Export tax rebates and resource misallocation: Evidence from a large developing country*

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Abstract. The export tax rebate (ETR) policy is one of the most frequently used policy instruments by Chinese policy makers. This paper provides a vital analysis of its allocation effects. We use customs transactions, tax administration, and firm-level data to measure the effect of variation in export tax rebates, taking advantage of the large policy change in 2004. A difference-in-difference approach allows us to compare the production and pricing decisions of eligible versus non-eligible firms and the distributional implications. We tie these distributional results to a structural model akin to Hsieh and Klenow (2009) where incomplete tax rebates act as a tax on revenue of export sales. A reduction in tax rebates shifts production away from rebate-eligible firms and decreases allocative efficiency. Our takeaway is that by adjusting its VAT policy as a part of broader policy objectives, China introduces an allocative efficiency dimension that must be taken into consideration.

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1 Introduction

Chinese exports have grown at spectacular rates since the 1980s, prompting a line of intense research on policies that have played a role in China's transformation. This paper is a contribution to this literature. Specifically, we focus on the role of China's export tax rebates (hereinafter, ETRs), which work like a negative value-added tax (VAT). The Chinese government uses ETRs to refund part of its domestic taxes to Chinese exporters while also taxing imports. Our focus is on how the adjustment of ETRs distort Chinese firms' allocation of resources between its exporting and non-exporting firms.

Theoretical studies claim that the use of a VAT system cannot promote the country's exports or improve its competitiveness (see, e.g., Feldstein and Krugman (1990)). By contrast, empirical evidence seems to indicate that trade performance is affected by VATs and rebates for exports. This is likely due to the fact that theoretical models (such as in the aforementioned paper) assume that complete export rebates are combined with VATs. Intuitively, exports should be exempted from VAT payments to avoid double taxation as they will be taxed in the destination country. In the case that complete export refunds are *not* a given, as is the case in China, then lowering (raising) the export rebates is akin to raising (lowering) taxes on exporters. This paper aims to quantify the implications of a policy where ETRs are not complete and can therefore be adjusted. We argue that this adjustment of ETRs is important not only because it affects export performance, but that it has allocative efficiency consequences as well. A VAT system that does not fully rebate taxes to exporters creates a distortionary wedge on export sales (that is not present for non-exporters), and leads to an inefficient allocation of resources.

We connect to the trade and macro literatures by linking the incomplete tax rebate for exporters to an aggregate measure of misallocation. We start with micro data that allows us to estimate the impact of changes in the ETR policy across rebate-eligible and non-eligible firms. Consistent with the literature on trade performance, reducing the rebate reallocates production away from eligible firms. Our novel argument is that the non-rebated VAT paid by exporters can be viewed as a distortionary tax that are not subjected to the rest of the firms. This is because, as highlighted in Feldstein and Krugman (1990), a "neutral" policy would be to rebate all VAT to exporters who then pay tax on sales abroad that is consistent with their competition in that destination. We demonstrate the allocative efficiency consequences of incomplete rebates in a simple macroeconomic model with trade, where non-rebated VAT inflicts a wedge between total revenues and total output for eligible firms. The model is used to build a measure of misallocation at the aggregate industry level that is a result of aggregating firm-level responses. We argue that China's decision to reduce rebates in 2004 had the predicted effect of raising misallocation as it further distorted decisions made by exporters.[1]

This study contributes to the literature with a rigorous empirical examination of the impact of ETRs on firms' production decisions and its implication for resource allocation between exporting and non-exporting firms. This is possible by combining not only Chinese firm-level production data with customs transactions as is the case in virtually all related studies (see below), but also by adding data on export tax rebate rates from the State Taxation Administration (EVATR). The EVATR data set records export tax rebates at the HS 10-digit. With information in the production and customs databases along with EVATR, we can calculate a firm-specific ETR along with sales and export performance. In contrast to the policy of complete rebates that is standard, China does not rebate all VAT paid to exporters and in fact the government continuously alters the policy as a part of its broader objectives. We leverage the fact that China significantly reduced the rebates across the board in 2004 and compare the differential effects to eligible and non-eligible firms.

We first restrict the sample to the customs transactions of eligible exporters, with data on their export values, quantities and net tax paid, in order to show that a decrease in rebates leads to a lower quantity sold. Although we expect that eligible firms should lower their

[1]The focus on the 2004 policy change is due to data availability. In 2018, China *raised* its rebates as it faces disruptions amid the trade dispute with the United States. This reinforces the fact that research on the consequences of adjusting rebate rates is necessary.

prices, the evidence here is inconclusive as we find that the change in unit values for eligible firms in response to lower rebates are not distinguishable from zero. However, the large drop in export values provides evidence for the distortionary effect on eligible firms. When we aggregate to the firm level and merge with the national census that provides revenue and input data, we can then produce firm-level productivity and markup estimates. By construction these confound domestic and export sales, thus obscuring the direct effect of the tax rebate. However, the distributional effects allow us to measure the reallocation of production between eligible and non-eligible firms as a consequence of the rebate policy. A difference-in-difference specification identifies the effect on eligible firms using non-eligible firms as a control group. We verify that the parallel trends assumption is satisfied when revenue productivity is used as an outcome and argue that a decrease in tax rebates shifts production away from eligible firms and reduces their profits. For example, although there is no difference in log value added per worker trends between the two groups before 2004, the log value added per worker in eligible firms drops on average 7.7% relative to non-eligible firms post-2004. This is robust to the inclusion of controls for other concurrent policy decisions made during this period and holds whether we allow for firm entry or hold the composition of firms fixed. The results are consistent with a model in which incompletely rebating the VAT acts as a tax on exporters relative to non-exporters.

The next part of the paper is devoted to studying the allocative efficiency implications of the policy change at the aggregate level. We do this by first modeling the incomplete rebates as a wedge akin to a tax on revenues that is heterogeneous across firms. The setup is similar to Hsieh and Klenow (2009), but firms are allowed to sell both in both the domestic and global market. A fraction of firm sales are exports, and these are subject to the incomplete VAT wedge, in addition to an export trade cost. This generates a result that is familiar in the misallocation literature, where revenue productivities are different across firms which is due solely to the firm-specific distortions. In the case of this paper, these differences in revenue productivity across firms is driven entirely by the fraction of firm sales that are exports. The model provides a testable prediction, that as export rebates decline, the difference in revenue productivity between eligible and non-eligible firms will increase. At the industry level, a higher share of eligible firms is associated with a larger dispersion in revenue productivity.

To test the direct prediction on allocative efficiency, we aggregate the data to the industry level to produce misallocation measures consistent with our model. The firm-level data is used to create a standard deviation of revenue TFP (TFPR), the firm TFP that is estimated using the production function estimation on revenue (and input) data. In addition, we produce alternative dispersion measures as well as dispersions of alternative firm outcomes. These measures are common in the literature (Hsieh and Klenow, 2009; Oberfield, 2013). We find robust evidence that a reduction of the rebates raises industry misallocation. On aggregate, the back-of-the-envelope implication is that the higher dispersion in TFPR lowers China's TFP by 1.8% relative to what it would have been in 2006 had the policy to reduce rebates not occurred in 2004.

There are a number of recent empirical studies that support the non-neutrality of export taxes. For example, Desai and Hines (2005) analyze data for 168 countries from 1950 and 2000, and obtain results consistent with the expectation that VATs reduce export performance. In their own words, "countries that rely heavily on VATs export and import less as a fraction of GDP than do other countries, and the negative relationship between VATs and exports persists after controlling for observable variables." Similarly, Nicholson (2013) analyzes bilateral U.S. data for 146 countries across 29 sectors and 12 years, and finds that VATs reduce exports. Similar results have been found by analyzing whether rebates on exports (a way to eliminate VATs on exports) can improve trade performance measures. Chandra and Long (2013) use firm-level Chinese data from 2000 to 2006 and find that a one percentage point increase in the average ETR rates raises the quantity of exports by 13%. Poncet et al. (2014) utilize HS6 product-level Chinese export volume data for a longer time period (from 2003 to 2012) and find a smaller but similar effect, i.e., a one percentage point increase in ETR rates leads to only a 7% increase in export volume.

We focus on China as well, which acts as an interesting test case because the VAT system is not neutral. In addition, ETRs have fluctuated in China over time and are heterogeneous across industries. Our paper diverges from the previously mentioned studies by focusing on Chinese production allocation instead of export performance. This is an important distinction as misallocation can be an important factor reducing productivity in developing countries (Restuccia and Rogerson, 2008). Therefore, aside from considering productive efficiency, there is room for policies to also consider seriously the effect on allocative efficiency. We study a case where a tax system treats exporters and non-exporters differently, which implies that there are idiosyncratic distortions.

Since we model the incomplete rebate as a tax on revenues, our paper fits most naturally in the aforementioned studies that incorporate distortions to the supply side of the firm. However, there is also literature, mostly within international trade, that studies demand-side distortions through non-homothetic preferences. In this case, firms face heterogeneous elasticities of demand, which leads to larger firms charging larger markups. Our paper is connected to this recent work on variable markups and allocative efficiency.[2] Our paper is also related to theoretical and empirical studies that highlight the links between trade policy and firm markups. Recent empirical work by DeLoecker and Warzynski (2012) uses plant-level data from the Slovenian manufacturing industry and identifies the markup advantages of exporters relative to non-exporters. Lu and Yu (2015) find that markup variation decreased in China due to tariff reductions implemented after joining the WTO. In our paper we control for such events by using industry-year fixed effects in our firm level analysis to compare across rebate eligible and non-eligible within industries. With industry-level variation this is not possible, but we control for the major policy reforms in China that have been documented in the literature. A related theoretical work is Demidova and Rodriguez-Clare (2009) who study a small economy with distortions and find that trade policy has allocative efficiency implications through its effect on the markup distortion. Demidova (2017) and Dalton

[2] A few examples include: Edmond et al. (2015), Dhingra and Morrow (2016), Peters (forthcoming), and Weinberger (2020).

and Goskel (2013) both apply the Melitz and Ottaviano (2008) model to policy. The former applies the model to the implementation of output tariffs, and the latter to a closed economy model where higher taxes result in higher markups. Since incomplete rebates raise taxes on exporters, our theoretical motivation is similar to the latter result. We are able to study the impact of such a tax on resource allocation by merging Chinese firm-level manufacturing data with customs transactions and tax information.

The paper proceeds as follows. Section 2 introduces the data and provides background on China's trade policy. In Section 3 we investigate firm-specific outcomes as a response to China's reduction of ETRs by comparing eligible to non-eligible firms across our sample period. We also implement various robustness specifications. A model with distortions that can be mapped to the rebate policy is described in Section 4 and its testable predictions are taken to industry data. Section 5 concludes.

2 Data and Policy

2.1 Data

We use three data sets for the empirical analysis: all state-owned manufacturing firms and the above-scale (sales above 5 million Renminbi) private manufacturing firm panel data from the Chinese National Bureau of Statistics' (NBS) annual surveys (CASIF); transactions-level trade data from China Customs (CCTS) that cover all transactions of Chinese exporters and importers; and export tax rebates rate data from State Taxation Administration (EVATR). All data sets are from 2000 to 2006. The industries covered include CIC (Chinese Industry Classification) 13-42 with each firm being given a four-digit and two-digit classification.[3] The CASIF data set contains the main balance sheet information, with more than 130 financial variables for each firm. The CCTS data set provides information on import and export

[3]The textile industry is not included in this data, which means the quota changes in 2005 should not affect our results.

values, quantities, destination at the HS eight-digit level for each trading firm. The EVATR data set records export tax rebates at the HS ten-digit level. With information from the CCTS data set and EVATR, we calculate a firm's average ETR. As a note, we deflate all nominal values using 2000 as the base year.

Although this data set contains rich information, a few variables are noisy and misleading. We follow Cai and Liu (2009) and Feenstra et al. (2014) in cleaning the sample as follows: First, we drop all firms with less than eight employees, as they fall under a different legal regime. Then, we drop all firms with missing or negative key financial variables (such as total assets, net value of fixed assets, intermediate inputs, and total wages payable). Finally, we drop all firms with value of total fixed assets or value of total flowing assets outweighing value of total assets, and value of exports that outweigh gross value of industrial output. We merge the manufacturing firm data with the transaction level trade data using two methods from Yu and Tian (2012). First, we merge manufacturing firm data with the transaction level trade data based on firm names and data year. We interpret two firms to be the same one if they use the same firm name in the two data sets in the same year. Second, we merge the two data sets based on zip code and the last seven numbers of the firm phone number and drop all invalid samples (including zip codes are less than six numbers and phone numbers are less than seven numbers in the two data sets). After cleaning the sample, there are 1,304,636 observations.[4] Exporting firms account for 396,422 and non-export firms 908,214 (close to 3/4 of the total number of observations). Table 1 reports statistics of the CASIF, CCTS, and merged data sets.

[Table 1 about here.]

[4]Comparing our total firms in the merged data to the number of firms in CASIF implies we lose about 7% of firms in our procedure above. The *non-merged* firms make up 4.15% of revenues, 4.6% of VA, and 6.2% of labor in the full CASIF sample. These firms are on average smaller than merged firms, although not largely so: their average labor force is 217 employees whereas the merged samples has an average of 226 employees.

2.2 Export Tax Rebate Policy

China is an interesting case study for the export tax rebate policy due to its constant adjustments. Although the policy is technically "destination-based," the policy has shifted from fully rebating exports (as in the European Union), to only partial rebates, with the fraction of the credit also varying over time. China began using ETRs in 1985, and starting in 1988 they fully refunded export VATs (Cui (2003)). Since then, China has frequently adjusted the ETR rates, or, equivalently, the VAT refund levels. For example, they reduced the rebates in 1995 and 1996 due to budget shortfalls and then raised them (for some products) after the Asian Financial Crisis in 1998 and 1999 (Ferrantino et al. (2012)). We show below that another large adjustment happened in 2003, during our sample period, which we leverage for the differences-in-differences analysis. Prior to 2004, these adjustments were aimed mainly at increasing foreign exchange reserves and boosting the country's economic growth. After 2004, however, adjustments were intended to influence industrial structure and promote the balanced growth of imports and exports.

The calculation of value added tax owed by Chinese firms has been documented in the previous ETR literature (Ferrantino et al., 2012; Poncet et al., 2014), but we summarize it here. To start, the tax payable for Chinese firms is:

$$\text{VAT Payable} = \text{Output VAT} - (\text{Input VAT} - \text{NCNR}) \tag{1}$$

where NCNR is the non-creditable and non-refundable amount and Input VAT is the tax paid on domestically sourced inputs. Since VATs are destination based, exports do not pay output VAT and the calculation for *exporters* becomes simply the difference of NCNR and Input VAT paid, with the following definition for NCNR:

$$\text{NCNR} = (\text{X} - \text{BIM}) * (\text{VAT} - \text{reb}) = (\text{X} - \text{BIM}) * (\tau_H), \tag{2}$$

where X represents export value, vat is the tax rate, reb is the tax-rebate rate, and BIM are tax-free imported inputs (allowed for "processing exporters" which we define in the next paragraph). The difference between the VAT and the rebate is non-zero in the case of incomplete rebates, and we summarize this difference with τ_H.

