as follows. Section 2 uses India's PSL policy to estimate the effects of credit constraints on exporting and non-exporting firms. Section 3 builds a model of credit constraints and selection into exporting, and Section 4 estimates this model by targeting the pattern of treatment effects found in Section 2. Section 5 explores the policy implications of the estimated model. Finally Section 6 concludes.

2 Are Exporters Credit Constrained?

In this section I exploit variation in eligibility for a directed credit policy, Priority Sector Lending (PSL), as a source of exogenous variation in the availability of credit. I find that eligible exporting firms borrowed more, hired more workers and sold more output, while eligible domestic firms did not respond in any economically or statistically significant way. My results suggest that credit constraints are more important for exporting firms than for similarly sized non-exporting firms. Subsection 2.1 discusses the details of the PSL policy and Subsection 2.2 introduces my data. In Subsection 2.3 I outline my estimation strategy, and 2.4 presents results.

2.1 Priority Sector Lending

Under India's Priority Sector Lending (PSL) policy, all banks are obliged to allocate at least 40% of net credit to the 'priority sector', which includes agriculture, transport, and small businesses (Banerjee and Duflo 2014). If a bank fails to reach this quota, it faces financial penalties.[3] Therefore, banks have a strong incentive to lend to firms in the priority sector, and priority sector firms enjoy favorable access to credit. Variation in PSL eligibility across firms thus has the potential to act as a source of exogenous variation in access to credit.

The priority sector includes manufacturing firms with plant and machinery (a subset of physical capital) below a certain cutoff. This cutoff has moved around over time. For example, Banerjee and Duflo (2014) studied the effects of an increase in the cutoff from 6.5 million rupees to 30 million rupees in 1998, as well as a subsequent decrease in 2000. I focus on a later change in the policy, when in 2007 the cutoff was raised from 10 to 50 million rupees, roughly 1.1 million USD. Thus in 2007, firms with plant and machinery below 50 million rupees became eligible for PSL, while firms with plant and machinery just above this level remained ineligible.

[3] Banerjee and Duflo (2014) show that for the bank they study, the share of lending to the priority sector is always close to 40%, suggesting that this constraint is binding.

In principle, banks could have increased lending to the newly eligible firms in two ways. First, if firms were credit constrained, banks could have offered to raise their credit limits. Second, banks could have lowered the cost of borrowing. In Appendix A I investigate the second possibility and find that PSL eligibility did not lower firms' borrowing costs. This is consistent with evidence presented by Banerjee and Duflo (2014), who find that interest rates did not fall for eligible firms. Therefore I interpret the effects of PSL eligibility reported below as evidence of the presence of credit constraints.

Since I will report separate results for exporting and non-exporting firms, it is important to note that PSL did not distinguish between these two types of firms. Credit extended to exporters was counted towards the quota if and only if the firm had plant and machinery below 50 million rupees. Thus, both types of firm were subject to the same policy; differences in treatment effects across these two groups therefore reflect differences between exporting and non-exporting firms, rather than differences in the application of the PSL policy.

Finally, firms eligible for PSL were also eligible for a number of other programs run by the Ministry for Micro, Small, and Medium Enterprises (MSME). In practice the vast majority (70%) of MSME's budget was devoted to credit guarantee and support schemes (Rotemberg 2019). We would expect these credit guarantees to have effects similar to those of PSL, and since my goal is not to measure the effects of PSL *per se*, but rather to use eligibility as a source of exogenous variation in credit supply, the presence of such credit guarantee schemes does not present a problem. MSME also provided entrepreneurs with access to to training programs, which would be expected to raise firm productivity. Rotemberg (2019) finds that eligibility had a negligible effect on firm productivity, suggesting such training programs were unimportant. I therefore follow Banerjee and Duflo (2014) and interpret eligibility as a shock to firms' access to credit.

2.2 Data

My main analysis relies on the Prowess dataset, compiled by the Centre for Monitoring the Indian Economy (CMIE). This is a panel of firms beginning in 1980 whose source is audited financial statements. I use information on the value of plant and machinery, total borrowing, wage bills, total sales, and export sales.[4] Table 1 reports summary statistics from Prowess for 2007. Prowess is not representative of the universe of Indian firms. Instead, it focuses on larger firms, and among these firms it has fairly complete coverage. For example, firms in Prowess account for $60 - 70\%$ of economic activity in the organized industrial sector and 75% of corporate taxes collected by the Government of India (De Loecker et al. 2016). Prowess does a good job of capturing firms affected by the PSL policy and is therefore ideal for this paper. Panel (a) of Figure 1 shows the density of plant and machinery across firms in 2007 alongside the cutoff for PSL eligibility. Roughly 35% of firms in 2007 had plant and machinery below 50m rupees and were therefore eligible for PSL. Panel (b) of Figure 1 shows that PSL affected both exporting and non-exporting firms; 18.7% of

[4] For most firms, Prowess does not report employment separately from wage bills. I therefore assume that all firms face the same wages, so that a firm's employment is proportional to its wage bill.

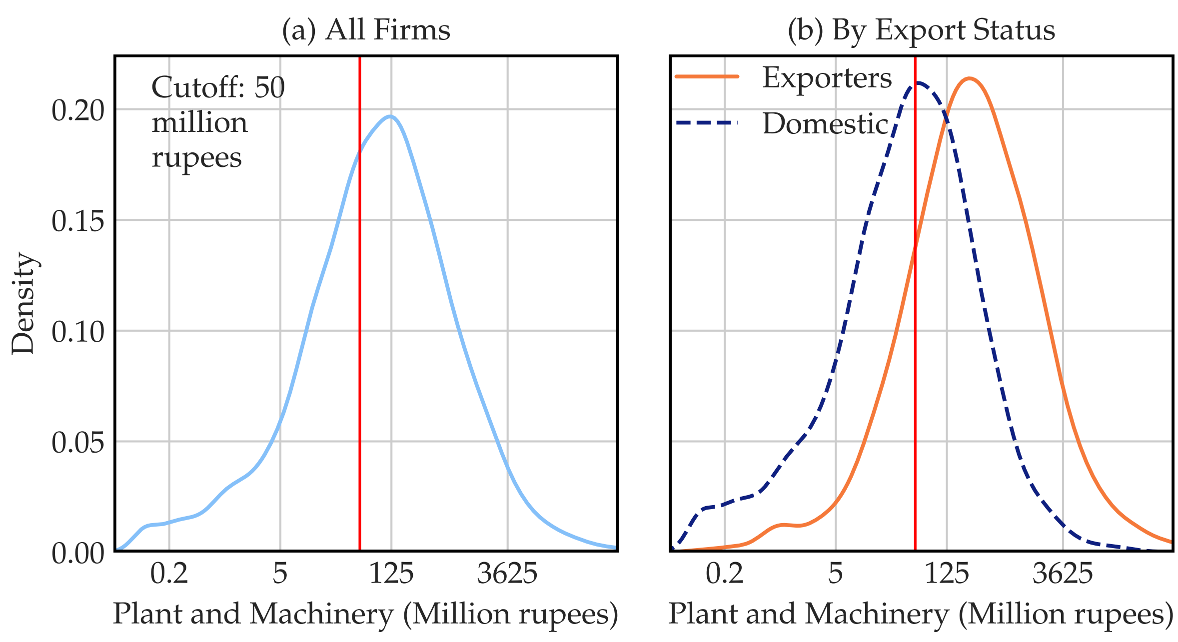
Table 1: Summary Statistics

	(1) *All Firms*	(2) *Exporters*	(3) *Non-exporters*
Number of Firms	7349	3449	3900
Median Sales	505	1257	205
Median Plant and Machinery	111	257	53
Plant and Machinery Below 50 Million, %	34.6	18.7	48.6

Source: Prowess dataset, manufacturing firms, 2007.

Note: Units for sales and plant and machinery are millions of rupees. 'Exporters' defined as firms with positive export sales in 2007.

Figure 1: Plant and Machinery Distribution



Source: Prowess dataset, manufacturing firms, 2007. *Notes*: Panel (a) shows the density of (log) plant and machinery in all manufacturing firms in 2007. The vertical line shows the 50 million rupee cutoff for PSL eligibility. Panel (b) shows the density (log) plant and machinery in exporting and non-exporting firms, where export status is defined using sales in 2007 exporters and 48.6% of non-exporters in Prowess were eligible for PSL in 2007. See Appendix A for more details.

2.3 Research Design and Estimation

The crucial feature of the policy described in Subsection 2.1 is that a firm's eligibility changed discretely as plant and machinery crossed the 50 million rupee threshold. I consider models of the form

$$\mathbb{E}\left[y_{it}|x_{i0}\right] = f_y(x_{i0}) + \beta_y \mathbb{I}\{x_{i0} \leq c\}$$

where i indexes firms and t indexes years. y_{it} is the outcome of interest — (log) loans, employment, and sales. x_{i0} is log plant and machinery in year 0, which I take to be 2007, and $c = \log(50)$ is the cutoff for PSL eligibility. β_y is the parameter of interest and measures the effect of PSL eligibility on the outcome y. Note that this is an average treatment effect for the set of firms with plant and machinery equal to 50 million rupees, rather than an average over all firms.

The effect of PSL eligibility, β_y, is identified under the assumption that the function $f_y(x_{i0})$ is continuous at $x_{i0} = c$. f_y represents the expected value of y_{it} in the absence of PSL. Assuming continuity of f_y is therefore equivalent to assuming that without PSL the outcomes y_{it} would have varied smoothly across the cutoff c. Any discontinuous jumps we observe can then be attributed to the effects of PSL.

As is standard in the regression discontinuity literature (Imbens and Lemieux 2008), I approximate the unknown function f_y using a local linear regression, so that estimating β_y reduces to estimating

$$y_{it} = \varphi_0 + \varphi_1(x_{i0} - c)\mathbb{I}\{x_{i0} \leq c\} + \varphi_2(x_{i0} - c)\mathbb{I}\{x_{i0} > c\} + \beta_y \mathbb{I}\{x_{i0} \leq c\}$$

by weighted least squares, with the weights determined by bandwidth and kernel choices. In choosing these values I follow Calonico, Cattaneo, and Titiunik (2014) — see Appendix C for details. In some specifications I include controls — year and industry fixed effects, and lagged values of the outcomes y_{it}. When I do so, I follow the advice of Calonico et al. (2019) and include them additively, without interacting them with the cutoff dummy.

2.4 Results

Main Results

Table 2 shows my main results. Each column reports results for three different outcomes: loans, employment, and sales. I categorize firms into exporters and non-exporters based on their sales in 2007. All outcomes are measured in logs, so the point estimates can be interpreted as the percentage difference between firms who were just eligible for PSL based on their plant and machinery in 2007 and those who were just ineligible.

Columns (1) and (2) report results for exporters and non-exporters, respectively, with outcomes measured between 2008 and 2012. Column (1) shows that eligible and ineligible exporters look very different. Eligible exporters borrowed 39% more and hired 23% more workers. They also sold more output, but this estimate is not statistically significant. By contrast, PSL eligibility did not have any effect on these outcomes for non-exporters. All the estimates in Column (2) are quantitatively small and statistically insignificant.

Next, in Columns (3) and (4) I control for outcomes in the pre-policy period, which I take to be 2005.[5] That is, each regression now includes log loans, employment and sales in 2005 as controls. Adding these controls has two benefits. First, by absorbing variation in the outcomes which existed prior to the policy, they allow me to estimate the effects of PSL eligibility more precisely. Second, these results provide a check on my identifying assumption. If the results with pre-policy controls differed sharply from those without, that would suggest that the results in Columns (1) and (2) reflected pre-existing differences between eligible and ineligible firms rather than the causal effect of PSL. Equally, finding similar results when these controls are included would suggest that the results in Columns (1) and (2) do indeed capture this causal effect. Column (3) continues to show that exporters responded strongly to PSL eligibility, by borrowing 33% more,

Table 2: Effects of Priority Sector Lending

Outcome	(1) *Exporters*	(2) *Domestic*	(3) *Exporters*	(4) *Domestic*
Loans	0.388**	0.022	0.328**	0.020
	(0.177)	(0.134)	(0.153)	(0.117)
Employment	0.233*	−0.027	0.246**	0.000
	(0.133)	(0.107)	(0.115)	(0.075)
Sales	0.121	0.033	0.224*	0.110
	(0.163)	(0.128)	(0.122)	(0.113)
Years	2008-12	2008-12	2008-12	2008-12
Pre-policy controls	No	No	Yes	Yes
N	13243	12757	11553	9382

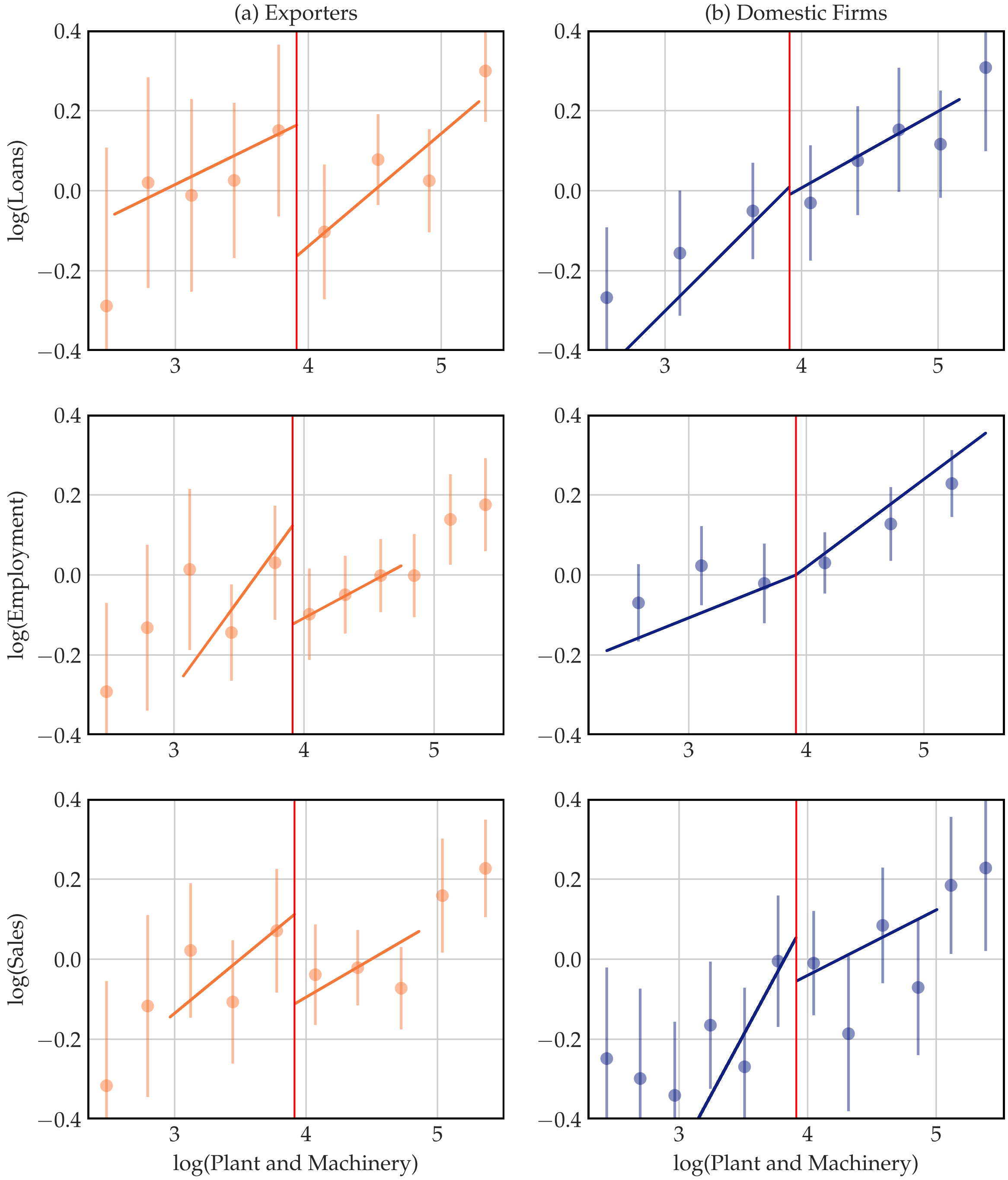
Source: Prowess Dataset, all manufacturing firms, 2005-2012

