Exporting and Investment under Credit Constraints*

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Abstract

We examine the relationship between firm performance and export entry credit constraints. The existing research assumes that variation in firm financial conditions identifies credit constraints. A critical assumption is that financial conditions do not affect real outcomes (performance, exporting, or investment). To relax this assumption, we focus on the direct effect of firm fundamentals and financial conditions on firm performance. This approach distinguishes between firms who choose not to export because it is unprofitable from firms that do not export because of binding credit constraints. Our empirical specification allows firm characteristics to enter both the selection into exporting and return from exporting regressions. The leverage response heterogeneity identifies the presence of credit constraints. Using administrative Canadian firm-level data, our findings show that new exporters (a) increase their productivity, (b) raise their leverage ratio and (c) increase investment. We estimate that 48 percent of Canadian manufacturers face binding credit constraints when deciding whether to enter export markets. Alleviating these constraints would increase aggregate productivity by 0.97-1.04 percentage points.

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1 Introduction

Are firms credit constrained from entering export markets? If so, do financial market imperfections limit the gains from trade?

This paper provides a new approach to the identification of credit constraints and quantification of the gains to exporting, through policy reform. Leveraging heterogeneous returns to exporting (e.g., productivity growth), we uncover the set of firms for which credit constraints bind. Specifically, we first estimate the returns to exporting across heterogeneous firms under a model of exporting and the constraints faced by the firm. Firm financial conditions are flexibly included in the equations governing both the decision to export and the return to exporting. We argue that if credit constraints bind for a subset of firms, we should expect that positive selection on unobservables would systematically increase estimated marginal returns to exporting *among constrained firms* but less so among unconstrained firms. Intuitively, the decision to export among unconstrained producers depends only on the opportunity to enter into markets. Constrained producers, in contrast, are those firms that are potentially profitable exporters but cannot access sufficient financing. Removing credit barriers would potentially induce entry among constrained agents, thus lowering the marginal return to exporting among constrained firms. Employing the Marginal Policy Relevant Treatment Effect (PRTE) proposed in Carneiro, Heckman, and Vytlacil (2010), we identify the subset of firms for which improved financial conditions reduce the estimated return to exporting. We find that the credit constraints are binding for nearly one half of incumbent Canadian non-exporters in respect to the decision to enter export markets.

While novel, our approach hinges on three important assumptions. First, we assume that constrained agents face different costs than otherwise similar unconstrained agents. Second, we assume that we can observe a variable (or set of variables) that is (are) correlated with financing costs. For instance, among firms that are considering whether or not to export, we assume that we cannot *ex ante* distinguish which firms are constrained, but we can determine what underlying variables affect firms' access to credit markets. Third, we assume that a sufficient number of firms are unconstrained and, likewise, a sufficient fraction is constrained. When this last assumption is violated, our approach will only allow us to say that all firms appear to operate under the same set of constraints but will not provide evidence whether any particular constraint binds. Under the above assumptions, the differences in marginal returns let the data speak to *whether* there are credit constraints among a group of otherwise similar firms and for *what fraction* of agents they are binding.

Our work contributes to two large and rapidly developing areas of empirical and quantitative research in international trade. The first branch of this literature employs well-established reduced-form methods to investigate the degree to which firms with poor financial conditions are less likely to enter export markets and, if so, whether they experience weaker firm performance thereafter. Evidence from a wide set of countries confirms that manufacturers with poor financial conditions are less likely to enter export markets, export fewer products, export to fewer destinations, or have lower export sales (Muûls (2015), Berman and Héricourt (2010), Bellone, Musso, Nesta, and Schiavo (2010), Manova, Wei, and Zhang (2015), Amiti and Weinstein (2011), Paravisini, Rappoport, Schnabl, and Wolfenzon

(2015)).[1] In contrast, Greenaway, Guariglia, and Kneller (2007) find little evidence that differences in financial conditions affect the propensity to enter export markets or firm performance in the U.K. Fundamentally, each of the above studies employs some form of a common identification assumption: if measures of firm-level financial health hold explanatory power conditional on relevant variables, then at least some fraction of firms are credit constrained.[2]

While this assumption is plausible, our approach cautions that this strong assumption potentially confuses firms that are constrained from exporting with those for which exporting is not profitable. For example, consider two firms that are identical in every respect but for their *ex-ante* financial conditions. A firm with a healthy financial balance sheet may plausibly secure inexpensive financing and thereby meet any upfront expenditures needed to enter export markets. Inexpensive financing also raises the gross return to exporting. For the unhealthy firm that chooses not to export, it is unclear whether the first constraint binds (insufficiently securing the financing needed to enter export markets), whether the second constraint binds (meeting a minimum profitability threshold), or both. Interpreting the above literature as identifying firms that are credit constrained on international markets requires assuming that the variable capturing financial conditions has no impact on the profitability of exporting (inclusive of the cost of loan repayment). Our approach to identifying credit constrained firms does not rely on this assumption and, as such, we need not assume that any financial characteristic affects access to credit but not the return to exporting itself.[3]

The second main branch of literature that we directly address posits highly stylized models of constrained firms. Using the model's structure, equilibrium outcomes can be disciplined by data and should reflect information about the agents' optimal decisions under the given set of constraints (see Caggese and Cunat (2013), Dovis and Brooks (2011), Kohn, Leibovici, and Szkup (2016), Leibovici (2020)).[4] For example, we might expect that a model of credit-constrained exporters should replicate both export patterns and the distribution of debt-to-asset ratios across exporting and non-exporting firms.[5] The fundamental drawback of this approach is that the quantitative implications of these models typically depend critically on the underlying parametric assumptions.[6] This is particularly salient in

[1]Collectively, the above citations cover the following countries: Bangladesh, Belgium, China, France, Indonesia, Japan, Morocco, Peru, Philippines, South Africa, Thailand, and Vietnam.

[2]Despite its wide use, in practice, this type of exclusion restriction is not generally verifiable.

[3]An adjacent literature studies whether firms self-report as credit constrained. For example, Minetti and Zhu (2011) study a survey of Italian producers that are asked whether they would have liked to obtain more credit at market interest rates. We applaud these pioneering studies that directly attempt to distinguish credit constraints from profitability constraints. However, they are likewise limited in that (i) credit constraints may bind even among firms that do not attempt to secure external finance, (ii) questionnaires rarely distinguish the purpose of additional finance, (iii) results depend on the ability of firms to accurately describe whether they are credit constrained, (iv) this approach cannot be broadly applied since this type of data is exceptionally rare.

[4]A similar approach is posited in Hennessy and Whited (2005), Miao (2005), Rossi-Hansberg and Wright (2007), Buero, Kaboski, and Shin (2011), Kaboski and Townsend (2011), Arellano, Bai, and Zhang (2012), Buero, Kaboski, and Shin (2013), Bond, Tybout, and Utar (2015), and Catherine, Chaney, Huang, Sraer, and Thesmar (2022), among others.

[5]There is a large literature that investigates a firm's financing decisions. See Rajan and Zingales (1995), Hovakimian, Hovakimian, and Tehranian (2004), Hovakimian (2004), Leary and Roberts (2005) and Huynh, Paligorova, and Petrunia (2018), who suggest firms aim for a target leverage ratio in their debt versus equity financing decisions.

[6]For example, Catherine, Chaney, Huang, Sraer, and Thesmar (2022) develop a model of investment and collateral constraints that successfully matches key features of the firm-level distribution of financial outcomes, among other empirical moments. As in numerous preceding studies, the robustness of the preferred specification is tested against six model alternatives, which share many structural features. Our approach complements this standard approach by providing a

this context since credit constraints are never directly observed and thus cannot be externally validated.

We likewise build on the literature that quantifies the potential gains from policy reform. As in Catherine, Chaney, Huang, Sraer, and Thesmar (2022), we quantify the impact of policy reform on aggregate productivity growth, but here we focus on gains mediated through entry into export markets. As in the preceding trade literature, we find that exporting causes firm productivity to improve but extend this line of inquiry by characterizing the degree to which realized gains are influenced by both trade-specific costs (e.g., tariffs) and also unobserved, non-trade barriers to entry (e.g., access to finance). We find minimal evidence that trade and financial reform are strong complements. Rather, for Canadian manufacturers trade and financial reform are roughly independent. While either form of liberalization causes an increase in aggregate productivity (0.84 percentage points for trade and 1.04 percentage points for financial reform over the 2000–2010 period), the productivity growth achieved through joint reform (trade and financial reform combined) is approximately the sum of the two individual reforms.

This finding stands in stark contrast to existing research where trade and financial policy are often bundled together. Identifying the individual impact of a specific policy event (e.g., trade liberalization) can be difficult to isolate in the presence of multi-dimensional policy reform (e.g., trade and financial liberalization). Indeed, a number of recent contributions have carefully investigated the degree of complementarity between trade and financial reform in industrial trade flows (Manova (2008)), why trade is more sensitive to financial market imperfections than domestic production (Ahn (2011), Feenstra, Li, and Yu (2014)), or the extent to which access to external finance or trade credit can compensate for weak financial institution (see Manova, Wei, and Zhang (2015) and Antràs and Foley (2015)).[7] Much of our knowledge relies on cross-firm, cross-country, and cross-industry comparisons that only hold under the strong assumption that differences in ownership structure, industry affiliation or country location do not directly affect differential responses other than through credit access. However, as we document below, a) firm-level financial conditions vary to a much greater degree *within* industries than *across* industries in Canada and (b) measures of firm efficiency are generally insufficient to rationalize within-industry differences. Moreover, this approach also has a limiting qualification; in general, it does not distinguish whether the estimated differential response in exporting or trade flows are *due to unobserved differences in credit constraints* or characterize *whether those constraints are binding*. Incorporating treatment heterogeneity into our empirical work allows us to address both of these features under a minimal set of behavioural assumptions.

Within the larger trade and productivity branches of literature, our work updates the empirical relationship between exporting, financial development and/or productivity growth, as in studies of learning-by-exporting,[8] to account for differences in firm-level financial conditions and to characterize

measure of potentially constrained firms using a non-parametric estimation approach.

[7] Related papers studying the impact of credit access of firm or household outcomes include Rajan and Zingales (1995), Booth, Aivazian, Demirguc-Kunt, and Maksimovic (2001), Ahlin and Townsend (2007), Huynh and Petrunia (2010), Huynh, Petrunia, and Voia (2010), Poncet, Steingress, and Vandenbussche (2010), Kaboski and Townsend (2011) and Breza and Kinnan (2018), among others.

[8] See Clerides, Lach, and Tybout (1998), Bernard and Bradford Jensen (1999), Baldwin and Gu (2003), Aw, Roberts, and Winston (2007), De Loecker (2007), Atkeson and Burstein (2010), Lileeva and Trefler (2010), and Aw, Roberts, and Xu (2011).

the degree to which they influence firm responses to policy reform. We estimate that relaxing financial constraints will increase productivity among existing non-exporters induced to export through financial reform by 15 percent over a 10-year period, or roughly 2 percent per year on average. Likewise, this paper is also related to studies of the interaction of financial development and trade,[9] financial development and productivity growth,[10] firm dynamics in export markets,[11] and the impact of financial frictions on firm/industry dynamics.[12] Our estimates indicate that exporting caused firm-level efficiency to grow by at least an additional 35 percent among new Canadian exporters between 2000 and 2010.[13] We likewise document that the estimated productivity gains are mediated through increases in both firm leverage and investment, confirming that access to credit is a key determinant of firm-level productivity growth.

More broadly our paper relates to existing studies of constrained agents (e.g., students) in their decision to invest (e.g., go to college) and improve individual welfare (and wages). Indeed, our structure is very similar to those proposed in Carneiro and Heckman (2002) and Cameron and Taber (2004). In each of these cases, credit constraints are identified from differences in behaviour across groups with differential access to short-term financing (e.g., family income) or differential cost shocks (e.g., access to a local college) that must be financed immediately but have less impact on the long-term value of investment. Our approach does not require the presence of information that can plausibly exclude from the long-run value of investment tomorrow but affect the short-term decision to engage in investment today. Instead, we leverage intuition from differential returns to investment, as highlighted by Cameron and Taber (2004). By applying the marginal treatment framework[14] and identifying policy-relevant treatment effects for marginal producers, as in Zhou and Xie (2019), we employ the estimated heterogeneity in the returns to investment to identify the number of producers who are potentially credit constrained from entering export markets.[15]

We outline the costs and benefits in the remainder of our paper. The next section introduces data from the Canadian manufacturing sector and establishes the strong correlation between exporting, firm financial conditions and firm performance. Section 2 outlines a highly stylized model of exporting and

[9] See Kletzer and Bardhan (1987), Beck (2002), Matsuyama (2005), Becker, Chen, and Greenberg (2013), Braun and Raddatz (2008), Do and Levchenko (2007), Ju and Wei (2008).

[10] Levine (1997), Levine and Zervos (1998), Neusser and Kugler (1998), Rousseau (1998), Zingales (1998), Rousseau and Wachtel (2000), Benhabib and Spiegel (2000), Arellano, Bai, and Zhang (2012), Levine and Warusawitharana (2021).

[11] De Loecker (2007), Melitz, Helpman, Marin, Verdier, and Costantini (2007), Atkeson and Burstein (2010), Aw, Roberts, and Xu (2011), Bustos (2011), De Loecker (2011), Bastos, Dias, and Timoshenko (2018), Berman, Rebeyrol, and Vicard (2019), Garcia-Marin and Voigtländer (2019).

[12] Cooley and Quadrini (2001), Albuquerque and Hopenhayn (2004), and Miao (2005) provide theoretical models suggesting that financial friction matters to the growth and survival prospects of firms. Empirical studies by Huynh and Petrunia (2010), Huynh, Petrunia, and Voia (2010) and Huynh, Jacho-Chávez, Petrunia, and Voia (2011) find that a firm's debt-to-asset ratio impacts its future growth and survival prospects. Petrunia (2007) provides empirical evidence that a firm's financial position has long-term impacts. Catherine, Chaney, Huang, Sraer, and Thesmar (2022) quantify the aggregate effects of financing constraints.

[13] On average, we estimate an annual export premium that ranges between 3 and 4 percent per year.

[14] Heckman and Vytlacil (1999, 2005, 2007a,b) provide a discussion of marginal treatment effect.

[15] While it may be plausible that short-run cost shocks to college access would not necessarily influence the long-run wage premium, this logic does not clearly transfer to other settings. Indeed, should firms incur financing costs to undertake a new investment, this would not only influence the likelihood of investment but also the overall return from investment, conditional on having undertaken it. As such, financial cost shocks clearly cannot be excluded from either the decision to invest or the evaluation of the outcome.

investment under credit constraints to describe standard identification challenges and provide intuition for our empirical approach. Section 3 describes the Canadian export context and provides an outline of our benchmark data. Section 4 defines the marginal treatment effect and connects the key model insights with the econometric approach used to identify the set of firms that are potentially credit constrained from entering export markets. Section 5 documents our findings while section 6 discusses exporting, policy reform, and credit constraints. Finally, section 7 concludes the paper.

2 Model

We posit a simple, one-period model that marries the heterogeneous gains from the trade liberalization structure of Lileeva and Trefler (2010) with the exporting under credit constraints framework outlined in Manova (2013). To keep our exposition as simple as possible, we assign details to the appendix.

Consider a setting with two markets, home and foreign, and one sector. A continuum of heterogeneous firms produce differentiated goods in both countries and face a constant elasticity of substitution (CES) demand curve in each market: $q = p(\omega)^{-\sigma}A$ at home and $q^* = p^*(\omega)^{-\sigma}A^*$ abroad, where A and A^* are measures of domestic- and foreign-market size, respectively, ω is the firm index, $p(\omega)$ and $p(\omega)^*$ are the prices charged at home and abroad by firm ω, inclusive of trade and transport costs.

2.1 Production, Investment & Exporting

As in Melitz (2003), we assume firms incur a sunk cost to enter the market and recover an initial Hicks-neutral productivity level, $\varphi_0'(\omega)$, from the distribution $G(\varphi)$. Successful entrants then employ debt and equity markets to purchase collaterizable assets, K. We allow annual debt service liabilities, δ, to vary across firms and reflect initial differences in the cost of raising start-up funds.[16] A firm with productivity level φ_0' produces according to the production function $q = \varphi_0' l$, where l represents a sector-specific input bundle that costs c. We assume that there are no fixed costs associated with serving the domestic market and that all domestic market activities can be financed with cash flows from current sales. We normalize the input cost to $c = 1$, work with the following transformation of productivity, $\varphi \equiv (\sigma-1)^{\sigma-1}\sigma^{-\sigma}(\varphi')^{\sigma-1}$, and suppress the variety index ω.

Let $\pi(\varphi, E, I)$ denote the profits earned by firms with different export (E) and investment (I) decisions. Let each of these variables take the value one if the firm engages in the activity and zero otherwise. The net operating profits earned by a firm that does not export or invest can be written as:

$$\pi(\varphi_0, 0, 0) = \varphi_0 A - \delta. \tag{1}$$

As in Lileeva and Trefler (2010), firms can potentially increase profits by investing, exporting or en-

[16] For simplicity, we do not explicitly model the determination of initial productivity or debt holdings here but take their existence as given. Extending Melitz (2003) and Manova (2013) to generate the above heterogeneity in initial debt-asset ratios is straightforward; the authors are glad to provide an example upon request. Likewise, we assume that the capital necessary for production is identical across firms due to technological requirements. Allowing for differences in capital holdings, among other dimensions of firm heterogeneity, would not change the analysis below and is omitted only for expositional clarity. These assumptions are not imposed on the empirical work that follows.

gaging in both activities.