Next, we define "eligible" and "non-eligible" firms. The most obvious set of non-eligible firms are non-exporters who cannot be affected by the rebate policy. When we make use of the whole set of domestic producers, non-exporters make up the majority of the non-eligible firms. However, there are also non-eligible exporters, which are comprised of "processing exporters". These firms import intermediate inputs from abroad – with tariff and VAT exemptions – then process/assemble them in China in order to export the final good. These processing exporters are the ones that are allowed to buy the BIM and hence reduce their tax liability, while regular exporters do not have that option. Since regular exporters differ in the sense that they cannot write-off imported products, we expect normal exporters to react more strongly to changes in rebates.

Although the rebate policy is rather complicated, we believe our parsimonious modeling of export rebates above is able to capture the differential effect between eligible and non-eligible firms. For regular exporters with zero BIM, $NCNR = (X) * (vat - reb) = (X) * (\tau_H)$. We interpret τ_H as a distortion on exporters relative to the case where VATs are fully refunded for exporters, as assumed by Feldstein and Krugman (1990), and the policy undertaken by European Union countries. In Section 4 we provide a model of misallocation with distortions on output due to the magnitude of τ_H, which acts as a tax on revenue that is only imposed on export sales.

For all other firms, we assume $\tau_H = 0$. We do attempt to control for some of the firm characteristics brought up in Ferrantino et al. (2012) that might affect the tax and rebates that is actually applied to firms.[5] For example, we control for firm capital intensity, an

[5] Another note about the rebate policy is that capital purchases cannot be deducted, which leads to higher tax rate in capital-intensive sectors. We do have data on capital intensity at the firm-level to control for the discrepancy. In 2004, China allowed 6 sectors in 3 North Eastern provinces to deduct capital purchases as well. For that reason it is important to control for capital intensity of the firm, as well as use region fixed

import dummy, output and input tariffs, plus other time-varying controls. Finally, in some specifications we also include province fixed effects to control for policies that are applied in specific regions (such as the capital purchases deduction studied in Liu and Lu (2015)).

2.2.1 Firm-Level Rebate Calculations

Rebates vary at the firm level since they depend on the products that they export, each product subject to its own VAT tax and rebate rate. Since we have this product level tax data plus exports at the product level, we report the summary statistics of economy-wide export tax rebates using individual firms' weighted average ETR rates. The firm level ETR rates is defined as follows:

$$reb_f = \sum \frac{value_{hs8}}{value} reb_{hs8}, \tag{3}$$

which is a weighted average of rebates (at the HS8 level) the firm receives based on each products' weight in the total export value of the firm. Table 2 shows the average ETR rate over our data sample. The ETR rates are about 15% on average from 2000-2003 but fell to under 13% on average in 2004 and beyond. The change is due to a drop in rebates across most products in 2004 as there is a very small change in the dispersion of rates across firms.[6]

[Table 2 about here.]

We also compute the ratio of ETR rate divided by VAT rate as a measure of the tax on exports relative to refunding all VATs on exports, $rv_f = \frac{reb_f}{vat_f}$. According to Poncet et al. (2014) and our definition above, a complete rebate means the ETR rate that a firm gets from the government is equal to the VAT rate it pays in its production process. The firm receives closer to a complete rebate if this ratio is higher (it can be up to 100%). We compute the

effects.

[6]Since the ETR policy was established in 1983, it has been adjusted many times. In 1995 and 1996, the zero rate for export products was adjusted to 3%, 6%, and 9% (three different rates). Facing the Asian economic crisis in 1998, the government increased the ETR rate of some products: to 5%, 13%, 15%, and 17%. On Jan. 1st 2004, in order to adjust the industry structure of exports and balance the economic development, the government adjusted the ETR rate to 5%, 8%, 11%, 13%, and 17%, and the average ETR rate decreased to below 13%.

firms' weighted average ETR to VAT ratio, with a ratio below 100% signifying a positive distortion, τ_H. Table 2 reports the results for the ratio of the rebates to VAT tax in the last two columns. Before 2003, the average value of this ratio is between 88.4% and 90.2%, and decreases to 75.1%-75.2% between 2004 and 2006. Unsurprisingly, the patterns follow exactly from the firm rebates.

In the empirical analysis below, our benchmark specifications leverage the *time* variation in export tax rebates (ETRs). As mentioned above, we compare a set of rebate-eligible firms to non-eligible firms during this time period. There are two similar ways that we can compare the effect of ETR changes on eligible versus non-eligible firms. First, without using the actual magnitude of the rebates, we conduct a differences-in-differences specification of relative outcomes before and after 2004, when all ETRs were reduced. Second, we also use the variation of *industry-average* ETRs to compare relative outcomes at different industry ETR rates. Only when we restrict ourselves to the customs data, and therefore exporters only, do we use the actual product level rebates.[7]

2.3 Other Trade Policy

We also summarize concurrent trade policy by China that we will attempt to control for in the empirical analysis. Table 3 summarizes the average import, input, and export tariff rates in China across the years in our sample. Import tariffs decrease from 19.15 in 2000 to 9.56 in 2006, which reflects China's liberalization post WTO accession. Input tariffs are much lower due to the way China structures its tariff schedule but are roughly constant across time. Finally, we also compute average export tariffs given the tariffs Chinese firms face to sell abroad. As our study mostly compares Chinese exporters to non-exporters it is important to control for the evolution in this policy.

[Table 3 about here.]

[7] We use this only as motivational evidence in Section 3.1 as in this case there is no control group (non-exporters).

There are other potentially important policy changes during this period. First, the gradual elimination of quotas on Chinese textiles impacts exporters in certain industries. Khandelwal et al. (2013) provide data on fill rates which we use to measure industry exposure to the MFA over time. Second, China alters its export license requirements across industries (Bai et al., 2017). We use the industry data provided in that study. As with tariff rates, industry-year fixed effects control for changes in these policies in the firm specifications. However, we check that the policies do not affect differential outcomes within industries by interacting the policy measure with the firm eligibility indicator. The industry-level time-varying policy measures are also used in the robustness analysis of the industry level specification (where we cannot control for industry-year fixed effects).

3 Firm Level Empirical Results

In this section we provide robust evidence that the reduction in the rebate rate in 2004 led to changes in both the production and profits of goods by exporters relative to non-eligible firms. Customs data allows us to track a panel of product level transactions which we use to show that exporters reduce quantity sold of their products with bigger reduction in rebates. We then turn to the firm level census data and compare exporters to non-exporters. The main finding is that less complete rebate policy, or higher τ_h, shifts production away from exporters and lowers their profits. Having established in this section that non-complete rebates act as distortions on exporters, in Section 4 we sketch a model of misallocation where the VAT-rebate difference acts as a differential tax on output for exporters relative to non-exporters, and reduces allocative efficiency in the economy.

3.1 Exporters Only

In order to take advantage of the product-level customs data, we start with restricting ourselves to exporters only. We calculate the price and quantity response to changes in rebates for eligible firms. In models with heterogeneous distortions across firms, which we describe below, firms with large distortions under-produce relative to a non-distorted economy. Since we hypothesize that incomplete rebates act as distortions, we test this in the customs data by studying the price and quantity changes in response to changes in ETRs. Although we can only construct markups and profits at the firm level where we have financial variables, export prices and quantities are available from customs transactions. We expect quantities of eligible firms to decrease unambiguously in response to a lower rebate rate. In a constant markup environment, such as ours in Section 4, prices should increase as firms pass-through tax increases.[8] We check these predictions using the customs transaction level data that provides exports at the destination-HS8-firm level.

The following specification applies the customs data to only *eligible* firms:

$$y_{fpt} = \delta rv_{fpt} + \beta_1 X_{ft} + \beta_2 X_{pt} + \gamma_{fp} + \gamma_t + u_{fpt}. \quad (4)$$

δ is the elasticity of changes in the magnitude of rebates on production and prices, where rebates vary at the product level.[9] y_{fpt} is the outcome variable (log quantity, log prices, and log value) at the firm (f)- HS8 product (p)- year (t) level. The fraction of VAT taxes rebated back to the firm for a specific product is rv_{fpt}. Note that before we aggregate to the firm level, the rebate can vary within firms if they sell multiple products. X_{ft} and X_{pt} are time varying firm and product characteristics such as capital/labor ratio, foreign ownership share, import share, and HS6 product tariff rates. We also include firm-product and year fixed effects separately. Therefore, δ captures the variation in rebates over time *within* firm-product pairs (where product rebates change over time). We capture firm responses across their multiple products depending on the heterogeneous product rates. Note that we could use firm-product-destination transactions instead, since the current specification

[8]This latter effect does not seem to be present in the data, and we return to this when we analyze markup responses.

[9]This is true as long as we use transaction-level data and do not aggregate within the firm.

necessitates aggregating the destination transactions to the product level. However, the results are essentially identical and we use the current specification since rebates are at the product level.[10]

The results for specification (4) are presented in Table 4. The variable of interest is the fraction of VAT that is rebated. For each outcome, we run the specification with all eligible firms, as well as a balanced panel of firms, which controls for any endogenous entry/exit into exporting at the firm-level.[11] As expected, a rise in the fraction of the VAT that is rebated leads to a rise in quantity produced (columns (1)-(2)), with 10 percentage point increase in rv_{fpt} (e.g. from 75% of the VAT to 85%) is associated with a 2.7-3.5% increase in quantity produced (multiply coefficient by 0.1 since rv_{fpt} is a fraction). In Table 2 we showed that China's policy reforms reduced the average fraction of VAT rebated about 15 percentage points in 2004, making the potential effect on quantities exported economically significant.

[Table 4 about here.]

In the next two columns, we report the response of log prices, once again for all firms and a balanced panel. As expected, the sign for the effect of rebates on prices is negative. However, it is small and not statistically significant, which could be because the pass-through of the rebate to prices is small,[12] or a sign that markups are changing due to macroeconomic trends. As a note that foreshadows the difference-in-difference results below, we do find a trend of decreasing markups for eligible firms, although not necessarily variation across eligible and non-eligible products.[13] A rise in competition, which likely starts after China

[10] In addition, we drop any transactions to Hong Kong for the well known problem of "re-exports."

[11] For the balanced panel, we keep only firm-product combinations that are present in every year of the data. Therefore, we control for any endogenous entry/exit into exporting as a response to the policy.

[12] This would be consistent with studies that have found very small short-term pass-through of costs to prices. It is also the case that we cannot control for quality since prices are simply unit value of exports. It is possible that there is an endogenous quality response that affects the unit value of the exported good.

[13] A comparison across the specifications is difficult due to where the variation is coming from. The firm-level specification in the next sub-section is *within firms* and it is likely that firms don't necessarily maximize profits at the product level but at the firm level.

joins the WTO in 2001, is not something we focus on in this paper, but we are careful to account for this in the rest of the paper.

Finally, the last two columns report the effect on the value of export transactions at the firm-product level, which increase with the rebate. This is not surprising given the much larger effect on quantities. It is also consistent with the firm-level results below, where firm revenues and profits decrease significantly after a reduction in the rebate. The model in the following section predicts a rise in the dispersion of firm level revenue productivities, which we are able to capture with the large adjustment in quantities. Overall, there is strong evidence that exporters reallocate their own production towards the products with higher rebates, and that this is done mostly through changes in quantity, without large changes in prices.

3.2 Exporters versus Non-exporters

The advantage of the customs data used above is the availability of unit values and quantities, and the fact that we have product level transactions. However, there is no information on input use and other financial variables, and restricts the analysis to exporters only. The previous specification also does not adequately account for the fact that omitted variables could be driving quantities and prices for *all* exporters. Next, we take a specification that can more accurately identify the causal effect of rebates by comparing eligible exporters to non-eligible firms. We utilize the manufacturing firm-level census which also allows us to study a much broader set of outcomes. The eligibility definition is straightforward, as a firm must be an exporter and additionally it cannot be a "processing trade with supplied materials" type.[14]

We start by using the firm data to produce revenue productivity and markup estimates. These are the measures that have been the focus of models that study misallocation through

[14]The latter characteristic is used to characterize non-eligibility in Poncet et al. (2014), but they use aggregate (HS6) data while we use transaction-level data and then aggregate to the firm-level; therefore, we can identify firms that are both exporters and non-processing firms.

firm-specific supply-side frictions.[15] Our data allows us to estimate markups using revenue-based production function, as in DeLoecker and Warzynski (2012) (DLW) and Lu and Yu (2015).[16] We also use other parts of the firm statistics, such as profits and production measures, to fully investigate the distributional effects between eligible and non-eligible firms.

3.2.1 Production Function Results

Given the data in the Industrial Survey, we estimate markups and productivity as in DeLoecker and Warzynski (2012). The estimation requires firms' revenue, labor input, capital stock, material input, and export behavior. To recover markups, the strategy is to first estimate production function coefficients using the method of Ackerberg et al. (2015) (ACF) with a Translog gross output production function.[17] To control for the fact that production functions may be different for exporters and non-exporters, we include the lag of the export indicator as a state variable (see Kasahara and Rodrigue (2008)). The firm markup is the ratio of the *materials*' coefficient in the production function and *materials*' share of total input costs. The Translog specification allows us to recover both firm-specific material cost shares and material output shares. As an example, the output coefficient for materials are calculated as: $\theta_m = \beta_m + 2\beta_{mm}m_{ft} + \beta_{lm}l_{ft} + \beta_{km}k_{ft} + \beta_{lkm}l_{ft}k_{ft}$, given the Translog production function. Additionally, we calculate firm TFP using the output coefficients, following ACF.

Table 5 lists our results from estimating input coefficients at the two-digit industry level and the resulting markups. Since the coefficients are firm-specific, we report the industry average, along with the median markup and the number of observations (these include firms

[15] Hsieh and Klenow (2009) is the oft-cited study on revenue productivity dispersion. Liu and Lu (2015) measures markup dispersion.

[16] The latter measure markup dispersion in China, with the policy interest being the effect of entry into the WTO.

[17] The Translog production function is the following: $y_{ft} = \beta_l l_{ft} + \beta_{ll} l_{ft}^2 + \beta_k k_{ft} + \beta_{kk} k_{ft}^2 + \beta_m m_{ft} + \beta_{mm} m_{ft}^2 + \beta_{lk} l_{ft} k_{ft} + \beta_{lm} l_{ft} m_{ft} + \beta_{km} k_{ft} m_{ft} + \beta_{lkm} l_{ft} k_{ft} m_{ft} + \omega_{ft} + \epsilon_{ft}$. l_{ft}, k_{ft}, and m_{ft} represent log values at time t of firm employment, capital stock, and materials respectively. ω_{ft} is log TFP which is observed by the firm but not the econometrician (the object estimated in the ACF procedure).

more than once since it aggregates all years). The median markup ranges from 1.10-1.25, with the median across all industries at 1.14. These estimates are within the range of the previous literature. We have also calculated a simpler price-cost margin which we call $Markup(profit) = \frac{sales-wages-inputs}{sales}$. These result in a very similar median markup, as the median margin is also 14%. In the regression results below, we report both measures as outcomes.[18]

[Table 5 about here.]

3.2.2 Firm Level Specification

We will use a difference-in-difference specification to compare the outcome differences between eligible and non-eligible firms for the rebate before and after a policy change. Compared to the product level specification, we aggregate to the firm level where all outcomes combine domestic and export activities. In this case the non-eligible firms are mostly non-exporters who sell only domestically. "Eligibility" is fixed so that the firm cannot switch across types over time. In the benchmark specification we restrict the sample to a balanced panel of firms to abstract away from the response in terms of entry and exit that is due to the ETR reduction, as well as other policy changes. The share of eligible firms (the treatment group) makes up 16.8% of the sample in every year.[19]

We run two types of difference-in-difference specifications possible, each with their own tradeoffs. One possibility is to use the interaction $\{Year \geq 2004 * Eligible\}$ because there is a large policy variation in 2004 and beyond relative to the pre-2004 period. Although the ETR policy is not fixed in the other years, Table 2 shows that the variation in other years is

[18] We have also investigated alternative production function estimation methods, such assuming a Cobb-Douglas production function and estimating output coefficients with OLS estimation. These yield even lower coefficients on capital (which are already low in our estimation). The median markup with those methods is about 20%.