Note: Columns show results for different specifications; rows show results for different outcomes. Each estimate reports the discontinuity in the outcome at plant and machinery equal to 50 million rupees. Plant and machinery measured in 2007; export status defined using sales in 2007. All outcomes are measured in logs and a positive number indicates a positive effect of being eligible for Priority Sector Lending. (1) and (2) show results with year, industry, and firm age fixed effects. (3) and (4) additionally control for pre-policy outcomes measured in 2005. Standard errors clustered at the firm level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

[5] I exclude 2006 because the change in the PSL threshold was announced, but not implemented, in this year.

Figure 2: Regression Discontinuity Plots



Source: Prowess dataset, manufacturing firms, 2005-2012. *Notes*: Panel (a) shows results for exporters, Panel (b) for non-exporters. Each plot shows a binned scatterplot of an outcome (log loans, employment or sales), plotted against the log of 2007 plant and machinery, in a window around the cutoff for PSL eligibility. As in Columns (3) and (4) of Table 2, I control for year, industry, and firm age FE, as well as pre-policy outcomes. This cutoff is shown by the vertical line. The solid lines on either side of the cutoff plot local linear regressions fitted to the underlying data. Note that in each plot the outcome variable has been shifted by a constant so that the y-axis is centered on zero. The discontinuity in the solid line at the cutoff corresponds to (minus) the treatment effects reported in Columns (3) and (4) of Table 2.

hiring 25% more workers and selling 22% more output, although again this last outcome is more noisily measured. Consistent with Column (2), all the estimates for non-exporters in Column (4) are quantitatively small and statistically insignificant.

Figure 2 visualizes the results in (3) and (4) by showing binned scatterplots of the three outcomes in Table 2 against plant and machinery, with local linear regressions shown by the solid lines. The plots for exporters in Panel (a) show discontinuous jumps at 50 million rupees, whereby firms just below the cutoff borrowed more, hired more workers and sold more output. In contrast the plots for non-exporters in Panel (b) show small discontinuities with inconsistent signs. Columns (3) and (4) represent my preferred specification, and will serve as targets for the model I estimate in Section 4.

Threats to Identification

Above I assumed that the function $f_y(x_{i0})$ was continuous at $x_{i0} = c$ in order to identify β_y, the causal effect of PSL eligibility. In my setting, the leading threat to identification is the manipulation of plant and machinery close to the cutoff. If firms can perfectly choose their 2007 plant and machinery,[6] and if the firms which choose to become eligible for PSL are systematically different than those which do not, then this kind of sorting could bias my results. Below I present two pieces of evidence that suggest that such sorting is not a problem in this case.

First, I check whether firms 'bunched' to the left of the 50 million rupee cutoff, which would suggest manipulation of plant and machinery (McCrary 2008). Figure 9 in Appendix A shows histograms of plant and machinery close to the cutoff, separated by export status. These histograms show no evidence of bunching. In Appendix A I also report the results of formal statistical tests which do not reject the null of no bunching. If anything, these tests find there are slightly too few firms to the left of the cutoff, just the opposite of what we would expect if firms were manipulating their plant and machinery to become eligible for PSL.

Second, I perform a placebo test. In Table 3, I repeat the specification in Columns (1) and (2) of Table 2, continuing to use 2007 plant and machinery as the running variable, but now I use outcomes measured before the policy was implemented. In particular, I use outcomes from 2005. The idea here is that if firms that became eligible in 2007 differ from those that did not become eligible only because of the causal effect of PSL, then we should detect no effect of PSL eligibility on any outcome before the policy was implemented. Columns (1) and (2) report the results of this exercise for exporters and non-exporters, respectively. All of the estimates are statistically insignificant, and for exporters the sign of the estimates varies across the different outcomes. I conclude that eligible and ineligible firms were not significantly different prior to the introduction of PSL, and only diverged after the policy was implemented. Together with the bunching

[6] Note the qualifier 'perfectly.' Lee (2008) considers a setting in which agents can influence their assignment into treatment, and shows that if this is imperfect, i.e., if eligibility is at least partly determined by some random component, then a regression discontinuity design continues to identify the causal effect of treatment. In my setting, this random component might come from some randomness in the rate at which capital depreciates, for example.

check reported above, this placebo check suggests that sorting around the cutoff is not driving my results. Instead, they represent the causal effect of becoming eligible for PSL.

Rotemberg (2019) points out that PSL also had indirect effects that operated through changes in equilibrium prices, and that these indirect effects likely varied across sectors. Such equilibrium effects do not pose a threat to identification for my regression discontinuity design, even if they vary across sectors or across exporters and non-exporters. To see this, note that while exposure to equilibrium effects might be correlated with plant and machinery (because, for example, plant and machinery varies systematically across sectors), we would not expect this correlation to jump discontinuously at the 50 million rupee cutoff for PSL eligibility. Therefore, indirect effects do not violate the assumption that the potential outcomes $f_y(x_{i0})$ are continuous functions of plant and machinery at the cutoff.

Robustness Checks

In Appendix A I investigate the robustness of the results in Columns (3) and (4) of Table 2. I show that they are not sensitive to the choice of bandwidth; to dropping observations very close to the cutoff for PSL eligibility; or to the years I use to measure the outcome variables. I obtain qualitatively similar point estimates when I drop all controls, although these estimates are noisy.

Table 3: Effects of Priority Sector Lending: Placebo

Outcome	(1) *Exporters*	(2) *Domestic*
Loans	−0.136	−0.007
	(0.171)	(0.133)
Employment	0.113	−0.075
	(0.143)	(0.120)
Sales	−0.006	0.112
	(0.164)	(0.180)
Years	2005	2005
N	2731	2694

Source: Prowess Dataset, all manufacturing firms, 2005-2012

Note: Columns show results for different specifications; rows show results for different outcomes. Each estimate reports the discontinuity in the outcome at plant and machinery equal to 50 million rupees. Plant and machinery measured in 2007; export status defined using sales in 2007; outcome measured in 2005. All outcomes are measured in logs and a positive number indicates a positive effect of being eligible for Priority Sector Lending. All regressions include year, industry and firm age fixed effects.

* p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors in parentheses.

Table 4: Effect of Priority Sector Lending on Exporting

Outcome	(1) *Extensive margin*	(2) *Intensive margin*
PSL Effect	−0.021	0.016
	(0.024)	(0.029)
Years	2008-12	2008-12
N	26000	11790

Source: Prowess Dataset, all manufacturing firms, 2005-2012

Note: Each estimate reports the discontinuity in the outcome at plant and machinery equal to 50 million rupees. Plant and machinery measured in 2007; export status defined using sales in 2007. In (1), the outcome is a dummy equal to 1 if the firm has positive export sales, and the sample is all firms. In (2), the outcome is the share of sales exported, and the sample is all firms with positive export sales. All regressions include year, industry and firm age fixed effects. All regressions also control for values of the outcome in the pre-policy period, i.e 2005.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Are Export Sales Especially Constrained?

Table 2 showed that PSL eligibility had a significant effect on borrowing, employment, and sales in exporting firms. This might reflect the fact that exporting is a uniquely finance-intensive activity. For example, Manova (2012) points out that export sales may be particularly dependent on access to external financing because they involve large upfront costs and long lags between production and payment. Table 4 investigates this possibility by estimating the effect of PSL eligibility on extensive and intensive margin export decisions. Columns (1) studies the extensive margin of exporting: were eligible firms more likely to enter the export market? The outcome here is a dummy equal to 1 if the firm has positive export sales. As in Columns (3) and (4) of Table 2, I control for values of the outcome in the pre-policy period. The result in Column (1) is small and not statistically significant, indicating that PSL eligibility did not affect the extensive margin of exporting. Column (2) shows that the same is true for the intensive margin, i.e., the share of output a firm sells abroad, conditional on being an exporter. Eligible firms increase the share of their sales made abroad by about 1.6 percentage points, but this effect is not statistically significant. Together, the results in Columns (1) and (2) do not show any significant effect of PSL eligibility on either the extensive or intensive margins of exporting.

Summary

PSL eligibility relaxed firms' credit constraints and caused exporters to borrow more, hire more workers and sell more output. In contrast, non-exporters did not respond to this change in their credit constraints. I infer that credit constraints must be binding for a significant fraction of exporters close to the 50 million rupee cutoff, while they are less important for similarly sized non-exporters. PSL eligibility did not differentially affect the export sales of exporting firms; instead, it caused exporters to expand their foreign and domestic sales symmetrically. I therefore conclude that my results are not driven by export sales *per se*. Rather, the kind of firm which chooses to export must be particularly likely to find credit constraints binding.

3 Model

I now develop a model of credit constraints and selection into exporting, with two objectives in mind. First, I aim to make a tight connection between the model and the natural experiment analyzed in Section 2, allowing me to exploit my empirical results to quantify the aggregate importance of credit constraints for exporters and non-exporters. Second, I will use the estimated model to explore the effect of different policies that target exporters in Section 5.

3.1 Environment

Time is discrete. Entrepreneurs are the key agents of the model, and begin each period with a state $\omega \equiv (z, f, k, a)$. z denotes productivity, which evolves exogenously over time according to

$$\log z' = \rho_z \log z + \sigma_z \epsilon, \quad \epsilon \sim \mathcal{N}(0,1). \tag{1}$$

Entrepreneurs also differ in a shock to the fixed cost of exporting f, which is exogenous and time-invariant. They endogenously accumulate physical capital k and liquid assets a over time.

In each period the entrepreneur takes the wage w as given, demands labor ℓ and produces according to

$$y = z k^{\alpha} \ell^{1-\alpha}. \tag{2}$$

Entrepreneurs face a working capital constraint, in that they must pay for the labor they hire before production takes place. These payments may be made by directly using liquid assets a, or by borrowing using physical capital k as collateral. Formally, the entrepreneur faces the constraint

$$w\ell \leq a + b$$

where

$$0 \leq b \leq \lambda k.$$

b is the total borrowing of the entrepreneur, which is limited by the amount of physical capital they can offer as collateral. Borrowing is costly; if the entrepreneur borrows b, they incur interest $r_b b$.

The entrepreneur can potentially sell in two markets, domestic and foreign. Let y_d denote the amount the entrepreneur sells domestically and y_x the amount sold abroad. The entrepreneur chooses these quantities subject to

$$y_d + \tau y_x = y$$

where $\tau \geq 1$ is an iceberg trade cost. Each market is monopolistically competitive, and the entrepreneur sets prices subject to CES demand with elasticity σ,

$$p_d y_d = \left(\frac{p_d}{P}\right)^{1-\sigma} D, \quad p_x y_x = \left(\frac{p_x}{P^*}\right)^{1-\sigma} D^* \tag{3}$$

where p_d and p_x are the prices charged in each market. P is the domestic price level, D is total domestic demand and P^* and D^* are their foreign analogues.

In addition to the iceberg cost, exporters also incur a fixed cost F every period. I model this fixed cost as

$$F(\omega) = f z^{\vartheta} \tag{4}$$

where ω indexes the entrepreneur's state and f is a time-invariant shock to the entrepreneur's fixed cost. I assume

$$\log f \sim \mathcal{N}\left(\mu_f, \sigma_f^2\right). \tag{5}$$

Notice that (4) allows the cost of exporting to depend directly on productivity z. This relationship may be positive or negative depending on the sign of ϑ. If $\vartheta < 0$, for example, then more productive entrepreneurs face lower fixed costs. Intuitively, this might capture the idea that an entrepreneur who is skilled at producing goods is also skilled at overcoming the logistical and regulatory hurdles involved in selling internationally. As we will see in Subsection 3.5, ϑ is a key parameter in determining the drivers of selection into exporting.

3.2 Static Problem

First I focus on an entrepreneur who chooses to sell only domestically. Rearranging (3) and substituting, this entrepreneur's profit function may be written

$$\begin{aligned} \pi_d(\omega) = \max_{\ell, b} \quad & \tilde{p}_d \left(z k^{\alpha} \ell^{1-\alpha}\right)^{\frac{\sigma-1}{\sigma}} - w\ell - r_b b \\ \text{s.t} \quad & w\ell \leq a + b, \quad 0 \leq b \leq \lambda k, \end{aligned} \tag{6}$$

where

$$\tilde{p}_d = \left(P^{\frac{\sigma-1}{\sigma}} D^{\frac{1}{\sigma}}\right). \tag{7}$$

Notice that CES demand creates a source of decreasing returns to scale at the firm level, with returns to scale determined by $\left(\frac{\sigma-1}{\sigma}\right)$.