Specifically, firms can potentially increase their productivity to $\varphi_1 > \varphi_0$ by incurring a fixed investment cost, F^I.[17] We assume that the potential productivity gain φ_1 is also firm-specific and known to the firm but only realized with probability p_I. Analogous to the financial constraints in Manova (2013), firms need to raise outside capital for a fraction, $D(\delta/K)$, $0 < D(\cdot) < 1$, of the fixed investment cost. Unlike Manova (2008), we allow the firm's reliance on external finance to vary with the firm's initial debt-asset ratio: producers have to borrow DF^I to increase productivity. In general, we follow the literature, which suggests that there is an optimal ratio of debt to assets for individual firms. For example, firms with a high debt-to-asset ratio, $\delta/K > \overline{\delta/K}$, may have more room to finance future investment by issuing equity and will not be as reliant on external finance.[18] The profitability of investment will depend on the value of D and on the nature of the lending contract, as outlined below.

To enter export markets, each producer must pay a fixed export cost, F^E. There are also additional variable trade costs that are modeled as an iceberg trade cost so that $T > 1$ units of a product need to be shipped for 1 unit to arrive in the destination market. Firms face credit constraints in financing the costs associated with international trade. We follow the Manova (2013) construction of financing needs and financial contracting, though this structure is not essential to our empirical work. We proceed with the simplest case where all variable trade costs can be financed internally and the fraction of fixed export costs, which require external finance, D^X, is the same as the fraction associated with the fixed investment cost, $D^X = D$.[19] The financing needs of an exporter who does not invest in raising productivity are DF^E, while the financing needs of an exporter who invests and exports are $D(F^E + F^I)$.

2.2 Borrowing & Lending

In the model, there are a continuum of identical investors interested in financing firm projects. Firms can pledge tangible assets as collateral. Financial contracting begins with each firm making a take-it-or-leave-it offer to a potential investor. The contract specifies the amount of funds borrowed, the repayment $\rho(\varphi, \delta, E, I)$ if the contract is enforced, and the collateral the investor can claim if the firm defaults. Revenues are realized after production, and the investor receives payments at the end of the period. Investors (rationally) expect to be repaid with the probability $1 - p_D$ where p_D captures the endogenous probability of defaulting. When the firm defaults and the creditor claims the collateral K,

[17] We restrict attention to a simple model where investment costs do not vary across firms and the firm cannot increase expected productivity gains by incurring greater investment. The appendix extends the model to this more complex setting and demonstrates that the primarily lessons from the model are preserved even when we allow for endogenous investment size.

[18] See Miao (2005) for a detailed exposition. To the extent that lower debt-equity ratios indicate that the firm is overcapitalized (and thus has outsized shareholder payments), we might expect that D is an decreasing function of the debt-to-asset ratio. In contrast, if lower debt-to-asset ratios are indicative of better terms for existing debt service, the opposite may be true. For our purposes it is not essential which interpretation is correct. What is essential is that there is variation in the observed debt-to-asset ratio and that it affects future borrowing conditions.

[19] This assumption does not affect the qualitative predictions of the model. The underlying assumption is that firms cannot use profits from past periods to finance future operations, for example, because they have to distribute all profits to shareholders due to principal-agent problems. Alternatively, D is the fraction of outlay that needs to be financed externally after all retained earnings have been used up.

the firm absconds with its realized profit. The key feature of this framework is that ex-ante differences in existing financial conditions create differential access to credit markets.

Let B represent the investor's profits and normalize the returns they can earn outside of the model to r.[20] The investor's participation constraint is written as

$$B \equiv -D(EF^E + IF^I) + (1 - p_D)\rho(\bar{\varphi}(I), \delta, E, I) + p_D rK \geq rD(EF^E + IF^I), \tag{2}$$

where $\bar{\varphi}(I)$ denotes the firm's expected productivity given its investment decision. In the presence of competitive credit markets, the repayment schedule brings the investor to their participation constraint.[21] We write the repayment schedule as

$$\rho(\bar{\varphi}(I), \delta, E, I) = \frac{(1 + r)D(EF^E + IF^I) - p_D rK}{1 - p_D}. \tag{3}$$

The most a firm can promise to repay is the firm's net profits in the current period. To borrow funds for exporting and/or investment, firms must satisfy the credit constraint associated with their decisions: the expected profits must be at least as large as the repayment to the investor,

$$\mathbf{E}[\pi(\varphi, E, I)] \geq \rho(\bar{\varphi}(I), \delta, E, I), \tag{4}$$

where the expectation operator accounts for the uncertainty of the investment's success.[22]

There are four important features of equations (2)–(4) that merit comment. First, firms with greater financing needs (higher D) will have to promise larger repayments in order to satisfy the investor's participation constraint. Second, higher default probabilities and greater reliance on external finance increase the price of obtaining credit. We write the effective price of credit, q, as

$$q(\varphi_0, \varphi_1, E, I) = \frac{(1 + r)D(EF_E + IF_I) - p_D rK}{(1 - p_D)D(EF_E + IF_I)} - 1. \tag{5}$$

Letting $\mathcal{D}$ represent the need for the external finance to assets ratio, $\mathcal{D} = D(EF_E + IF_I)/K$, it is straightforward to show that[23]

$$\frac{\partial q}{\partial p_D} > 0 \text{ if } D(EF_E + IF_I) > K \text{ and } \frac{\partial q}{\partial \mathcal{D}} > 0.$$

Third, more productive firms will expect higher revenues, will be better able to meet the repayment requirements and, as such, will be better able to secure external finance. Lastly, firms that invest and export face larger financing needs than those that only export. Firms with steeper financing needs will require higher returns from investment in order to meet the credit constraint, all things being equal.

[20] As discussed at length in Manova (2013), this assumption is without loss of generality.

[21] We assume the firm can commit to investing in productivity improvement.

[22] We could likewise allow for uncertainty in export payoffs. Allowing for this additional source of uncertainty has no effect on the model's implications for our purposes.

[23] The restriction $D(EF_E + IF_I) > K$ is the only interesting case since, if the opposite were true, the bank would always receive full repayment regardless of the state of the world. Further, if the probability of default is zero p_D, then $q = r$.

2.3 The Decision to Export and Invest

Our primary interest lies in characterizing productivity gains driven by exporting, and we focus on the firm's decision to jointly export and invest.[24] The nature of the firm's problem is succinctly characterized by the threshold condition required to induce a non-exporting, non-investing firm to take on both exporting and investment:

$$\mathbf{E}[\pi(\varphi,1,1)] - (1-p_D)[\rho(\varphi_0,\delta,1,1) - rK] - [\pi(\varphi_0,0,0) + rK] = 0. \tag{6}$$

Because the lending contract appears directly in threshold condition (6), the decision to export is a function of all of the firm's underlying characteristics including the terms of its lending contract. Naturally, unfavorable lending terms will also reduce the potential gains from exporting and investment.

The firm's optimal exporting and investment choices are illustrated in Figure 1. This figure plots initial productivity, φ_0, against the expected productivity gains per dollar of investment, $\frac{E[\Delta\varphi]}{\chi(D,E,I)}$, where $E[\Delta\varphi] = p_I(\varphi_1 - \varphi_0)$ and $\chi(D,E,I) = (1+rD)(EF_E + IF_I)$. Intuitively, when the expected productivity gains are small the firm never invests, and the firm only exports if its initial productivity is sufficiently large, as in Melitz (2003). In Figure 1 this occurs when initial productivity is sufficiently large, $\varphi_0 > \frac{\chi(D,1,0)}{T^{-\sigma}A^*}$, but the expected productivity growth from investment is small, $\frac{E[\Delta\varphi]}{\chi(D,0,1)} < \frac{1}{A+T^{-\sigma}A^*}$. A subtle difference with Lileeva and Trefler (2010) is that each threshold depends on both productivity and the need for external finance. As the need for external finance grows, either due to larger investments (F_E, F_I) or poorer financial conditions (D), the expected productivity growth must also rise to justify exporting and/or investment.

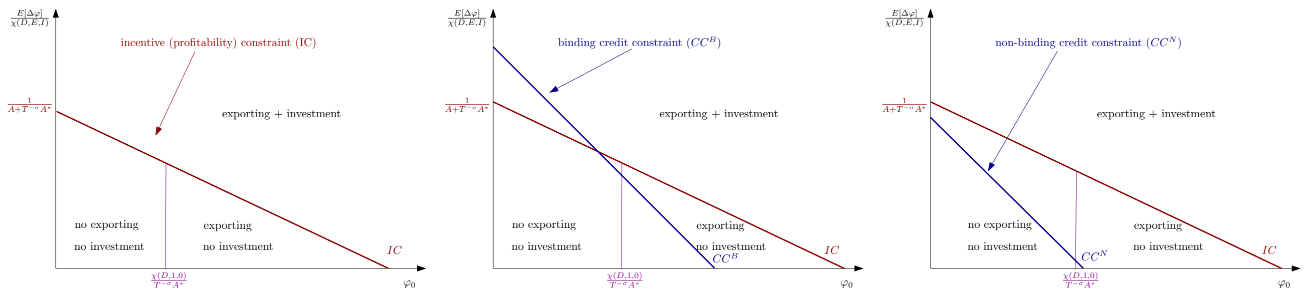
When the expected productivity gains from investment are sufficiently large the firm will want to export (and vice-versa, the firm will want to invest but only if it can spread fixed costs over greater export-induced sales). Among initial non-exporters, this decision is determined by whether the expected gains from exporting and investment are larger than the minimum gain needed to justify investment, $\Delta\varphi^{EI}$:

$$\frac{\mathbf{E}[\Delta\varphi^{EI}]}{\chi(D,1,1)} = \frac{1}{A+T^{-\sigma}A^*} - \left(\frac{T^{-\sigma}A^*}{A+T^{-\sigma}A^*}\right)\frac{\varphi_0}{\chi(D,1,1)}. \tag{7}$$

Intuitively, this locus is downward sloping since firms that are initially more productive will require smaller gains to meet the export and investment threshold.

[24] Model details, a discussion of the individual decisions to export or invest alone, and an extension to multiple investment levels can be found in the appendix.

Figure 1: Optimal Exporting and Investment



(a) Incentive Constraint (b) Binding Credit Constraint (c) Non-Binding Credit Constraint

Notes: The IC curve captures the incentive constraint, while the CC^B represents binding credit constraints among financially unhealthy (healthy) firms.

Panel (a) is similar to the two-dimensional problem in Lileeva and Trefler (2010). The only difference is that the expected return to exporting per dollar of investment, $\mathbf{E}[\Delta\varphi/\chi(D,E,I)]$, depends on both productivity and financial conditions. In panel (b) we incorporate a binding credit constraint in the form of that from Manova (2013), that is, firms that are marginally able to secure financing must be at least able to meet the repayment conditions in expectation:

$$\mathbf{E}[\pi(\varphi,1,1)] - \rho(\bar{\varphi},\delta,1,1) \;=\; \bar{\varphi}[A + T^{-\sigma}A^*] - \delta + (1-D)(F_E + F_I) + rK - \rho(\bar{\varphi},\delta,1,1) = 0.$$

We again hold financial conditions fixed and plot the relationship between the firm's initial conditions and the expected gain required to convince the lender to provide the needed credit for exporting and investment, as illustrated by the CC^B locus. The CC^B locus is downward sloping since initially productive firms do not need large productivity gains to meet their lending obligations:

$$\frac{\mathbf{E}[\Delta\varphi^{CC}]}{\chi(D,1,1)} \;=\; \frac{1}{A+T^{-\sigma}A^*}\left(1 + \frac{\delta - rK/p_I}{\chi(D,1,1)} + \frac{D(1+r)(1-p_I)}{(1+rD)p_I}\right) - \frac{\varphi_0}{\chi(D,1,1)}. \tag{8}$$

Relative to the incentive constraint, (7), the credit constraint, (8), is steep since the lender does not care whether the firm's choice is optimal, only if the firm will be able to meet its repayment obligations.

Importantly, we refer to this as a *binding* credit constraint because for some subsets of (initially low productivity) firms, the expected productivity growth needed to satisfy the lender is greater than that needed to meet the firm's own profitability (incentive) constraint. Comparing equations (7) and (8) we observe that among initially unproductive firms this will generally be the case.[25] The credit constraint locus, CC^B, has a steeper slope than that of the incentive compatibility constraint, IC. While this feature is a direct implication of the model, the intuition is simple: the lender only cares if it is likely that the action taken (exporting and investment) will result in a sufficient payoff (profits) to meet the contractual obligations, not whether the action is better than the firm's next best option. Among firms with poorer financial conditions the slope of (8) will be relatively flat, increasing the share firms for which the credit constraint lies above the incentive constraint, *ceteris paribus*. This

[25] For instance, if $\delta > rK/p_I$, then it will surely be the case.

feature is of paramount importance to our work: all firms face a lending constraint that varies with firm characteristics and financial conditions. Firms are only credit constrained if the lending constraint is their most binding constraint.

The placement of the credit-constraint locus above the profitability constraint in Figure 2b is admittedly arbitrary. For instance, among firms with small amounts of debt, the credit constraint may lie uniformly below the profitability constraint, as depicted in Figure 2c. We refer to this as a *non-binding* credit constraint since, for all firms with this particular set of financial conditions, the profitability constraint is the only relevant determinant of export-driven productivity growth.

In sum, our simple framework captures four important features of constrained decision making: (i) financial conditions influence the required return from exporting through both the profitability and credit constraints, (ii) financial conditions influence whether a firm exports and the complementary investment, (iii) firms with better initial conditions face lower profitability and credit constraints, and (iv) even among firms with similar financial conditions, credit constraints are unlikely to bind equally across all firms. As we argue next, these features present particular identification challenges for empirical researchers but also suggest a novel approach for studying the effect of credit constraints on firm behaviour.

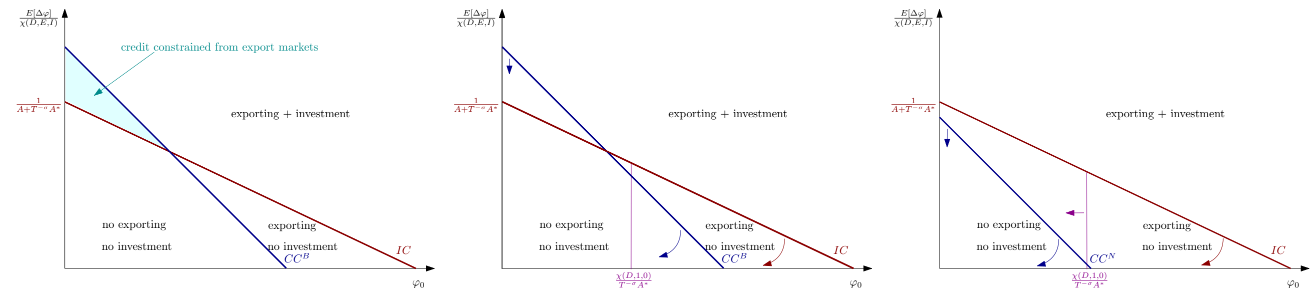
2.4 Identification Lessons

Before describing the empirical framework, we establish key lessons from our theoretical framework that guide our empirical work.

Lesson 1: Financial conditions cannot generally be used to identify credit constraints.

Despite the widespread use of financial conditions or financial reform to identify the presence of credit constraints, we argue that this common approach does not distinguish the presence of credit constraints from changing the return to investment itself. As illustrated in Figure 3a, only a subset of firms are credit constrained in the usual sense; we define credit-constrained firms as those that would choose to export in the absence of the credit constraint (or if the credit constraint was relaxed). These firms are captured by those that have sufficiently large productivity returns to meet the profitability threshold (the IC curve) but insufficiently large returns to meet the credit constraint (the CC^B curve). For example, in our simple model, improved financial conditions at the firm (e.g., a lower value of D/K) or market level (e.g., a lower value of r) would shift the CC^B curve downwards and allow some firms presently constrained from export markets due to credit frictions to export and invest.

Figure 2: Credit Constraints & Policy Reform



(a) Constrained Firms (b) Reform w/ Binding Constraint (c) Reform w/o Binding Constraint

Notes: The blue area represents firms that are credit constrained from exporting but would optimally prefer to export and make a small investment. The IC curve captures the incentive constraint, while the CC^B (CC^N) represents binding (non-binding) credit constraints among financially unhealthy (healthy) firms.

This does not suggest, however, that policy reform (e.g., financial reform) necessarily provides meaningful (exogenous) variation to identify credit constraints. Indeed, our simple model again suggests otherwise. Changes in firm financial conditions, lender reform, and shocks to the lender's outside options affect the credit constraint (CC^B, CC^N) and profitability constraint (IC). This feature is true both when credit constraints are binding (Figure 3b) and when they are not (Figure 3c).[26]

Meaningful correlation between policy reform and exporting is *always expected* but does not clearly indicate any particular notion of binding credit constraints. We would expect any study that employs variation-based firm/industry financial characteristics or financial reform to find significant correlation with standard firm outcomes (exporting, productivity growth, etc.). Moreover, classifying firms into treated (e.g., policy-exposed, external-finance dependent) and untreated (non-exposed) does not alleviate the identification problem: selection-on-unobservable gains in exporting should imply that the marginal response among likely exporters is inherently different than that from unlikely exporters for all measures of financial conditions and regardless of whether credit constraints bind.