[19] In the case where we do not restrict the sample to only firms that are reported in every year of the data, share of eligible firms changes across years. From 2000-2006, the share of eligible firms is respectively: 0.143, 0.140, 0.136, 0.124, 0.134, 0.132, 0.105.

very small compared to 2004, so effects due to ETR policy should be especially pronounced in 2004-2006 relative to the behavior in 2000-2003. The specification is:

$$y_{fjt} = \delta Year \geq 2004_t * Eligible_f + \beta_1 X_{fjt} + \beta_2 X_{jt} * Eligible_f + \gamma_f + \gamma_{jt} + \eta_{fjt}, \tag{5}$$

where f stands for firm (export and domestic transactions), j is 4-digit industry, and t is the year. Alternatively, robustness exercises will substitute the $Year \geq 2004_t$ indicator with a continuous and time-varying measure of the rebate at the industry level. The exercise is similar to the main specification since most of the variation in rebates happens in 2004 and is a shift down across all industries.

We also conduct a separate analysis that allows us to check for the possibility that eligible and non-eligible firms have different trends prior to the policy change. In order to examine the policy's impact *before and/or after* its adoption, we modify our difference-in-differences specification to include not just a dummy for the post-2004 period, but also add dummies for different number of years before and after the policy implementation. This event study specification is the following:

$$y_{fjt} = \sum_{t=2000}^{t=2006} \delta_t year_t \left(Eligible_f + Char_f\right) + \beta_1 X_{fjt} + \beta_2 X_{jt} * Eligible_f + \gamma_f + \gamma_{jt} + \eta_{fjt}. \tag{6}$$

where $Eligible_f$ is time-invariant and equal to one for eligible firms, and it is interacted with year dummies for each year of the sample. We also interact the year dummies with other firm dummies ($Char_f$), including whether the firm is a processing firm or an importer, since these firms might be hit by varying concurrent policy changes. This allows us to plot the set of $\{\delta_t^{Eligible}\}$ over time for each outcome, which can be interpreted as the reform's effect on eligible firms relative to non-eligible firms in each year relative to the effect in 2003 (the dropped year dummy).

Our specifications control for time-invariant firm fixed effects, and time-variant industry fixed effects. The latter allows us to capture within-industry relative outcomes between eligible and non-eligible firms while controlling for any industry-specific shocks.[20] For example, changes in industry-level output and input tariffs are controlled for, assuming that there are no large variations across more disaggregate products. There are other large policy shocks to Chinese firms during this time, such as the elimination of quotas for exporters of textiles (Khandelwal et al., 2013) and loosening of export license requirements (Bai et al., 2017). This restrictive specification allows us to interpret δ as the distributional effect between eligible and non-eligible firms within their industry, and thus to control for heterogeneous exposure to these shocks across industries. However, we also control for the interaction of industry tariffs with firm eligibility (β_2) in case changes in tariffs have distributional effects through competition.[21] X_{fjt} is a vector of time-varying firm controls which includes capital intensity, "processing" and import dummies, firm age (as a trend control), share of the firm owned by foreign investors, and log exports. All specifications include standard errors that are clustered at the 4-digit industry and firm employment weights.

Results In order to visualize the effect of the policy, Figure 1 plots specification (6) for a variety of outcomes. The top row plots relative value added per worker and TFP (using the productivity estimation procedure) for eligible vs non-eligible firms. The bottom panel does this for the two types of markups. From the top panel, there are no discernible pre-trends in revenue productivity (value added per worker or TFP) for eligible firms relative to non-eligible firms. Eligible firms do seem to have a rise in productivity in 2001, likely in anticipation to China's entry into the WTO, but the coefficient is already insignificant by 2002, and through 2004. After 2004, there is a decrease in revenue productivity for eligible firms that materializes in 2005, and is very large (the reduction in much larger than the

[20]We have also conducted the same specification but with j being 2-digit sectors, therefore capturing within effects in more broadly defined sectors. Results are very similar so we report only results with 4-digit industries.

[21]We have also checked with an interaction with MFA exposure and do not see important effects. This would reduce the years of our sample so we do not report it here. We report these in the industry level results since controlling for time-varying industry shocks is more difficult in that specification.

positive one in 2001). The reduction in revenue productivity in 2005-2006 looks like a clear break relative to the preceding years. We also report the main event-study for all firms, allowing for firm entry. Figure A3 in the Appendix repeats the analysis for the unbalanced panel and there are no discernible differences.

[Figure 1 about here.]

Given the rise in the effect in 2001 and reduction before the reforms, we attempt to control for a linear trend. Notice that the plots in Figure 1 include a trend line interpreted as follows: we regress each outcome on the "$Elig * Trend$" interaction plus controls and fixed effects *only* for pre-reform years. Then, we take the coefficient on the eligible-firm trend and set it as a constant linear trend that grows linearly from 2000 to 2006, where we normalize it to 0 in 2003.[22] It is clear that, outside of panel (C), the coefficients in 2005 and 2006 are far below the trend implied by the pre-reform years.

By the nature of this event study, a simple linear trend interacted with the eligible dummy (for all years) would not be identified, so we attempt several robustness checks. First, one can test for trends by merging years of data. We merge the first two years first, then the last two years before the reform, and re-run specification (6) with an interaction of the eligible dummy with a trend.[23] The results are displayed in Figures A4-A5 of the Appendix. In merging the first and last combination of pre-reform years together, we do find now that there is virtually no trend between 2001 and 2003, while are still big drops in revenue productivity of eligible firms after 2004. We also find that the trend-eligible interaction, which captures the trend in the merged years, is very small and statistically insignificant for value added per worker and TFP.

Second, we test if there is a *break* in the trend line after 2003. With all years now back in the sample, we replace the year dummy interactions with the following interaction term:

[22] We do acknowledge that the few available pre-reform years in the data make the test for pre-trends a difficult task.

[23] We merge only two years because of the few pre-reform years available.

$Year \geq 2004_t * Eligible_f * Trend_t$. If this is negative, then any possible negative pre-trend is dwarfed by the post-reform effects. Table A1 reports the results, with a large and significant effect on this coefficient for TFP, log value added per worker, and the profit estimation of the markup. The "$Elig * Trend$" coefficient is also negative, which is the average trend *across all years*, but the effect after 2004 is clearly much larger. This is consistent with the trend lines in Figure 1, where the negative effects after 2004 are much larger than predicted by the simple linear trend created for the pre-reform years. It is possible that the negative trend for eligible firms would be easier to see with more years of data before the reform, but we take the fact that the trend-eligible interaction coefficient is so close to zero as reason for cautious optimism of our approach.

The results for markups provide a larger reason for caution. Once again, it is evident that eligible firms suffer in the latter period, as markups clearly decrease for eligible firms. However, there is also a visible trend in the pre-period for markups estimated with the DeLoecker and Warzynski (2012) procedure, which decrease for eligible firms between 2000 and 2002. In Figure A4, there is still evidence of a pre-trend. We do note however that markups calculated with the simple price-cost margin do not display pre-trends. Although the drop in markups after 2004 is also smaller, the last column of Table A1 reports that the trend is more negative after 2004 relative to the pre-reform period. This could be evidence of a violation of the parallel trends assumption in the specification with production function estimated markups, although it is the case that there is a deceleration of the markup decrease between 2002 and 2004, before accelerating again in 2005. In all the further regression specifications we control for an interaction of the eligibility dummy with firm age, which acts as a trend, as well as the interaction of the dummy with possible contemporaneous shocks such as tariffs, which might lead to differential trends in the two groups. Still, we caution that the interpretation of markup responses is limited by the fact that exporter markups appear to decrease continuously from the time China enters the WTO.[24]

[24]It would not necessarily be surprising that exporter markups are decreasing as they enter a competitive world market. Chinese exporters likely have small margins as is implied in the results of the recent U.S.

The relative responses to the 2004 policy change are reported in Table 6. We are interested in the interaction term in the first row whose coefficient is δ in (5). The main results are reported using *only firms in the dataset in 2000.* We use this set of firms to abstract away from the entry that might result in response to the policy change, but the results for all firms – allowing for entry and exit into the dataset – are reported in Table A2 of Appendix A.2. In Appendix A.1 we describe the entry and exit dynamics observed in the dataset, and some of the possible reasons behind these dynamics. Still, even with a much larger number of observations in the case with all firms, we do not find qualitatively different results. As a separate robustness exercise, we also repeat the analysis with a *continuous measure* of the industry-level rebate instead of the post-2004 indicator. The variable of interest is the interaction between the continuous industry rebate and firm eligibility dummy, with the rest of the specification as in (5). Again, we conduct the analysis with all firms and with only firms alive in 2000, with both tables in the Appendix (Tables A3 and A4).

[Table 6 about here.]

The results in each column reflect the distributional implications across a variety of outcomes between eligible and non-eligible firms in response to lower ETRs. From the first column, eligible firms lower their markup relative to non-eligible firms after the rebate rates are reduced in 2004. Markups decrease for eligible firms by 0.8% relative to non-eligible firms.[25] Conversely, when rebates increase, eligible firms raise their markup (Tables A3 and A4). These results are similar for alternative outcomes that reflect the firm profit margins (column (2)). We note that the magnitudes for markup changes, like for prices, are small.

The last two columns report the differential effect on log value added per worker and log TFP. Eligible firms have a decrease in value added per worker of 7.7% relative to non-eligible firms after 2004. A similar effect is seen in TFP, which is also a revenue productivity

tariffs on China (Fajgelbaum et al., 2020; Amiti et al., 2019).

[25] Outcomes in columns (1)-(2) and (4) are in logs. Markup(profit) (column (2)) is a ratio), so that the coefficient can be interpreted as percentage point changes.

measure, estimated using the Ackerberg et al. (2015) procedure. TFP for eligible firms drops on average 2.4% relative to non-eligible firms after the reduction of the rebates. These magnitudes are large and consistent with the fact that exporters raise the quantity sold on products that have a relative rise in rebates.

All of these results hold as well when we interact the eligibility dummy with the continuous measure of rebates. For example, we can compare the case with no entry (Table A3 in the Appendix) with our benchmark results. In Table 2 we report that the average share of VAT rebated is reduced from about 0.90 to 0.75 (this is the continuous rebate measure we use). Multiplying the coefficients in the first row of Table A3 by -0.15 can be interpreted as the reduction in log value added per worker for an eligible firm relative to an ineligible firm as a response to a drop in rebates experienced by the average firm. If one were to compare this product with the coefficients in the first row of Table 6, they are very similar, the effect reported in Table A3 being about 75% as large.[26]

We interpret the reduced rebate as a distortionary tax on exporters relative to non-exporters, and it is evident that indeed exporters make lower profits and their revenue productivity is lower when rebates are reduced in 2004. This is consistent with the results that utilize customs data, where lower rebates are associated most strongly with a reduction in quantity and value sold of eligible products. Overall, the results imply that in China's case, where fiscal issues lead them to reduce rebates, there is a reallocation of production from eligible to non-eligible firms. In the next section, we model firm production allowing for lower rebates to act as a friction that forces a divergence between revenue productivity of domestic and foreign sales. Thus, the observed reallocation is a key building block of our overall story, that China's ETR policy introduces an allocative efficiency dimension that must be taken into consideration.

[26]For this reason we believe that either specification essentially picks up the large across-the-board variation in rebates that happens in 2004. We do not focus on the variation in how much the rebate drops *across only eligible firms*, but we note this can lead to reallocation across eligible firms which might have extra distortive effects. We thank a referee for pointing this out.

4 Aggregate Misallocation

The results of the previous section demonstrate that a reduction in export rebates lowers revenue productivity and profit margins for rebate-eligible firms. In this section, we describe a macroeconomic framework that is consistent with the micro-level results and allows us to speak to the aggregate misallocation related to incomplete rebates. The value-added tax acts to reduce firm revenue and so we can connect this to the literature on firm-level wedges. As in the discussion above, we interpret a full rebate on exports as the non-distorted case akin to Feldstein and Krugman (1990). A less complete rebate maps to a higher tax on output, a distortion faced by rebate-eligible firms.

4.1 Model

Following the empirical analysis in the previous section, in this section we compare the response *across industries* to the reduction in export rebates, using their exposure to the policy. Industries are comprised of firms with output y_f which create industry production using a CES aggregate:[27]

$$Y_j = \sum_{f \in \Omega_j} \left(y_f^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}} \tag{7}$$

Ω_j represents the set of varieties produced in industry j and σ is the elasticity of substitution across varieties within an industry. Firms produce output at constant returns to scale with the following production function:

$$y_f = A_f F_f, \tag{8}$$

[27] It would be trivial to aggregate output across industries – the majority of the literature assumes Cobb-Douglas aggregation. We model a representative industry to then compare industries given their exposure to export rebates.

where A_f determines the firm-specific TFP and F_f represents a composite of firm factors.[28]

Firm production can either be sold domestically or exported. Let $p_{fx} = \tau_x p_{fd}$ represent the price of exported goods, which are merely scaled by a trade cost τ_x relative to the domestic price. Note that we include τ_x to account for the fact that a subset of firms export a part of their production. CES aggregation results in firm-level prices of $p_{fd} = P_j \left(\frac{Y_j}{y_f}\right)^{\frac{1}{\sigma}}$, where P_j represents the CES price index (at the industry level).

Firm revenues are the sum of domestic and export revenues. We express domestic sales given that firms face CES demand and then scale by trade costs to get export revenues (following Melitz (2003)). However, in this case export revenues will include a distortion due to an incomplete rebate of VAT taxes paid. Domestic revenues are given by $r_{fd} = p_{fd} y_{fd} = P_j Y_j^{\frac{1}{\sigma}} (A_f F_f)^{\frac{\sigma-1}{\sigma}}$. Export revenues are written as a function of domestic revenues, $r_{fx} = (1 - \tau_{fh}) \tau_x^{1-\sigma} r_{fd}$. Total firm revenues are a weighted average of their domestic and export revenues:

$$\begin{aligned} r_f = \lambda r_{fd} + (1-\lambda) r_{fx} &= r_{fd} \left(\lambda_f + (1-\lambda_f)(1-\tau_{fH})\tau_x^{1-\sigma}\right) \\ &= P_j Y_j^{\frac{1}{\sigma}} (A_f F_f)^{\frac{\sigma-1}{\sigma}} \left(\lambda_f + (1-\lambda_f)(1-\tau_{fH})\tau_x^{1-\sigma}\right). \end{aligned} \tag{9}$$

First, λ_f represents the share of firm sales that are sold domestically, which allows us to express total revenues as a weighted average of domestic and export sales. More importantly, export revenues depend not only on trade costs, but a distortion determined by the share of VAT taxes *not* rebated on exports. This distortion is tied to the definition in (2). The proportion not rebated, τ_{fH}, acts as a tax on firm level revenues. This introduction of the distortion caused by incomplete rebates follow closely the framework of Hsieh and Klenow (2009) given that their output distortion has a very similar intuition as the tax we examine in this paper. We simplify their framework to some degree because there is no reason to

[28]The analysis follows equally with a more realistic Cobb-Douglas production function made of capital and labor, $y_f = A_f K_f^{\alpha} L_f^{1-\alpha}$. Since we are not interested in the capital-labor allocation we aggregate factors into a composite of capital and labor which we call F.

expect that incomplete VAT rebates will affect the relative capital-labor allocation within firms. Hence, we eliminate their capital distortion and focus on a production function with only one factor.