The profit maximization problem of an entrepreneur who chooses to export is similar. First, given total output y, the optimal choice of y_d and y_x satisfies

$$\frac{y_d}{y_x} = \tau^{\sigma} \left(\frac{P}{P^*}\right)^{\sigma-1} \left(\frac{D}{D^*}\right).$$

Given this allocation across markets, the exporter then solves a problem analogous to (6).

$$\pi_x(\omega) = \max_{\ell,b} \quad \tilde{p}_x \left(zk^{\alpha}\ell^{1-\alpha}\right)^{\frac{\sigma-1}{\sigma}} - w\ell - r_b b \tag{8}$$
$$\text{s.t} \quad w\ell \leq a + b, \quad 0 \leq b \leq \lambda k,$$

where

$$\tilde{p}_x = \left(\left(P^{\frac{\sigma-1}{\sigma}} D^{\frac{1}{\sigma}}\right)^{\sigma} + \tau^{-\sigma}\left(P^{*\frac{\sigma-1}{\sigma}} D^{*\frac{1}{\sigma}}\right)^{\sigma}\right)^{\frac{1}{\sigma}}. \tag{9}$$

Comparing (7) and (9), we can see that the only difference between exporting and non-exporting firms is that exporters effectively face a higher output price, i.e $\tilde{p}_x > \tilde{p}_d$. An entrepreneur exports if doing so is sufficiently profitable to justify paying the fixed cost, i.e if $\pi_x(\omega) - F(\omega) \geq \pi_d(\omega)$. Given this decision, the overall profits of an entrepreneur with state ω are

$$\Pi(\omega) = \max\{\pi_d(\omega), \pi_x(\omega) - F(\omega)\}.$$

3.3 Dynamic Problem

Given a state ω, the entrepreneur solves the static problem above to obtain profits $\Pi(\omega)$. The entrepreneur must then choose a', next period's stock of liquid assets, and i, investment in physical capital, to solve an infinite horizon dynamic programming problem. I assume that physical capital is subject to a fixed adjustment cost. Letting $V(\omega)$ denote their value function, the entrepreneur solves

$$V(\omega) = \max_{a',i} \quad \log(c) + \beta\mathbb{E}\left[V(\omega')|\omega\right], \tag{10}$$
$$\text{where} \quad \omega' = \left(z', f, k', a'\right),$$
$$a' + i + c = \Pi(\omega)\left(1 - \phi\mathbb{1}\{i \neq 0\}\right) + (1 + r_a)a,$$
$$a' \geq 0, \quad c \geq 0, \quad k' = (1-\delta)k + i \geq 0.$$

c is the entrepreneur's choice of consumption, β is their discount factor, δ is the rate of depreciation of physical capital, and r_a is the interest the entrepreneur earns on their savings of liquid assets. (10) defines the value function $V(\omega)$ and also the entrepreneur's policy function $g(\omega)$, which describes the optimal choice of a' and i, and therefore k', given an initial state ω. Three features of (10) are worth noting:

(i) The entrepreneur faces a fixed cost of adjusting their physical capital stock. In particular, I follow Cooper and Haltiwanger (2006) and model this cost as a fraction ϕ of profits.

(ii) The entrepreneur must choose k' before learning next period's productivity z'. This 'time to build' creates an additional source of friction in the entrepreneur's problem.

(iii) The entrepreneur is constrained to hold $a' \geq 0$, implying they cannot issue *intertemporal* debt to finance investment or consumption. Thus the only borrowing in my model is *intratemporal* debt, issued to fund the hiring of workers.

Together (i)-(iii) impede the entrepreneur's ability to adjust their capital stock and thus imply that conditional on capital there will be some dispersion in productivity z. This dispersion is important in relating the model to the natural experiment. There, by construction, eligible and ineligible firms had very similar capital stocks, but differed widely in their responses to changes in their credit constraints. In the model, dispersion in z conditional on k will be an important driver of these differing responses.

3.4 Aggregation and Equilibrium

So far, I have taken interest rates, wages, and prices as given and studied the decisions of individual entrepreneurs. This is all that is needed for the estimation in Section 4. However, when I study policy interventions in Section 5 it will be necessary to specify how markets clear and how prices are determined.

I assume that the model represents a small manufacturing sector within a larger economy. Entrepreneurs in this sector can purchase physical capital at a fixed price, normalized to 1. They can also save at a fixed interest rate r_a and borrow to hire labor at a fixed interest rate r_b. Aggregate expenditure on manufactured goods is fixed at an exogenous level D. The supply of labor to the manufacturing sector is fixed at L, and the wage w adjusts to clear this market. Finally, for simplicity, I assume that the foreign and domestic economies are symmetric, so that $P = P^*$, $D = D^*$ and so on.

The export fixed cost, $F(\omega)$, is paid using an 'entry good'. One unit of this good is produced using one unit of labor. Let L_e denote the total labor used in the production of the entry good, and L_p labor used in the production of goods. Then

$$L = L_e + L_p.$$

The assumptions above imply that there are two endogenous prices to be determined, the nominal wage w and the CES price index P. These prices adjust to satisfy the labor and goods market clearing conditions

$$\int \ell(\omega) d\mathcal{G}(\omega) = L_p, \tag{11}$$

$$\int R(\omega) d\mathcal{G}(\omega) = D \tag{12}$$

where $\ell(\omega)$ is the labor demand and $R(\omega)$ the revenue of a firm with state ω, and $\mathcal{G}$ is the joint distribution over states. Having stated these market clearing conditions, I now define a static equilibrium:

Definition 1 (Static Equilibrium)**.** Given a joint distribution $\mathcal{G}$ over states ω, a *static equilibrium* consists of nominal wages w and a CES price index P, such that when entrepreneurs solve the static problem in Subsection 3.2 the labor and goods market clearing conditions (11) and (12) are satisfied.

Any change in the environment will also change the joint distribution of states $\mathcal{G}$ in the long run. In my counterfactual experiments, I will focus on equilibria in which $\mathcal{G}$ has converged to its new steady state.

Definition 2 (Steady State)**.** A *steady state* is a distribution of states $\mathcal{G}_{ss}$, a policy function g, a nominal wage w and a CES price index P, such that (i) given G_{ss}, w and P are a static equilibrium, and (ii) when z evolve according to its exogenous law of motion (1) and k and a evolve according to the policy function g, the resulting joint distribution over states is $\mathcal{G}_{ss}$.

3.5 Exporting and Credit Constraints

The model developed above is rich but not analytically tractable. In this subsection, I make the following simplifying assumptions to illustrate the fundamental forces in the model:

(i) I abstract from physical capital and assume $\alpha = 0$. Then an entrepreneur's ability to hire labor is entirely determined by liquid assets a.

(ii) I assume that $\sigma_f = 0$, so that there is no exogenous heterogeneity in the fixed cost of exporting. Each entrepreneur then faces a cost $F(\omega) = \exp(\mu_f)z^{\vartheta}$.

(iii) I assume that $\rho_z = 0$, so that z is identically and independently distributed over time. This implies that the distribution of assets is independent of productivity. I further assume that the asset distribution is exogenous and constant.

First let us suppose $\vartheta = 0$. An entrepreneur exports if the extra profits from doing so exceed the fixed cost of exporting. Formally, an entrepreneur exports if

$$\Delta\pi(z, a) \geq \exp(\mu_f) \tag{13}$$

where $\Delta\pi(z, a) = \pi_x(z, a) - \pi(z, a)$ denotes the extra profits the entrepreneur earns by exporting. Integrating (13) over productivity z defines the probability that a firm exports conditional on assets a. Likewise, integrating over a defines the probability a firm exports conditional on z. The solid lines in Figure 3 plot these conditional probabilities.

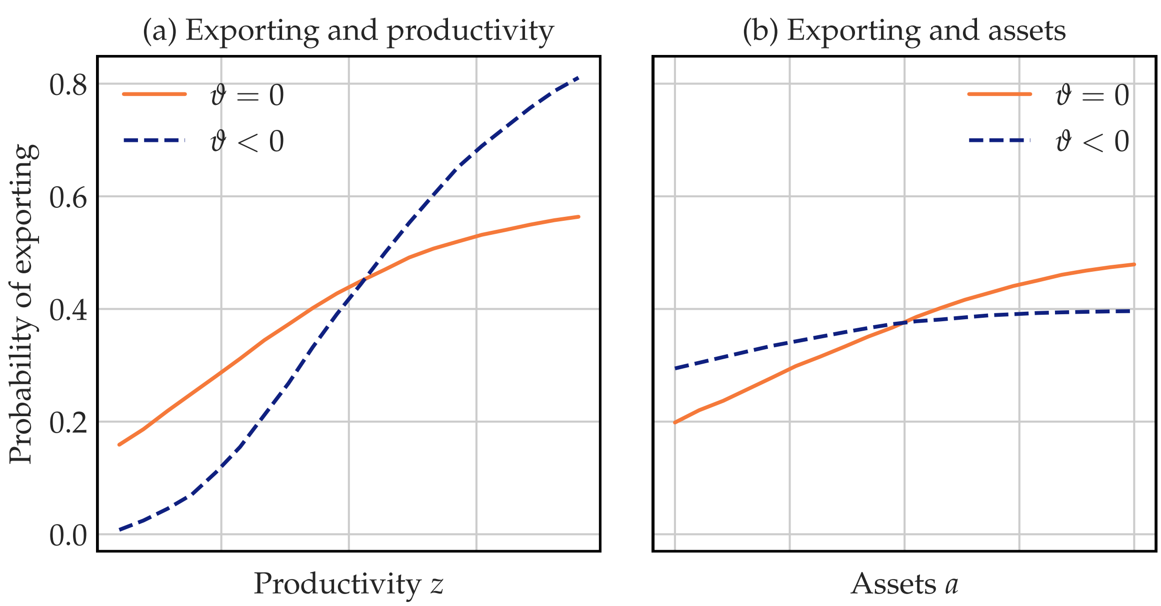
The solid line in Panel (a) of Figure 3 shows that the probability a firm exports increases with z. More productive firms are larger, and this makes overcoming a given fixed cost more worthwhile. This is the usual driver of selection into exporting in models with heterogeneous firms and fixed costs (Melitz 2003).

But Panel (b) shows that there is a second force at work in my model: the probability a firm exports is also an increasing function of its liquid assets a. A firm with low liquid assets cannot hire many workers and so produces at a small scale. Therefore such a firm doesn't find it worthwhile to start exporting. Moreover, even given its size, a firm with low a will not be able to expand when it does enter the export market, making $\Delta\pi(z, a)$ small for such a firm. Thus, the probability an entrepreneur exports increases in both z and a.

Importantly, z and a have opposing effects on the probability an entrepreneur is credit constrained. Holding a fixed, a higher productivity z raises labor demand and makes the firm more likely to hit a binding constraint. Holding z fixed, a higher a relaxes this constraint. Therefore the fact that exporters are selected on both of these dimensions makes it theoretically ambiguous whether exporters are more or less likely than non-exporters to be constrained.[7] In particular, if the decision to export is driven mainly by an entrepreneur's assets, few exporters and many non-exporters will be constrained. An immediate implication is that exporters will be inefficiently large, and policies that encourage them to expand will worsen misallocation.

Now let us suppose $\vartheta < 0$, so that more productive entrepreneurs are better at paying the fixed cost of exporting; this will turn out to be the empirically relevant case in Section 4. The dashed lines in Figure 3 show how $\vartheta < 0$ changes the decision to export. As ϑ becomes negative, the

Figure 3: The Determinants of Exporting



Notes: Panel (a) shows the probability a firm exports as a function of productivity z, in the simple model (i.e imposing Assumptions (i) - (iii) above). The solid line assumes ϑ, the productivity-fixed cost elasticity, is 0. The dashed line assumes $\vartheta < 0$. Panel (b) shows the same probability as a function of liquid assets a.

[7] To make this concrete, consider the following (trivial) special case: suppose $\sigma_z = 0$, so that entrepreneurs differ only in their assets a. The decision to export then depends only on a, and, if the fixed cost μ_f is sufficiently high, it is possible to show that no constrained entrepreneur will ever choose to export, because they cannot expand enough to recoup the fixed cost. This special case is conceptually similar to one analyzed by Bai, Jin, and Lu (2019), who show that if selection into exporting is entirely driven by distortions then the gains from trade must be negative.

probability a firm exports becomes more sensitive to productivity z. In addition to the scale motive that was present when $\vartheta = 0$, now a higher productivity directly lowers the export fixed cost. This is captured by the relatively steep dashed line in Panel (a). As z becomes a more important driver of exporting, assets a necessarily become less important, as shown by the relatively shallow dashed line in Panel (b).

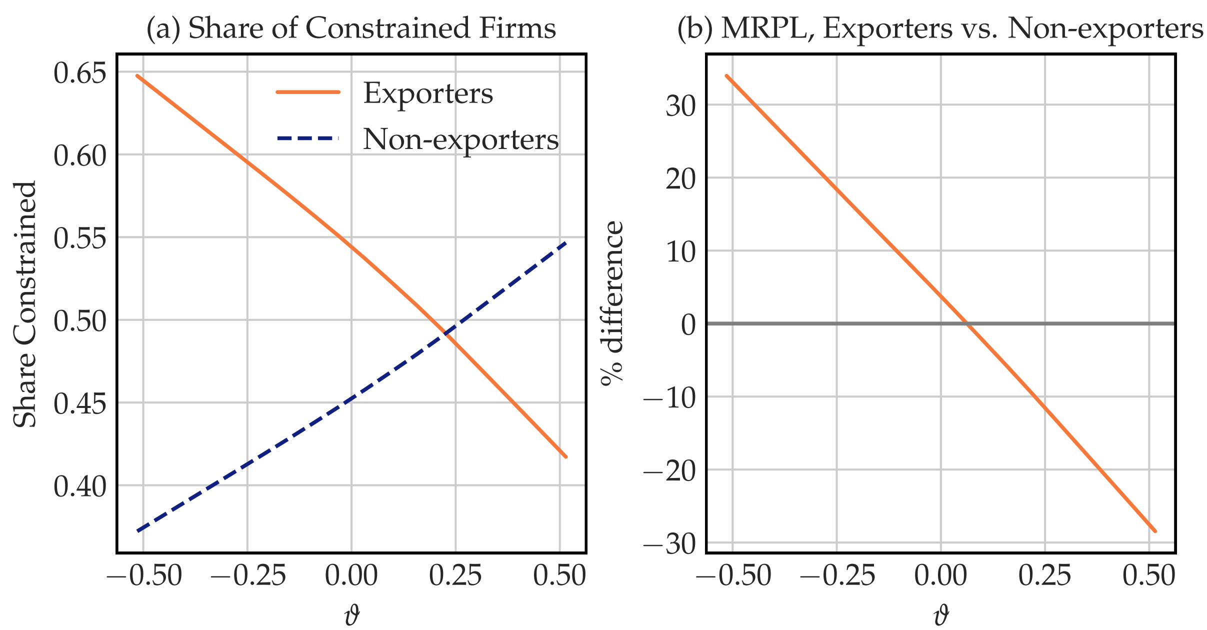
Figure 4 shows how this change in the drivers of the decision to export translates into a change in the characteristics of exporters and non-exporters. The solid line in Panel (a) plots the share of constrained firms among exporters, and the dashed line plots this share among non-exporters, as ϑ varies between -0.50 and 0.50. As ϑ rises, selection into exporting is less and less driven by productivity, so exporters become less likely to be constrained. Notice that when ϑ is sufficiently positive, constrained firms are more common among non-exporters. The following theorem makes the intuition above precise.