Although the exact form of these equations are particular to our model, the problem they imply is not: regardless of whether the firm's credit constraint is binding, the marginal return from exporting and investment is a function of the firm's underlying financial characteristics, even in settings with competitive financial markets. As such, we would expect to find that financial characteristics are important determinants of realized productivity gains irrespective of whether the firm is in fact credit constrained. Although a large number of existing, reduced-form studies test for the existence of credit constraints by estimating whether agents with different *ex-ante* financial conditions respond differently to (exogenous) credit supply shocks or policy change,[27] our simple model suggests that this logic is

[26]As documented in the appendix, allowing for multiple investment options reinforces the notion that policy reform changes both the opportunities and constraints faced by the firm.

[27]A large number of papers, including the previous work of the authors, use a similar variation to test for the presence of financial frictions and/or credit constraints. While most papers acknowledge the importance of controlling for cost differences separately from financial characteristics, rarely do the impacts of financial shocks feature in the measurement of costs themselves. See, for example, Harrison and McMillan (2003), Manova (2008), Bellone, Musso, Nesta, and Schiavo (2010), Berman and Héricourt (2010), Huynh and Petrunia (2010), Poncet, Steingress, and Vandenbussche (2010), Amiti and Weinstein (2011), Manova (2013), Feenstra, Li, and Yu (2014), Antràs and Foley (2015), Manova, Wei, and Zhang

likely to often fail in practice.[28]

Lesson 2: Heterogeneous returns to exporting may identify credit constrained firms.

The intersection of the credit constraint and the profitability constraint represents the threshold where credit constraints no longer bind. This intersection defines a threshold productivity level, φ_{0I}^{CC}, which distinguishes firms that would potentially choose to invest if they could in fact secure a loan to do so:

$$\varphi_{0,I}^{CC} = \frac{\chi(D,1,I)}{A}\left(\frac{\delta - rK/p_I}{\chi(D,1,I)} + \frac{D(1+r)(1-p_I)}{(1+rD)p_I}\right). \tag{9}$$

The presence of threshold (9) implies that financial reform should have a differential impact on the marginal exporter and, as such, provides a strategy to potentially identify credit-constrained firms. Consider a set of initial non-exporters and suppose one had sufficient information to identify the locus of marginal exporters, as in Figure 2b. This locus would represent the upper envelope of the CC^B and IC curves up to export threshold (7), as outlined in Figure 3. Suppose further that we were also able to estimate the locus of marginal returns to exporting *under* financial reform, in the potential outcomes sense. In the absence of credit constraints, only the IC curve remains relevant to the firm's decision to export and the associated returns to exporting. Estimating the intersection of marginal returns to exporting – should it exist – before and after financial reform would potentially allow us to identify a set of firms that are credit constrained.[29] For the particular set of firms in Figure 3a, this intersection is captured by the initial productivity level, $\varphi_{0,I}^{CC}$.

Figures 3a–3c further highlight two implicit requirements for the credible identification of credit constraints. First, we need to be able to reasonably estimate the marginal return to exporting under the baseline policy and one in which credit constraints are less binding. Effectively, this is what stylized, quantitative models of credit constraints propose to do. By specifying a functional form for the interaction of lenders and firms and then targeting particular firm characteristics, such as the distribution of debt-asset holdings, quantitative models recover an equilibrium credit allocation mechanism.

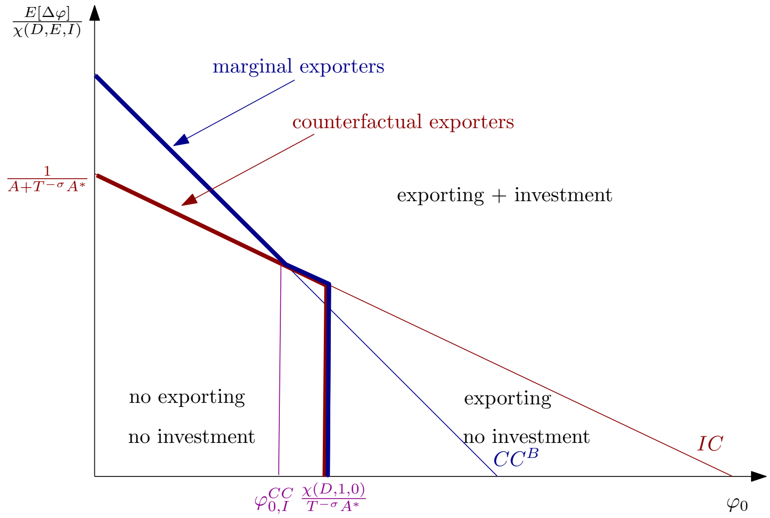
Second, credibly identifying constrained firms through counterfactual policy reform requires that at

(2015), Muûls (2015), Rho and Rodrigue (2015), Dao, Camelia, and Ostry (2017), Levine and Warusawitharana (2021), and Leibovici (2020). Important exceptions include Minetti and Zhu (2011) and Paravisini, Rappoport, Schnabl, and Wolfenzon (2015). Minetti and Zhu (2011) use a firm survey asking Italian firm managers if they would have preferred to obtain a larger amount of credit at market rates than they were able in order to identify "constrained" firms. They then estimate whether constrained firms are less likely to export conditional on other firm characteristics. Paravisini, Rappoport, Schnabl, and Wolfenzon (2015) exploit exceptionally rich Peruvian data, which allows the authors to separate the impact of an exogenous shock to financing costs on the firm's profitability conditions from their export entry decisions. They find that few firms are credit constrained from entering export markets, but exporting does increase the variable cost of exporting and, as such, reduce export sales. A similar argument is highlighted in Bond and Van Reenen (2007), where the authors argue that financial variables can be used to identify financial constraints in an investment model if and only if it is possible to directly control for the change in the cost of investment. In the absence of such controls, it is unclear how to distinguish credit constraints from model misspecification.

[28] Given that changes in the cost of investment – even the cost of acquiring information, for instance – simultaneously affect the return to investment, this is generally a difficult exclusion restriction to satisfy in practice.

[29] As we argue below, an empirical analog of each locus could be recovered by estimating the locus of marginal treatment effects (Heckman and Vytlacil (2007a,b)) and the locus of marginal policy relevant treatment effects (Carneiro, Heckman, and Vytlacil (2010), Zhou and Xie (2019)).

Figure 3: Heterogeneous Returns & Identification



Notes: The dark blue curve captures the locus of marginal (new) exporters under the baseline policy. The dark red curve delineates the locus of marginal exporters after policy reform. The intersection of both lines at $\varphi_{0,I}^{CC}$ distinguishes credit-constrained firms from those that are constrained by the profit (incentive compatibility) condition.

least some firms are unconstrained, while others are constrained. To identify the empirical equivalent of the intersection of the profitability and credit constraints, we need to be able to recover the marginal return when constraints bind and when they do not. Again, this is similar in spirit to the quantitative literature: counterfactually evaluating changes in firm borrowing allows quantitative structural models to recover the unobserved distribution of constrained firms. Our empirical approach is similar in spirit – we will want to counterfactually recover estimates of a set of constrained firms – but leverages inherent heterogeneity in the return to exporting to avoid the same set of strong functional form assumptions. We return to this issue in the description of our empirical approach but highlight the feature that if all firms are either constrained or unconstrained we are unlikely to be able to credibly identify the marginal returns under any counterfactual policy of interest.

Our simple model suggests that exploiting the heterogeneous-returns framework may provide an intuitive solution to the above challenges: comparing the estimated and counterfactual returns to exporting and the nature of credit constraints faced by the firm. What is less obvious is that these differential returns are *essential* to identification in this context. To clarify this aspect of our work, it is illustrative to consider a model without heterogeneity. For instance, suppose, as in Bustos (2011), that if a firm chooses to invest in new technology, its realized productivity grows by a constant proportion, $\gamma > 1$, such that $\varphi_1 = \gamma\varphi_0$ for all investing firms.

Given this restriction, we reconsider the incentive compatibility and credit constraints above. When we impose the restriction $\varphi_1 = \gamma\varphi_0$, the incentive compatibility constraint (7) reduces to

$$\varphi_{0,I}^{IC} = \frac{\chi(D,E,I)}{(\gamma-1)p_I(A+T^{-\sigma}A^*)+T^{-\sigma}A^*}, \tag{10}$$

where φ_0^{IC} denotes the marginal exporter's productivity. The lack of heterogeneous responses implies that the incentive compatibility constraint is a single threshold productivity level for exporting for every value of D, rather than a function of initial productivity as defined in equation (7).

In a similar fashion, applying $\varphi_1 = \gamma\varphi_0$ to equation (8) yields a single credit-constraint threshold for every value of D and each investment choice:

$$\varphi_{0,I}^{CC} = \chi(D,E,I)\left[\frac{1 + \frac{\delta - rK/p_I}{\chi(D,E,I)} + \frac{D(1+r)(1-p_I)}{(1+rD)p_I}}{(\gamma-1)p_I(A + T^{-\sigma}A^*) + A + T^{-\sigma}A^*}\right]. \tag{11}$$

Comparing equations (10) and (11), we observe that it is not possible to unequivocally determine which condition, (10) or (11), binds, for any given value of D.[30] This has a stark implication for identification: given D, credit constraints always bind for all marginal exporters or none of them.

3 Data

3.1 T2-ASM Database

This research uses the T2-ASM database, which is a unique firm- and plant-level database of manufacturers created and maintained by Statistics Canada. The T2-ASM database has two sources: (i) the General Index of Financial Information-Corporate Tax Return File (T2) and (ii) the annual survey of manufacturers (ASM). The T2-ASM database merges information from these two sources for the period 2000–2010. The T2 tax files provide tax and balance sheet information on Canadian firms, while the ASM provides production information on Canadian manufacturing plants. The T2 database tracks all incorporated firms that file a T2 form with Revenue Canada at the five-digit NAICS industry level. This database is used to assess firm-specific, annual financial variables such as profit, total debt, short-term debt, long-term debt, equity, total assets, current assets, capital assets, sales and location. Measured by either output or employment, the GIFI-T2 database includes the universe of Canadian firms. The firm composition includes both publicly traded and privately held firms (the latter group forms a larger portion of the Canadian business sector (see Huynh, Paligorova, and Petrunia (2018)). The ASM database provides production information for Canadian manufacturing plants, such as the value of shipments, value-added of shipments, productivity, intermediate inputs and employment. The T2-ASM database contains the necessary information (shipments and financial variables) to allow us to empirically consider the exports and investment of manufacturing firms in the context of financial constraints.

One consideration is the level of aggregation. Although most manufacturers operate in one location, there is not necessarily a one-to-one match between the two databases. The ASM contains plant/establishment-level information, while the T2 contains firm/enterprise-level information. For a multi-plant manufacturing firm, each of its individual plants has unique production information taken from the ASM. However, the corresponding financial information is identical for each of the firm's plants since the T2 tax file is unique to a firm. This paper works with the plant-level aggregation of

[30] Specifically, both the numerator and denominator in equation (10) are smaller than their counterparts in equation (11).

variables, so that financial variables are identical for each plant within a firm. Combining the T2 and ASM datasets allows us to consider the impact of firm-level financial variables on plant-level operations. This database contains the universe of manufacturing plants in Canada whose firm files a tax return.

Some discussion is necessary with respect to calculated variables and the sample of plants. First, we impute the following variables at the firm-level and assign them to their corresponding plant-level observations. Leverage ratio (lev), one measure of a firm's financial state, is the ratio of total debt to total assets, or the debt-to-asset ratio, while short-term leverage, a second measure of a firm's financial state, is the ratio of short-term debt to total assets. The ratio of tangible assets to total assets measures the tangibility of a firm. Next, we impute the following using plant-level observations. Measures of productivity include total factor productivity (TFP) and labour productivity. Following Tomlin (2014), we implement well-known control function approaches to measure total factor productivity (Olley and Pakes (1996), Levinsohn and Petrin (2003), Ackerberg, Caves, and Frazer (2006), and Wooldridge (2009), among others) for our data sample. Labour productivity is measured as value added per worker, where a plant's employment is the sum of production workers and salaried employees. A plant's total shipments is a measure of firm-size. The sample of plants includes all non-exporting plants in 2000 that survived until 2010, which captures the beginning and ending years of the dataset. The focus of the empirical analysis is a comparison of outcomes between non-exporting plants in 2000 that started exporting by 2010 (i.e., new exporters) and those plants that remained non-exporters in 2010.[31]

3.2 Tariff rates

To estimate causal effects, we follow an instrumental variable approach relying on industry-specific tariff rates that the U.S. applies to its trading partners. Variation in U.S. trade policy changes the degree of competition that Canadian producers face from foreign firms and their willingness to enter Canada's main export market. At the same time, there were no changes in U.S. trade policy towards Canada as the North American Free Trade Agreement (NAFTA) was still in place during our sample period.

To calculate U.S. industry-specific tariff rates, we assemble annual data from the U.S. Census on U.S. bilateral imports on each of its trade partners and the corresponding applied tariff rates, by HS 10 product code, for the years 2000 and 2010. We match the 10-digit HS codes in the trade data to 5-digit NAICS codes (the classification of our plant-level data) using the corresponding table from Pierce and Schott (2012). To aggregate the 10-digit HS tariff rates to 5-digit NAICS codes, we use import values for the year 2000 for both years in our sample.[32] We then take a trade-weighted average across trading partners to obtain 5-digit NAICS industry-specific tariff rates.

[31] For the start date of the sample, we choose the year 2000 because it is the first year where we observe financial information at the firm level. For the end year, we follow Lileeva and Trefler (2010) and focus on a 10-year period. We experiment with using a 7-year window (ending in 2007) and find that the results are robust (see Appendix).

[32] 5-digit NAICS codes are the lowest level of aggregation of industry codes that are harmonized between the U.S. and Canada. Using the beginning-of-period values for aggregation addresses the concern of endogenous changes in trade due to tariff changes in 2010.

3.3 Summary Statistics

Tables 1 and 2 provide summary statistics. The exporter sub-sample includes 2,707 plants that started exporting between 2000 and 2010. The non-exporter sub-sample contains 7,660 plants that did not export in 2000 and continued not to export in 2010. Table 1 provides summary statistics for these two groups of firms and compares their characteristics in 2000. The comparison indicates that plants that start exporting tend to be larger, have higher TFP and labour productivity, are older, and have lower leverage ratios relative to non-exporting plants. In all comparisons, these differences are statistically significant at the 1 percent level for the leverage, productivity and size variables, and at the 5 percent level for age.

Table 2 documents the growth rate of each variable between 2000 and 2010 as calculated as the log difference between 2010 and 2000. This table compares differences in dynamics over the 2000–2010 period in the states of the two samples of plants. For both groups, Table 2 indicates that leverage ratios and short-term leverage ratios fell (negative growth on average) and that these declines were relatively large among non-exporters. In contrast, TFP, labour productivity, tangible assets, and tangibility rose (positive growth on average) for both groups, with new exporters experiencing larger average growth values for TFP, labour productivity and tangible assets, and non-exporters having larger average growth values for the tangibility variable. These differences between the two groups are statistically significant at the 1 percent level for the growth rates of labour productivity and tangible assets. Table 2 also adds the summary statistics for the tariff changes for non-exporters and new exporters. In sectors with a large fraction of non-exporters, the average U.S. tariff rate declined by 0.4 percentage points, whereas in the sectors with a disproportionate number of new exporters, the tariff rate fell by 0.2 percentage points. Between 2000 and 2010, the change in average U.S. tariff rates vis-à-vis the rest of the world was significantly higher in sectors with a larger number of non-exporters relative to sectors where exporting was common. A t-test reveals that the difference is statistically significant at the 1 percent level. This suggests that competitive pressure from foreign firms was significantly lower in sectors where new Canadian exporters operated. Below, we formally test whether the lower tariff rates had a significant impact on the likelihood of firms entering export markets.

3.4 Productivity, Financial Health, and Exporting

Estimating our model requires overcoming two key empirical concerns. First, there must be sufficient variation in financial health to capture meaningful differences across plants, even after we condition on standard observables such as productivity, size, industry affiliation, etc. If plant-level financial health and productivity are strongly correlated with each other, for example, it may be difficult to isolate the individual impact of financial conditions on productivity growth.

Figure 4a plots the density of initial financial health, measured as the leverage ratio[33] in 2000, for high, medium and low productivity firms, where firms are grouped by productivity terciles. Casual observation suggests that the highest productivity manufacturers are associated with slightly lower average leverage ratios, while their least productive counterparts are characterized by moderately higher

[33]For ease of presentation, the leverage ratio was normalized to the [0,1] interval.

debt-average ratios. Perhaps most strikingly though, the differences across groups are *small*, while the variation within each group is *large*. Indeed, each of the three density plots are remarkably similar and suggest that widely different degrees of financial health are common to each productivity group.

In Figure 4b we extend this analysis to study differences in leverage ratios across 3-digit NAICS industries. Again, we observe small differences across industries but a large degree of variation within any given industry. This is striking since the limited differences across industries are often employed to measure dependence on external finance and act as key variables in tests of firm-level credit constraints (see Rajan and Zingales (1995), Braun (2003), and Manova (2013), among others).

We first characterize the impact of financial conditions on the firm-level decision to export by positing a simple probit model where the firm's decision to export is a function of initial productivity, initial financial health and a series of additional firm characteristics (firm age, age-squared, firm size, average industrial leverage). Financial health is measured as the (absolute) distance of the firm's leverage ratio from its industry average (see Miao (2005) and Levine and Warusawitharana (2021)). Productivity is measured as the (log) total factor productivity from an industry-specific Cobb-Douglas production function estimated as in Tomlin (2014), while firm size is measured as the value of total shipments per year conditional on firm characteristics.[34] For ease of presentation we presently bin both the productivity and financial health variables into terciles (high, medium, and low).