Firm profits are given by:

$$\pi_f = P_j Y_j^{\frac{1}{\sigma}} \left(A_f F_f\right)^{\frac{\sigma-1}{\sigma}} \left(\lambda + (1-\lambda)(1-\tau_{fH})\tau_x^{1-\sigma}\right) - p_F F_f \tag{10}$$

In order to solve for firm output, we use profit maximization to solve for the allocation of factors and output:

$$F_f^* = Y_j \left(\frac{\sigma-1}{\sigma} p_F^{-1} P_j\right)^{\sigma} A_f^{\sigma-1} \left(\lambda_f + (1-\lambda_f)(1-\tau_{fH})\tau_x^{1-\sigma}\right)^{\sigma} \tag{11}$$

$$y_f^* = A_f F_f = Y_j \left(\frac{\sigma-1}{\sigma} p_F^{-1} P_j\right)^{\sigma} A_f^{\sigma} \left(\lambda_f + (1-\lambda_f)(1-\tau_{fH})\tau_x^{1-\sigma}\right)^{\sigma} \tag{12}$$

The solution for firm output then allows us to derive the revenue productivity of the firm:

$$TFPR_f = p_f A_f = \frac{\sigma}{\sigma-1} p_F \left(\lambda_f + (1-\lambda_f)(1-\tau_{fH})\tau_x^{1-\sigma}\right)^{-1} \tag{13}$$

As in Hsieh and Klenow (2009), an important conclusion from our framework is that revenue productivities are equalized across firms as long as long as $\tau_x = 1$ and $\tau_{fH} = 0$. For now, we put aside trade costs as they are not the focus of this study. For $\tau_{fH} \neq 0$, revenue productivities will diverge across firms and lead to an inefficient allocation. In (13), a reduction in the rebate raises their tax and increases the difference in revenue productivity between a non-eligible and eligible firm, so that a more "incomplete" rebate raises the level of misallocation. This is because an incomplete rebate acts as a positive tax on exporters. It is clear that we can follow a large part of the misallocation literature (Hsieh and Klenow, 2009; Restuccia and Rogerson, 2008; Oberfield, 2013), and interpret a rise in misallocation though increases in the dispersion of firm revenue productivities within an industry.

Note also that there is a confounding variable in that a rise in export tariffs would lead to a similar divergence in revenue productivities between more export intensive firms. In the empirical analysis, we control for changes in export tariffs. We note, however, that export tariffs are *decreasing* during this time as Chinese firms see an expansion in market access. Therefore, to the degree that we fail to control for all changes in export tariffs, it will bias down the distortion on exporting firms that we measure as a response to a reduction in the rebates.

4.2 Raw Trends

To start, we investigate measures of misallocation in China as a whole during our sample. Following (13) above, we compute measures of dispersion in TFPR in China. We find that several measures of dispersion increase in 2004, consistent with a rise in misallocation in the latter half of our sample period. It also appears that this is especially the case in industries with a large share of firms eligible for rebates. As an alternative measure of misallocation, we also compute a "distorted" Solow residual (Petrin and Levinsohn, 2012; Baqaee and Farhi, 2019), which also implies that allocative efficiency is lower in the latter period. In the next subsection, to identify the effect of the rebate policy, we return to the specification in the previous section and examine industries with eligible firms relative to industries with less exposure to the rebates.

Figure 2 panel A plots the standard deviation of revenue productivity across the whole economy. The standard deviation is computed *by industry* and we take the average across industries to aggregate to the economy-wide measure.[29] This simple time series shows a gradual decline in the TFPR dispersion up until 2003, with a an increase in 2004 and beyond.[30] The same can be done for a subset of industries labeled as "high share" versus those labeled as "low share" in terms of share of eligible firms (we define this below). Panel B of Figure 2 displays the dispersion for each of these subset of industries. There is a roughly

[29]The average is not weighted, although the results are very similar when we use industry value added weights.

[30]Still, the dispersion in 2006 is below that in 2000.

parallel trend of a reduction in the dispersion up until 2003. In 2004, the dispersion increases for the "high-share" industries but not for the "low-share" ones. This is a visualization of the raw data that we will exploit for the more rigorous difference-in-difference specification in the next subsection.

[Figure 2 about here.]

In the Appendix, we report the time series of alternative measures of misallocation. First, we investigate other dispersion measures that we expect to follow a similar pattern to the standard deviation above. In the top two panels of Figure A6 we plot the difference in TFPR across firms in the 90th (75th) relative to firms in the 10th (25th) percentile of the distribution in each year. As expected, in both cases we find that dispersion picks up in the latter period which is consistent with the rise the in the standard deviation of revenue productivity. Alternatively, we compute a type of "reduced form" misallocation following measures in the aggregate productivity literature. Petrin and Levinsohn (2012) calculate growth rates in allocative efficiency as a residual of the growth of aggregate productivity that is not explained by technical efficiency. We leave the description of this approach to the Appendix as it does not map directly to the model in this paper.[31]

4.3 Empirical Analysis of Misallocation

We will once again identify the effect of changes in the rebate policy with a differences-in-difference specification. Equation (13) allows only for export tariffs and differential sales taxes to create distortions across firms. In reality, there are likely various industry characteristics that determine the level of distortions within industries. For this reason, a cross-sectional analysis as in Hsieh and Klenow (2009) would not necessarily report larger dis-

[31] The *growth rate* of the distorted Solow residual is displayed in Figure A7. Throughout the time period, there is a negative growth rate in allocative efficiency, which means that reallocation is taking away from aggregate productivity growth. The dispersion in revenue productivity is a more direct measure of the causal effect of changes in ETRs because the model in the previous subsection allows us to map a rise in τ_{fH} to a larger difference in TFPR across eligible and non-eligible firms.

tortions in industries with a greater share of exporters. However, given the drastic policy change in the share of VAT that is rebated to exporters, we can compare changes in the level of distortions across industries based on their exposure to the rebates. We therefore run the following specification:

$$SD(TFPR)_{jt} = \delta Year \geq 2004_t * HighShare_j + \beta_1 X_{jt} + \beta_2 X_{jt} * HighShare_j + \gamma_j + \gamma_t + \eta_{jt}. \tag{14}$$

We construct our measures at the 4-digit industry level (j) in each year (t). We define $HighShare_j$ as a dummy equal to one if an industry is above the median in terms of the fraction of firms that are eligible for rebates (fixed over time). Similarly, we will also report results that replace this dummy with the continuous measure of the share of eligible firms in the industry (once again fixed). In mapping the specification to the model, the fixed share of eligible firms is represented by the average of $(1 - \lambda_f)$ across firms within an industry. All else equal, changes in the level of tax distortions are more important in industries with a higher share of eligible firms. We also control for possible confounding time-varying industry characteristics and government policies. X_{jt} includes industry export intensity, average capital-labor intensity, and the Herfindahl Index as a measure of competition. The interaction, $X_{jt} * HighShare_j$, allows for other policy changes to affect the rebate-exposed industries differentially. We include import tariffs, input tariffs, and export tariffs, where each is interacted with the industry exposure dummy.

Table 7 displays the results from from specification 14. The main coefficient of interest in the first three columns corresponds to the interaction of $Year \geq 2004_t$ with $HighShare_j$. In the last three columns we replace this dummy with the continuous share of eligible firms. For both cases, we incrementally add controls to each successive column. In column (1), we add only the time-varying industry controls. In the second column, we also add the interaction of import and export tariffs with the $HighShare_j$ dummy.[32] In the third column we also add

[32]We have also added input tariffs to this specification but these do not alter the results at all.

an interaction of $HighShare_j$ with exposure to MFA quotas (Khandelwal et al., 2013) and industry export license requirements (Bai et al., 2017).[33] The coefficient in the first row of column (2) can be interpreted as follows: in industries with a higher share of eligible firms, the average standard deviation of log revenue TFP from 2004-2006 relative to 2000-2003 is .024 higher than in industries with a low share of eligible firms. The results are similar with and without the tariff interactions, although, as expected, the post-2004 effect is larger when we control for changes in tariffs.[34] Including the last two interactions does not materially affect the results.

[Table 7 about here.]

The results are very similar when we use a continuous measure of the share of eligible firms (last three columns). Again, it is clear that the difference in dispersion of TFPR across time periods grows as the eligibility share increases. Also, the effect is larger when we control for the differential effect of the change in tariffs. Overall, our results imply that, controlling for the major industry-level policy reforms in China during this period, there is a clear rise in the dispersion of TFPR in industries most exposed to the rebate reduction relative to industries that are less exposed (and these results are significant at the 5% level).

We conduct robustness analysis using alternative outcome measures, with tables in the Appendix. First, instead of calculating TFPR as revenue TFP from the structural productivity estimation (Ackerberg et al., 2015), we use log value added per worker as our productivity measure. We then construct the same dispersion measure as the standard deviation across firms within industries. We repeat the two full specifications that include all tariff interactions. The results are displayed in Table A5 and can be similarly interpreted to the benchmark results. After 2004, the dispersion of high-share industries increases relative to low-share industries. In the case where industry exposure is a continuous measure the

[33] We add these separately because the MFA data is not available for all years and so the sample becomes smaller.

[34] See the discussion above on negative trends for export tariffs.

coefficient of interest is still positive and statistically significant.[35] Second, we also investigate the dispersion of markups. The model above does not provide a clear relationship with markups, but given the CES demand that firms face, there would be no markup dispersion in the case with no distortions. Therefore, we do expect markup heterogeneity to increase as a result of a greater tax on exporters. The final two columns of Table A5 replace the outcome measure with the standard deviation of log markups (as used in column (1) of Table 6). Although the results are weaker, we again find that the dispersion post-2004 rises relatively more in industries with more eligible firms.

Finally, in Table A6 we report the results when the measure of dispersion is the 90th relative to the 10th percentile of firms. The first two columns report the dispersion of TFPR from the production estimation and the latter two columns compute the dispersion of log value added per worker. Once again, the dispersion increases relatively more after 2004 in industries with more eligible firms. In summary, the evidence points toward an increase in misallocation after 2004 in industries that had a large fraction of rebate-eligible firms.

Aggregate Implications for Productivity Our results from estimating the specification in (14) provide the difference in revenue productivity dispersion that can be attributed to the policy implementation (coefficient δ). Next, we compute a back-of-the-envelope implication for aggregate productivity. To do so, first we attempt to answer: how much larger is the dispersion of TFPR in China in 2006 due to the policy change in 2004. This is done using a simple counterfactual where we compare TFPR dispersion in China in 2006 when δ turns to 1 in 2004 (the actual predicted dispersion) relative to the case where δ stays equal to 0 through the sample period (the counterfactual), and all other coefficients are assumed to stay constant. Instead of using the "HighShare" measure of industry j, we use the continuous measure of the fraction of eligible firms in the industry. We do this for every industry j, and then aggregate the standard deviation up to the country level using revenue shares. This

[35] Although we don't show the results without tariff interactions, it is again the case that the coefficient is smaller if we were to exclude these controls.

procedure yields the result that under the counterfactual case of no policy implementation, the variance of log TFP dispersion is 0.012 lower relative to the variance with the policy in 2006. Figure A8 (Appendix) displays the time series of the predicted TFPR dispersion over time compared to the predicted dispersion without the policy.

To map this number into an aggregate productivity difference, we rely on Hsieh and Klenow (2009). As a convenient functional form, we borrow their extreme example of the case where quantity and revenue productivities are jointly log normally distributed. This yields that the the difference in TFP in the two scenarios is proportional to the difference in TFPR variance (scaled by σ, the elasticity of substitution across products), as long as there is no difference in quantity productivity in the two scenarios.[36] The variance difference computed above, combined with fixing $\sigma = 3$, means that aggregate TFP would have been 1.8% higher in 2006 absent the policy. Therefore, the back-of-the-envelope implication is a large TFP contraction over a 3 year period relative to a world where rebates are held constant in 2004.[37]

5 Conclusion

The Chinese government has many levers to push when it comes to trade policy. In this study, we investigate one, a rebate on VAT paid on export sales, that has played an important role in its domestic finances. Many countries use a VAT system and rebates are common across the world, but the case of China is especially interesting because it has frequently adjusted its export tax rebates. China's adjustment thereby provides a useful laboratory in which to

[36]See Equation 16 in Hsieh and Klenow (2009), where they show that revenue productivity dispersion would be perfectly correlated with the aggregate productivity losses under this particular functional form assumption.

[37]The usual disclaimer about this type of counterfactual, that all the parameters are held constant over time, should be taken into account. Furthermore, notice that assuming that quantity and revenue productivities are jointly log normally distributed is fairly restrictive, and the counterfactual change in aggregate TFP depends on the fixed value of σ. Since σ simply scales the difference in TFPR variance, it is simple to see how the result varies with different σ. For example, doubling this parameter to be equal to 6, doubles the implied TFP contraction over the 3 year period to 3.6%.

measure the implications of such a policy. The theoretical literature has found that a policy that fully rebates VAT paid on export sales is "neutral" in that it should have no effect on global competitiveness. Given that China does not provide a full rebate to exporters, we hypothesize that this policy will have distortionary effects on the economy by inefficiently reducing exporters' sales relative to non-exporters.

The evidence we provide shows that a decrease in rebates results in lower quantities produced, lower revenue productivity, and lower profits for firms that are eligible for the rebates. However, we do not find that eligible firms, that saw their rebates reduced, meaningfully altered their prices. The results are obtained from a difference-in-difference specification which leverages the 2004 policy change to compare relative outcomes between eligible and non-eligible firms. To the furthest extent possible, we control for the concurrent policy shocks in China during this time.

We tie these distributional results to a structural model akin to Hsieh and Klenow (2009) by incorporating incomplete tax rebates as a tax on revenues on export sales. The model predicts that more "incomplete" rebates will create larger differences in revenue productivity between domestic and export sales. Thus, smaller rebates create more misallocation in industries with a higher share of rebate-eligible firms, which would be reflected in the increasing dispersion of revenue productivity. We use the firm data to create industry level dispersion measures and do indeed find that misallocation increases in industries with a larger share of firms that rely on rebates. One takeaway is that by adjusting this policy as a part of its broader policy objectives, China introduces an allocative efficiency dimension that must be taken into consideration. It is likely the reason why most countries choose to fully rebate VAT paid on exports.