Theorem 1. *Suppose ϑ, the elasticity of fixed costs with respect to productivity, falls (i.e., becomes more negative). Suppose also that the average fixed cost μ_f varies so that the share of exporters remains constant. Then the share of constrained exporters rises, and the share of constrained non-exporters falls.*

See Appendix B for a proof.

Panel (b) of Figure 4 relates these constraints to misallocation. Here I follow Hsieh and Klenow (2009) and use the marginal revenue product of labor ($MRPL$) as a measure of misallocation.

Figure 4: Exporter Characteristics and ϑ



Notes: Panel (a) shows how the share of constrained firms among exporters (solid line) and non-exporters (dashed line) varies with ϑ, the productivity-fixed cost elasticity. Panel (b) shows the percentage difference in average marginal revenue products of labor between exporters as non-exporters. Consistent with Theorem 1, as ϑ varies I vary μ_f so that the share of exporters remains 0.43, its value in the Prowess dataset in 2007.

$MRPL$ is defined as

$$MRPL \equiv \frac{dR}{d\ell}$$

where R denotes a firm's revenue and ℓ its labor input. When $MRPL$ differs across firms, it is possible to raise aggregate output by reallocating labor towards high $MRPL$ firms. In my model, credit constraints create variation in $MRPL$. Unconstrained firms hire workers until $MRPL$ is equal to the wage w, while constrained firms have $MRPL > w$. Panel (b) shows that when ϑ is low, and many exporters are constrained, average exporter $MRPL$ is about 30% higher than non-exporter $MRPL$, i.e., exporters are inefficiently small. As ϑ rises, this gap changes sign and exporters become inefficiently large.

Summary

In this simple model, two forces — productivity z and assets a — shape an entrepreneur's decision to export. Which of these two forces dominates determines how many exporters are constrained relative to non-exporters, and in turn whether exporters are inefficiently large or small. The elasticity of fixed costs with respect to productivity, ϑ, governs the relative strength of these two forces.

The same two forces appear in the full model, i.e., without assumptions (i)-(iii) above. When z is not iid, productivity and assets will likely be positively correlated. As long as they are not perfectly correlated, however, both will play a role in the decision to export. Physical capital k, acting as collateral, behaves similarly to liquid assets by increasing an entrepreneur's ability to hire labor.

4 Estimation

I now estimate the parameters of the model developed in Section 3. I begin by externally calibrating several parameters. I follow Buera, Kaboski, and Shin (2021) and set the discount factor β equal to 0.85. I set the depreciation rate δ to 0.06 and the capital share α to 0.33. I set the elasticity of demand σ equal to 6.67, so that firm level returns to scale are $\left(\frac{\sigma-1}{\sigma}\right) = 0.85$ as in Midrigan and Xu (2014). I choose the iceberg trade cost τ so that in the model exporters sell 25% of their output abroad, the average in Prowess in 2007. Interest rates in the model are exogenous, so I follow Buera, Kaboski, and Shin (2021) and set the real interest rate on savings, r_a, to 0. I set the real interest rate on borrowing, r_b, to 5%, based on average (real) borrowing costs of firms in Prowess in 2007.[8]

Above I focused on a special case of the model with no dispersion in the export fixed cost shock (i.e., $\sigma_f = 0$) and showed that ϑ, the elasticity of fixed costs with respect to productivity, is a key parameter in determining the relative importance of credit constraints across exporters and

[8]The model also requires values for total demand D and for total employment L. I set these to 1 without loss of generality.

non-exporters. The same intuition continues to apply once we allow $\sigma_f > 0$, but what matters now is the magnitude of ϑ relative to σ_f. For this reason, it is useful to note that at each point in time productivity z and the overall fixed cost of exporting F are log normally distributed with correlation coefficient θ, where

$$\theta = \frac{\vartheta \sigma_z}{\sqrt{\vartheta^2 \sigma_z^2 + \sigma_f^2}}.$$

Note that θ is a monotonically increasing function of ϑ, and always has the same sign as ϑ. Since it is natural to think in terms of this correlation, I report my results in terms of θ rather than ϑ. Therefore, the parameters to be estimated are

(i) θ — the correlation between productivity and export fixed costs.

(ii) λ — the collateralizability of physical capital,

(iii) ρ_z and σ_z — the autocorrelation and standard deviation of the productivity process,

(iv) μ_f and σ_f — the mean and standard deviation of the export fixed cost shock,

(v) ϕ — the fixed cost of capital adjustment.

I estimate these parameters, plus a parameter that scales the size of the PSL policy (introduced below), by targeting 11 moments. These are the six treatment effects in Columns (3) and (4) of Table 2 and five descriptive statistics: the standard deviation of log sales growth, the autocorrelation of log sales, the fraction of firms that export, the average difference in log sales between exporters and non-exporters, and the frequency of investment 'spikes', defined as changes in firm-level capital stock above 20% in absolute value. These moments are summarized in Column (1) of Table 5. Note that the number of target moments exceeds the number of parameters to be estimated, i.e., the model is overidentified.

Mapping Model to Natural Experiment

The first part of my estimation strategy asks the model to match the effects of PSL in the data. I implement the PSL policy within the model by defining a cutoff c at the 35th percentile of the capital distribution.[9] Firms below the cutoff become more able to borrow, while those above experience no change. Formally I suppose the parameter λ, which determines the collateralizability of physical capital, now depends on k

$$\log \lambda(k) = \begin{cases} \log \lambda + T & \text{if} \quad k \leq c \\ \log \lambda & \text{if} \quad k > c \end{cases}$$

[9] I choose the 35th percentile because in 2007, 50 million rupees, the cutoff for PSL eligibility, was at this point in the plant and machinery distribution.

Table 5: Estimation

	(1) Data	(2) Model	(3) Model ($\theta = 0$)
(a) Moments			
Treatment Effects			
Non-exporter Loans	0.020	0.030	0.098
Non-exporter Employment	-0.000	0.027	0.083
Non-exporter Sales	0.110	0.015	0.049
Exporter Loans	0.328	0.335	0.215
Exporter Employment	0.246	0.284	0.181
Exporter Sales	0.224	0.162	0.109
Descriptive Statistics			
Standard deviation of log sales growth	0.526	0.526	0.523
Autocorrelation of log sales	0.971	0.970	0.971
Fraction of exporters	0.434	0.434	0.434
Log sales difference, exporters vs. domestic	2.403	2.410	2.410
Fraction of changes in capital above 20%	0.227	0.228	0.228
(b) Parameters			
Externally Calibrated			
β — Discount factor		0.850	0.850
δ — Deprecation rate		0.060	0.060
σ — Demand elasticity		6.667	6.667
α — Capital share		0.330	0.330
Estimated			
θ — Productivity - fixed cost correlation		-0.261	0.000
λ — Collateralizability		0.730	0.716
T — PSL scale		1.100	1.060
ρ_z — Productivity persistence		0.898	0.898
σ_z — Productivity standard deviation		0.055	0.055
μ_f — Export cost shock, mean		0.485	0.976
σ_f — Export cost shock, standard deviation		53.34	2.706
ϕ — Adjustment costs		0.230	0.228

Note: Column (1) of Panel (a) shows moments calculated from the Prowess dataset in 2007 — see Appendix C for details. Columns (2) and (3) show the same moments generated by the estimated model, with (3) imposing the restriction $\theta = 0$. Panel (b) shows values of externally calibrated and estimated parameters.

where $T \geq 0$ is a parameter to be estimated. As in Section 2 I measure the effects of this policy on log loans, employment and sales using a regression discontinuity design, and separate firms into exporters and non-exporters. Two points on timing are worth mentioning. First, in the model, I assume firms choose whether to export or not and then learn of the policy. This implies that the only immediate effect of the policy is to enable some firms to expand employment. Second, I measure the effects of the policy in the period in which it was implemented. This is different than in the data, where, motivated by the possibility that this policy might have been implemented with a lag, I estimated its effects over five years. To the extent that these estimates pick up long-run effects that differ significantly from short-run effects, my model's targets are off. However, in robustness checks in Appendix A I show that the estimated effects (in the data) are not very sensitive to the choice of years, suggesting this is not a serious problem.

To understand what parameters the natural experiment identifies,[10] consider taking a first order approximation around $T = 0$. Let C be an indicator equal to 1 if a firm is constrained and let E be an indicator equal to 1 if a firm exports. The average effect of PSL eligibility on (log) borrowing is then

$$\beta^b_{E=1} = [\mathbb{P}(C = 1|E = 1, k = c)]\, T,$$
$$\beta^b_{E=0} = [\mathbb{P}(C = 1|E = 0, k = c)]\, T.$$

$\beta^b_{E=1}$ is the effect on exporters and $\beta^b_{E=0}$ on non-exporters. Note that the policy only changes borrowing for constrained firms, and so its effects are scaled by the shares of constrained firms $\mathbb{P}(C = 1|E = 1, k = c)$ and $\mathbb{P}(C = 1|E = 0, k = c)$. Notice also that these shares are conditional on the firm having physical capital k equal to c, the cutoff for eligibility.

Recall from Subsection 3.5 that θ, the correlation between the export fixed cost and productivity, plays a key role in determining the relative importance of constraints for exporters and non-exporters, i.e., in determining $\mathbb{P}(C = 1|E = 1, k = c)$ and $\mathbb{P}(C = 1|E = 0, k = c)$. When $\theta < 0$, for example, exporters are strongly selected on productivity and many are constrained. Therefore $\beta^b_{E=1}$ and $\beta^b_{E=0}$ help identify T, the overall size of the treatment, and θ, which controls the difference in treatment effects between exporters and non-exporters.

Panels (a) and (b) of Figure 5 illustrate this argument. Here I fix all the other parameters at the values estimated in Table 5. Panel (a) varies T between 0 and 1.50 and plots the resulting treatment effect on borrowing for exporters and non-exporters. Both treatment effects increase monotonically as the scale of the PSL policy rises. Notice, however, that both treatment effects are concave functions of T because as the credit constraint is relaxed, many firms become unconstrained and stop borrowing more. Panel (b) shows the same treatment effects varying θ between -0.40 and 0.0. The exporter treatment effect falls as θ rises, while the non-exporter treatment effect rises. Thus, the difference between these two treatment effects is informative about this parameter.

Turning to employment, and continuing to use a first order approximation, these treatment

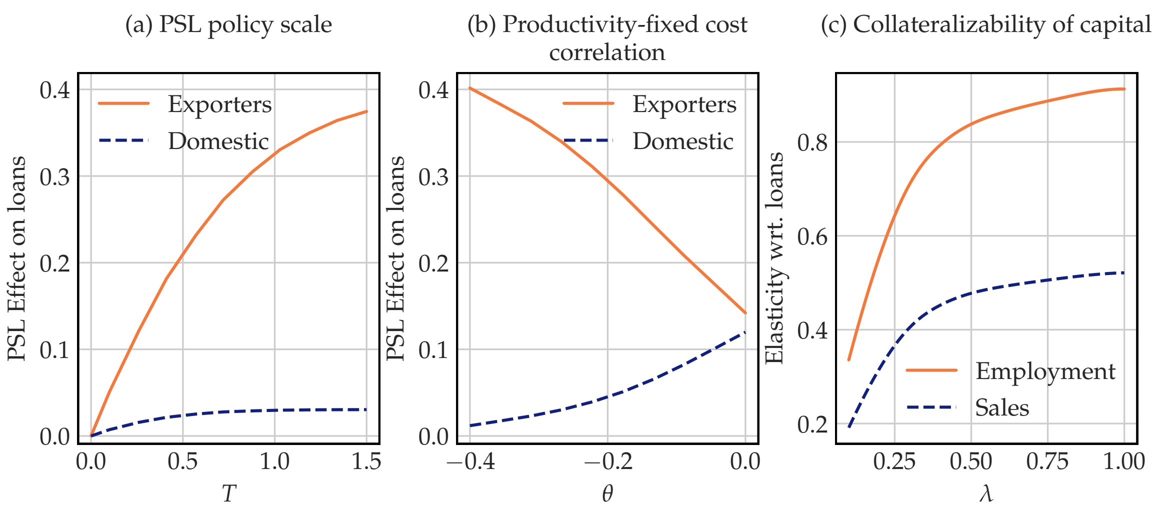
[10] Formally, all the targeted moments jointly identify all the parameters, so this discussion is purely heuristic.

effects are

$$\beta^{\ell}_{E=1} = \left[\int \left(\frac{\lambda c}{\lambda c + a} \right) d\mathcal{G}_{ss}(a|C=1, E=1, k=c) \right] \beta^{b}_{E=1},$$
$$\beta^{\ell}_{E=0} = \left[\int \left(\frac{\lambda c}{\lambda c + a} \right) d\mathcal{G}_{ss}(a|C=1, E=0, k=c) \right] \beta^{b}_{E=0},$$

where $\mathcal{G}_{ss}$ is the steady state joint distribution of states. $\beta^{\ell}_{E=1}$ is the treatment effect on employment for exporters and $\beta^{\ell}_{E=0}$ is defined analogously. The key object here is $\left(\frac{\lambda c}{\lambda c + a}\right)$, which is the elasticity of employment with respect to borrowing for a constrained firm at the cutoff with assets equal to a. This elasticity differs across firms depending on a, hence the integrals above. When λ is large, firms rely primarily on borrowing, captured by λc, to meet their financing needs, and this elasticity will be large. When λ is small, they instead rely on retained earnings, captured by a, and this elasticity will be small. Therefore the size of the treatment effects on employment helps identify λ. Of course, this argument ignores the fact that changes in λ will also change the steady state distributions of capital and assets. The solid line in Panel (c) of Figure 5 plots the employment elasticity as a function of λ, allowing these distributions to vary. The employment elasticity is a monotonically increasing function of λ, in line with the simple intuition above.

Figure 5: Parameters Identified by the Natural Experiment



Notes: Each plot is constructed by varying the parameter on the x-axis while holding all other parameters constant at their values in Column (2) of Table 5. The y-axis plots the moment(s) identifying each parameter. (a) shows the scale of the PSL policy T; (b) shows the productivity-fixed cost correlation θ; and (c) shows the collateralizability of physical capital λ.