Figure 5 documents the predicted marginal effects for each combination of productivity and financial health. It is immediately clear that both improved initial productivity and financial health are always strongly associated with higher export probabilities. Remarkably, the estimated gains of moving from low to medium productivity or from medium to high productivity are nearly identical across firms with different financial conditions, even though the marginal effect is estimated separately for each productivity-financial health combination. In contrast, the average gains from moving from poor financial health to medium financial health are modestly larger than that from moving from medium to high financial health for any productivity group. While these correlations are consistent with the notion that improved financial conditions encourage exporting, in no case can we conclude that the improved financing relaxed the binding credit constraints rather than simply improving the return from exporting itself.

3.5 Instruments

Implementing our empirical approach requires identifying the impact of exporting on productivity growth. However, identification is threatened by the presence of endogeneity bias in the form of reverse causality. Exporters are generally more productive, have a larger sales volume and can post larger collateral, which eases the access to external capital. At the same time, the decision to export depends on access to external capital. We mitigate part of this endogeneity concern by comparing firms within the same cohort and considering the effect of initial (beginning of sample) financial conditions on current

[34]Specifically, we first run an auxiliary regression of firm size on TFP and our measure of the firm's financial health. The residuals from this regression now provide the measure of net firm size. This procedure allows us to distinguish the impact of individual co-variates in an intuitive manner. We further discuss the impact of this modelling decision in Section 5 and document results for both the level measure of size and net size in Table 4.

returns to exporting. Still, initial financial conditions are correlated with the initial productivity of the firm and, to the extent that productivity is persistent over time, with current returns to exporting.

To circumvent this issue in our estimation approach, we use exogenous variation in the competitiveness of Canadian firms in its main foreign market, the United States. We suppose that U.S. trade policy with respect to other trading partners is exogenous to Canadian firms. Further, Canadian-U.S. tariffs are fixed during this period due to NAFTA. In this situation, differential changes in U.S. tariff rates across industries and trading partners will affect the competition Canadian firms face in the U.S. market. Higher U.S. tariffs for non-NAFTA member countries reduce competition among Canadian exporters to the U.S. and increase their willingness to enter the U.S. market.

Our instrument consists of changes in the trade-weighted average tariff rate that the U.S. applies to its trading partners at the 5-digit industry level. We find an increase in the U.S. applied tariff rates for 54 sectors and a decrease for 105 sectors. The average change in the two samples is a -0.36 percentage point decrease in the rate with a maximum of -4.9 percentage points in the "dairy products" industry (NAICS code 31151). The maximum increase in the applied tariff rate was -2.3 percentage points in the "millwork" industry (NAICS code 32191). These results show that there is significant variation across sectors in U.S. tariff rates applied to the rest of the world and these differences might have had a significant impact on the export decisions of Canadian manufacturing plants.

The main identification concern is the exogeneity of the instrument. The U.S. trade policy variable is measured at the 5-digit industry-level. A violation of exogeneity occurs if the U.S. trade policy is correlated with different underlying conditions and trends for Canadian firms in different industries. We provide two pieces of evidence to suggest the instrument is exogenous. As a first piece of evidence, we consider whether the trade policy of the U.S. is correlated with the trade policy in Canada. If correlation occurs, then the improved access to the U.S. for Canadian exporters is not a consequence of higher U.S. tariffs applied to the rest of the world but lower import competition in Canada due to higher Canadian import tariffs for the rest of the world. This is a concern because Canadian trade policy is subject to lobbying efforts from Canadian firms and thus endogenous (Grossman and Helpman, 1994). Figure 6 plots the correlation between changes in the industry-specific average applied tariff rate of the U.S. between 2000 and 2010 vis-à-vis the rest of the world versus the average applied tariff rate of Canada vis-à-vis the rest of the world, over the same period. We do not find any significant relationship between the two trade policies.

As a second piece of evidence for instrument exogeneity, we regress U.S. tariff changes on initial conditions of firms. We are concerned that our instrument picks up differences in initial firm characteristics that make firms more or less likely to export rather than cause firms to change their exporting behaviour. Our regressions test for any correlation between U.S. trade policy and the observed firm characteristics in our analysis – total factor productivity, size, and financial health (distance from the industry optimal debt-asset ratio) in 2000. Table 3 presents these results with the first three columns including only one of the firm variables and the fourth column including all three firm variables as controls. All of the coefficient estimates are statistically insignificant across the four specifications. Further, tests also fail to reject the null hypothesis of overall model insignificance in all four cases. The next section presents the corresponding estimation results.

4 Estimation

Our empirical objectives are three-fold. First, we want to estimate the impact of exporting on productivity growth across constrained and unconstrained firms. Second, we want to determine *the fraction* of firms that cannot enter export markets *due to credit constraints* but might otherwise be profitable exporters. Third, we want to quantify the impact of relaxing credit constraints on export-driven productivity gains. Fundamentally, each of these empirical objectives requires first recovering the marginal treatment of exporting on firm-level productivity growth.

4.1 The Marginal Treatment Effect

To evaluate the heterogeneous impact of exporting on productivity growth, we follow Lileeva and Trefler (2010) and extend the framework developed by Heckman and Vytlacil (1999, 2005, 2007a,b) to our setting. We define $\Delta\varphi_1$ as the log productivity growth if the firm chooses to export and, likewise, let $\Delta\varphi_0$ be the log productivity growth if the firm chooses not to export. The relationship between productivity growth and exporting can then be written as

$$\Delta\varphi_1 = \mu_1(X) + U_1 \text{ and } \Delta\varphi_0 = \mu_0(X) + U_0. \tag{12}$$

We allow the average return to exporting, $\bar{\beta}$,to depend on firm characteristics, X, such that $\mu_1(X) \equiv \mathbf{E}[\Delta\varphi_1|X] = \bar{\beta}(X) + X\gamma'$ and $\mu_0(X) \equiv \mathbf{E}[\Delta\varphi_0|X] = X\gamma'$ while $U_1 = \epsilon + U$ and $U_0 = U$. The matrix of co-variates, (X), includes variables capturing productivity and financial health, along with additional measures of firm and industry characteristics. The impact of exporting on productivity growth also depends on the firm-specific ability to learn from exporting:

$$\mathbf{E}[\Delta\varphi_1 - \Delta\varphi_0] = \bar{\beta}(X) + U_1 - U_0 = \bar{\beta}(X) + \epsilon. \tag{13}$$

To map the above structure into an empirical model of the firm-level decision to export, let matrix Z represent all co-variates, inclusive of our instrument. We then represent the decision to export through the latent variable, D^*, as $D^* = \mathbf{E}[\pi(E=1,Z) - \pi(E=0,Z)] = \mu_D(Z) - V$, where $\mu_D(Z) = \mathbf{E}[\pi(E = 1,Z) - \pi(E=0,Z)|Z]$ is a deterministic function of observable instruments, Z, while $V = \mathbf{E}[\pi(E = 1,Z) - \pi(E=0,Z)] - \mu_D(Z)$ is a mean-zero unobserved stochastic component. The latent variable model of exporting is then

$$D^* = \mu_D(Z) - V, \qquad D = 1 \quad \text{if } D^* \geq 0, \; D = 0 \quad \text{otherwise}. \tag{14}$$

A plant either exports, that is, $D = 1$, if $D^* \geq 0$, if is not.

The random variable V captures the firm-specific components of productivity growth, ϵ and U. Since $\mathbf{E}[\pi(E=1,Z) - \pi(E=0,Z)]$ is strictly increasing in the value of ϵ, the random variables V and ϵ are negatively correlated when ϵ is independent of φ and U. That is, we expect that a plant with a larger value of $U_1 - U_0 = \epsilon$ will gain more from exporting, and hence the value of V is low. Because plants with a high value of ϵ – plants that expect large productivity gains from exporting – are more likely to self-select exporting, it must be that $\mathbf{E}[U_1 - U_0|V]$ decreases in V. The literature refers to V as the unobserved resistance to treatment.

Although V is unobserved, we can use propensity scores to recover the marginal treatment effect of exporting on productivity growth. Specifically, we assume that the distribution of V, denoted by F_V, is continuous and strictly increasing. Then, let $P(Z)$ denote the probability of exporting conditional on Z so that $P(Z) = \text{Prob}(\mu_D(Z) > V) = F_V(\mu_D(Z))$. If we define uniform random variables as $U_D \equiv F_V(V)$, the export decision can be written as $D = 1$ if $P(Z) \geq U_D$ and otherwise as $D = 0$. Since $\mathbf{E}[U_1 - U_0|V]$ strictly decreases in V, so does $\mathbf{E}[U_1 - U_0|U_D]$ in U_D. We then define the marginal treatment effect (MTE) as

$$\Delta^{MTE}(x, p) = \mathbf{E}[\Delta\varphi_1 - \Delta\varphi_0|X = x, U_D = p] = E[\bar{\beta}(X) + U_1 - U_0|X = x, U_D = p]. \tag{15}$$

The MTE quantifies heterogeneous selection on unobservables across the distribution of firms. In our case, the MTE captures the mean impact from exporting on productivity growth among plants with $X = x$ and $P(Z) = p$ when the realization of the unobserved random variable U_D is such that the plant is indifferent about whether to export or not.[35] For the given instrument Z, we are able to identify the expected return from exporting, conditional on $U_D = p$, the estimated probability of exporting.

As in Lileeva and Trefler (2010), we estimate the expected return from exporting for each value of U_D within the support of P and construct the empirical counterpart to the locus of productivity growth across firms, as outlined in Figure 3. However, due to the differential effect of financial conditions on endogenous selection into exporting, we recover a different locus of productivity returns for each level of firm-level financial conditions.

For concreteness, let X include measures of initial productivity, φ_0, and a measure of the firm's financial conditions. Specifically, we treat the difference between the firm's debt-to-asset ratio based on the industry's average debt-to-asset ratio as a signal of the firm's financial conditions.[36] Last, we also include a vector of additional co-variates consistent with the literature (firm age, initial firm size, industry-specific differences in the debt-to-asset ratio) and, in some specifications, industry fixed effects to capture differential trends across industries.[37] We then write expected productivity growth as

$$E[\Delta\varphi|X = x, P(Z) = p] = x'\gamma + px'\alpha + H(p) \tag{16}$$

[35]The MTE is identified under the assumption that (U_1, U_0, V) is independent of Z then conditional on X, and that the equation (15) is additively separable in X and U; and the shape of the MTE schedule is determined by $E[U_1 - U_0|U_D]$, which decreases in U_D.

[36]This result is broadly consistent with the literature, which suggests that there is an optimal debt-to-asset ratio for a given industry (Miao, 2005) or the debt-to-asset ratio depends on the nature of competition within an industry (Brander and Lewis, 1986). An optimal debt-to-asset ratio results from balancing the bankruptcy risk of more debt with exploiting profitability, within an industry.

[37]In practice, these additional co-variates help capture variation in initial firm efficiency and financial conditions, which are entirely captured by our benchmark measures of productivity and firm financial conditions. Since our benchmark specification is in the first difference industry specific, fixed-effects in the level equation have already been eliminated from the level equation.

and the associated marginal treatment effect as

$$\Delta^{MTE}(x,p) = x'\alpha + H'(p), \tag{17}$$

where $H(p) = E[U_1 - U_0 | U_D \leq p]p$ and $H'(p)$ is the first derivative of $H(p)$. We estimate γ, α, and $H(p)$ by a partially linear regression of $\Delta\varphi$ on X and $P(Z)$ (Robinson, 1988) with local polynomial regressions. The MTE allows us to compute all the conventional treatment parameters, such as the average treatment effect (ATE), the average treatment effect of the treated (ATT), and the average treatment effect of the untreated (ATUT), as weighted averages of the MTE, each computed with a different weighting function (Heckman and Vytlacil, 1999, 2005, 2007a,b).

5 Estimated Treatment Effects

5.1 The Decision to Export

Table 4 presents the first-stage results from the probit regression for the decision to export in 2010. The first two rows capture our primary measure for the firm's financial health condition, distance from the (industry-specific) optimal debt-asset ratio, and our primary measure for initial firm efficiency, total factor productivity. Rows 3–7 include additional variables measuring the average industry-specific leverage ratio in 2000, firm size (e.g., firm sales), and firm age (and age squared). These co-variates provide additional explanatory power for firm-level financial conditions, financial access, and dependence on external finance. They also reflect heterogeneity in initial firm efficiency, which is not captured by our measure of productivity alone. The eighth row captures the impact of our excluded instrument on the decision to export and the tariff change between the U.S. and other (non-Canadian) trading partners, while the remaining rows document underlying model specification, goodness of fit, the significance of the excluded instrument, etc.

In the first column we observe that plants with weaker financial conditions, as proxied by the distance from average initial leverage, are less likely to enter export markets. In contrast, the coefficient on productivity is counterintuitively estimated to be negative. This result can be explained by the fact that initial productivity is highly co-linear with initial firm size and firm age. Excluding these variables reverses the sign of the productivity variable but also requires that we drop key co-variates, such as age and size. Eliminating age and size from the first-stage model significantly reduces the differences in predicted export probabilities of firms that chose to export (treated) versus firms that did not choose to export (untreated). These differences between the treated and the control group provide information on the unobserved heterogeneity in the returns from exporting and are important for the identification of the different treatment effects. As such, we choose to include these variables but recognize that the entire vector of initial productivity variables – total factor productivity, size and age – helps characterize the firm's initial degree of productive efficiency and financial conditions.

To make this latter point clear, in column 2 we first run an auxiliary regression of firm size on TFP and our measure of the firm's financial health. The residuals from this regression now provide the measure of net size. This procedure allows us to distinguish the impact of individual co-variates in an intuitive manner. Indeed, weak financial conditions tended to reduce the propensity to export over our sample period, while larger, more productive and older firms were more likely to start exporting in the period. The third column repeats the benchmark probit estimation (column 1) but includes disaggregated 2-digit industry fixed effects. These have little impact on the estimated coefficients but significantly increase the overall goodness of fit. As we document below, their inclusion has a somewhat larger impact on our subsequent findings.[38]

The last row of the top panel captures our excluded instrument, the change in U.S. tariffs for the rest of the world over the 2000–2010 period. This variable consistently indicates that when the U.S. raises tariffs for other countries, Canadian producers have better access to U.S. markets and are more likely to begin exporting. The bottom panel of Table 4 documents the marginal effect at the mean of the tariff change variable. Given that the marginal impact of tariff changes ranges between 0.17 and 0.41 and the coefficients are always statistically significant, we conclude that our instrument captures a significant determinant of export decisions among Canadian manufacturers.

5.2 Treatment Effects

Table 5 documents results for our primary outcome variable, the change in log total factor productivity between 2000 and 2010. Each column corresponds to the same empirical specification as reported in the corresponding column of Table 4: (i) the first column includes all level variables (less the excluded instrument), (ii) the second employs the net-size variable, and (iii) the third includes disaggregated fixed effects. Our dependent variable is measured in log changes; differencing eliminates unobserved, time-invariant heterogeneity at either the firm- or industry-level from the estimation of the marginal treatment effects. In this sense, the inclusion of industry fixed-effects further controls for differential industry-specific trends over time. We treat the results in the second column as our benchmark findings but note differences in the results where appropriate.

The top panel of Table 5 reports the estimated coefficients for the control group (non-exporters) while the middle panel reports estimates for the difference between treated (new exporters) and control firms. The bottom panels display the coefficients from the export propensity control function, the estimated treatment effects and p-values for two statistical tests. The first test is a joint test of whether the treatment effect differs across observable firm-level characteristics while the second is a test of essential heterogeneity, which tests whether the marginal treatment effects are the same across firms. We reject the null hypothesis for both tests at the 1 percent level, that is, we find consistent evidence of heterogeneous responses to trade liberalization in terms of firm observables and unobservables in each specification. Next, we focus our discussion on the estimated regression coefficients together with the treatment effects.

Consistent with existing evidence, we find that initially larger and older non-exporters grow more

[38]The additional first-stage co-variates (age, size) are generally estimated with intuitive signs across specifications. One exception is initial leverage, which is negative in the first two columns but positive in the last column. As documented by Arellano, Bai, and Zhang (2012), firm leverage may correlate positively or negatively with financial access (and thereby exporting) depending on the development of a country's financial system. Intuitively, we expect that initial leverage would correlate positively with financial access in a developed country such as Canada.

slowly, all things being equal. For new exporters, the age effects are muted relative to their non-exporting counterparts while the size effects change signs in columns 1 and 2. Non-exporting firms with weaker initial financial positions are estimated to grow more rapidly on average, though this is only statistically significant in column 2. Financial conditions take different signs across specifications and are not clearly predictive for future growth among exporters. While these findings may seem counterintuitive, the literature provides a number of explanations for this result. For instance, better initial access to external finance can support existing growth and simultaneously act as a barrier to subsequent investment.

We do not strongly interpret any of the financial variables. Instead, the effect of the fragility of the estimated coefficient on the variables capturing financial conditions highlights the interpretation challenges associated with reduced-form evidence of firm-level credit constraints. While a positive coefficient might be consistent with the notion that only firms that were able to acquire inexpensive finance were able to grow rapidly in subsequent years, one might equally well suggest that a negative coefficient is consistent with evidence that over or underleveraged firms grow more slowly than comparable well-financed firms. We consider an alternative approach to identifying credit-constrained firms in Section 6 that does not rely on a particular interpretation of any given coefficient.