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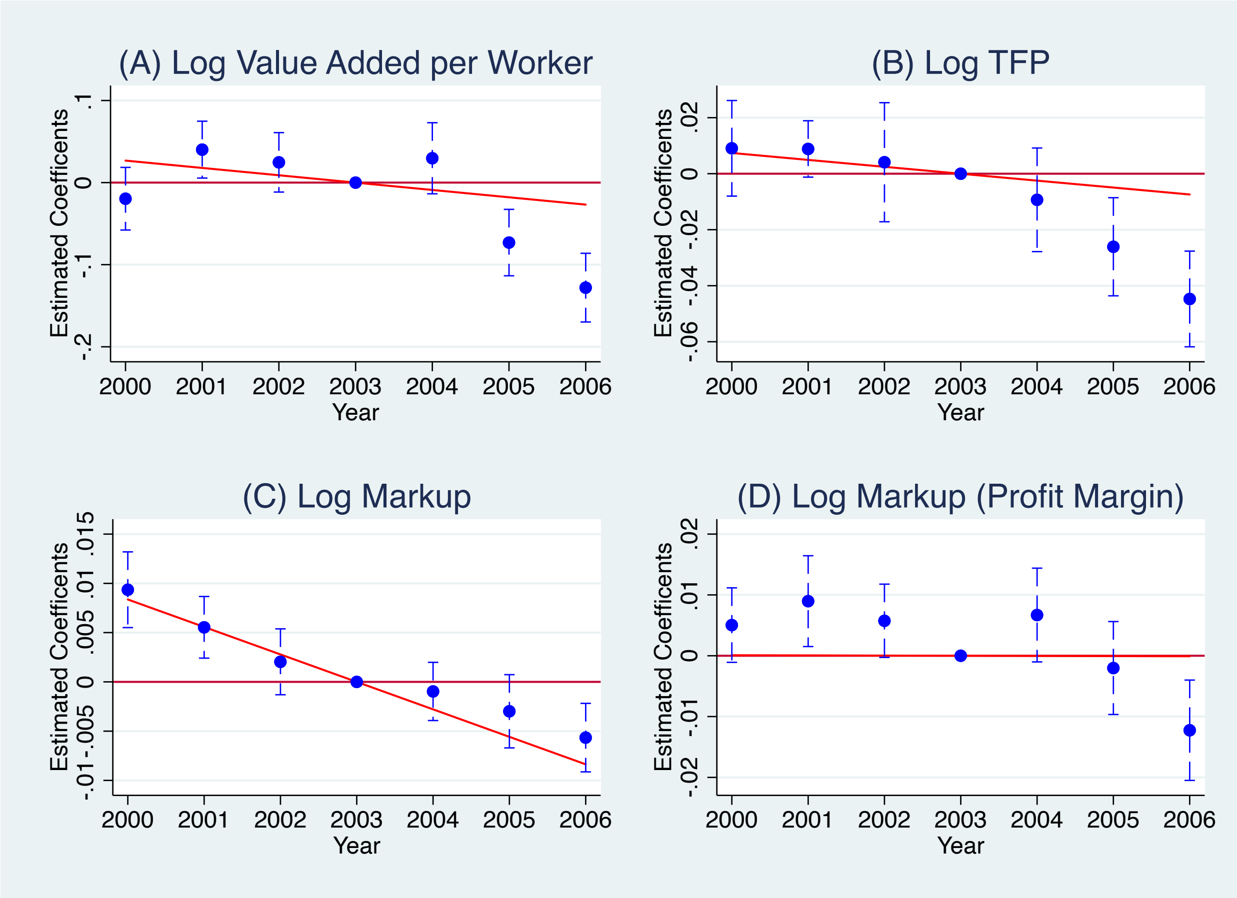


Figure 1: Dynamic Effects: Coefficients in Years Before and After Policy Implementation

NOTES: The figures plot the coefficients we obtain from a specification that regresses the outcome on the interaction of the eligibility dummy with each year (Equation 6). We also include the interaction of year dummies with indicators of whether the firm imports or is a "processing exporter". We drop the interaction with $year = 2003$, so coefficients can be interpreted as the differential outcome of eligible relative to non-eligible firms relative to their differences in 2003. For each figure, there is a trend line which can be interpreted as follows: we regress each outcome on the "Elig*Trend" interaction plus controls and fixed effects (no year dummy interaction) only for pre-reform years. Then, we take the coefficient on the eligible firm trend and set it as a constant linear trend that grows linearly from 2000 to 2006, where we normalize it to 0 in 2003. Log value added per worker simply uses the raw value added and employment data. TFP and markups are estimated based on revenue productivity estimations as in DeLoecker and Warzynski (2012). The profit margin markup measure is equal to: $Markup(profit) = \frac{sales - wages - inputs}{sales}$. 95% confidence intervals are shown in the figures.

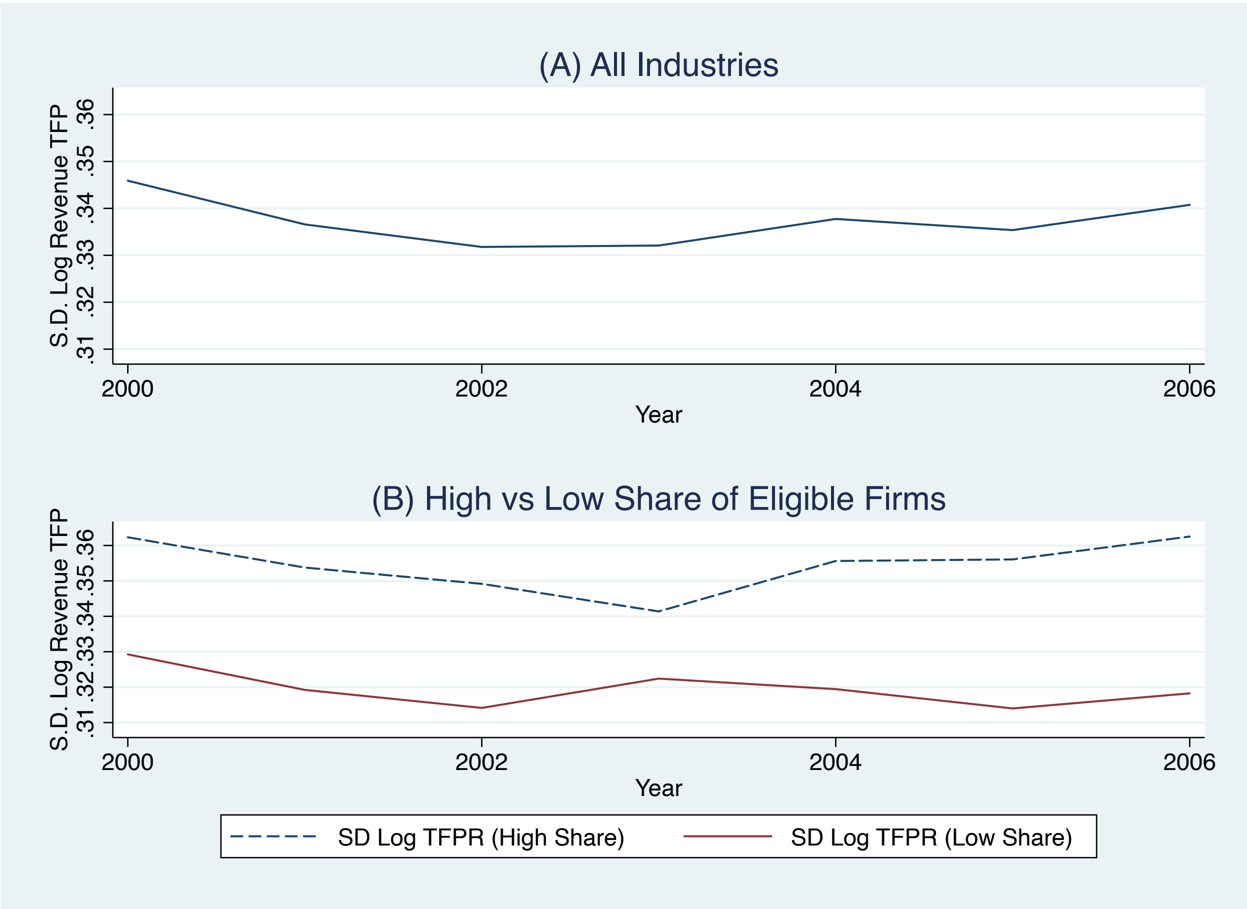


Figure 2: Standard Deviation of Revenue Productivity

NOTES: This figure plots the the standard deviation of revenue productivity over time. Revenue productivity refers to the TFP as estimated using the Ackerberg et al. (2015) procedure.

Table 1
Firm information of the merged data

Year	# of firms in CASIF	# of firms in CCTS	# of firms in merged data	# of firms with zero export value	# of firms with positive export value
2000	140311	59868	125013	2940	33269
2001	149348	62210	136864	3464	37067
2002	160860	57739	147116	3293	40933
2003	176370	52257	165102	3123	46485
2004	255312	77029	235517	3314	69565
2005	247489	106846	234161	7772	68955
2006	274864	73857	260863	3750	72492

NOTES: This table summarizes the number of firms generated in the merged data set comprising of CASIF and CCTS, for years 2000-2006.

Table 2
Firm ETR rate 2000-2006

Year	Observations	Mean reb_f	SD reb_f	Mean rv_f	SD rv_f
2000	15956	0.15	.024	0.885	0.13
2001	18559	0.152	0.022	0.9	0.118
2002	19328	0.153	0.022	0.903	0.118
2003	19020	0.151	0.023	0.894	0.126
2004	32093	0.128	0.018	0.755	0.096
2005	38729	0.128	0.019	0.758	0.098
2006	22251	0.129	0.017	0.76	0.091

NOTES: Firm ETR rate is as defined as reb_f in the main text. rv_f uses the same reb_f as firm ETR rate divided by the firm VAT rate computed with the same weighting. We then take the mean and standard deviation across all eligible firms in each year. The minimum rebate (and ratio) is 0. The maximum firm rebate is 0.17, which is equal to the VAT rate and therefore has a ratio of 1.

Table 3
Tariff Policy

Year	Mean Output Tariff	Mean Input Tariff	Mean Export Tariff
2000	19.15	1.49	11.14
2001	17.8	1.50	10.97
2002	13.6	1.52	10.42
2003	12.05	1.52	9.74
2004	10.62	1.52	9.50
2005	9.93	1.53	9.60
2006	9.56	1.50	9.17

NOTES: Tariffs are based on data from WITS-Trains. Output tariffs are unweighted mean of applied tariffs across industries. Input tariffs are constructed using output tariffs and input output table. Export tariffs are based on mean tariffs charged by all China trade partners on Chinese goods.

Table 4
Customs Data: Eligible Firms Only

	Log Quantity		Log Price		Log Value	
	(All)	(Balanced)	(All)	(Balanced)	(All)	(Balanced)
Rebates/VAT (RV)	0.267*** (0.067)	0.348*** (0.113)	-0.025 (0.022)	-0.041 (0.041)	0.242*** (0.065)	0.308*** (0.106)
HS6 tariffs	-0.006 (0.013)	0.010 (0.026)	0.003 (0.006)	0.011 (0.013)	-0.002 (0.012)	0.021 (0.025)
K/L	0.016*** (0.005)	0.035*** (0.012)	-0.002 (0.002)	0.003 (0.005)	0.014*** (0.005)	0.038*** (0.011)
Foreign share	0.011 (0.017)	0.001 (0.042)	0.012 (0.008)	-0.021 (0.018)	0.023 (0.017)	-0.021 (0.041)
Importer	0.075*** (0.009)	0.050*** (0.018)	-0.012*** (0.004)	-0.013 (0.008)	0.064*** (0.009)	0.037** (0.018)
FEs	Firm-Product, Year	Firm-Product, Year	Firm-Product, Year	Firm-Product, Year	Firm-Product, Year	Firm-Product, Year
R^2	0.852	0.832	0.942	0.929	0.781	0.750
N	577646	138350	577646	138350	577706	138364

NOTES: This Table reports the results for specification (4). There are 3 outcomes (log quantity, log prices, and log value), with 2 specifications for each: including all eligible firms and with a balanced panel. In all specifications we include firm-product and year fixed effects. Standard errors (in parenthesis) are clustered at the 4-digit industry level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

Table 5

Firm Data: Output Coefficients and Markups by 2-digit Industry

Industry (2 dig)	Industry Name	Labor Coeff	Capital Coeff	Materials Coeff	Median Markup
13	Food Process.	.07	.01	.85	1.10
14	Food Manuf.	.05	.01	.91	1.18
15	Beverage Manuf.	.01	.03	.93	1.24
16	Tobacco Process.	.18	.09	.85	1.36
17	Textile Industry	.05	.02	.87	1.10
18	Garments, Shoes, Hats	.13	.01	.85	1.12
19	Leather, Furs, Down	.10	.02	.83	1.05
20	Timber Process, other wood	.00	.02	.89	1.15
21	Furniture	.03	.01	.96	1.25
22	Papermaking & Paper	.05	.01	.90	1.15
23	Printing and Media	.11	.02	.89	1.20
24	Stationery	.07	.02	.88	1.13
25	Petroleum Process, other fuel	.03	.01	.92	1.18
26	Raw Chemical	.04	.03	.85	1.09
27	Medical & Pharma	.04	.02	.93	1.20
28	Chemical Fibers Manuf.	.03	.00	.93	1.13
29	Rubber products	.03	.03	.87	1.12
30	Plastic products	.04	.03	.85	1.06
31	Non-metal Mineral	.01	.02	.90	1.19
32	Ferrous Metal Process.	.04	.02	.91	1.13
33	Non-ferrous Metal Process.	.04	.01	.92	1.14
34	Metal Products	.07	.02	.87	1.10
35	Ordinary Machinery Manuf.	.06	.03	.87	1.12
36	Special Purpose Equipment	.08	.03	.83	1.08
37	Transportation Equipment	.04	.02	.91	1.18
39	Electric Equipment & Machinery	.06	.03	.86	1.08
40	Electronics & Telecommunications	.07	.03	.87	1.14
41	Instruments, other Machinery	.11	.03	.79	1.05
42	Crafts & Other Manufacturing	.07	.01	.89	1.16

NOTES: Data source is the Chinese National Bureau of Statistics (NBS) and annual surveys (CASIF). Production function output coefficients are estimated using the procedure of Ackerberg et al. (2015). The firm markup is the ratio of the *materials* coefficient in the production function and *materials* share of total input costs. The Translog specification allows us to recover both firm-specific material cost shares and material output shares. As an example, the output coefficient for materials are calculated as: $\theta_M = \beta_M + 2\beta_{MM} M_{it} + \beta_{LM} + L_{it} + \beta_{KM} K_{it} + \beta_{LKM} L_{it} K_{it}$. Additionally, we calculate firm TFP using output coefficients.