Finally, the sales treatment effects are

$$\beta^s_{E=1} = \left[(1-\alpha)\left(\frac{\sigma-1}{\sigma}\right)\right]\beta^\ell_{E=1},$$
$$\beta^s_{E=0} = \left[(1-\alpha)\left(\frac{\sigma-1}{\sigma}\right)\right]\beta^\ell_{E=0}.$$

The sales treatment effects are mechanically related to the employment treatment effects — $(1-\alpha)\left(\frac{\sigma-1}{\sigma}\right)$ is just the elasticity of sales with respect to employment. Including the sales treatment effects therefore does not add anything to identification. Instead, they effectively provide an additional observation on the employment treatment effects and therefore help me estimate λ more precisely. The dashed line in Panel (c) of Figure 5 illustrates this point. The sales elasticity rises with λ and is proportional to the employment elasticity.

Additional Descriptive Statistics

The second part of my estimation strategy pins down the remaining parameters using the five descriptive statistics in Panel (a) of Table 5. I calculate these descriptive statistics using the Prowess dataset in 2007 — see Appendix C for details. The parameters ρ_z and σ_z have a direct relationship with the persistence of log sales and the standard deviation of log sales growth. μ_f controls the level of the fixed cost of exporting and is closely related to the share of firms choosing to enter the export market. Given μ_f, σ_f then determines the intensity of selection into exporting. When σ_f is large, the decision to export is almost random, and the sales of exporters will be only slightly larger than those of non-exporters. When $\sigma_f = 0$, by contrast, the sales distributions of exporters and non-exporters will be entirely disjoint, and average sales among exporters will be much higher than among non-exporters. The difference in average sales between the two groups therefore identifies σ_f. Finally, the frequency of investment 'spikes' pins down the capital adjustment cost ϕ, because a large ϕ incentivizes high investment rates conditional on adjustment (Cooper and Haltiwanger 2006; Asker, Collard-Wexler, and Loecker 2014).

Estimation Procedure

I estimate the parameters in (i) - (v) above, plus the scale of the PSL policy T, by the Simulated Method of Moments. For ease of notation I collect the 8 parameters to be estimated into a vector Ψ. I calculate the 11 moments in Panel (a) of Table 5 in the data and collect them into a vector $\widehat{M}$. Given a guess on Ψ, I solve for steady state distribution of states $\mathcal{G}_{ss}$ from Definition 2 in Section 3. I draw a sample of firms from $\mathcal{G}_{ss}$ and calculate the model analogues of the moments in $\widehat{M}$, and collect these model moments into a vector $M(\Psi)$. I define a loss function

$$L(\Psi) = \left(\widehat{M} - M(\Psi)\right)' W \left(\widehat{M} - M(\Psi)\right)$$

where W is a weight matrix — I use a diagonal matrix which weights each moment by the inverse of the square of its standard error. Finally I choose Ψ to minimize $L(\Psi)$. For more details, please see Appendix C.

4.1 Results

Parameter Estimates

Column (2) of Table 5 shows my main results. Panel (a) shows how the model fits the target moments and (b) shows the estimated parameters. I start by discussing the estimated parameters. The values of σ_z, ρ_z, and ϕ are similar to existing estimates (Cooper and Haltiwanger 2006; Asker, Collard-Wexler, and Loecker 2014).

I estimate that $\theta < 0$, implying that more productive entrepreneurs typically face lower export fixed costs. I also estimate a large σ_f, implying that the fixed cost of exporting f is very dispersed. This is a result of finding that selection into exporting is strongly driven by productivity, i.e., $\theta < 0$, while still matching the difference in average sales between exporters and non-exporters (the fourth moment in Table 5). A large σ_f implies that conditional on productivity z, exporters are almost randomly selected, which keeps the difference in average sales between exporters and non-exporters in the model consistent with its value in the data.

I estimate $\lambda = 0.73$, implying entrepreneurs can collateralize a large fraction of their physical capital. As Panel (c) of Figure 5 shows, this is a consequence of the relatively large employment and sales treatment effects I estimated for exporters. Finally, I estimate that $T = 1.10$, implying that PSL had a large effect on the borrowing constraints of eligible firms. Note that T is much larger than the effect of PSL eligibility on borrowing, even among exporters, because the constraint is only binding for a fraction of firms and because the treatment effect on borrowing is a concave function of T, as shown in Panel (a) of Figure 5.

Targeted Moments

Panel (a) shows how successful the model is in matching the target moments. It fits the five descriptive statistics (almost) perfectly. Figure 6 therefore focuses on the model's ability to replicate the six treatment effects in Column (1) of Panel (a). The orange dots show the exporter treatment effects from the data, with 95% confidence intervals, while the blue dots show non-exporter treatment effects. The solid bars show the corresponding treatment effects implied by the estimated model. The model is quantitatively successful in capturing the pattern observed in Section 2: significant effects of PSL eligibility for exporters and negligible effects for non-exporters.

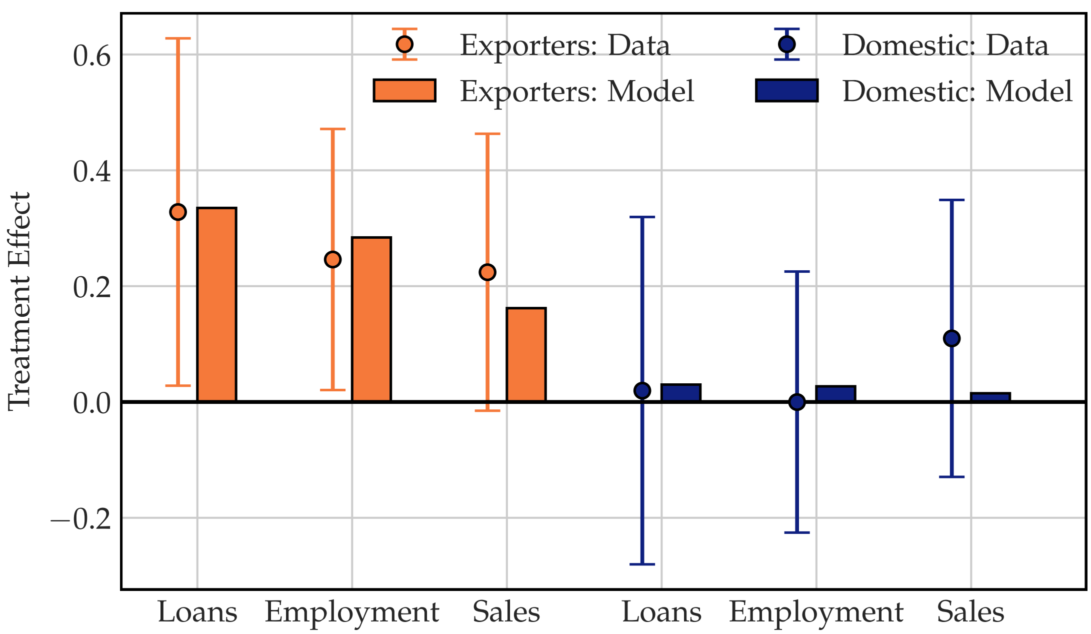
In Column (3) of Table 5, I explore the role of the productivity-fixed cost correlation θ by re-estimating the model imposing $\theta = 0$. While this model still fits the five descriptive statistics well, it is less successful in matching the pattern of treatment effects across exporters and non-exporters. In particular, it struggles to generate a large difference between exporters and non-exporters. For example, compared to Column (2), the difference between the exporter and non-exporter treatment effect on loans falls from 30.5 percentage points to 11.7 percentage points. I conclude that allowing for a rich pattern of selection into exporting by incorporating this correlation is crucial for enabling the model to match the estimated treatment effects.

Untargeted Moments

In addition to fitting the target moments well, the model also matches a number of untargeted moments, shown in Table 6. First, recall from Section 2 that in the data PSL eligibility had a negligible effect on the extensive margin of exporting, i.e., the probability a firm exports. The first row of Table 6 reproduces this figure and compares it to the same object in the model. Consistent with the data, in the model PSL eligibility has a tiny effect on the extensive margin of exporting — it raises the probability a firm exports by 0.00036. This is a natural consequence of the finding that the decision to export is largely driven by productivity. In Figure 3, we saw that when $\vartheta < 0$, so that productive entrepreneurs are more able to export, exporting is very insensitive to changes in an entrepreneur's ability to borrow.[11]

The next two moments relate to the the dynamics of exporting. In the data exporting is very persistent: 93% of firms that export in a given year continue to do so in the following year. The model generates a similar degree of persistence, with just over 90% of exporters in a given year

Figure 6: Treatment Effects: Model vs Data



Notes: 'Data' treatment effects from Columns (3) and (4) of Table 2. Error bars show 95% confidence intervals. 'Model' shows the treatment effects produced by the model at the parameters in Column (2) of Table 5.

[11]Note that in the simple model of Section 3, 'ability to borrow' is really a just an entrepreneur's liquid assets. But increasing an entrepreneur's assets has very similar effects to increasing its ability to collateralize physical capital, i.e to the PSL policy.

exporting in the following year. In the data export entry is also accompanied by fast sales growth. To measure this, I regress log sales growth on an indicator equal to 1 if a firm starts exporting in that year. The third row of Table 6 reports results from the data and from the model. The model replicates the pattern of fast sales growth upon entry found in the data, but in fact produces too much. This is likely because, in the model, the fixed cost shock f is time-invariant and so moves in and out of exporting are largely driven by shocks to productivity z, which is highly correlated with sales.

The final two rows report differences in input demands across exporters and non-exporters. In each case I regress log input demands (i.e., employment and physical capital) on industry fixed effects and a dummy for export status. The results in Column (1) indicate that exporters in the data hire more labor and use more capital than non-exporters, and that the gap is smaller for capital. Column (2) shows that the model is qualitatively consistent with these facts, and also gets the relative size of the differences in capital and labor right. Relative to the data, however, exporters in the model use too much labor and too little capital.

4.2 Implications for Misallocation

The estimated model implies that credit constraints are concentrated among exporters. Among firms close to the PSL cutoff in the model 45% of exporters are at a binding credit constraint, compared to only 8% of non-exporters. Panel (a) of Figure 7 shows how these numbers vary across the capital distribution. Among both exporters and non-exporters, the share of constrained firms falls as capital rises. At every point in the capital distribution, there is a large difference between exporters and non-exporters. Overall 37% of exporters and 8% of non-exporters are constrained.

An immediate implication is that inputs are misallocated across exporters and non-exporters. To measure this misallocation in the model and relate it to the data, I use the marginal revenue products of labor and capital. Letting R denote a firm's revenue and ℓ and k its labor and capital

Table 6: Untargeted Moments

Moment	(1) *Data*	(2) *Model*
Extensive margin effect of PSL	-0.021	0.000
Persistence of export status	0.933	0.905
Sales growth of new exporters	0.402	0.794
Log employment difference, exporters vs. non-exporters	2.066	2.309
Log capital difference, exporters vs. non-exporters	1.820	1.481

Note: 'Data' moments calculated using manufacturing firms in Prowess, 2006-07. See Appendix C for details. 'Model' moments calculated using a panel of simulated firms from the estimated model, parameterized following Column (2) of Table 5.

inputs, these are defined as

$$MRPL \equiv \frac{dR}{d\ell} = (1-\alpha)\left(\frac{\sigma-1}{\sigma}\right)\frac{R}{\ell}, \quad MRPK \equiv \frac{dR}{dk} = \alpha\left(\frac{\sigma-1}{\sigma}\right)\frac{R}{k}.$$

where the second equality is a consequence of the Cobb-Douglas and CES demand assumptions. These objects are a natural measure of misallocation. If one firm has a higher $MRPL$ than another, reallocating labor towards that firm would raise aggregate revenue. Similarly, if $MRPL$ is typically high among exporters, then reallocating labor towards exporters will raise aggregate revenue.

I begin by asking how $MRPL$ and $MRPK$ differ between exporters and non-exporters in the model. Formally I simulate data from the estimated model and run the regressions

$$\log\left(\frac{R_i}{\ell_i}\right) = \tilde{\alpha}_\ell + \varphi_\ell e_i + \epsilon_i \tag{14}$$

$$\log\left(\frac{R_i}{k_i}\right) = \tilde{\alpha}_k + \varphi_k e_i + \eta_i \tag{15}$$

where e_i is a dummy equal to 1 if firm i exports. Note that $\left((1-\alpha)\left(\frac{\sigma-1}{\sigma}\right)\right)$ and $\left(\alpha\left(\frac{\sigma-1}{\sigma}\right)\right)$ get absorbed by the additive constants $\tilde{\alpha}_\ell$ and $\tilde{\alpha}_k$. The coefficients φ_ℓ and φ_k capture differences in average $MRPL$ and $MRPK$ between exporters and non-exporters. The results are reported in Column (1) of Table 7, and indicate that exporter $MRPL$ is 9.4% and higher and exporter $MRPK$ is roughly 93% higher.

I also report results for revenue total factor productivity ($TFPR$), which aggregates $MRPK$ and $MRPL$ to give an overall measure of misallocation and is defined by

$$TFPR = MRPK^{\alpha} MRPL^{1-\alpha}. \tag{16}$$

The final row of Column (1) shows that exporter $TFPR$ is about 37% higher than non-exporter $TFPR$, and hence exporters are on average inefficiently small. Panel (b) of Figure 7 plots the distributions of $TFPR$ for both exporters and non-exporters. The distribution for exporters is clearly shifted to the right, but it is also true that there is substantial heterogeneity within each of these sets of firms. As we will see in Section 5, both dimensions of misallocation — between exporters and non-exporters, and within each group — will play an important role in determining the effects of any policy which targets exporters.

Given the Cobb-Douglas production and CES demand assumptions, I can also calculate $MRPL$ and $MRPK$ directly from the Prowess dataset and repeat these regressions.[12] Note an advantage of the log specification I use here is that variation in the capital and labor shares across industries can be absorbed by including industry fixed effects. I report the results in Column (2) of Table 7. Qualitatively the results are similar to those from the estimated model: exporter marginal revenue

[12] Given the log specification, in principle these MRPL and MRPK numbers could have been directly inferred from the log differences in sales and inputs between exporters and non-exporters, i.e., the fourth moment in Table 5 and the fourth and fifth moments in Table 6. In practice these two procedures give slightly different results, because employment or capital data are missing for some firms.