The bottom panel reports common treatment-effect parameters used to evaluate the impact of exporting on productivity growth. In general, we never observe a statistically significant estimate for the average treatment effect. However, self-selection is a common finding in the literature on export decisions and, as such, we expect that firms with greater productivity returns will be more likely to enter export markets. The average treatment effect among firms that select into exporting (ATT) is large, positive and significantly higher than the average treatment effect among non-exporters (ATUT). We estimate that exporting increased cumulative total factor productivity by 13–43 percent, or roughly 1–4 percent per year, among firms that endogenously chose to start exporting over the 2000–2010 sample period.[39]

Figure 7 displays the positive selection effect and highlights the heterogeneity across the export propensity distribution. Firms that are unlikely to export or those that have a low resistance to treatment require much higher productivity gains from exporting to justify entering export markets. Beyond the least likely exporters, the marginal treatment effect declines monotonically across the support of the export propensity distribution. These findings, consistently estimated across specifications, mirror those in Lileeva and Trefler (2010): among new exporters, the smallest Canadian producers enjoy large productivity gains, and the estimated degree of productivity growth declines sharply with the propensity to export.

[39] This difference between the ATE and ATT is consistent with the literature. While in numerous early studies, exporting has little effect on productivity (see Clerides, Lach, and Tybout (1998) and Bernard and Bradford Jensen (1999)), a number of recent contributions find at least modest support for the hypothesis that exporting improves productivity, at least for a subset of producers (see Baldwin and Gu (2003), Aw, Roberts, and Winston (2007), De Loecker (2007), Atkeson and Burstein (2010), and Lileeva and Trefler (2010)).

5.3 Mechanisms

Table 6 documents the impact of exporting on two underlying mechanisms for the strong export-driven productivity growth documented above: the deployment of external finance, as measured by changes in the leverage ratio, and firm-level investment, proxied by the change in tangible assets. Panels (b) and (c) of Figure 9 display the corresponding marginal treatment effects. In both cases, we observe that exporting induces the largest changes in the leverage ratio and the largest increase in capital holdings among new exporters with low resistance to treatment, that is, not only do the least likely exporters demonstrate the largest productivity gains but also the largest firm-level investment. Our findings further suggest that this investment is disproportionately financed with large expansions of firm leverage. Although this is consistent with the prevailing notion that credit constraints provide a significant barrier to export entry for many firms, it does not demonstrate that non-exporters are unable to acquire sufficient leverage to enter export markets.[40]

Across all outcomes, we consistently find that initial financial conditions are key determinants of exporting, productivity improvement, leverage growth and investment. Nonetheless, it remains unclear whether credit constraints bind among the remaining non-exporters. On the one hand, the average treatment effect on the untreated is insignificantly different from zero in Tables 5–6, suggesting that treating the remaining non-exporters may not have a significant impact on investment, leverage or productivity growth among firms that did enter export markets. On the other hand, the large number of non-exporting producers may respond more strongly to exporting if they had better access to external finance. Our next section focuses on those firms that are particularly likely to enter export markets in response to policy change, whether that be driven by trade liberalization, financial market reforms, or both. We then propose a novel measure of credit-constrained firms, given the proposed policy actions, and evaluate the impact of policy reform on aggregate productivity.

6 Exporting, Policy Reform, and Credit Constraints

Our benchmark estimates confirm that exporting has a substantial impact on firm-level productivity and that these estimates vary with firm-level financial conditions. They do not reveal whether trade and financial reform are complementary, the degree to which joint reform enhances export-driven productivity growth, or whether credit constraints restrict Canadian producers from exporting. To characterize the impact of trade and financial reform through exporting, we consider three alternative policies. For each policy experiment we compute the mean effect of going from a baseline policy to an alternative policy per plant that shifted into exporting. This object is commonly known as the Policy Relevant Treatment Effect (PRTE), proposed by Heckman and Vytlacil (2005, 2007b).

The first experiment isolates the impact of financial reform on exporting and productivity growth.

[40] We perform a wide set of further robustness and specification checks. Specifically, we consider specifications with initial size instead of the net-size variable (see Appendix Table 12) and again employment labour productivity as an alternative measure of productivity with the initial variable (see Appendix Table 12), and restrict attention to a sample period that excludes the great financial crisis (2000–2007; see Appendix Tables 10–11). All specifications return very similar results to those presented in Tables 4–6, and we omit further discussion hereafter.

We model this policy change by setting the firm's initial debt-to-assets ratio equal to the sectoral average, reflecting the benefit of financial reform on an individual firm's financial conditions. In this case, the PRTE captures the causal impact of exporting on productivity growth precipitated by financial reform alone. This experiment, as outlined in the next section, also allows us to recover a measure of credit constrained exporters.

The next experiment addresses the impact of higher tariffs between the U.S. and the rest of the world, excluding Canada and Mexico, the two NAFTA trading partners. As trade barriers rise between the U.S. and the rest of the world, the U.S. should be an increasingly attractive export market for Canadian firms, which in turn encourages greater investment and productivity growth. The cost of shipping goods to the U.S. from non-NAFTA trading partners counterfactually rises by 10 percentage points; that is, $\tau_f^* = \tau_f + 0.1$ where τ_f is the foreign tariff rate.

Third, we also consider joint financial and trade reform. Packaged policy reform often presents a challenge for evaluating the individual contribution of each policy change to trade growth or aggregate productivity (Manova, 2008). This is particularly true for firm-level or industry-specific policy evaluation since few data sets span periods where both trade and financial liberalization can be cleanly separated.

6.1 Using Counterfactuals to Identify Credit-Constrained Exporters

The empirical framework identifies the mean impact from exporting on productivity growth among plants with $X = x$ and $U_D = p$. Less obviously, it also inherently implies where unobserved credit constraints bind. Recall the logic of our simple model: two marginal firms, identical in every respect except access to credit, should reveal different marginal returns to exporting if and only if those firms are differentially constrained from credit markets.

Let X^h represent an index of financial conditions and define $\bar{x}^h$ as the optimal value of X^h. Further, let $\tilde{X}$ and $\tilde{Z}$ capture the full set of co-variates and instruments, *except* that we have counterfactually set $x^h = \bar{x}^h$ for all firms and, likewise, define the counterfactual probability of exporting as $p^h = P(\tilde{Z})$. The counterfactual marginal treatment effect associated with an improvement in financial conditions is

$$\Delta^{MTE}(\tilde{X}, p^h) = \tilde{X}'\alpha + H'(p^h) \tag{18}$$

for any firm. Denote the difference between the estimated and counterfactual marginal treatment effects as

$$\Delta_{CC}^{MTE}(X, \tilde{X}, p, p^h) \equiv \Delta^{MTE}(X, p) - \Delta^{MTE}(\tilde{X}, p^h). \tag{19}$$

Among highly productive firms with strong financial health, the counterfactual policy should have a very small, non-increasing impact on the marginal treatment effect, $\Delta_{CC}^{MTE}(X, \tilde{X}, p, p^h) \leq 0$. As the initial likelihood of exporting declines, we expect that $\Delta_{CC}^{MTE}(X, \tilde{X}, p, p^h)$ is strictly positive; relaxing the firm's financial constraints induces new exporters to enter export markets and causes the estimated productivity gain from the marginal exporter to fall. Let $p^{CC}(x^h) \equiv \max\{p(Z|X^h = x^h)|\Delta_{CC}^{MTE}(X, \tilde{X}, p, p^h) > 0\}$ denote the export propensity such that the counterfactual marginal treatment effect lies clearly below the benchmark MTE among firms with initial financial health $X^h = x^h$. Integrating over the set of potentially constrained firms provides a benchmark estimate of the firms for which credit constraints are binding:

$$\text{Fraction Constrained from Exporting} = \int_{x^h} \int_0^{p^{CC}(x^h)} g(p|X^h = x^h) g(x^h) dp dx^h, \tag{20}$$

where $g(p)$ is the density of initial export propensities and $g(x^h)$ is the density of financial health. A virtue of this measure is that it captures a meaningful notion of credit constraints: the fraction of firms that are most constrained from reaching export markets for credit reasons, not because exporting is insufficiently profitable per se.

Each of the above claims rests strongly on the assumption that we can separably identify the marginal treatment effect for credit-constrained and unconstrained firms alike. We formalize this assumption below.

Assumption 1 *(A1. Relevance) There exists a co-variate, X^h, directly pertaining to firm-level financing that is irrelevant for the decision to export among some portion of the support of $P(Z|X^h = x^h) = P(Z|X^h = \bar{x}^h)$ and highly relevant for the decision to export for the remaining portion of the support of $P(Z|X^h = x^h) < P(Z|X^h = \bar{x}^h)$.*

Assumption 2 *(A2. Excludability) There exists a co-variate, X^h, that satisfies A1 but does not influence the profitability of exporting except through financing costs.*

Under the above assumptions, the MTEs also provide a meaningful measure of credit-constrained exporters. Moreover, given estimates of $\Delta_{CC}^{MTE}(X, \tilde{X}, p, p_h)$, the vector of initial firm co-variates X, and initial export propensities, we can, *expost*, compare the underlying characteristics of constrained firms relative to their unconstrained counterparts.

While the fraction of firms for which credit constraints are binding can be computed with a minimal number of structural assumptions, there are three caveats to our approach that merit comment. First, if $\Delta_{CC}^{MTE} \geq 0$ for all firms, this would not necessarily indicate that credit constraints do not bind. Rather, it would imply that credit constraints bind for all firms or none at all; the data do not, and cannot, separately identify these two cases. Second, the choice of $\bar{x}^h$ is likely to be important. While it is natural to choose the optimal sample value, in the presence of measurement error, we may want to be conservative in this choice.[41] Third, if we cannot observe variation that can be *uniquely* tied to a particular constraint, then we cannot identify how changes in the constraint would influence firm behaviour. For instance, in our simple model, trade liberalization induces exporting by relaxing credit constraints for a subset of firms. However, trade liberalization, by creating new profitable export opportunities, may also plausibly relax other firm-level constraints that bind in a similar fashion (skill shortages, improved logistics management, new marketing, etc.) but were excluded from the model

[41]For instance, mismeasurement of $\bar{x}^h$ may bias $\Delta^{MTE}(\tilde{X}, p_h)$ downwards and Δ_{CC}^{MTE} upwards. Sensitivity analysis suggested that this value had a small impact in our particular application.

for parsimony. In this sense, identifying *credit-constrained* exporters requires variation that influences firm behaviour through financial conditions alone.

Nonetheless, we argue that Assumptions 1 and 2 are minimal requirements for the identification of unobserved constraints and no stronger than the implicit assumptions in numerous papers at the intersection of financial development and exporting. On the one hand, we do not need data that differentiates the impact of financial shocks on the selection and return equations to identify credit constraints, that is, we do not need to make strong assumptions on how financial variables affect the return to exporting from the decision to export. On the other hand, our approach to identifying credit constraints offers important advantages over approaches with stronger parametric assumptions. In fact, while the model in Section 2 is useful for building intuition and defining the notion of credit-constrained exporters in a particular case, it is not necessary for our empirical interpretation beyond motivating Assumptions 1 and 2. In this sense, we consider our approach a useful first step in determining which co-variates reflect variation that constrains firms from export markets and on which further theoretical or quantitative models can be developed.

6.2 Results 1: Would Policy Reform Induce Productivity Growth?

Panel (a) of Figure 8 displays the marginal treatment effects of improved financial conditions on productivity growth, panel (b) on the change in the leverage ratio and panel (c) on investment (growth of tangible assets). The PRTEs for all of our experiments are documented in Table 7.

In the first experiment we set each firm's initial financial conditions at their respective industry optimum and determine the impact on exporting, productivity growth, leverage and investment. We estimate that financial reform would increase export driven productivity gains by 16 percent. Although this estimate is quite large and substantially larger than the small, insignificant ATE in Table 5, we recall that the estimated ATT was even larger. Intuitively, the firms already induced to export under current financial conditions benefit even more than those induced to export due to financial reform.

Panels (b) and (c) of Figure 8 indicate that the counterfactual changes in productivity manifest themselves alongside large increases in leverage among firms induced to export but a smaller rise in average tangible assets. Indeed, leverage more than doubles among the set of complying firms. The estimated difference between leverage and investment can partially be attributed to differences in the distribution of the policy weights. In panel (c) the distribution is skewed to the right, implying that policy compliers come almost exclusively from plants with low propensity scores and high returns to exporting. In contrast, the distribution of weights for investment (panel (b)) moves right and does not display as much skewness. This difference likely reflects the fact that larger and more productive plants, with larger pre-existing capital stocks, invest at higher rates to replace depreciated capital.

Figure 9 considers the impact of improved export market access. Across all three outcome variables, we observe similar qualitative results relative to the financial reform exercise. The trade reform experiment predicts a 17 percent increase in average total factor productivity among firms induced to export. Intuitively, the average increase in leverage and tangible assets grows less strongly with trade liberalization than it did with financial reform.

Both trade and financial reform are predicted to encourage exporting, investment and productivity. However, in many settings these policy changes are collected and implemented in a single reform package, making it difficult to disentangle the degree of complementarity across policy reforms. Previous literature has demonstrated significant complementarity in trade and financial reform by employing the differences in external finance dependence across industries (see Manova (2008)). While compelling, standard approaches do not allow one to characterize the degree of within-industry complementarity across policy change. Given that there is a great deal more variation in financial conditions within, rather than across, industries (Figure 4), understanding the differential responses of heterogeneous producers is of natural policy interest.

To shed light on the interdependence of trade and financial policy, we consider a counterfactual setting where Canadian producers enjoy better financial conditions (as in experiment 1) and U.S. market access improves (as in experiment 2). In general, the estimated PRTEs from joint policy reform, illustrated in Figure 10, are close to the same as that in the preceding experiments; the PRTE for joint policy reform is 16 percent among the firms induced to export.

In comparing PRTEs across experiments, joint policy reform has little impact on the marginal returns to exporting relative to single-policy reform. It is tempting to conclude that this automatically implies that trade and financial reform are largely substitute, rather than complementary, sources of policy reform for Canadian manufacturers. However, this inference requires that both average change *and* the propensity to export are equal across policy experiments. Given that both policy variables are significant determinants of the propensity to export, the joint experiment suggests that any additional improvement in aggregate growth is determined by additional entry into export markets rather than a significant change in the return to exporting. Section 6.4 examines the joint impact of both financial reform and trade liberalization on *aggregate* productivity. But first we examine whether these potential gains can be attributed to relaxed credit constraints.[42]

6.3 Results 2: Are Canadian Firms Credit Constrained from Export Markets?

Employing estimates from column 2 of Table 5, we compute the expected difference between the marginal treatment effect and its counterfactual estimate after financial reform, $\Delta_{CC}^{MTE}(X, \tilde{X}, p, p^h)$, and estimate the fraction of firms for which credit constraints bind for export decisions. We find that credit constraints are the most binding constraint for nearly one half (48 percent) of all existing non-exporters.

As a first check on our interpretation of credit-constrained firms, we examine whether the firms that are identified as constrained differ from their unconstrained counterparts. Table 8 compares the mean differences of constrained and unconstrained firms across a host of firm characteristics. We find that the firms we identify as constrained are significantly more leveraged, less productive, smaller and have poorer financial health relative to their unconstrained counterparts.

[42]Table 7 documents PRTEs for both our benchmark net size specification and one that employs the size variable in level form. While the point estimates are similar across all experiments, the careful reader will note that standard errors in the second experiment increase substantially. This result is due to the imprecisely estimated impact of financial conditions in column 1 of Table 4.

To further validate our interpretation, we proceed by binning $\Delta_{CC}^{MTE}(X, \tilde{X}, p, p^h)$ according to the firm's initial export propensity. Figure 11 illustrates the change in $\Delta_{CC}^{MTE}(X, \tilde{X}, p, p^h)$ and the associated confidence intervals over the export propensity distribution.[43] Consistent with our model, we observe that the binned values of $\Delta_{CC}^{MTE}(X, \tilde{X}, p, p^h)$ are positive for firms with low export propensities, reflecting that the marginal treatment effect is larger than counterfactual marginal treatment effect. As the propensity to export grows, $\Delta_{CC}^{MTE}(X, \tilde{X}, p, p^h)$ declines. On average, this crossing occurs around an export propensity of 0.25, though we remind the reader that Figure 11 is only illustrative, masking heterogeneity within each bin. As highlighted by the confidence intervals, to the right of the initial threshold there is little evidence that $\Delta_{CC}^{MTE}(X, \tilde{X}, p, p^h)$ is greater than zero.

While these results are striking, their interpretation merits a word of caution. On the one hand, the estimates imply that credit constraints are the most binding constraint among a large set of non-exporters. In this sense, we find that firm export decisions are strongly subject to financial considerations. The finding that many firms are credit constrained follows directly from the fact that many firms are initially small, relatively unproductive and in poor financial health.[44] It is not surprising that only a few are able to secure the necessary funding for risky investments in foreign markets.

On the other hand, although credit constraints are estimated to bind for many firms, this does not imply that relaxing credit constraints would necessarily induce exporting or productivity growth. While our counterfactual experiment improves financial health, it holds both size and initial productivity as fixed. Even if credit constraints were relieved for all non-exporters, exporting would remain unprofitable and out-of-reach for a large fraction of firms. Understanding the gains from financial reform or any policy change, requires assessing both the impact that policy reform has on gains from exporting and the propensity to export itself. We turn to this issue next.