Table 6
2004 Policy Change on Eligible vs Non-Eligible Firms: No Entry

	Markup	(Markup (Profit)	TFP	VA/worker
	(1)	(2)	(3)	(4)
$\geq$ 2004*Elig.	-0.007***	-0.019***	-0.024***	-0.077***
	(0.002)	(0.007)	(0.008)	(0.026)
Tariff*Eligible	-0.001	-0.027	-0.054	-0.100
	(0.006)	(0.035)	(0.044)	(0.091)
InputTariff*Eligible	-0.004	0.025	-0.002	0.016
	(0.004)	(0.016)	(0.024)	(0.036)
ExportTariff*Eligible	-0.002	0.003	0.003	-0.005
	(0.002)	(0.013)	(0.014)	(0.044)
K/L	0.005***	0.009***	0.034***	0.252***
	(0.001)	(0.003)	(0.006)	(0.013)
Foreign share	0.003	-0.082	-0.019	-0.006
	(0.003)	(0.091)	(0.017)	(0.045)
Processing	0.004	0.002	-0.014	-0.037
	(0.007)	(0.014)	(0.026)	(0.076)
Importer	0.001	-0.040	-0.012	-0.012
	(0.002)	(0.043)	(0.008)	(0.022)
Log Exports	0.000	0.001	0.003***	0.006**
	(0.000)	(0.001)	(0.001)	(0.002)
R^2	0.92	0.24	0.89	0.84
N	129500	129500	129500	129500

NOTES: This table displays relative outcomes differences between ETR-eligible and non-eligible firms to the policy change in 2004. There are 4 outcomes measured: log markup, log markup using a profit margin, log TFP, and log value added per worker. δ is the coefficient on the main variable of interest: the interaction of a dummy if the year is 2004 or after, with the firm dummy for eligibility. Firms are eligible if they export and are not "processors". All controls are reported in the table, except we omit the firm age and its interaction with the eligibility dummy coefficients (included to control for trends). "TFP" is an index for TFP using the production function estimation of Ackerberg et al. (2015), with gross output data. "K/L" stands for capital intensity, and "foreign share" is the percent of the firm owned by a multinational. "Processing" and "Importer" are dummies. We keep only firms are alive in 2000. Standard errors (in parenthesis) are clustered at the 4-digit industry level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 7
2004 Policy Effect Across Industries: Standard Deviation of TFPR

	SD TFPR					
	(1)	(2)	(3)	(4)	(5)	(6)
$\geq$ 2004*HighShare	0.018*	0.024**	0.028**			
	(0.009)	(0.010)	(0.013)			
$\geq$ 2004*EligShare				0.074	0.119*	0.138*
				(0.059)	(0.067)	(0.077)
Tariff*HighShare		-0.072	-0.038			
		(0.060)	(0.070)			
ExportTariff*HighShare		0.009	-0.007			
		(0.012)	(0.014)			
Tariff*EligShare					-0.477**	-0.165
					(0.240)	(0.301)
ExportTariff*EligShare					0.054	-0.018
					(0.053)	(0.070)
Avg L/K	0.015	0.016	0.025	0.017	0.019	0.029
	(0.012)	(0.013)	(0.023)	(0.012)	(0.012)	(0.022)
Avg Exports	0.017	0.021	0.007	0.017	0.019	0.000
	(0.021)	(0.022)	(0.038)	(0.021)	(0.022)	(0.037)
HHI	0.047	-0.000	-0.050	0.050	0.000	-0.043
	(0.090)	(0.082)	(0.121)	(0.092)	(0.083)	(0.121)
LicenseReq*HighShare			-0.003			
			(0.091)			
LicenseReq*EligShare						-0.231
						(0.304)
MFA*HighShare			0.018			
			(0.067)			
MFA*EligShare						0.073
						(0.236)
R^2	0.77	0.78	0.80	0.77	0.78	0.80
N	3007	2690	1527	2986	2677	1520

NOTES: This table displays the effect of the policy change in 2004 on relative TFPR dispersions between industries that are treated depending on their exposure. The outcome in all columns is the standard deviation of TFPR within an industry-year. In the first two columns, the main coefficient (δ) is the interaction of a dummy if the year is 2004 or after, with the industry dummy for "$HighShare$". In the last two columns we interact the time dummy with a continuous measure of the share of firms that are rebate eligible (fixed in the year 2001). Firms within an industry are eligible if they export and are not "processors". All controls are reported in the table, except a trend interaction with the eligibility dummy coefficients. The interaction of input tariffs with rebate exposure is dropped due to collinearity. All specifications include industry and year fixed effects. Standard errors (in parenthesis) are clustered at the 4-digit industry level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

A Appendix

A.1 Entry, Exit, and Competition

The current literature on gains from trade has moved away from entry to some degree to concentrate on the effect of trade on incumbents. This is due to the use of the tractable Pareto distribution to describe the productivity distribution of firms.[38] In examining the misallocation measures below we measure dispersions of all producing firms and in this way ignore the effects of entry and exit as in Hsieh and Klenow (2009). In the firm level regressions we check that our results are not driven by a shifting composition of firms. Here, we check the dynamics of entry and exit that might be present in the background. Figure A1 plots the number of entrants and exiting firms for each year, where exit is defined as a firm not in the dataset in the next year. There are a couple of reasons for the seemingly huge entry jump in 2004. Brandt et al. (2012) point out that many private firms were left out before 2004 simply due to the identification of firms by the census. Moreover, Bai et al. (2017) point out that export licenses made it very difficult for private firms to export directly before 2004, with preference given to foreign and state-owned enterprises. This makes it difficult to study the effects of ETR on competition.

An interesting observation is that firms that exit the next year tend to have higher markups. The median markup of exiting firms is 14% across all years (after winsorizing the data), compared to the median of 12.7% for the whole sample. The entry of private firms most likely led to increasing departures of older and state-owned firms with high markups. On the other hand, entrants have lower markups than both incumbents and exiting firms. Markup distributions for the 3 types of firms are shown in Figure A2. Therefore, aside from the effect of ETRs, there is most likely tougher competition in the export market after 2003.

[Figure 3 about here.]

[Figure 4 about here.]

A.2 Firm Level Robustness Results

[Figure 5 about here.]

[Figure 6 about here.]

[Figure 7 about here.]

[38] Arkolakis et al. (2017) is one of the better-known examples. See Feenstra (2018) for a way to reintroduce entry into trade models.

[Table 8 about here.]

[Table 9 about here.]

[Table 10 about here.]

[Table 11 about here.]

A.3 Industry Level Raw Trends

[Figure 8 about here.]

A.3.1 Description and Summary of Petrin and Levinsohn (2012) APG Measure

Aggregate productivity growth (APG) is the annual change in industry final demand corrected for growth in factor expenditures.[39] Technical efficiency is the weighted average of productivity growth coming from plants generating more output holding inputs constant, which we compute as the average of growth rates in revenue productivity across all incumbent firms.[40] Then, we compute the "distorted" Solow residual, which is the growth in aggregate productivity not explained by the average growth in firm productivity.

Recently, Baqaee and Farhi (2019) have generalized the interpretation of this distorted Solow residual. We do not measure their change in allocative efficiency directly given data limitations. Instead, as in Petrin and Levinsohn (2012), we compute APG. In Baqaee and Farhi (2019), the analogous term to APG is written as $dlogY - \sum_f \Lambda_f dlogL_f$. Then, continuing with the terminology in Baqaee and Farhi (2019), in order to compute changes in allocative efficiency, we subtract from APG: $\sum_f \Lambda_f dlogA_f$. A_f is firm physical TFP, which we assume we can measure with revenue productivity although this is of course not correctly measured physical TFP. Λ_f is the sales share in that paper, where we use value added weights. Due to data limitations, we do not incorporate the input-output structure of that paper.

The *growth rate* of the distorted Solow residual is displayed in Figure A7. Throughout the time period, there is a negative growth rate in allocative efficiency, which means that reallocation is taking away from aggregate productivity growth. Furthermore, consistent with the observed rise in dispersion of TFPR, the reduction in allocative efficiency is the largest in the latter half of the sample period. We do note, however, that this captures *all* changes in APG that are not captured by growth in technical efficiency. The dispersion in revenue productivity is a more direct measure of the causal effect of changes in ETRs because the model in the previous subsection allows us to map a rise in τ_{fH} to a larger difference in TFPR across eligible and non-eligible firms.

[39] Given the difficulty in computing capital prices, we follow Petrin and Levinsohn (2012) and assume that labor is the only factor. APG is then the differences between the growth rate in total revenues and total wages.

[40] Weights are firm value added shares within the industry. We use the average weights across the two years. For this analysis, we compute the measures from a *balanced* panel of firms since the average growth of firm productivity only captures firms in the two consecutive years. As a robustness, we have also checked the dispersion measures using a balanced panel of firms, but the results are similar so we do not report them here.

[Figure 9 about here.]

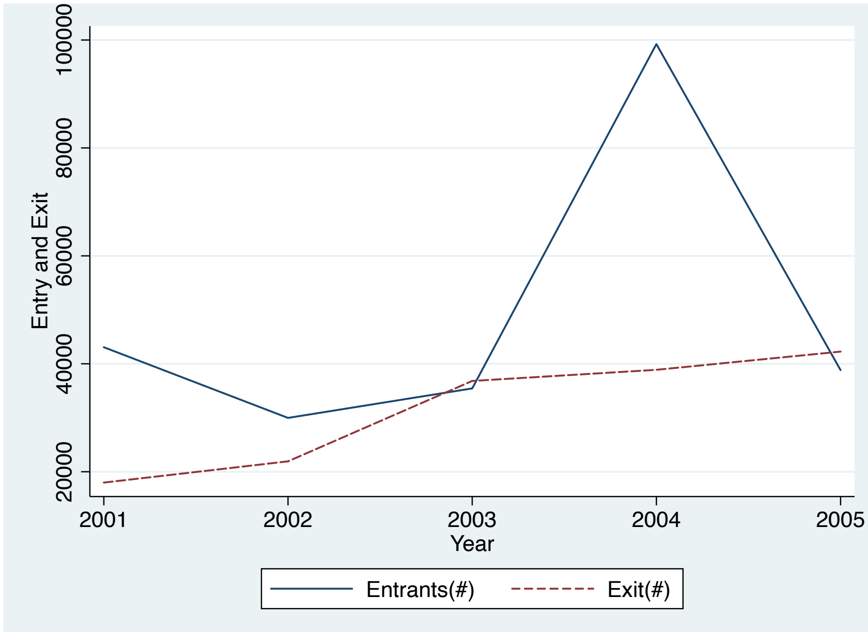
A.4 Industry Level Robustness Results

[Table 12 about here.]

[Table 13 about here.]

[Figure 10 about here.]

Figure A1: Number of Entry and Exit per Year



NOTES: This Figure plots the number of firms that enter the dataset and exit the dataset in each year. Exit is defined as a firm not being in the dataset in year $t+1$.

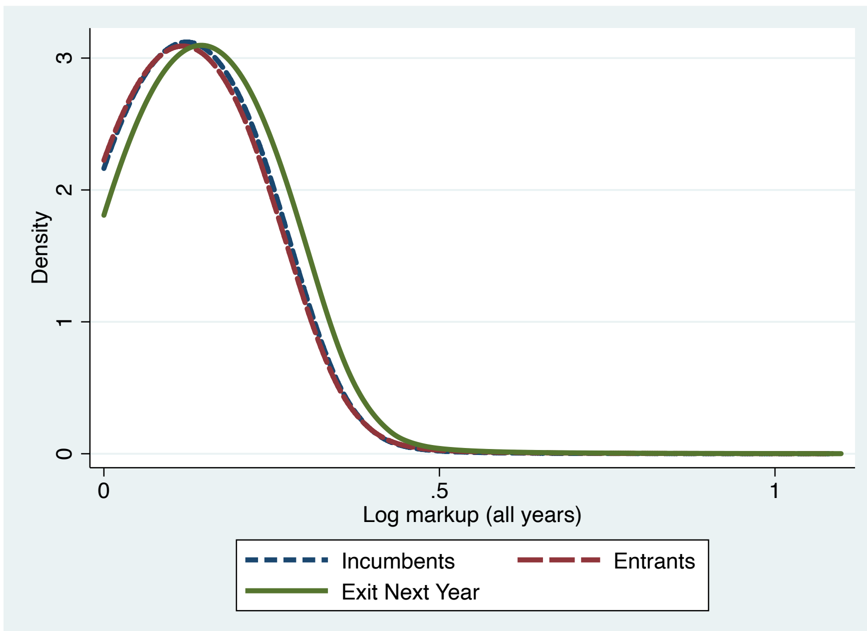


Figure A2: Kernel Density: Incumbents, Entrants, and Exiting Firms

NOTES: This Figure plots the density function of log markups (DLW estimation) for incumbents, entrants, and exiters (the next year).

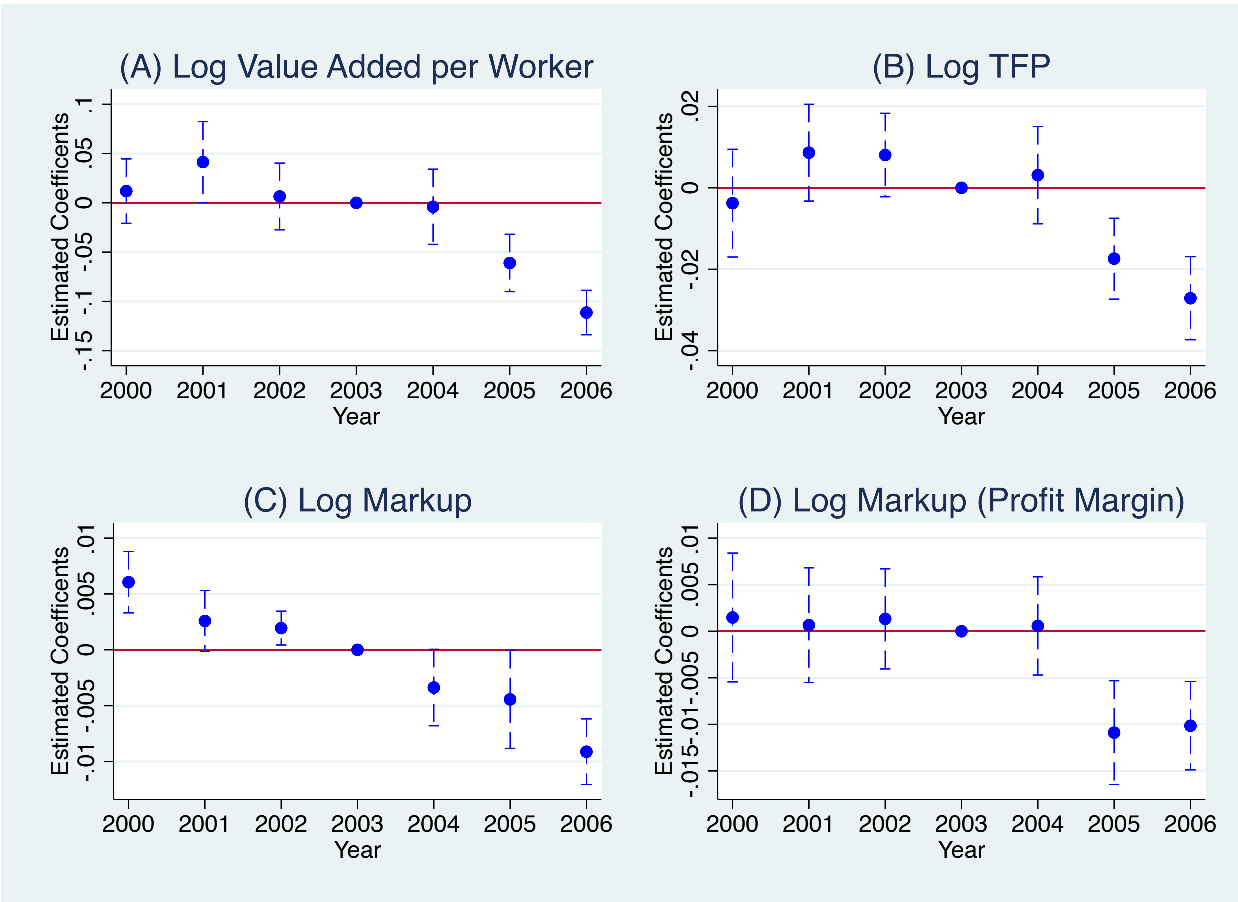


Figure A3: Dynamic Effects: Coefficients in Years Before and After Policy Implementation: All Firms (with entry)

NOTES: The figures plot the coefficients we obtain from a specification that regresses the outcome on the interaction of the eligibility dummy with each year (Equation 6), for the case where we allow for free entry and exit and report results for the unbalanced panel. We drop the interaction with $year$ = 2003, so coefficients can be interpreted as the differential outcome of eligible relative to non-eligible firms relative to their differences in 2003. Log value added per worker simply uses the raw value added and employment data. TFP and markups are estimated based on revenue productivity estimations as in DeLoecker and Warzynski (2012). The profit margin markup measure is equal to: $Markup(profit) = \frac{sales-wages-inputs}{sales}$. 95% confidence intervals are shown in the figures.