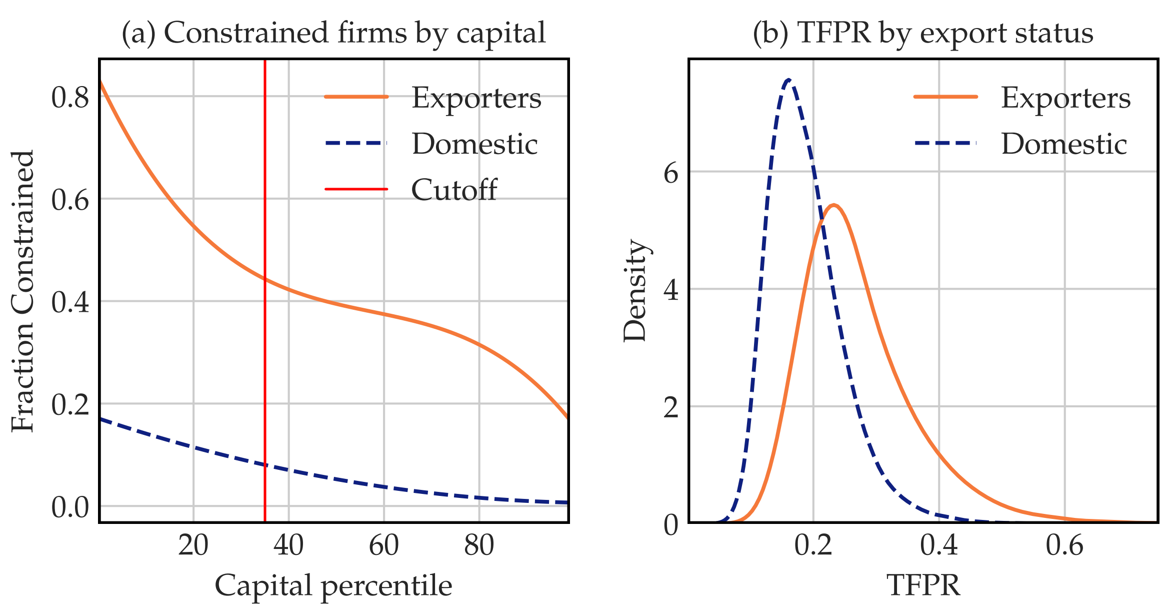
Table 7: Marginal Revenue Products: Exporters vs. Non-exporters

	(1) Model	(2) Data
Labor (MRPL)	0.094	0.162***
	-	(0.030)
Capital (MRPK)	0.930	0.515***
	-	(0.038)
Total (TFPR)	0.370	0.278***
	-	(0.030)

Note: Column (1) reports the (log) difference in marginal revenue products between exporters and non-exporters in the model, estimated using the regressions (14) and (15). The final row aggregates these with weights $(1-\alpha)$ and α to form an estimate of the the difference in log TFPR between exporters and non-exporters. Column (2) reports the same difference using data from manufacturing firms in the Prowess dataset in 2007. Note that the regressions in (2) include industry fixed effects. The final row of (2) is produced in the same way as the final row of (1).

* p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors in parentheses.

Figure 7: Exporting, Credit Constraints and Misallocation



Notes: Plots based on simulated data from estimated model, parameterized as in Column (2) of Table 5. Panel (a) plots the fraction of constrained firms in each percentile of the capital distribution; the dashed line shows domestic firms and the solid line shows exporters. Panel (b) plots the densities of revenue total factor productivity (TFPR) for exporters (solid) and non-exporters (dashed).

products are higher, and the gap is particularly large for capital. However, relative to the model, the data shows a larger gap in $MRPL$ and a substantially smaller one in $MRPK$. Aggregating as in (16), in the data exporter $TFPR$ is 28% higher than non-exporter $TFPR$.

5 Does Targeting Exporters Raise Productivity?

The key findings from the estimation in Section 4 are: (i), credit constraints are binding for many exporters but few non-exporters; and (ii), exporters are inefficiently small, and reallocating labor and capital towards them would raise aggregate productivity. These findings have the potential to provide an efficiency rationale for the many real-world policies which target exporting firms. For example, surveying the literature on East Asia's 'miracle economies', Itskhoki and Moll (2019) point out that policies which subsidized the input purchases of exporters or provided them with favorable access to credit were widespread. As I show below, my results also imply a source of gains, or losses, from trade absent from models in which inputs are allocated efficiently.

Subsection 5.1 defines the outcome of interest — total factor productivity (TFP) — and shows theoretically how changes in the allocation of input across firms may affect it. Subsection 5.2 studies the effects of two specific policies on TFP. The first policy directly relaxes the credit constraint of exporting firms, while the second subsidizes their employment. These two policies cause comparable amounts of reallocation towards exporters. Surprisingly, however, I show that they have very different consequences for aggregate productivity. Finally, Subsection 5.3 uses the estimated model to quantify the effect of reductions in trade costs on TFP. I show that any gains from reallocation are modest, and contrast this finding with the results of a model in which misallocation is the result of exogenous wedges in input markets.

5.1 Reallocation and Total Factor Productivity

First, some notation. Let K denote total capital and L_p total labor used in production (recall the export fixed cost absorbs some labor). Recall from Section 3 that $\omega \equiv (z, f, k, a)$ indexes an entrepreneur's state and that $\mathcal{G}$ denotes the distribution over those states. Let Ω_x denote the set of states ω in which an entrepreneur chooses to export and let Ω_d be the states in which they choose to produce only for the domestic market. Let

$$S_{\ell x} = \left(\int_{\Omega_x} \ell d\mathcal{G} \right) L_p^{-1}, \quad S_{kx} = \left(\int_{\Omega_x} k d\mathcal{G} \right) K^{-1}$$

denote the shares of labor and capital held by exporters, and define $S_{\ell d}$ and S_{kd} analogously. Finally, let

$$s_\ell(\omega) = \begin{cases} \left(\frac{\ell(\omega)}{L_p} \right) S_{\ell x}^{-1} & \text{if} \quad \omega \in \Omega_x \\ \left(\frac{\ell(\omega)}{L_p} \right) S_{\ell d}^{-1} & \text{if} \quad \omega \in \Omega_d \end{cases}$$

be the labor demand of an entrepreneur with state ω, relative to total labor demand of the group — exporters or non-exporters — to which ω belongs. Define $s_k(\omega)$ analogously.

Now we are in a position to define TFP, denoted by Z. Aggregating over firms, total real output in this economy can be written

$$Y = ZK^{\alpha}L_p^{1-\alpha}.$$

Because inputs are not necessarily allocated efficiently, Z depends on how inputs are allocated across firms. Since my focus is on reallocation between exporters and non-exporters, a helpful way of writing Z decomposes it into the allocation of inputs *between* these two sets of firms, and the allocation of inputs *within* each of these sets of firms. Formally

$$Z = \left(\left(S_{kd}^{\alpha} S_{\ell d}^{1-\alpha} \right)^{\frac{\sigma-1}{\sigma}} Z_d^{\frac{\sigma-1}{\sigma}} + \left(1 + \tau^{1-\sigma} \right)^{\frac{1}{\sigma}} \left(S_{kx}^{\alpha} S_{\ell x}^{1-\alpha} \right)^{\frac{\sigma-1}{\sigma}} Z_x^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}} \tag{17}$$

where

$$Z_d = \left(\int_{\Omega_d} \left(z s_k^{\alpha} s_{\ell}^{1-\alpha} \right)^{\frac{\sigma-1}{\sigma}} d\mathcal{G} \right)^{\frac{\sigma}{\sigma-1}}, \quad Z_x = \left(\int_{\Omega_x} \left(z s_k^{\alpha} s_{\ell}^{1-\alpha} \right)^{\frac{\sigma-1}{\sigma}} d\mathcal{G} \right)^{\frac{\sigma}{\sigma-1}}. \tag{18}$$

We can think of Z_x and Z_d as the productivity of a representative exporter and non-exporter, respectively.

Taking a first order approximation, any change in the allocation of capital and labor can also be decomposed into changes in between-group misallocation, and changes in within-group misallocation[13]

$$d \log Z = \underbrace{\left(\frac{\sigma}{\sigma - 1} \right) \left(dS_{kx} \left(\overline{MRPK}_x - \overline{MRPK}_d \right) + dS_{\ell x} \left(\overline{MRPL}_x - \overline{MRPL}_d \right) \right)}_{\textit{Between}} \tag{19}$$

$$+ \underbrace{\left(\rho_d d \log Z_d + \rho_x d \log Z_x \right)}_{\textit{Within}},$$

where ρ_x is the share of exporters in total revenue and ρ_d the share of non-exporters. $\overline{MRPK}_x$ is the average (input-weighted) marginal revenue product of capital among exporters and $\overline{MRPK}_x$, $\overline{MRPL}_x$, and $\overline{MRPL}_d$ are defined analogously.

This expression shows the basic logic underlying any policy directed towards exporters. Exporters have high marginal revenue products relative to non-exporters, i.e., $\overline{MRPK}_x > \overline{MRPK}_d$ and $\overline{MRPL}_x > \overline{MRPL}_d$. Therefore reallocating inputs from non-exporters towards exporters should raise TFP. But it also highlights that this argument comes with an important caveat: such reallocation is beneficial provided it does not simultaneously worsen misallocation within the sets of exporting or non-exporting firms, as captured by $d \log Z_x$ and $d \log Z_d$.

[13] In this decomposition I hold the sets of exporting and non-exporting firms Ω_x and Ω_d fixed. Such extensive margin changes turn out to be quantitatively negligible for the counterfactuals I consider.

5.2 Policy Interventions

Relaxing the Credit Constraint of Exporters

I suppose the policymaker can relax the credit constraint of exporting firms. Formally, the parameter λ now depends on a firm's export status and is denoted by λ_d for non-exporters and λ_x for exporters. I assume $\lambda_d = 0.73$, the value estimated in Section 4, but that for exporters it rises according to

$$\log \lambda_x = \log \lambda_d + \Delta_x.$$

Lacking a natural magnitude for Δ_x, I choose the size of the PSL policy estimated in Section 4 and set $\Delta_x = 1.10$. Starting from the steady state implied by the parameters estimated in Section 4, I shock the economy with this change in λ. I solve for the new equilibrium in the short run, holding the joint distribution of states (productivity, export fixed costs, capital, and liquid assets) constant; and in the long run, allowing this distribution to converge to its new steady state.

In the short run the share of employment in exporting firms rises by 2.30%, and in the long run this rises to 2.68%. Exporters also increase their share of the capital stock by 0.85%. Column (1) of Table 8 shows the effects on TFP. In keeping with (19), I decompose the overall effect into two components. First, I calculate a 'Between' component by holding the allocation of inputs within each set of firms constant and allowing the aggregate shares S_{kx}, etc., to vary. Second, I hold the aggregate shares fixed and calculate a 'Within' component by allowing the input share of each firm within the set of exporters and non-exporters to vary. Overall, TFP rises by 2.47% in the short run and by 3.33% in the long run. Both components make a positive contribution at every time horizon, but the largest source of gains is reduced misallocation within each set of firms. Constrained exporters drive this positive effect. As the credit constraint is relaxed, these firms, which have relatively high marginal products, expand and pull labor out of less productive firms.

Table 8: Decomposing Policy Effects on TFP

	(1) Credit Policy		(2) Employment Subsidy	
	Short Run	*Long Run*	*Short Run*	*Long Run*
Between	0.09	0.07	0.09	0.24
Within	2.36	3.20	-0.04	-0.37
Overall	2.47	3.33	0.05	-0.12

Note: Each row shows the percentage increase in TFP caused by each component for each policy. 'Between' shows the effect of each policy, holding the allocation of inputs within the sets of exporting and non-exporting firms constant but allowing the aggregate shares of exporters and non-exporters to vary. 'Within' holds the aggregate shares constant but allows the allocation within the sets of exporting and non-exporting firms to vary. 'Overall' allows both to vary. Note that 'Within' and 'Between' need not sum to exactly 'Overall' because of second order terms.

Subsidizing Exporter Employment

The experiment above shows that relaxing the credit constraint of exporters can yield substantial TFP gains. It is not obvious, however, that this is a margin a policymaker can manipulate. I now ask whether a policymaker can achieve similar results using simpler instruments. In particular, I suppose a policymaker observes the high average marginal revenue product of labor among exporters and reasons that this misallocation could be resolved by an employment subsidy directed towards exporters. If workers receive a wage w, the wages facing exporters and non-exporters are

$$w_x = w(1 + t - t_x), \quad w_d = w(1 + t).$$

where t is a tax chosen so that the government budget balances.[14] I set the subsidy $t_x = 0.083$, chosen so that in the short run the percentage increase in exporter employment caused by this policy matches the short run effect of the credit policy above. Thus the two policies cause comparable amounts of reallocation between exporters and non-exporters.

The results of the employment subsidy policy are shown in Column (2) of Table 8. Again I show effects in both the short and long run and decompose these into between and within components. In the short run, the employment subsidy causes a very small increase in TFP. In the long run, however, its effects change sign and TFP falls by 0.12%. Looking at the decomposition, we can see that the subsidy and credit policies have similar effects on reallocation between exporters and non-exporters (by construction). But the two policies have dramatically different effects on misallocation within each set of firms.

Understanding the Two Policies

Figure 8 illustrates the difference between these two policies, focusing on exporters. I summarize the extent of misallocation in each firm using revenue total factor productivity (TFPR), as defined in (16). For each policy and each firm, I calculate the change in its share of capital, $\Delta \log s_k$, and the change in its share of labor, $\Delta \log s_\ell$. I aggregate these to form $\Delta \log s$, defined by

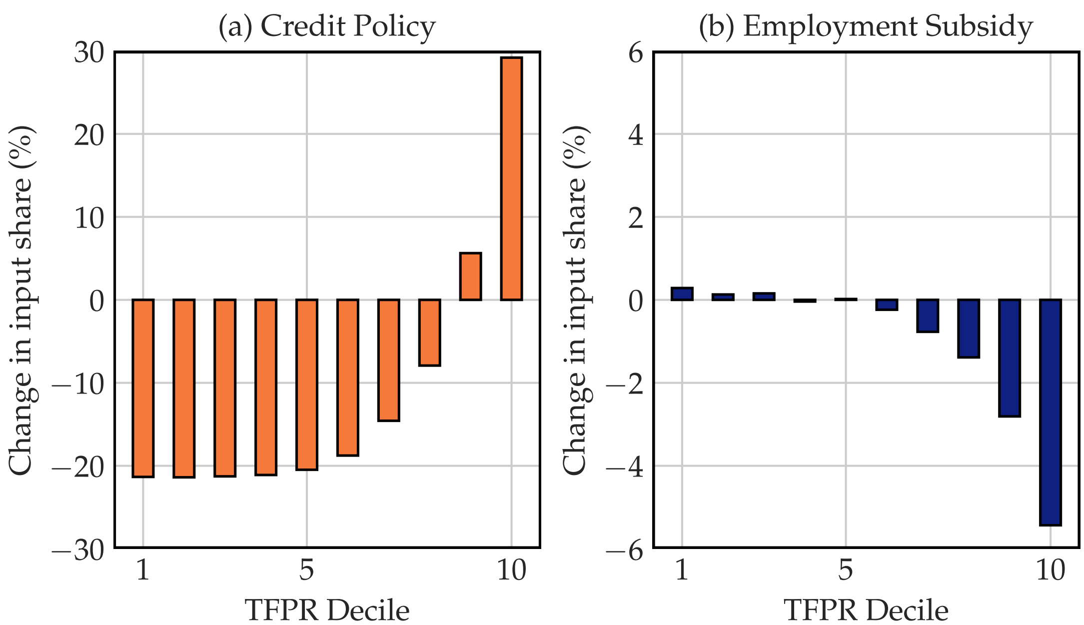
$$\Delta \log s = \alpha \Delta \log s_k + (1 - \alpha) \Delta \log s_\ell.$$

Finally, I plot averages of $\Delta \log s$ by TFPR deciles for each policy. Figure 8 shows the results; a positive number indicates that firms in that TFPR bin on average grew as a result of a particular policy. Panel (a) shows the credit policy. The primary beneficiaries are constrained exporters with high TFPR. Since these firms were initially inefficiently small, aggregate TFP rises.