6.4 Results 3: Do Credit Constraints Hinder Aggregate Productivity Growth?

The impact of policy change on aggregate productivity growth depends both on the mean response (as reported by the PRTE) and the change in the propensity to export. The latter effect is not captured in the preceding estimates. Likewise, the PRTE does not take into account that individual firms do not represent the same amount of economic activity. While unlikely exporters benefit from disproportionate productivity growth (conditional on entry), standard measures of aggregate productivity typically give larger firms more weight.

To measure the aggregate impact of policy change, we define aggregate productivity at time t as

$$A_t = \sum_i \omega_i \Delta TFP_{it}, \tag{21}$$

where ΔTFP_{it} is firm i's measured productivity in year t and ω_i is the firm-weight.[45] Consistent with

[43]Due to vetting constraints at Statistics Canada, we bin firms by initial export propensities into eight groups by initial export propensity. Changing the number of bins does not affect the qualitative nature of Figure 11.

[44]This can be seem in any of Figures 7–10 by recognizing that the large majority of firm-weights are heavily skewed to the left of the export propensity distribution. It is likewise confirmed in Table 8.

[45]We exclude incumbent exporters from this calculation because (i) they are excluded from our estimation exercise and (ii) these firms do not respond to policy change by starting to export. We could include these firms in an aggregate

existing literature, we employ revenue weights: ω_i is the *observed* revenue share of firm i in the base period. If the firm is induced to start exporting by a counterfactual policy change, the counterfactual (log) productivity in period t is $\widetilde{\Delta TFP}_i = \Delta TFP_i + MPRTE_i$, where $MPRTE_i$ is the marginal policy relevant treatment effect for firm i. Counterfactual aggregate productivity can then be computed as

$$\begin{aligned}\widetilde{\Delta A} &= \sum_i \omega_i \left\{[P(\tilde{Z}_i) - P(Z_i)]\widetilde{\Delta TFP}_i + [1 - (P(\tilde{Z}_i) - P(Z_i))]\Delta TFP_i\right\} \\ &= \Delta A + \left\{\sum_i \omega_i [P(\tilde{Z}_i) - P(Z_i)](MPRTE_i)\right\}\end{aligned}$$

so that the total gain between period $t-1$ and period t due to policy reform reduces to

$$\widetilde{\Delta A} - \Delta A = \sum_i \omega_i [P(\tilde{Z}_i) - P(Z_i)] MPRTE_i. \tag{22}$$

Equation (22) indicates that counterfactual aggregate productivity gains are driven by predicted productivity growth, $MPRTE_i$, the increased propensity to export, $P(\tilde{Z}_i) - P(Z_i)$, and their correlation with measured economic importance, ω_i.

Table 9 documents the implied productivity growth for each of our counterfactual experiments. First, we consider the aggregate productivity effects of financial reform. Our benchmark specification predicts an additional 1.04 percentage points of aggregate productivity growth over a ten-year period from financial reform. Financial reform affects marginal productivity growth both by drawing in new marginal exporters (e.g., by changing export propensities) and by changing the return to exporting (e.g., the direct impact of financial health on the investment return).

For comparison, we next consider the productivity growth induced by greater export market access. Improved market access brought about by the counterfactual policy reforms increases the investment opportunities for *all* firms. This affects their export decision and leads to entry into foreign markets and productivity-enhancing investment of both constrained and unconstrained firms. Tariff reform induces a 0.85 percentage point increase in aggregate productivity.[46]

Last, but not least, we document the impact of joint reform on aggregate productivity growth. We find that within the Canadian manufacturing industry these policy reforms are roughly *independent* in aggregate: the joint impact of simultaneous trade and financial reform is almost identical to the sum of the two individual policy experiments.

While this result contrasts sharply with existing literature, suggesting that trade and financial reform are strong complements (e.g., Manova (2008)), there are three subtle differences that merit comment. First, we find that joint reform increases aggregate productivity growth well beyond that implied by a single policy reform. Although trade and financial reform individually induce a similar degree of exporting and productivity growth, the policies do not affect the *same* group of firms. As such, each reform induces a different subset of non-exporters to enter export markets.

productivity calculation under various ad hoc assumptions but chose to exclude them for the sake of transparency.

[46]This finding is comparable to preceding measures for the Canadian manufacturing sector. Trefler (2004) finds that NAFTA raised manufacturing labour productivity by 7.4 percent, or 0.93 percent per year.

Second, we find that the aggregate gains from joint reform are modest relative to studies that do not allow for response heterogeneity.[47] This, however, does not suggest that our findings are small; in our 10-year sample of Canadian manufacturing plants, baseline productivity growth is only estimated to be 0.82 percentage points in aggregate. In this sense, existing trade and financial frictions remain significant barriers to Canadian manufacturing growth.

Third, an important caveat to our findings is that the approach in this paper admittedly abstracts from the possibility of gains through policy-induced structural changes in the economy or broader general equilibrium effects, among other considerations. Each of these are plausible sources of greater long-run gains from joint reform.

7 Conclusions

This paper studies the impact of exporting on firm productivity and growth in an environment where firms are constrained from entering export markets due to trade and financial frictions. We find that among firms induced to export through policy reform, export-induced productivity gains are large. Specifically, we apply the marginal treatment effect framework to exporting and productivity growth among Canadian manufacturers. We recover estimates that document firm-level efficiency growth by roughly 4 percent per year among new and initially small exporting firms. Complementary evidence confirms that these gains are mediated through increases in both firm leverage and investment. This finding provides credence to the hypothesis that access to credit is a primary mechanism through which firm-level productivity growth manifests.

This paper also demonstrates that this standard estimation approach can be used to identify the fraction of firms that are plausibly restricted from export markets because of credit constraints. We argue that firms that benefit from financial reform should have systematically higher marginal returns to exporting. Estimating marginal treatment effects before and after counterfactual financial policy reform, we demonstrate that the estimated model is consistent with this prediction. Our estimates suggest that roughly one half of non-exporting Canadian manufacturers are potentially limited from enjoying export-induced productivity growth because of binding credit constraints. Relaxing these constraints is estimated to increase firm-level productivity by 15 percent over a ten-year period among those firms pushed into exporting through financial reform. Aggregate productivity gains, in contrast, are much more modest over the same time period. We estimate aggregate 10-year productivity gains of 1.04 percentage points across experiments and find that trade and financial reform are roughly independent sources of policy-driven productivity growth in the Canadian manufacturing sector.

[47] By comparison, Catherine, Chaney, Huang, Sraer, and Thesmar (2022) conduct a similar policy experiment for a quantitative model tailored to fit the investment behaviour of publicly traded U.S. firms. In that case, financial frictions induce aggregate productivity losses of 1.4 percent.

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8 Tables

Table 1: Summary Statistics of New Exporters versus Non-exporters - Comparison in 2000

	Non-exporters		New Exporters		Difference
	Mean	Std. dev.	Mean	Std. dev.	t-stat
Initial leverage ratio	0.726	0.489	0.669	0.383	5.575
Initial labour productivity	0.778	1.081	0.876	1.257	-3.874
Initial total factor productivity	11.56	0.573	11.74	0.572	-14.13
log(age)	2.836	0.214	2.846	0.215	-2.233
log(age) squared	8.087	1.194	8.148	1.200	-2.293
Initial size	1.719	1.458	2.803	1.290	-34.229
Average initial leverage ratio	0.864	0.109	0.847	0.111	6.996
|Initial leverage - av. initial leverage|	0.338	0.252	0.302	0.227	6.585
Number of observations	7660		2707		

Notes: A new exporter is a plant switching export status from non-exporter to exporter between 2000 and 2010.

Table 2: Summary Statistics of New Exporters versus Non-exporters - Growth from 2000 to 2010

	Non-exporters		New Exporters		Difference
	Mean	Std. dev.	Mean	Std. dev.	t-stat
Δ labour productivity	0.157	0.895	0.221	0.809	-3.258
Δ total factor productivity	0.019	0.073	0.022	0.092	-1.407
Δ leverage ratio	-0.214	1.108	-0.187	0.830	-1.270
Δ short-term leverage	-0.224	0.967	-0.202	0.894	-0.923
Δ tangible assets	0.556	1.103	0.742	1.155	7.445
Δ tangible over total assets	0.274	0.989	0.212	0.968	2.801
Δ US tariff with ROW	-0.004	0.009	-0.002	0.007	-1.650
Number of observations	7660		2707		

Notes: This table presents the average change between 2010 and 2000 for each variable, where for firm i in the sample the change in variable X is $\ln X_{2010,i} - \ln X_{2000,i}$.

Table 3: Correlation Between Change in Tariffs and Initial Variables

Dependent variable:	Change in U.S. tariffs 2000–2010			
Initial size	0.0043			-0.0012
	(0.0039)			(0.0013)
|Initial leverage - av. initial leverage|		-0.0028		0.0006
		(0.0053)		(0.0036)
Initial total factor productivity			0.0292	0.0306
			(0.0230)	(0.0236)
R2	0.001	0.001	0.034	0.035
Number of observations	10,367	10,367	10,367	10,367

Notes: Standard errors are in parentheses.

Table 4: The Decision to Export

Dependent variable:	Export status in 2010		
|Initial leverage - av. initial leverage|	-0.015	-0.302	-0.374
	(0.0611)	(0.060)	(0.063)
Initial total factor productivity	-0.149	0.424	0.395
	(0.029)	(0.025)	(0.038)
Average initial leverage ratio	-0.175	-0.175	1.194
	(0.133)	(0.133)	(0.169)
log(age)	-2.575	-2.575	-2.599
	(2.223)	(2.223)	(2.293)
log(age) squared	0.351	0.351	0.376
	(0.398)	(0.398)	(0.411)
Initial size	0.363		0.381
	(0.0125)		(0.0133)
Initial net size		0.363	
		(0.0125)	
Change in U.S. tariffs 2000–2010	0.639	0.639	1.361
	(0.165)	(0.165)	(0.388)
Industry fixed effects	no	no	yes
LR chi(2)	1203.11	1203.11	1833.03
Pseudo R2	0.101	0.101	0.154
Number of observations	10,367	10,367	10,367
Marginal effect at the mean			
Change in U.S. tariffs 2000–2010	0.196	0.196	0.407
	(0.051)	(0.051)	(0.116)

Notes: This table reports the first-stage results for the binary decision to export. Standard errors are in parentheses.

Table 5: Benchmark Productivity Results

Dependent variable:	Log-change in total factor productivity		
Non-exporters			
|Initial leverage - av. initial leverage|	0.0004	0.0435	0.0044
	(0.0085)	(0.0104)	(0.0069)
Initial total factor productivity	0.0447	-0.0416	0.0386
	(0.0056)	(0.0100)	(0.0051)
Average initial leverage ratio	0.0253	0.0253	-0.0621
	(0.0229)	(0.0244)	(0.0266)
log(age)	-0.4210	-0.4210	-0.5113
	(0.3525)	(0.3635)	(0.2772)
log(age) squared	0.0784	0.0784	0.0901
	(0.0626)	(0.0652)	(0.0494)
Initial size	-0.0547		-0.0348
	(0.0072)		(0.0049)
Initial net size		-0.0547	
		(0.0075)	
Difference: exporters vs. non-exporters			
|Initial leverage - av. initial leverage|	0.0100	-0.0581	0.0676
	(0.0266)	(0.0388)	(0.0816)
Initial total factor productivity	-0.0639	0.0723	-0.0109
	(0.0189)	(0.0348)	(0.0215)
Average initial leverage ratio	-0.0545	-0.0545	0.0000
	(0.0619)	(0.0678)	(0.0000)
log(age)	0.3815	0.3815	-0.0450
	(1.0944)	(1.0919)	(0.0152)
log(age) squared	-0.0660	-0.0660	0.4707
	(0.1929)	(0.1957)	(0.8544)
Initial size	0.0862		-0.0745
	(0.0318)		(0.1517)
Initial net size		0.0862	
		(0.0304)	
Control function			
Polynomial	-1.2293	-1.2293	-0.2307
	(0.5000)	(0.4695)	(0.3012)
Marginal effects			
ATE	-0.0470	-0.0470	0.0190
	(0.1246)	(0.1204)	(0.0899)
ATT	0.3584	0.3584	0.1331
	(0.0695)	(0.0736)	(0.0514)
ATUT	-0.2102	-0.2102	-0.0202
	(0.1846)	(0.1753)	(0.1239)
LATE	0.2120	0.2118	0.1421
	(0.0564)	(0.0574)	(0.0500)
Observable heterogeneity (p-value)	(0.0000)	(0.0002)	(0.0000)
Essential heterogeneity (p-value)	(0.0000)	(0.0000)	(0.0000)
Industry fixed-effects	no	no	yes
Number of observations	10,367	10,367	10,367

Notes: This table reports the results from estimating equation (16) using TFP as an outcome variable. We employ the benchmark, net size and fixed effects (industrial trend) specifications in columns (1)–(3), respectively. Standard errors are in parentheses.

Table 6: Mechanisms Underlying Productivity Growth

Dependent variable:	Change in leverage ratio	Log-change in tangible assets
Non-exporters		
|Initial leverage - av. initial leverage|	0.1914	0.1130
	(0.0965)	(0.0979)
Initial total factor productivity	-0.0986	-0.1675
	(0.0863)	(0.0462)
Average initial leverage ratio	0.4224	-0.5883
	(0.1758)	(0.2088)
log(age)	6.9515	-19.1030
	(3.2991)	(4.2713)
log(age) squared	-1.2014	3.2009
	(0.5900)	(0.7643)
Initial net size	-0.0664	-0.0502
	(0.0578)	(0.0691)
Difference: exporters vs. non-exporters		
|Initial leverage - av. initial leverage|	1.0993	-1.0711
	(0.3886)	(0.3660)
Initial total factor productivity	0.0598	0.7381
	(0.2764)	(0.1645)
Average initial leverage ratio	-1.7275	2.2585
	(0.5333)	(0.7474)
log(age)	-10.6201	38.1291
	(10.2310)	(16.2902)
log(age) squared	1.8960	-6.6551
	(1.8354)	(2.9092)
Initial net size	0.1145	0.1081
	(0.2212)	(0.2892)
Control function		
Polynomial 1	-2.1261	-8.5497
	(3.7277)	(4.9096)
Marginal effects		
ATE	0.5501	-0.5653
	(0.8202)	(1.1812)
ATT	1.2205	1.8896
	(0.6054)	(0.7079)
ATUT	0.2781	-1.5515
	(1.2519)	(1.7514)
LATE	0.9950	0.7419
	(0.4295)	(0.7173)
Observable heterogeneity (p-value)	(0.0060)	(0.0000)
Essential heterogeneity (p-value)	(0.0000)	(0.0000)
Number of observations	10,367	10,367

Notes: This table reports the results from estimating equation (16) using the leverage ratio and tangible assets as outcome variables. We employ the net-size specification in each column. Standard errors are in parentheses.

Table 7: Policy Relevant Treatment Effects

Dependent variable:	Total factor productivity	Leverage ratio	Tangible assets
Policy Experiment 1: Financial Reform			
Benchmark Specification	0.152	0.985	0.422
	(0.067)	(0.441)	(0.652)
Size specification	0.180	1.040	0.568
	(1.901)	(62.69)	(171.1)
Policy Experiment 2: Tariff Reform			
Benchmark Specification	0.161	0.795	-0.188
	(0.061)	(0.419)	(0.834)
Size specification	0.161	0.795	-0.188
	(0.063)	(0.405)	(0.849)
Policy Experiment 3: Joint Reform			
Benchmark Specification	0.139	0.867	-0.323
	(0.063)	(0.509)	(0.792)
Size specification	0.160	0.805	-0.194
	(0.058)	(0.458)	(0.730)

Notes: This table documents policy relevant treatment effects for each outcome variable (productivity, leverage ratio, tangible assets, leverage ratio) and each experiment (trade reform, financial reform, joint reform). Standard errors are in parentheses.

Table 8: Financial Reform: Constrained vs. Unconstrained Plants

	Unconstrained		Constrained		Difference
	Mean	Std. dev.	Mean	Std. dev.	t-stat
Initial leverage ratio	0.64	0.36	0.78	0.54	-15.75
Initial labour productivity	0.91	1.33	0.69	0.85	9.80
Initial total factor productivity	11.75	0.61	11.46	0.50	26.84
log(age)	2.85	0.21	2.82	0.21	5.71
log(age) squared	8.17	1.19	8.03	1.19	5.80
Initial size	3.09	1.12	0.85	0.82	115.33
Initial size (net)	0.58	0.87	-1.16	1.14	87.58
Average initial leverage ratio	0.84	0.11	0.88	0.11	-17.48
|Initial leverage - av. initial leverage|	0.30	0.22	0.35	0.27	-9.75
P(export)	0.37	0.12	0.14	0.05	124.82
P(export) financial reform	0.41	0.12	0.17	0.06	129.01
P(export) trade reform	0.40	0.12	0.16	0.06	125.43
P(export) joint reform	0.40	0.12	0.17	0.06	125.83
Number of observations	5310		5057		

Notes: This table documents summary statistics for plants that are credit constrained from export markets according to equation (19) relative to those that are not given (i) the financial reform counterfactual experiment and (ii) the use of TFP as an outcome variable.