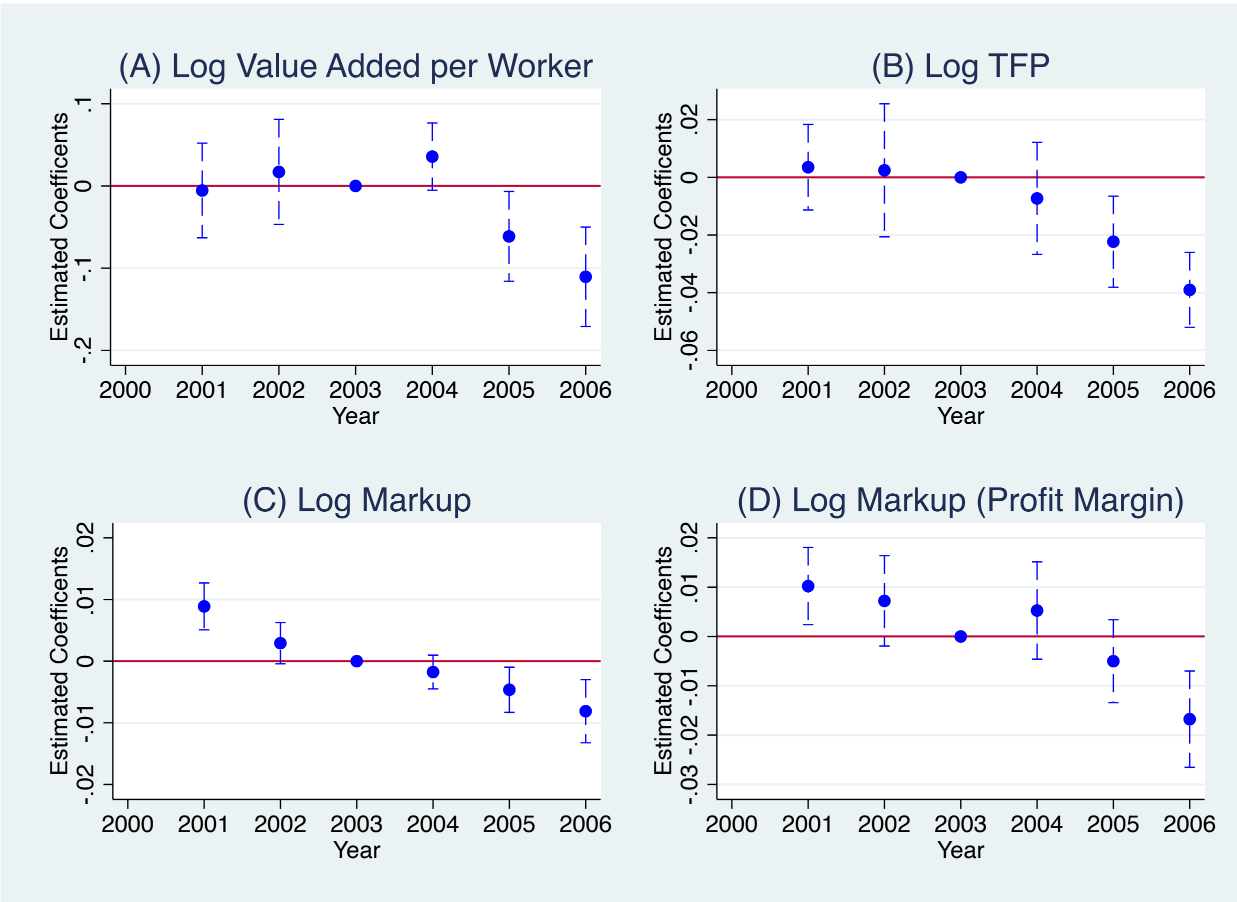


Figure A4: Dynamic Effects: Coefficients in Years Before and After Policy Implementation: Control for Trend by merging 2000 and 2001

NOTES: The figures plot the coefficients we obtain from a specification that regresses the outcome on the interaction of the eligibility dummy with each year (Equation 6). Relative to Figure 1 in the main text, in this case we add a control for trend. To do this, we merge together the 2000 and 2001 data, so that the trend variables are not completely captured by the year dummies. Then we add a trend-eligible interaction as well as the trend variable as controls. We drop the interaction with $year = 2003$, so coefficients can be interpreted as the differential outcome of eligible relative to non-eligible firms relative to their differences in 2003. Log value added per worker simply uses the raw value added and employment data. TFP and markups are estimated based on revenue productivity estimations as in DeLoecker and Warzynski (2012). The profit margin markup measure is equal to: $Markup(profit) = \frac{sales-wages-inputs}{sales}$. 95% confidence intervals are shown in the figures.

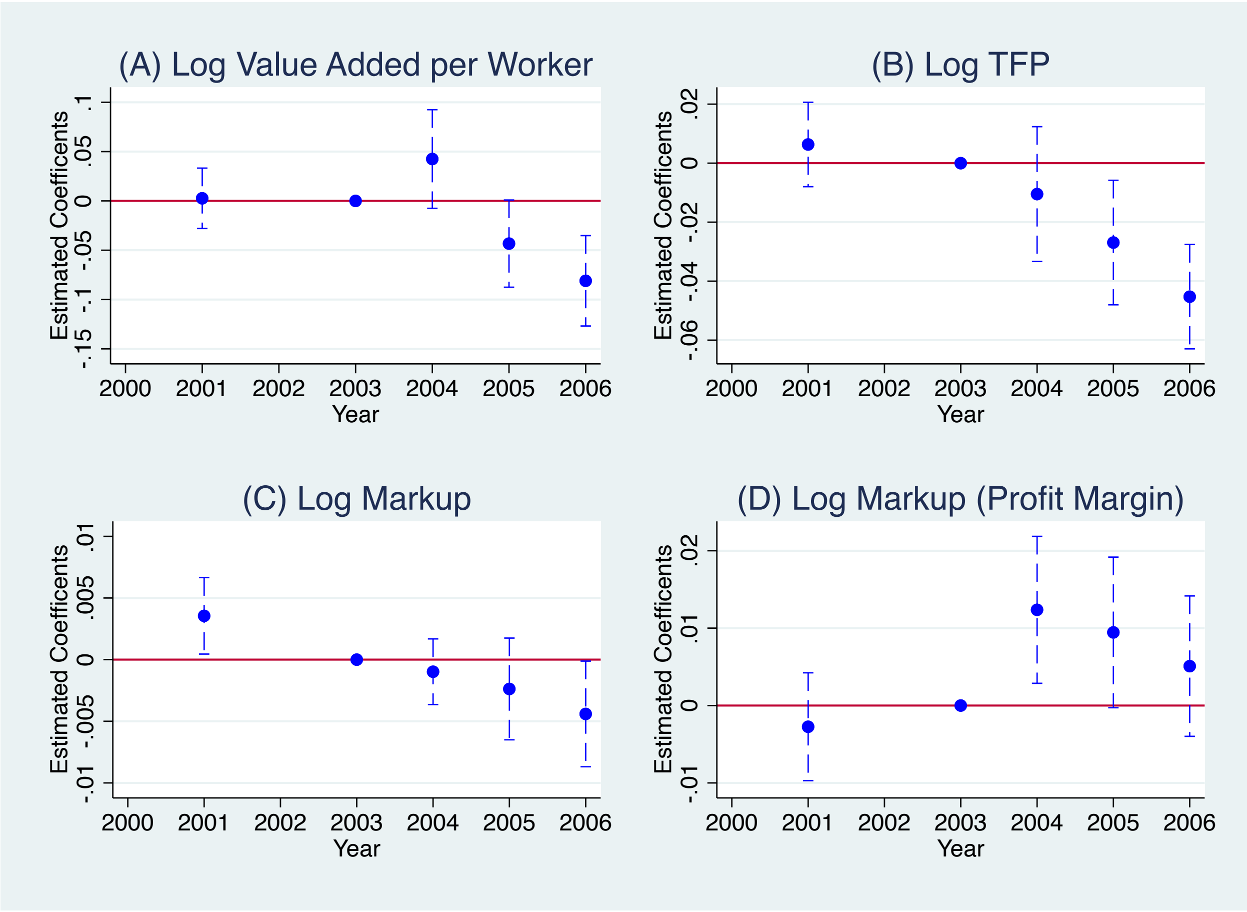


Figure A5: Dynamic Effects: Coefficients in Years Before and After Policy Implementation: Control for Trend by merging 2003 and 2002

NOTES: The figures plot the coefficients we obtain from a specification that regresses the outcome on the interaction of the eligibility dummy with each year (Equation 6). Relative to Figure 1 in the main text, in this case we add a control for trend. To do this, we merge together the 2003 and 2002 data, so that the trend variables are not completely captured by the year dummies. Then we add a trend-eligible interaction as well as the trend variable as controls. We drop the interaction with $year = 2003$, so coefficients can be interpreted as the differential outcome of eligible relative to non-eligible firms relative to their differences in 2003. Log value added per worker simply uses the raw value added and employment data. TFP and markups are estimated based on revenue productivity estimations as in DeLoecker and Warzynski (2012). The profit margin markup measure is equal to: $Markup(profit) = \frac{sales-wages-inputs}{sales}$. 95% confidence intervals are shown in the figures.

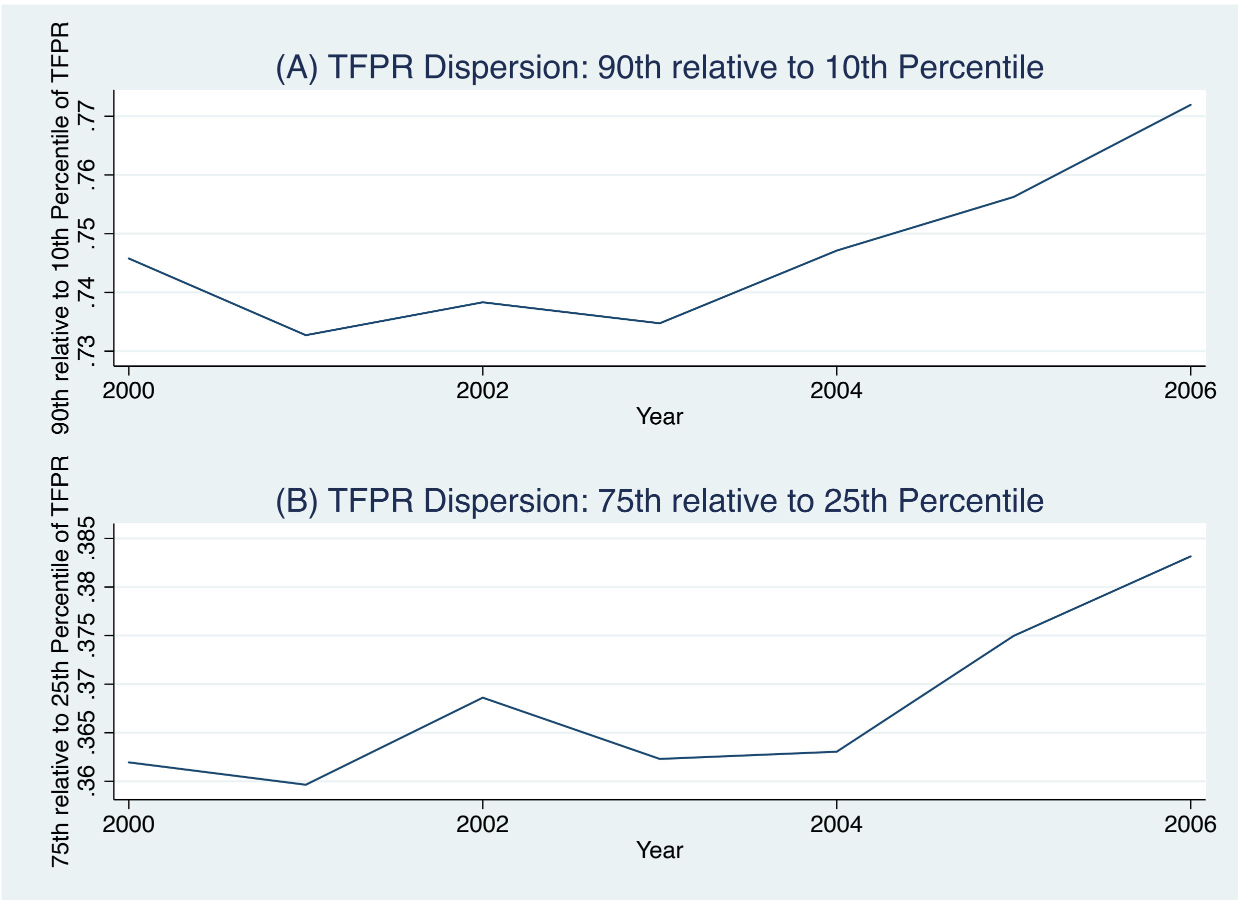


Figure A6: Alternative Measures of Dispersion

NOTES: This Figure plots alternative measures of misallocation. The two panels display alternative dispersion measures: the difference in TFPR across firms in the 90th (75th) relative to firms in the 10th (25th) percentile of the distribution in each year. Revenue productivity refers to the TFP as estimated using the Ackerberg et al. (2015) procedure.

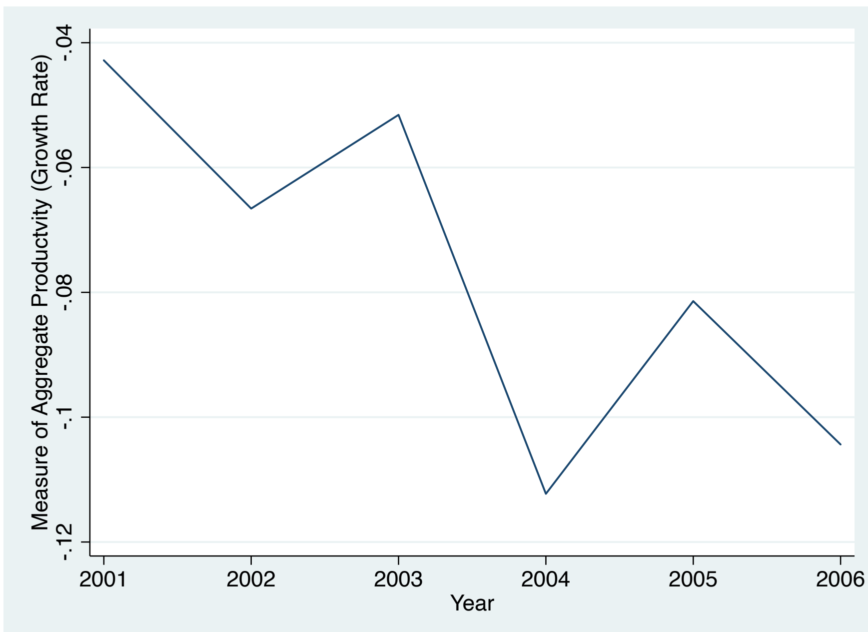


Figure A7: Alternative Measure of Dispersion: Allocative Efficiency as in PL (2012)

NOTES: This Figure plots an alternative measure of misallocation: the growth rate of allocative efficiency, or the "distorted Solow residual". The measure is produced as the difference between aggregate productivity growth and the average growth rate of revenue productivity of incumbent firms in each year.