Panel (b) shows the employment subsidy. This policy causes less reallocation within exporters than the credit policy; notice that the scale in Panel (b) is smaller. More importantly, the reallocation that this policy does cause worsens misallocation. To see why this should be the case,

[14] Note that because labor is supplied inelastically, the symmetric tax t is not distortionary. All the effects of this policy are therefore the result of the fact that it targets exporters.

Figure 8: Reallocation Among Exporters Under Two Different Policies



Notes: Each plot bins exporters by TFPR and shows the average change in inputs within each bin. Panel (a) shows the effect of relaxing the exporter credit constraint; this policy has the largest effect on the exporter's with the highest TFPR. Panel (b) shows the effect of the exporter employment subsidy; this policy caused the highest TFPR exporters to contract relative to low TFPR exporters.

compare the elasticity of labor demand with respect to the wage subsidy between constrained and unconstrained firms. For a constrained firm this elasticity is

$$\frac{d\log \ell}{d\log t_x} = 1,$$

but for an unconstrained firm this elasticity is

$$\frac{d\log \ell}{d\log t_x} = \left(\frac{1}{1-(1-\alpha)\left(\frac{\sigma-1}{\sigma}\right)}\right) \simeq 2.5.$$

Thus the subsidy causes unconstrained exporters to expand much faster than constrained ones. Since constrained firms typically have high marginal products, this explains the pattern in Panel (b) of Figure 8. Since low TFPR exporters expand at the expense of high TFPR exporters, aggregate TFP falls.

These results suggest that subsidies are not effective instruments in resolving the misallocation created by credit constraints. Even though in my counterfactual the subsidy was targeted to a group with a high share of constrained firms (i.e exporters), it backfired because it primarily benefitted unconstrained firms with low marginal products. The basic logic of this point goes beyond this specific example: subsidies are effective if the targeted firms expand in response, but almost by definition, constrained firms are unable to do this. Therefore subsidy policies will typically cause exactly the wrong firms to expand and worsen misallocation. On the other hand, an intervention that directly addresses the source of misallocation can yield a large improvement in TFP.

5.3 Trade, Reallocation and Credit Constraints

Since Melitz (2003) reallocation across firms has been central to accounts of the effects of trade liberalization, and two recent papers (Berthou et al. 2019; Bai, Jin, and Lu 2019) ask how this reallocation might affect aggregate productivity when inputs are misallocated across firms. I revisit this question using my model, in which misallocation is the result of specific distortions, i.e., credit constraints, as well as adjustment costs in physical capital. I start by extending the decomposition (19) to incorporate changes in the iceberg trade cost τ

$$
\begin{aligned}
d\log Z = & \underbrace{-(1-x_d)d\log\tau}_{\textit{Direct}} + \underbrace{(\rho_d d\log Z_d + \rho_x d\log Z_x)}_{\textit{Within}} \\
& + \underbrace{\left(\frac{\sigma}{\sigma-1}\right)\left(dS_{kx}\left(\overline{MRPK}_x - \overline{MRPK}_d\right) + dS_{\ell x}\left(\overline{MRPL}_x - \overline{MRPL}_d\right)\right)}_{\textit{Between}}
\end{aligned} \tag{20}
$$

where x_d is the share of expenditure devoted to domestically produced goods. The 'Direct' term here reflects the effect of falling trade costs holding the allocation of inputs across firms constant. Atkeson and Burstein (2010) show that in a large class of models this term captures all the gains from trade, but in my model this is not the case. Instead, the reallocation of inputs caused by falling trade costs may have a first order effect on productivity. These gains, or losses, from reallocation are captured by the final two terms in (20).

I shock the model with an exogenous decrease in the iceberg trade cost. I choose $d\log\tau = -0.10$, so that trade costs fall by roughly 10%.[15] I solve for the new equilibrium in both the short run and the long run, and decompose the change in TFP following (20). The results are shown in Column (1) of Table 9. Overall TFP rises by 0.428% in the long run, but reallocation contributes very little to this figure for two reasons. First, the reduction in τ causes very little reallocation between exporters and non-exporters, as can be seen from the relatively small magnitude of the 'Between' component in Table 9. Second, any gains from reallocation between these two groups are largely offset by worsening misallocation within each group. This is because lower trade costs are in this respect similar to the exporter employment subsidy studied in Subsection 5.2. The exporters most able to expand in response to falling trade costs are the relatively unproductive, unconstrained ones, who then drag aggregate TFP down.

These results suggest that merely knowing that in the cross-section exporters have relatively high TFPR is insufficient to conclude that trade will raise aggregate productivity via reallocation.

[15] I choose a relatively small shock so that the first order approximation in (20) is reasonably accurate.

To make this point concrete, I repeat this exercise in a model in which misallocation is the result of exogenous wedges in input markets, as in Hsieh and Klenow (2009) — similar models have been used by Berthou et al. (2019) and Bai, Jin, and Lu (2019) to study the gains from trade under misallocation. Formally, I construct a second economy in which labor and capital can be hired freely (i.e., without credit constraints, adjustment costs etc.), but the prices of these factors vary across firms depending on exogenous wedges. I choose distributions for these wedges so that this model exactly replicates the joint distribution of sales, employment, capital and export status from the original model. The two models thus have identical cross-sectional implications, and in particular yield identical differences in marginal revenue products between exporters and non-exporters. For complete details see Appendix D.

Column (2) of Table 9 shows the effect of a 10% reduction in trade costs in this second model. Three things are worth nothing. First, by construction the 'Direct' term is in identical in both models, since this does depend on how firms respond to changes in their environment. Second, much more reallocation occurs in the model with exogenous misallocation — the 'Between' gains in this model are about seven times larger than those from the original model. This is a natural consequence of removing the frictions in the original model — there, inputs were misallocated precisely because reallocation was difficult. Third, falling trade costs do not have any effect on 'Within' misallocation in this model. Conditional on export status, all firms respond symmetrically to falling trade costs, leaving the distributions of inputs within the sets of exporters and non-exporters unchanged. The net result is that the model with exogenous misallocation implies large gains from reallocation; these are roughly half as large as the direct effect of falling trade costs.

Overall, the results in Table 9 show that a model in which misallocation is the result of exogenous distortions is a poor guide to the effects of falling trade costs. Instead, accurately measuring

Table 9: Trade Cost Reductions and TFP

	(1) *Full Model*		(2) *Model with Exogenous Misallocation*
	Short Run	*Long Run*	
Direct	0.411	0.411	0.411
Between	0.004	0.029	0.200
Within	-0.003	-0.016	0.000
Overall	0.412	0.428	0.612

Note: Column (1) shows the effect of a 10% reduction in iceberg trade costs in the estimated model, in the short run and in the long run. Column (2) shows the effects of the same reduction in a model in which misallocation is the result of exogenous wedges — see Appendix D for details. Each column decomposes the effect on TFP into three components following (20). Note that the 'Overall' effect is not exactly the sum of these three components, because of second order terms absent from (20).

how policy changes interact with misallocation hinges on explicitly modeling the source of misallocation.

6 Conclusion

This paper's main findings can be summarized in three points. First, I used a natural experiment in India to show that exporters responded strongly to an exogenous increase in credit supply, while non-exporters did not. Second, I estimated a model of credit constraints and exporting by targeting the results of this natural experiment. I found that, in an environment with several dimensions of heterogeneity, productivity was the key driver of the decision to export. Since more productive firms are more likely to find credit constraints binding, the result is that credit constraints bind for many exporters; overall, 37% of exporters in the model are constrained, compared to only 8% of non-exporters. Finally, in counterfactual experiments I showed that different policy interventions that target exporters have very different effects on misallocation. Relaxing the credit constraint facing exporting firms significantly raises aggregate TFP because it allows the most productive exporters to expand. Simply subsidizing exporter employment has the opposite effect because the primary beneficiaries are unconstrained and relatively unproductive exporters. The same logic implies that credit constraints limit the productivity gains from reallocation driven by falling trade costs.

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A Empirical Appendix

Data

I use data from the Prowess dataset compiled by CMIE. Specifically, I consider firms in the 'manufacturing superset', as defined by CMIE. Among these firms, I drop any which have missing information on total borrowing, wage bills or sales. I also drop any missing plant and machinery in 2007, the baseline year for my regression discontinuity design. I also drop any firms which have very large growth rates in absolute value for any of these variables; in practice I drop firms for which these growth rates are in the top or bottom 1% across all firms.

Firms in Prowess differ in the dates on which they report financial statements. I adopt the following convention: if a report is made in the first six months of year t, I date it to year $t-1$ on the grounds that most of the production the report refers to took place in $t-1$. Firms also differ in the time span their financial statements cover: although most report information covering 12 months, a few report information for shorter timespans. Where this is the case, I rescale the flow variables (wage bills and sales) to a yearly frequency.

Regression Discontinuity Design Details

I implement the RDD estimation using the `rdrobust` Stata package created by Calonico et al. (2017). In particular, in each regression I use the MSE optimal one-sided bandwidth, a triangular kernel, and a first-order local polynomial. Standard errors are based on plug-in residuals, and are clustered at the firm level. In Table 5 I report the 'conventional' point estimates and standard errors, i.e.,without the robust bias-correction suggested by Calonico, Cattaneo, and Titiunik (2014). However I also report results with this correction in Column (4) of Table 11.

Borrowing Costs

In Table 10, I investigate whether PSL eligibility lowered firms' borrowing costs. Here I replicate the specification of Columns (3) and (4) of Table 2, but use as the outcome (log) borrowing costs. Prowess measures this as total expenses on financial services — i.e interest payments and fees — divided by total borrowing. Column (1) shows results for exporters and (2) for non-exporters. The positive number in Column (1) implies that if anything, exporters eligible for PSL faced higher borrowing costs relative to those that were ineligible. The discontinuity for non-exporters in Column (2) is much smaller and has the opposite sign. Neither estimate is statistically significant. I conclude that PSL did not change eligible firms' borrowing costs; in particular, lower borrowing costs cannot explain the positive effects of PSL eligibility on exporters in Table 2.

Bunching Checks

Figure 9 plots the density of (log) plant and machinery in 2007 for firms close to the cutoff. Panel (a) shows exporters and (b) shows non-exporters. Visual inspection shows that in neither figure is there a mass of firms just to the left of the cutoff, as one would expect if firms were strategically choosing their plant and machinery in order to become or remain eligible for PSL. To formalize this observation, I use the test statistic of Cattaneo, Jansson, and Ma (2020). I find values of 1.5729 (p-value $= 0.1157$) for exporters and 1.0793 (p-value $= 0.2804$) for non-exporters. The positive values indicate that if anything there are slightly too few firms to the left of the cutoff — the opposite of what we would expect if firms were choosing their plant and machinery to become eligible for PSL. Neither estimate is significantly different from zero, therefore I do not reject the null hypothesis of no bunching for either group.

Robustness Checks

Table 11 shows the results of a number of robustness checks. The baseline these results should be compared against is Columns (3) and (4) of Table 2. Each column shows results for loans, employment and sales among exporters in Panel (a) and non-exporters in Panel (b). The first three columns assess the sensitivity of my results to technical aspects of the regression discontinuity design specification. Column (1) halves the optimally chosen bandwidth and Column (2) doubles it. Although the point estimates move around a bit, qualitatively my results do not seem overly sensitive to the choice of bandwidth. Column (3) performs the 'donut hole' check suggested by Cattaneo, Idrobo, and Titiunik (2019), in which I drop the 5% of observations closest to the 50 million rupee cutoff. The idea here is that these are the observations most susceptible to manipulation, and so (3) acts as a check of how sensitive my results are to manipulation. Dropping these observations does not make a dramatic difference to my results. Column (4) applies the bias-correction suggested by Calonico, Cattaneo, and Titiunik (2014); this does not change my point estimates much, although because the bias must be estimated it inflates the standard errors.

Table 10: Effect of Priority Sector Lending on Borrowing Costs

	(1) *Exporters*	(2) *Non-exporters*
	0.094	-0.006
	(0.073)	(0.083)
Years	2008-12	2008-12
N	10145	6857

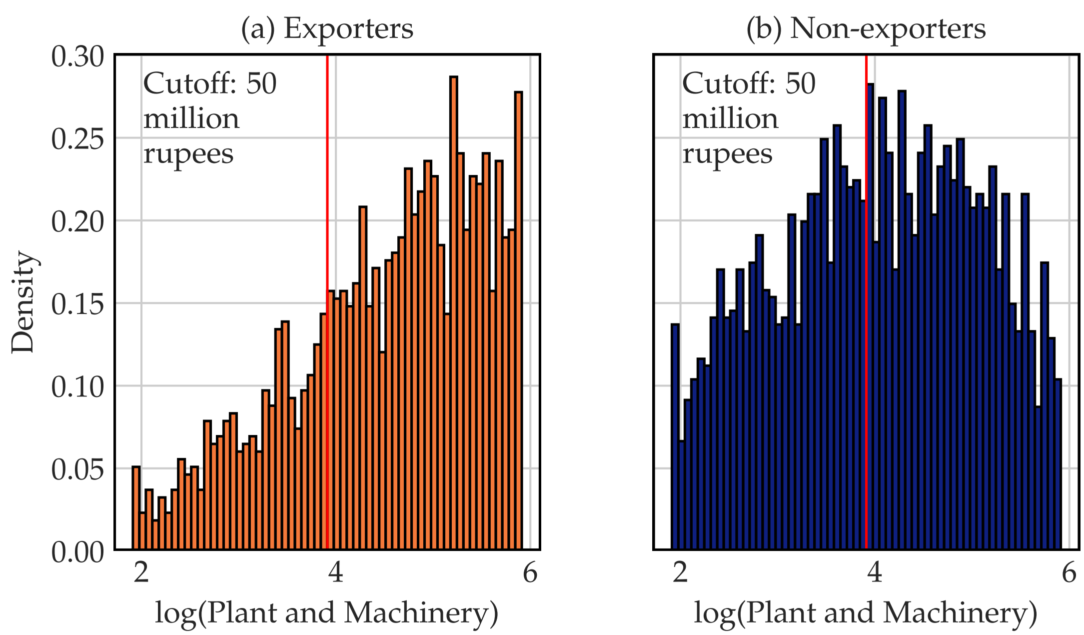
Source: Prowess Dataset, all manufacturing firms, 2005-2012

Note: Each column reports the estimated discontinuity in log borrowing costs at plant and machinery equal to 50 million rupees in 2007. The outcome is measured between 2008 and 2012. All regressions control for year, industry and firm age fixed effects. Additionally I control for 2005 loans, sales and employment, and 2005 log borrowing costs. (1) shows results for exporters and (2) shows results for non-exporters. Note that a positive number implies that firms eligible for PSL — i.e those to the left of 50 million rupees — faced higher borrowing costs.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Column (5) shows that my results are not sensitive to the choice of years I use for measuring outcomes; I drop the first and last years and continue to find very similar results. Finally Column (6) drops all controls other than year fixed effects. The results for exporters are noisier and not statistically significant, although qualitatively similar. For non-exporters the point estimates are somewhat larger than my baseline results, but still not statistically significant.