Table 9: Aggregate Productivity Changes

Panel A: Policy Experiment 1: Financial Reform	
sales weighted	1.04%
equal weights	0.97%

Panel B: Policy Experiment 2: Tariff Reform	
sales weighted	0.85%
equal weights	0.81%

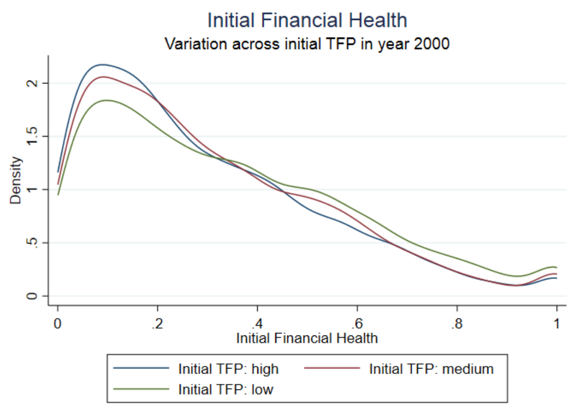
Panel C: Policy Experiment 3: Joint Reform	
sales weighted	1.71%
equal weights	1.57%

Notes: This table documents the aggregate productivity impact of policy reform. Policy driven productivity growth reports the percentage point growth in aggregate productivity caused by policy reform. Initial firm revenues are used as weights. The benchmark specification and total factor productivity are employed in all exercises.

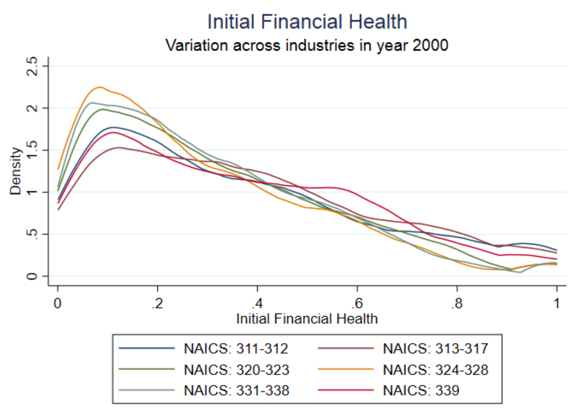
9 Figures

Figure 4: Distribution of initial financial health

(a) Specification: Initial total factor productivity

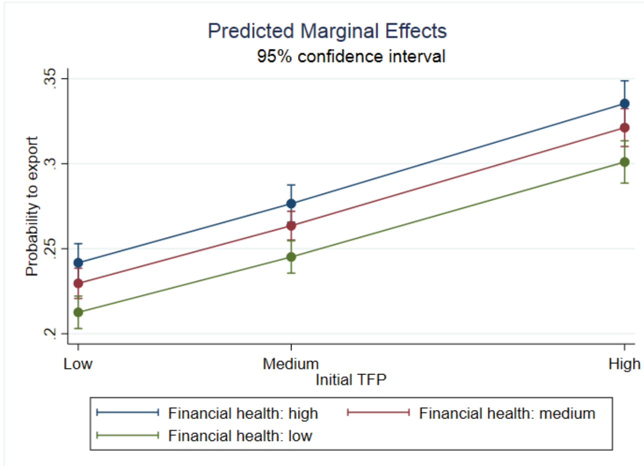


(b) Specification: 3-digit NAICS industries



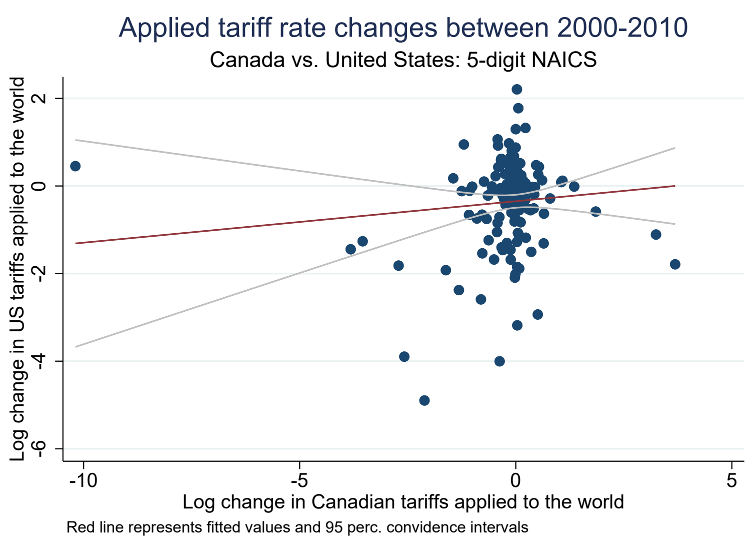
Notes: This figure displays the distribution of initial financial health for different levels of initial productivity and 3-digit NAICS industries.

Figure 5: Marginal treatment effects as a function of initial productivity and financial health



Notes: The figure shows the marginal effect of exporting at different levels of initial productivity and financial health. We distinguish between three categories of firms with low (lowest tertile), medium (second tertile) and high (highest tertile) initial total factor productivity as well the firms with low (lowest tertile), medium (second tertile) and high (highest tertile) initial financial health.

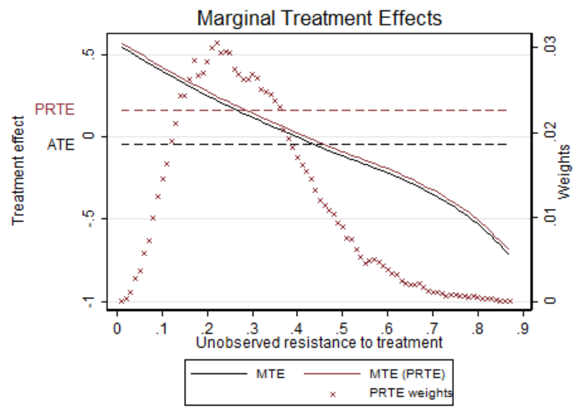
Figure 6: Correlation between changes in U.S. and Canadian trade policy (2000–2010)



Notes: The regression estimates the relationship between the industry-specific log change in the average applied tariff change in Canada for 2000–2010 on the log change in the average applied tariff change in the U.S. over the same period.

Figure 7: Total factor productivity

(a) Specification: Initial size



(b) Specification: Initial net size

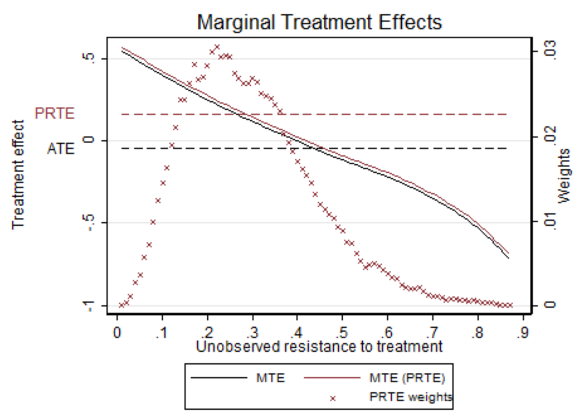
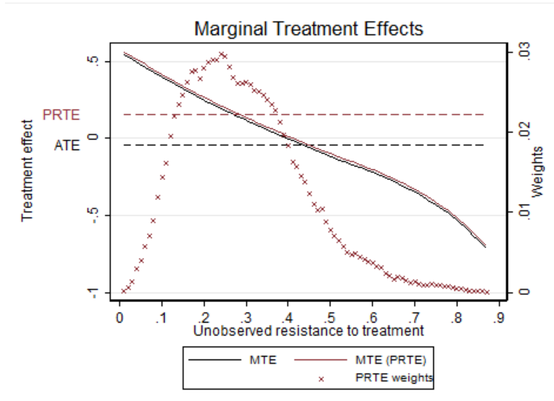
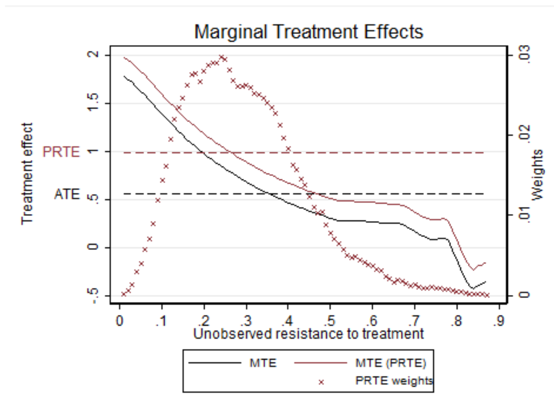


Figure 8: Ease of financial conditions (Benchmark Specification)

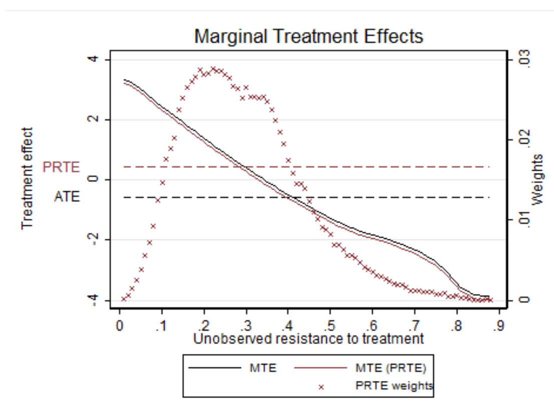
(a) TFP



(b) Leverage



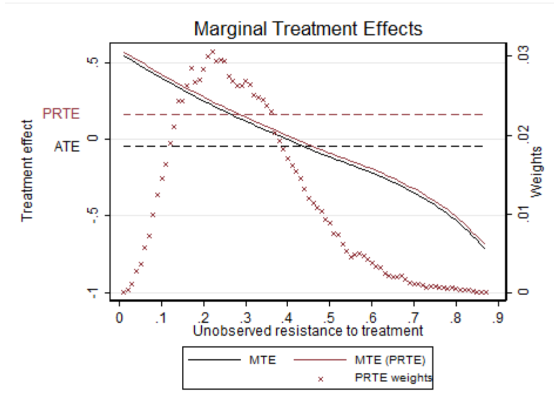
(c) Investment



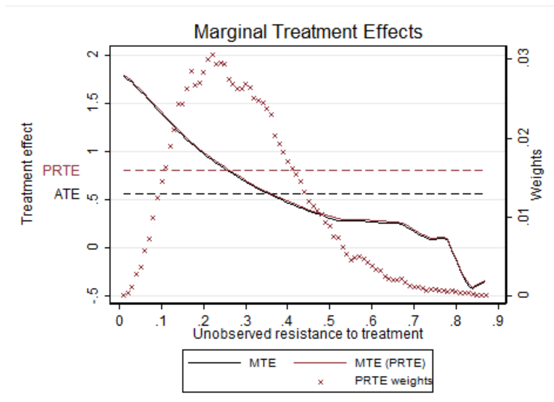
Notes: This figure displays the marginal treatment effect model before and after the policy intervention.

Figure 9: Higher U.S. tariffs (Benchmark Specification)

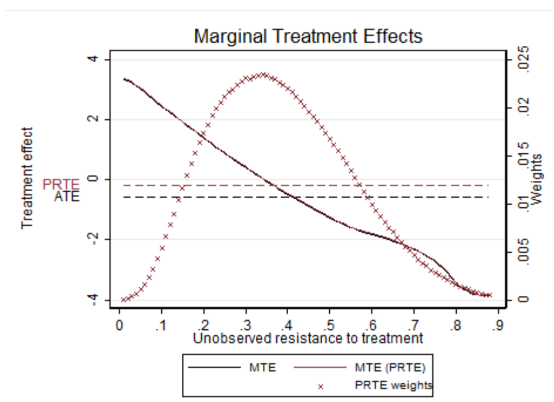
(a) TFP: average treatment effects



(b) Leverage: average treatment effects



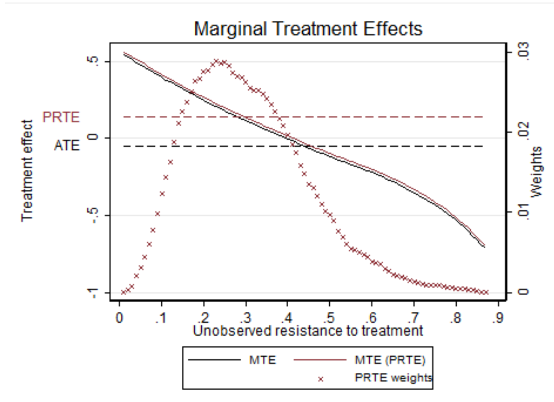
(c) Investment: average treatment effects



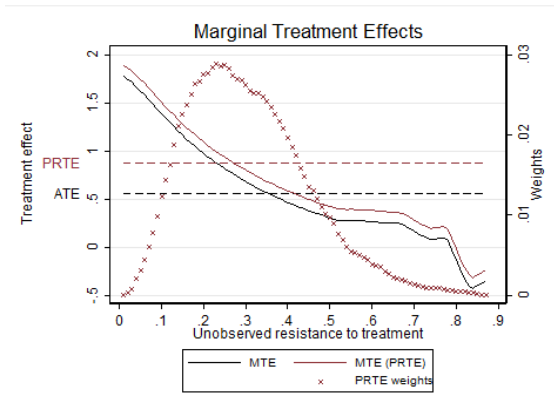
Notes: The figure displays the marginal treatment effect model before and after the policy intervention.

Figure 10: Higher tariffs and ease of financial conditions (Benchmark Specification)

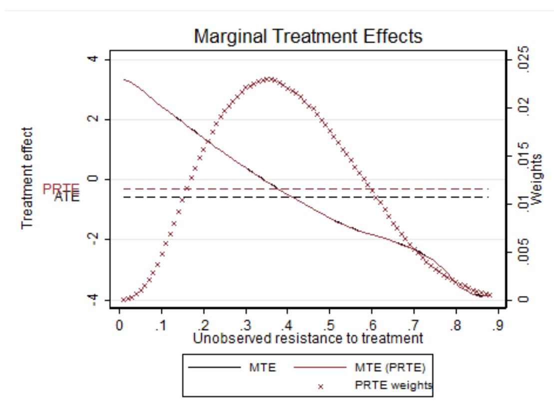
(a) TFP: average treatment effects



(b) Leverage: average treatment effects

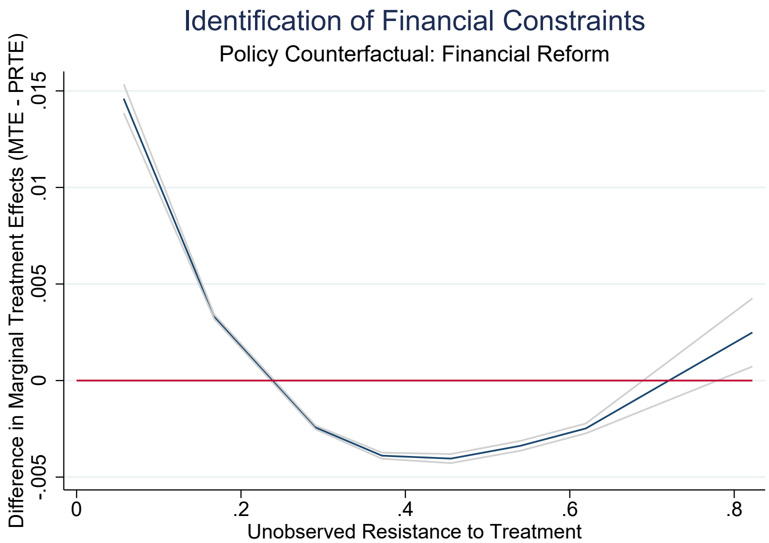


(c) Investment: average treatment effects



Notes: This figure displays the marginal treatment effect model before and after the policy intervention.

Figure 11: Share of financial constrained firms



Notes: The figure displays the difference between the marginal treatment effects model before and after the financial reform policy intervention. Due to vetting constraints, we binned the observations into eight bins and computed the mean difference for each bin. The grey lines represent the 95 percent confidence intervals around the mean estimate of the bin.

A Appendix: Tables

Table 10: The Decision to Export for the Periods 2000–2007 and 2000–2010

Dependent variable:	Export status in 2007			Export status in 2010		
|Initial leverage - av. initial leverage|	0.012	-0.325	-0.027	-0.015	-0.302	-0.073
	(0.055)	(0.055)	(0.057)	(0.061)	(0.060)	(0.063)
Initial total factor productivity	-0.136	0.538	-0.220	-0.150	0.424	-0.207
	(0.029)	(0.025)	(0.032)	(0.029)	(0.025)	(0.033)
Average initial leverage ratio	-0.133	-0.133	1.290	-0.175	-0.175	1.194
	(0.120)	(0.120)	(0.152)	(0.133)	(0.133)	(0.170)
log(age)	-1.553	-1.553	-1.779	-2.575	-2.575	-2.599
	(1.178)	(1.178)	(1.215)	(2.223)	(2.223)	(2.294)
log(age) squared	0.219	0.219	0.279	0.352	0.352	0.376
	(0.229)	(0.229)	(0.236)	(0.399)	(0.399)	(0.411)
Initial size	0.380		0.406	0.363		0.382
	(0.011)		(0.012)	(0.013)		(0.013)
Initial net size		0.380			0.363	
		(0.011)			(0.013)	
Change in U.S. tariffs 2000–2010	0.572	0.572	1.157	0.639	0.639	1.362
	(0.154)	(0.154)	(0.348)	(0.166)	(0.166)	(0.389)
Industry fixed-effects	no	no	yes	no	no	yes
LR chi(2)	1681.12	1681.12	2408.99	1203.11	1203.11	1833.03
Pseudo R2	0.114	0.114	0.164	0.101	0.101	0.154
Number of observations	12,758	12,758	12,758	10,367	10,367	10,367
Marginal effect at the mean						
Change in US tariffs 2000–2007	0.175	0.175	0.343	0.196	0.196	0.407
	(0.047)	(0.047)	(0.103)	(0.051)	(0.051)	(0.116)

Notes: This table reports the first-stage results for the binary decision to export for the period 2000–2007 in columns (1)–(3) and the period 2000–2010 in columns (4)–(6). Standard errors are in parentheses.