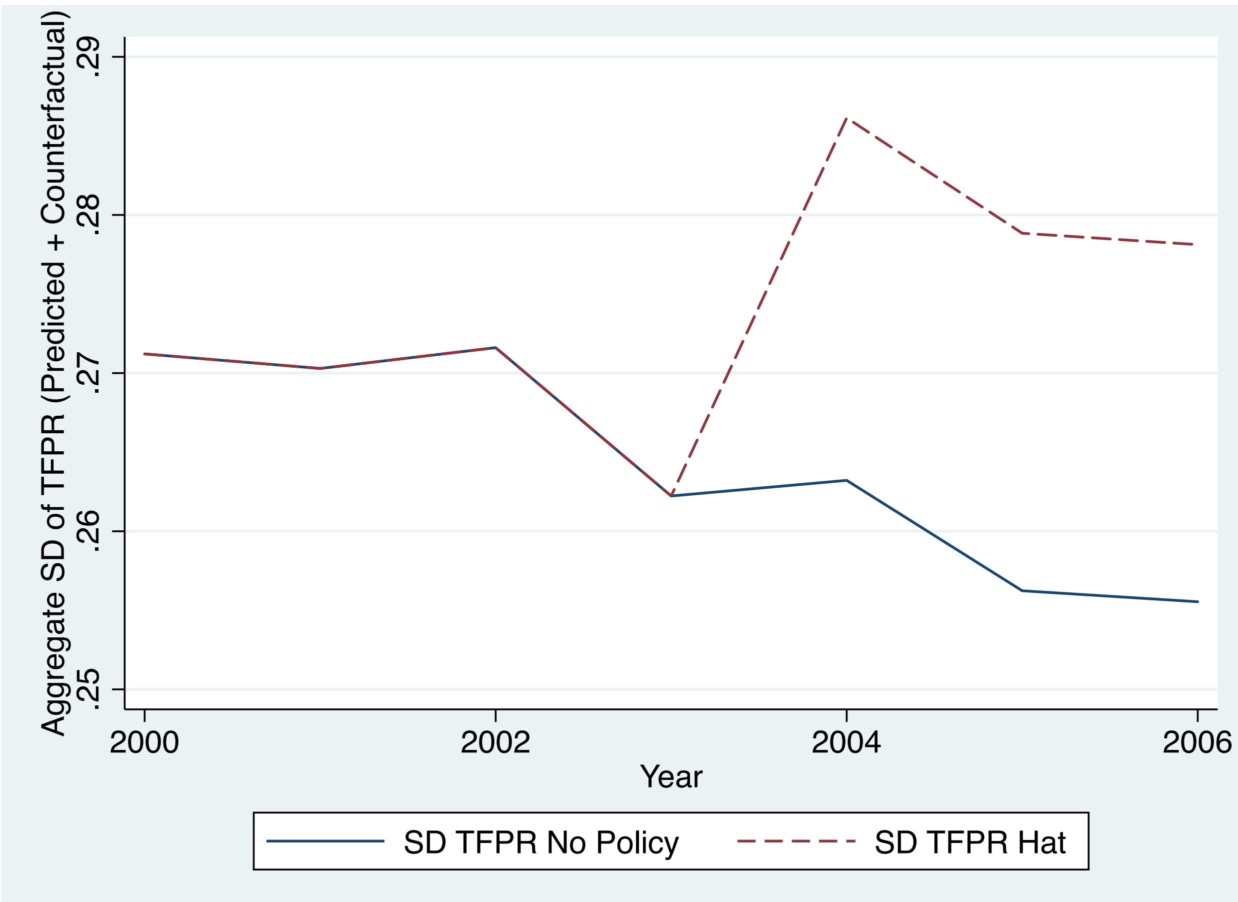


Figure A8: Aggregate Standard Deviation of Revenue Productivity: Predicted versus Counterfactual (with no policy)

NOTES: This figure plots the predicted standard deviation of revenue productivity over time under two scenarios: i) the predicted dispersion given the estimated parameters in (14), and ii) assuming $\delta = 0$ in (14). We use the continuous measure of the fraction of eligible firms in the industry instead of "HighShare". Predicted dispersions are constructed for each industry j, which we aggregate to the country level using revenue shares of each industry in 2000. The regression specification is identical to column (5) of Table 7.

Table A1

Trend Break After 2004

	Log VA/worker	TFP	Markup	Markup (profit)
	(1)	(2)	(3)	(4)
$\geq$ 2004_*Elig*Trend	-0.046***	-0.010**	0.001	-0.004
	(0.007)	(0.003)	(0.001)	(0.002)
Elig*Trend	-0.014**	-0.004**	-0.002***	-0.003
	(0.004)	(0.002)	(0.000)	(0.001)
R^2	0.84	0.89	0.92	0.52
N	129500	129500	129500	128617

In this Table we report results for evidence of a "break in trend." We are interested in the triple interaction in the first row, which is the differential effect on eligible firms relative to the pre-2004 trend. Standard errors (in parenthesis) are clustered at the 4-digit industry level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A2
2004 Policy Change on Eligible vs Non-Eligible Firms: All Firms (with entry)

	Markup	(Markup (Profit)	TFP	VA/worker
	(1)	(2)	(3)	(4)
$\geq$ 2004*Elig.	-0.007***	-0.005	-0.015***	-0.062***
	(0.002)	(0.009)	(0.005)	(0.017)
Tariff*Eligible	0.010*	-0.037	0.032	-0.037
	(0.006)	(0.023)	(0.032)	(0.056)
InputTariff*Eligible	-0.006	0.025**	-0.033**	0.029
	(0.004)	(0.012)	(0.016)	(0.028)
ExportTariff*Eligible	0.002	0.020	0.001	0.013
	(0.002)	(0.014)	(0.008)	(0.031)
K/L	0.003***	-0.001	0.023***	0.210***
	(0.001)	(0.010)	(0.005)	(0.008)
Foreign share	0.000	0.018	0.001	0.013
	(0.001)	(0.042)	(0.007)	(0.023)
Processing	0.002	-0.003	0.003	0.021
	(0.002)	(0.008)	(0.009)	(0.025)
Importer	-0.001	-0.004	0.005	0.022*
	(0.001)	(0.014)	(0.004)	(0.013)
Log Exports	0.000	0.001	0.004***	0.009***
	(0.000)	(0.001)	(0.001)	(0.001)
R^2	0.90	0.08	0.85	0.83
N	820214	820214	820214	820214

This table displays relative outcomes differences between ETR-eligible and non-eligible firms to the policy change in 2004. Unlike Table 6, in this case we allow for free entry and exit and report results for the unbalanced panel. There are 5 outcomes measured: log markup,log markup using a profit margin, log TFP, log value added per worker, and log revenue. δ is the coefficient on the main variable of interest: the interaction of a dummy if the year is 2004 or after, with the firm dummy for eligibility. Firms are eligible if they export and are not "processors". All controls are reported in the table, except we omit the firm age and its interaction with the eligibility dummy coefficients (included to control for trends). "TFP" is an index for TFP using the production function estimation of Ackerberg et al. (2015), with gross output data. "K/L" stands for capital intensity, and "foreign share" is the percent of the firm owned by a multinational. "Processing" and "Importer" are dummies. Standard errors (in parenthesis) are clustered at the 4-digit industry level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A3
Industry Rebate Rate Variation on Eligible vs Non-Eligible Firms: No Entry

	Markup	(Markup (Profit)	TFP	VA/worker
	(1)	(2)	(3)	(4)
Rebate*Elig.	0.039***	0.109***	0.112**	0.343***
	(0.010)	(0.040)	(0.048)	(0.132)
Tariff*Eligible	-0.001	-0.028	-0.056	-0.103
	(0.006)	(0.035)	(0.045)	(0.092)
InputTariff*Eligible	-0.005	0.023	-0.005	0.007
	(0.004)	(0.016)	(0.024)	(0.036)
ExportTariff*Eligible	-0.001	0.005	0.007	0.010
	(0.002)	(0.014)	(0.014)	(0.041)
K/L	0.005***	0.009***	0.035***	0.252***
	(0.001)	(0.003)	(0.006)	(0.013)
Foreign share	0.002	-0.082	-0.020	-0.009
	(0.003)	(0.091)	(0.017)	(0.046)
Processing	0.004	0.001	-0.015	-0.041
	(0.008)	(0.014)	(0.026)	(0.075)
Importer	0.001	-0.040	-0.012	-0.011
	(0.002)	(0.043)	(0.008)	(0.022)
Log Exports	0.000	0.001	0.003***	0.006**
	(0.000)	(0.001)	(0.001)	(0.002)
R^2	0.92	0.24	0.89	0.84
N	128836	128836	128836	128836

This table displays relative outcomes differences between ETR-eligible and non-eligible firms in response to changes in rebate levels at the industry level. There are 5 outcomes measured: log markup,log markup using a profit margin, log TFP, log value added per worker, and log revenue. δ is the coefficient on the main variable of interest: the interaction of the industry rebate rate in year t with the firm dummy for eligibility. Firms are eligible if they export and are not "processors". All controls are reported in the table, except we omit the firm age and its interaction with the eligibility dummy coefficients (included to control for trends). "TFP" is an index for TFP using the production function estimation of Ackerberg et al. (2015), with gross output data. "K/L" stands for capital intensity, and "foreign share" is the percent of the firm owned by a multinational. "Processing" and "Importer" are dummies. We keep only firms that are alive in 2000. Standard errors (in parenthesis) are clustered at the 4-digit industry level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A4
Industry Rebate Rate Variation on Eligible vs Non-Eligible Firms: All Firms (with entry)

	Markup	(Markup (Profit)	TFP	VA/worker
	(1)	(2)	(3)	(4)
Rebate*Elig.	0.040***	0.010	0.088***	0.292***
	(0.008)	(0.050)	(0.025)	(0.076)
Tariff*Eligible	0.010*	-0.037	0.031	-0.041
	(0.006)	(0.023)	(0.032)	(0.056)
InputTariff*Eligible	-0.007	0.024**	-0.034**	0.024
	(0.004)	(0.012)	(0.016)	(0.028)
ExportTariff*Eligible	0.002	0.021	0.002	0.021
	(0.002)	(0.014)	(0.007)	(0.030)
K/L	0.003***	-0.001	0.023***	0.210***
	(0.001)	(0.010)	(0.005)	(0.008)
Foreign share	0.000	0.018	0.002	0.013
	(0.001)	(0.042)	(0.007)	(0.023)
Processing	0.002	-0.004	0.002	0.018
	(0.002)	(0.008)	(0.009)	(0.025)
Importer	-0.001	-0.004	0.005	0.023*
	(0.001)	(0.014)	(0.004)	(0.013)
Log Exports	0.000	0.001	0.004***	0.009***
	(0.000)	(0.001)	(0.001)	(0.001)
R^2	0.90	0.08	0.85	0.83
N	815806	815806	815806	815806

This table displays relative outcomes differences between ETR-eligible and non-eligible firms in response to changes in rebate levels at the industry level. Unlike Table A3, in this case we allow for free entry and exit and report results for the unbalanced panel. There are 5 outcomes measured: log markup,log markup using a profit margin, log TFP, log value added per worker, and log revenue. δ is the coefficient on the main variable of interest: the interaction of the industry rebate rate in year t with the firm dummy for eligibility. Firms are eligible if they export and are not "processors". All controls are reported in the table, except we omit the firm age and its interaction with the eligibility dummy coefficients (included to control for trends). "TFP" is an index for TFP using the production function estimation of Ackerberg et al. (2015), with gross output data. "K/L" stands for capital intensity, and "foreign share" is the percent of the firm owned by a multinational. "Processing" and "Importer" are dummies. Standard errors (in parenthesis) are clustered at the 4-digit industry level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A5
2004 Policy Effect Across Industries: Standard Deviation of Value Added per Worker and Markups

	SD VA/Worker		SD Markup	
	(1)	(2)	(3)	(4)
$\geq$ 2004*HighShare	0.042**		0.003	
	(0.020)		(0.002)	
$\geq$ 2004*EligShare		0.231***		0.025*
		(0.084)		(0.013)
Tariff*HighShare	0.023		-0.050***	
	(0.116)		(0.014)	
ExportTariff*HighShare	0.018		-0.003	
	(0.024)		(0.002)	
Tariff*EligShare		0.111		-0.171***
		(0.523)		(0.061)
ExportTariff*EligShare		0.019		0.001
		(0.081)		(0.007)
Avg L/K	0.044	0.046	-0.007	-0.007
	(0.031)	(0.029)	(0.005)	(0.005)
Avg Exports	-0.025	-0.037	0.003	0.002
	(0.039)	(0.042)	(0.004)	(0.005)
HHI	-0.307**	-0.242*	-0.003	0.003
	(0.139)	(0.129)	(0.016)	(0.015)
R^2	0.53	0.54	0.65	0.65
N	2690	2677	2690	2677

NOTES: This table displays the effect of the policy change in 2004 on relative value added per worker and markup dispersions between industries that are treated depending on their exposure. The outcome in the first two columns is the standard deviation of log value added per worker within an industry-year. In the last two columns we replace the outcome with the standard deviation of log markups (using the DeLoecker and Warzynski (2012) method). The specifications follow those from Table 7. Firms within an industry are eligible if they export and are not “processors”. All controls are reported in the table, except a trend interaction with the eligibility dummy coefficients. The interaction of input tariffs with rebate exposure is dropped due to collinearity. All specifications include industry and year fixed effects. Standard errors (in parenthesis) are clustered at the 4-digit industry level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A6
2004 Policy Effect Across Industries: 90th relative to 10th Percentile of TFPR and Value Added per Worker

	90-10 Difference TFP		90-10 Difference VA/Worker	
	(1)	(2)	(3)	(4)
$\geq$ 2004*HighShare	0.024		0.097*	
	(0.020)		(0.055)	
$\geq$ 2004*EligShare		0.152*		0.489*
		(0.088)		(0.253)
Tariff*HighShare	-0.107			
	(0.136)			
ExportTariff*HighShare	0.029			
	(0.024)			
Tariff*EligShare		-1.197**		-0.374
		(0.530)		(1.342)
ExportTariff*EligShare		-0.043		0.071
		(0.100)		(0.179)
Avg L/K	0.044*	0.047*	0.086	0.114*
	(0.025)	(0.026)	(0.061)	(0.066)
Avg Exports	0.030	0.017	0.099	0.029
	(0.041)	(0.041)	(0.108)	(0.108)
HHI	-0.638***	-0.454***	-1.947***	-1.626***
	(0.218)	(0.173)	(0.401)	(0.405)
R^2	0.96	0.96	0.54	0.53
N	2698	2684	3016	2684

NOTES: This table displays the effect of the policy change in 2004 on relative dispersions post-2004 relative to the pre-period. In this Table we compute dispersion using the difference of the 90th percentile firm with the 10th percentile firm in the industry. In the first two columns, the dispersion is compute for revenue TFP. In the last two columns we compute the dispersion of log value added per worker. The specifications follow those from Table 7. Firms within an industry are eligible if they export and are not "processors". All controls are reported in the table, except a trend interaction with the eligibility dummy coefficients. The interaction of input tariffs with rebate exposure is dropped due to collinearity. All specifications include industry and year fixed effects. Standard errors (in parenthesis) are clustered at the 4-digit industry level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.