Figure 9: Bunching Checks



Source: Prowess dataset, manufacturing firms, 2007. *Notes*: Each panel shows a histogram of log plant and machinery for firms close to the 50 million rupee cutoff for PSL eligibility. Panel (a) shows exporters and Panel (b) shows non-exporters. In neither plot is there an obvious mass to the left of the cutoff, i.e there is no visual evidence of bunching.

Table 11: Robustness Checks

	(1) *Narrow Bw.*	(2) *Wide Bw.*	(3) *'Donut Hole'*	(4) *CCT*	(5) *Short Timespan*	(6) *No Controls*
(a) Exporters						
Loans	0.391*	0.275**	0.380**	0.331*	0.319*	0.259
	(0.208)	(0.114)	(0.175)	(0.182)	(0.181)	(0.184)
Employment	0.375***	0.161*	0.271*	0.282**	0.320***	0.172
	(0.136)	(0.087)	(0.164)	(0.135)	(0.119)	(0.151)
Sales	0.301**	0.177*	0.323**	0.227	0.255*	0.112
	(0.149)	(0.093)	(0.161)	(0.145)	(0.132)	(0.164)
(b) Non-Exporters						
Loans	−0.055	0.023	−0.037	0.021	0.086	0.128
	(0.155)	(0.092)	(0.139)	(0.136)	(0.132)	(0.131)
Employment	−0.013	−0.003	−0.181	0.008	0.006	0.122
	(0.097)	(0.061)	(0.127)	(0.086)	(0.087)	(0.132)
Sales	0.028	0.050	−0.038	0.142	0.126	0.193
	(0.145)	(0.089)	(0.138)	(0.132)	(0.124)	(0.160)
Years	2008-12	2008-12	2008-12	2008-12	2009-11	2008-12
Pre-policy controls	Yes	Yes	Yes	Yes	Yes	No

Source: Prowess Dataset, all manufacturing firms, 2005-2012

Note: Columns show results for different specifications; rows show results for different outcomes. Each estimate reports the discontinuity in the outcome at plant and machinery equal to 50 million rupees. Plant and machinery measured in 2007; export status defined using sales in 2007. All outcomes are measured in logs and a positive number indicates a positive effect of being eligible for Priority Sector Lending. (1) and (2) vary the bandwidth used in estimation; (3) drops the 5% of observations closest to the cutoff; (4) applies the bias-correction suggested by Calonico, Cattaneo, and Titiunik (2014); (5) measures outcomes in a shorter time window; and (6) drops all controls except year fixed effects.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

B Theory Appendix

Proof. Proof of Theorem 1

Let θ^0 and ϑ^1 denote two different values of ϑ, the productivity-fixed cost elasticity. Let μ_f^0 and μ_f^1 denote corresponding levels of the export fixed cost. Suppose $\vartheta^1 < \vartheta^0 \leq 0$ and suppose μ_f^1 is chosen so that the probability a firm exports does not change. Define

$$\Omega_x^0 = \{(z,a) | \Delta\pi(z,a) \geq \exp(\mu_f^0) z^{\vartheta^0}\},$$

$$\Omega_x^1 = \{(z,a) | \Delta\pi(z,a) \geq \exp(\mu_f^1) z^{\vartheta^1}\},$$

as the sets of exporters under these two parameterizations. Finally let the sets of 'entering' and 'exiting' exporters be

$$\text{Entering} = \overline{\Omega}_x^0 \cap \Omega_x^1$$

$$\text{Exiting} = \Omega_x^0 \cap \overline{\Omega}_x^1$$

where the bars denote complements. Suppose a firm with state (z,a) belongs to 'Exiting'. Then from the definitions of each of the sets Ω_x^0 and Ω_x^1,

$$\log z \leq \log \bar{z} \equiv (\vartheta^0 - \vartheta^1)^{-1} \left(\mu_f^0 - \mu_f^1\right).$$

The same reasoning implies that for any (z,a) in 'Entering'

$$\log z \geq \log \bar{z}.$$

Thus, every firm which starts exporting has a higher productivity than every firm which ceases exporting.

Now, let (z',a') be the productivity and assets of an arbitrary exiting firm, and (z'',a'') the productivity and assets of an arbitrary entering firm. We have established that $z'' \geq z'$ above. Because the entering firm did not initially export while the exiting firm did, the follow inequality must hold

$$\Delta\pi(z',a') \left(z'\right)^{-\vartheta^0} < \Delta\pi(z'',a'') \left(z''\right)^{-\theta^1}.$$

Each side is clearly increasing in z, and $\Delta\pi(z,a)$ is increasing in a. So this inequality can only hold if $a'' < a'$, i.e entering firms must have lower assets than exiting firms.

An exporting firm is constrained if

$$\log z > \left(\frac{1}{\sigma - 1}\right) \log a + B$$

where B is a constant. I have shown above that when ϑ falls, any firm which starts exporting has higher z and lower a than any firm which stops exporting. Thus if any firm which stops exporting is constrained, every new exporter is also constrained. If any firm which starts exporting is unconstrained, every firm which stops exporting is unconstrained. Either way, the net effect must be that the share of exporters who are constrained (weakly) rises. The same logic applies to non-exporters. When ϑ falls, every new non-exporter has lower productivity and higher assets than every exiting non-exporter. Thus the share of constrained non-exporters must (weakly) fall.

□

C Estimation Appendix

Targeted Moments

The estimation targets 11 moments. 6 of these are the PSL treatment effects in Column (3) and (4) of Table 2 and 5 are descriptive statistics calculated using the Prowess dataset. The descriptive statistics are:

(i) The standard deviation of log sales growth: I calculate this by first regressing log sales in 2006 and 2007 on industry fixed effects, to absorb variation created by industry level shocks absent from my model. I then calculate the standard deviation of the differences in the residuals from this regression. Note that I drop drop observations with very large growth rates in absolute value; in practice I drop firms with growth rates in the top or bottom 1% of the distribution of growth rates.

(ii) The autocorrelation of log sales: I follow the same procedure as in (i), but calculate the correlation between residuals in 2006 and 2007.

(iii) The fraction of exporters: I simply calculate the fraction of firms in Prowess in 2007 with positive export sales.

(iv) The difference in log sales between exporters and non-exporters: I regress log sales on industry fixed effects and an exporter dummy, again using firms from Prowess in 2007.

(v) The frequency of investment 'spikes': For each firm I calculate the change in log capital between 2006 and 2007. I define a spike as a log change greater than 0.20 in absolute value.

Estimation Procedure

As mentioned in the text, I choose a vector of parameters Ψ to minimize

$$L(\Psi) = \left(\widehat{M} - M(\Psi)\right)' W \left(\widehat{M} - M(\Psi)\right).$$

$M(\Psi)$ must be calculated by simulation.

I implement this by first solving the entrepreneur's dynamic programming problem from Section 3. I do this using discrete grids for z and f. In each case I use a grid with 15 points, having verified that my results are not sensitive to this choice. Instead of directly using capital k and liquid assets a as state variables, I define the state as total assets $t = k + a$ and the share of these held as capital, s. Clearly this is without loss of generality. For these endogenous state variables, I use cubic spline interpolation across discrete grid points. For total assets I use a grid which is evenly spaced in logs, and use 50 grid points. For the share of assets held as capital, I use an evenly spaced grid with 20 points.

Having solved this problem for a policy function g, I then draw a sample of N observations on the exogenous states f and z from their respective stationary distributions. Starting from an arbitrary distribution of k and a, I then simulate the policy function g, along with shocks to z, until the the joint distribution of states converges (this takes roughly 200 periods). I then calculate the descriptive moments (i)-(v) and simulate the PSL policy as described in the text. In practice I set $N = 20,000$, having verified that my results are not sensitive to this choice. I collect these moments into a vector $M(\Psi)$. To choose the value of Ψ which minimizes $L(\Psi)$, I use a global search algorithm (controlled random search with local mutation) from the Fortran implementation of NLOPT.

Untargeted Moments

In 6 I report values of 5 untargeted moments. The first of these is from Table 4, Column (1). I now describe the construction of the remaining four moments:

(i) The persistence of exporting: I calculate the probability that an exporting firm in Prowess in 2006 is still exporting in 2007, conditional on remaining active (i.e having positive overall sales).

(ii) Log sales growth of new exporters: I construct a dummy equal to 1 if a firm was active in 2006 but not an exporter, and exports in 2007. Call this $\textit{New Exporter}_{it}$. I then run the regression

$$\Delta \log R_{it} = \beta_0 + \beta_1 \textit{New Exporter}_{it} + \epsilon_{it}$$

where $\Delta \log R_{it}$ is the growth rate of sales between t and $t-1$. I use data from Prowess in 2007. Table 6 reports an estimate of β_1.

(iii) Differences in input demands: I run the regression

$$\log y_{its} = \gamma_s + \gamma_1 e_{its} + \epsilon_{its}$$

where y_{its} is an input — either capital or labor — and e_{its} is a dummy equal to 1 if the firm exports. s indexes sectors and γ_s is a sector fixed effect. Table 6 reports estimates of γ_1 for labor and capital.

D A Model with Exogenous Misallocation

In this Appendix, I develop a model in which misallocation is the result of exogenous, firm-specific wedges in input markets. I show that the wedges can be chosen so that the model has exactly the same cross-sectional implications as the model with endogenous misallocation developed in Section 3 and estimated in Section 4 — I refer to this as the full model. I also show that the model with exogenous misallocation has very different implications for the counterfactuals considered in Section 5. In particular I show that, for both the employment subsidy and the reduction in trade costs, this model predicts exactly zero change in the 'Within' component of misallocation. This explains the fact that in Column (2) of Table 9, the 'Within' component is exactly zero.

Firms differ in their productivities z, which are drawn from the stationary distribution of the AR(1) process (1). Production functions are Cobb-Douglas, as in (2), and demand is CES as in (3). Labor is hired at a wage w, without any credit constraints. However, firms differ in a labor wedge τ_ℓ. A firm with wedge τ_ℓ faces an effective wage $w\tau_\ell$. Likewise, capital is hired at a rental rate r_k, and firms have capital wedges τ_k. This implies that firms set their marginal revenue products equal to

$$MRPK = \tau_k r_k, \quad MRPL = \tau_\ell w.$$

Since $MRPK$ and $MRPL$ will generally differ across firms, the equilibrium allocation will not maximize total sales, and therefore aggregate TFP. Hence, these wedges give rise to misallocation. Finally, I assume that a firm's export status is exogenously given.

Now, suppose we observe a cross-section of firms generated by the full model. In particular, suppose we observe their sales, employment, capital and export statuses. For each firm from the full model, we can generate an identical firm in the model with exogenous misallocation. To see this, note that observing a firm's inputs, sales and export status pins down its productivity z. Then we can choose wedges τ_ℓ and τ_k for this firm so that its employment and capital exactly match those generated by the full model. Finally, choose the firm's export status to match that of the full model. Following this procedure, for any cross-section of firms generated by the full model, the model with exogenous misallocation can generate an identical cross-section. An immediate implication is that average $MRPK$ and $MRPL$ among exporters and non-exporters will be the same across the two models.

By aggregating over the decision of individual firms in the model with exogenous misallocation, we can arrive at the same expression for TFP as in (17). However, this is where the similarities between the two models end. Below I show that Z_x and Z_d, the productivity of a representative exporter and non-exporter, respectively, are entirely determined by the joint distributions of τ_ℓ, τ_k and z. These distributions are not altered by any policy interventions, i.e., changes in trade costs or the exporter employment subsidy. Hence, in this model, $d \log Z_x = d \log Z_d = 0$ and changes in the 'Within' component of misallocation are always zero. Intuitively, this is because exogenous wedges shift a firm's labor or capital demand up or down, but do not change the elasticities with which these demands respond to changes in prices or wages. Therefore, conditional on export status, all firms in a given group (i.e., exporters or non-exporters) respond symmetrically, leaving the productivity of the representative exporter or non-exporter unchanged.

In the model with exogenous wedges, the labor demand ℓ of any non-exporter is

$$\ell = c_d z^{\sigma-1} \tau_\ell^{\sigma-\alpha(\sigma-1)} \tau_k^{-\alpha(\sigma-1)}$$

where c_d is a constant common to all non-exporters. Denote the (exogenous) joint distribution of z, τ_ℓ and τ_k among non-exporters be $\mathcal{G}_d$. Integrate to obtain total employment in non-exporting firms

$$L_d = c_d \int z^{\sigma-1} \tau_\ell^{\sigma-\alpha(\sigma-1)} \tau_k^{-\alpha(\sigma-1)} d\mathcal{G}_d.$$

Now the share of any non-exporter is

$$s_\ell = c_d z^{\sigma-1} \tau_\ell^{\sigma-\alpha(\sigma-1)} \tau_k^{-\alpha(\sigma-1)} L_d^{-1}.$$

Observe that this share does not depend on any endogenous variables because c_d cancels. By the same steps, we can obtain an analogous expression for s_k — this also does not depend on any endogenous variables.

Now, consider the definition of Z_d (18). This is an integral over productivities z and the shares s_ℓ and s_k. We have just seen that that these shares are entirely determined by the (exogenous) joint distributions of productivities and wedges. So Z_d is also exogenously determined, and does not depend on iceberg costs or the exporter employment subsidy. Therefore $d \log Z_x = 0$ in response to either of these policies. A similar argument applies to Z_x, and implies $d \log Z_x = 0$. Since the 'Within' component is just a weighted average of these it is also zero.