Table 11: Productivity Results for the Periods 2000–2007 and 2000–2010

Dependent variable:	Log-change in total factor productivity 2000–2007			Log-change in total factor productivity 2000–2010		
|Initial leverage - av. initial leverage|	-0.007	0.047	0.002	0.000	0.044	0.004
	(0.007)	(0.007)	(0.005)	(0.009)	(0.010)	(0.007)
Initial total factor productivity	0.046	-0.062	0.033	0.045	-0.042	0.039
	(0.004)	(0.010)	(0.004)	(0.006)	(0.010)	(0.005)
Average initial leverage ratio	0.064	0.064	-0.040	0.025	0.025	-0.062
	(0.018)	(0.014)	(0.019)	(0.023)	(0.024)	(0.027)
log(age)	-0.224	-0.224	-0.240	-0.421	-0.421	-0.511
	(0.157)	(0.149)	(0.131)	(0.353)	(0.363)	(0.277)
log(age) squared	0.047	0.047	0.050	0.078	0.078	0.090
	(0.030)	(0.029)	(0.025)	(0.063)	(0.065)	(0.049)
Initial size	-0.061		-0.051	-0.055		-0.035
	(0.005)		(0.004)	0.007		0.005
Initial net size		-0.061			-0.055	
		(0.005)			(0.008)	
Difference: exporters vs. non-exporters						
|Initial leverage - av. initial leverage|	0.020	-0.058	-0.021	0.010	-0.058	0.068
	(0.022)	(0.023)	(0.018)	(0.027)	(0.039)	(0.082)
Initial total factor productivity	-0.058	0.097	-0.024	-0.064	0.072	-0.011
	(0.012)	(0.030)	(0.013)	(0.019)	(0.035)	(0.021)
Average initial leverage ratio	-0.138	-0.138	0.104	-0.055	-0.055	0.000
	(0.046)	(0.044)	(0.066)	(0.062)	(0.068)	(0.000)
log(age)	0.145	0.145	-0.037	0.382	0.382	-0.045
	(0.459)	(0.463)	(0.404)	(1.094)	(1.092)	(0.015)
log(age) squared	-0.026	-0.026	0.001	-0.066	-0.066	0.471
	(0.089)	(0.090)	(0.079)	(0.193)	(0.196)	(0.854)
Initial size	0.087		0.076	0.086		-0.074
	(0.019)		(0.016)	(0.032)		(0.152)
Initial net size		0.087			0.086	
		(0.019)			(0.030)	
Control function						
Polynomial	-0.948	-0.948	-0.790	-1.229	-1.229	-0.231
	(0.299)	(0.298)	(0.237)	(0.500)	(0.470)	(0.301)
Treatment effects						
ATE	-0.095	-0.095	-0.117	-0.047	-0.047	0.019
	(0.091)	(0.091)	(0.077)	(0.125)	(0.120)	(0.090)
ATT	0.289	0.289	0.191	0.358	0.358	0.133
	(0.048)	(0.053)	(0.038)	(0.069)	(0.074)	(0.051)
ATUT	-0.231	-0.231	-0.225	-0.210	-0.210	-0.020
	(0.129)	(0.127)	(0.105)	(0.185)	(0.175)	(0.124)
LATE	0.079	0.079	0.111	0.212	0.212	0.142
	(0.071)	(0.050)	(0.062)	(0.056)	(0.057)	(0.050)
Observable heterogeneity (p-value)	(0.0000)	(0.0002)	(0.0000)	(0.0000)	(0.0002)	(0.0000)
Essential heterogeneity (p-value)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Industry fixed-effects	no	no	yes	no	no	yes
Number of observations	12,758	12,758	12,758	10,367	10,367	10,367

Notes: This table reports the results from estimating equation (16) using TFP as an outcome variable. We employ the benchmark, net size and fixed effects (industrial trend) specifications in columns (1)–(3) for the period 2000–2007 and in columns (4)–(6) for the period 2000–2010. Standard errors are in parentheses.

Table 12: Measurement and Mechanisms: Size Specification

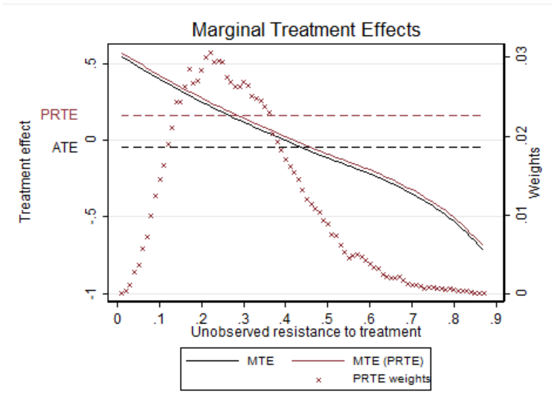
Dependent variable:	Log-change labour productivity	Change in leverage ratio	Log-change in tangible assets
Non-exporters			
|Initial leverage - average initial leverage|	-0.0032	0.1390	0.1130
	(0.1118)	(0.0977)	(0.1011)
Initial labour productivity	-0.1642		
	(0.1889)		
Initial total factor productivity		0.0063	-0.1675
		(0.0368)	(0.0462)
Average initial leverage ratio	0.6087	0.4224	-0.5883
	(0.3193)	(0.1657)	(0.2079)
log(age)	-5.8918	6.9515	-19.1030
	(3.5891)	(3.2869)	(4.4100)
log(age) squared	1.1519	-1.2014	3.2009
	(0.6312)	(0.5906)	(0.7900)
Initial size	-0.5608	-0.0664	-0.0502
	(0.0998)	(0.0572)	(0.0689)
Difference: exporters vs. non-exporters			
|Initial leverage - average initial leverage|	0.3368	1.1897	-1.0711
	(0.2757)	(0.3512)	(0.3762)
Initial labour productivity	-0.3743		
	(0.5149)		
Initial total factor productivity		-0.1212	0.7381
		(0.1413)	(0.1650)
Average initial leverage ratio	-1.6493	-1.7275	2.2585
	(0.8362)	(0.5045)	(0.7540)
log(age)	16.8925	-10.6201	38.1291
	(9.6975)	(9.6251)	(16.758)
log(age) squared	-3.1512	1.8960	-6.6551
	(1.6826)	(1.7242)	(2.9947)
Initial size	1.0575	0.1145	0.1081
	(0.5146)	(0.2190)	(0.2855)
Control function			
Polynomial 1	-19.2343	-2.1261	-8.5497
	(7.9056)	(3.5758)	(4.8309)
Marginal effects			
ATE	0.1538	0.5501	-0.5653
	(2.1301)	(0.8463)	(1.1667)
ATT	6.3376	1.2205	1.8896
	(1.0414)	(0.6018)	(0.7068)
ATUT	-2.2695	0.2781	-1.5515
	(3.0889)	(1.2548)	(1.7254)
LATE	3.2711	0.9952	0.7419
	(0.9395)	(0.4915)	(0.7132)
Observable heterogeneity (p-value)	(0.0010)	(0.0011)	(0.0000)
Essential heterogeneity (p-value)	(0.0000)	(0.0000)	(0.0000)
Number of observations	10,367	10,367	10,367

Notes: This table reports the results from estimating equation (16) using labour productivity, the leverage ratio and tangible assets as outcome variables. We employ the benchmark specification in each column. Standard errors are in parentheses.

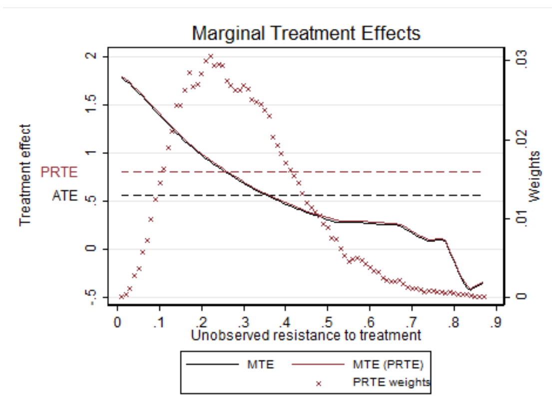
B Appendix: Figures

Figure 12: Higher U.S. tariffs (Size Specification)

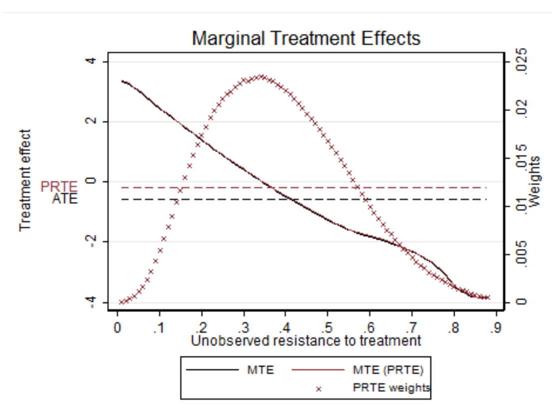
(a) TFP, average treatment effects



(b) Leverage, average treatment effects



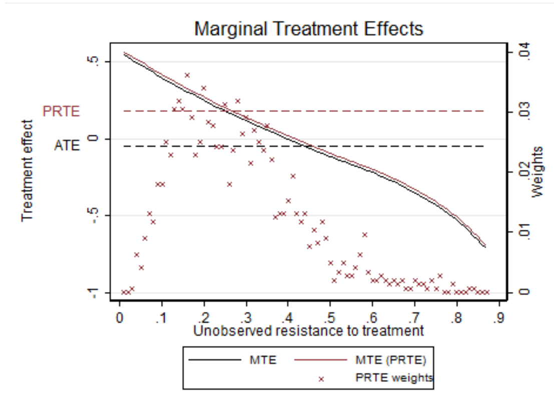
(c) Investment, average treatment effects



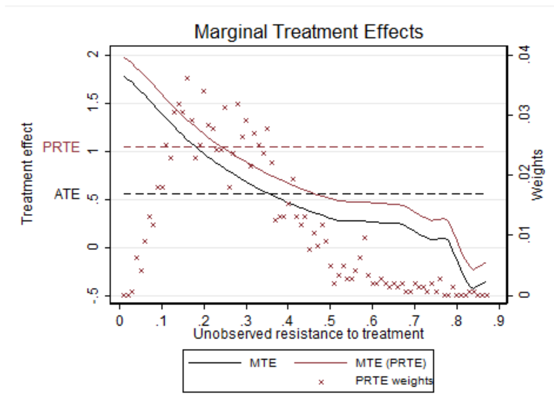
Notes: The figure displays the marginal treatment effect model before and after the policy intervention.

Figure 13: Ease of financial conditions (Size Specification)

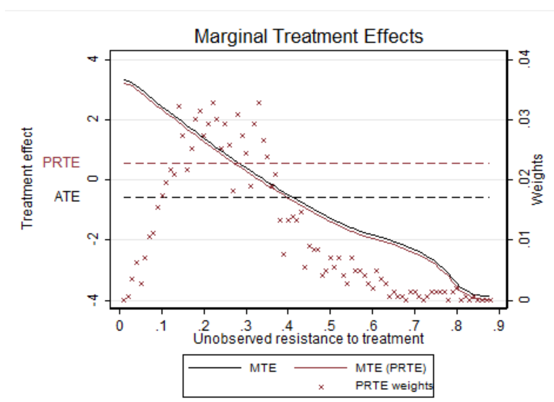
(a) TFP, average treatment effects



(b) Leverage, average treatment effects



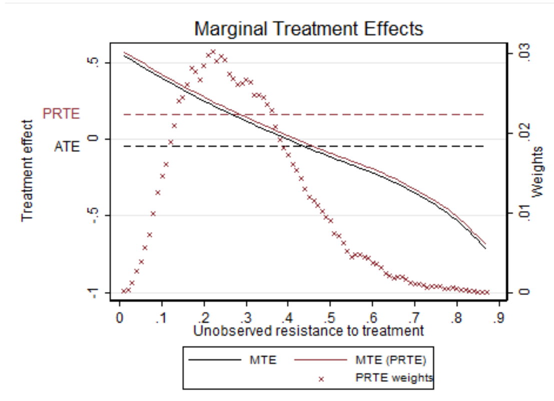
(c) Investment, average treatment effects



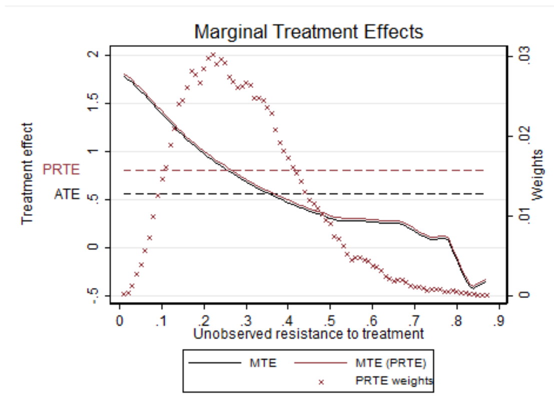
Notes: The figure displays the marginal treatment effect model before and after the policy intervention.

Figure 14: Higher tariffs and ease of financial conditions (Size Specification)

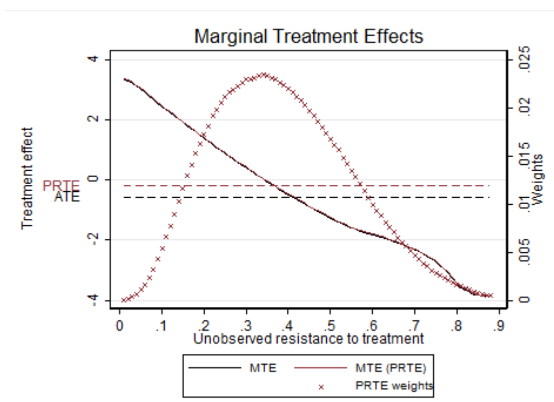
(a) TFP, average treatment effects



(b) Leverage, average treatment effects



(c) Investment, average treatment effects



Notes: The figure displays the marginal treatment effect model before and after the policy intervention.

C Model Details

This appendix provides background model details for the simple model. Details for the model with multiple investment levels are analogous to those presented below. We characterize the individual decision to invest or export in the absence of the other since these were omitted from the model description.

C.1 Exporting Alone

The firm will export whenever the net profit from exporting is greater than the profit from serving the domestic market alone, $\pi(\varphi_0, 1, 0) - \pi(\varphi_0, 0, 0) > 0$. The marginal exporter is the firm that is indifferent between exporting and only serving the domestic market:

$$\pi(\varphi_{0,1,0}) - (1 - p_D)[\rho(\varphi_{0,1,0}) - rK] - [\pi(\varphi_{0,0,0}) + rK] = 0. \tag{23}$$

The first term is the operating profits from exporting. We subtract the expected loan repayment and the firm's collateralizable assets from operating profits. The last term in square brackets is the net operating profits for the non-exporting/non-investing firm plus the market value of the firm's assets. The threshold export productivity is

$$\Rightarrow \varphi_0^E = \frac{(1 + rD)F_E}{T^{-\sigma} A^*}. \tag{24}$$

Since there is no uncertainty associated with exporting, the bank only lends to firms when export profits are sufficient to cover repayment. As such, there is never any default in equilibrium and $q = r$ for all non-investing exporters.

C.2 Investing Alone

We now consider the firm's decision to invest without exporting. The firm must satisfy an incentive compatibility constraint, $\mathbf{E}[\pi(\varphi, 0, 1)] - \pi(\varphi_0, 0, 0) > 0$. In contrast to exporting, investing is inherently risky. We define the marginal investor as

$$\mathbf{E}[\pi(\varphi, 0, 1)] - (1 - p_D)[\rho(\varphi_0, 0, 1) - rK] - [\pi(\varphi_0, 0, 0) + rK] = 0. \tag{25}$$

The marginal condition defines a threshold *gain* from investing: for every initial level of productivity φ_0, there is a minimum gain in productivity required to justify investing. Solving equation (25) we find

$$\frac{E[\Delta\varphi^I]}{\chi(D, 0, 1)} \equiv \frac{1}{A}. \tag{26}$$

The marginal condition (26) varies across firms depending on their reliance on external finance, D, but does not vary with their initial productivity, φ_0. The latter result is intuitive since the gains are proportional to market size A for all firms and the incentive compatibility constraint is not directly a function of the firm's financial conditions (δ, K). This does not imply that the financial conditions play no role here; rather, the amount needed to cover the same fixed investment costs, F_I, varies across firms and, as such, so does the amount of financing needed for investment.

We further observe that equation (26) is not directly a function of the default probability. Nonetheless, for some firms it implicitly depends on the probability of default through the probability that the firm's investment is successful. To see this, consider the set of firms whose initial productivity draws,

φ_0, are sufficiently high so that they will always be able to pay back the bank loan. For these firms, the probability of default is 0 and the price of investment q is the opportunity cost of bank r. In contrast, among firms that can only afford to repay the loan if the investment is successful, the probability of default is implicitly defined by the probability of investment success, $p_D = 1 - p_I$.

When $p_D > 0$, a firm must satisfy both its own incentive compatibility constraint, (25), and the bank's participation constraint, equation (4). The marginal firm(s), which are just able to meet these additional financing constraints, are defined as those where

$$\mathbf{E}[\pi(\varphi, 0, 1)] - \rho(\bar{\varphi}, 0, 1) \quad = \quad \bar{\varphi}A - \delta - (1-D)F_I + rK - \rho(\bar{\varphi}, 0, 1) = 0$$

or

$$\frac{E[\Delta\varphi^I_{CC}]}{\chi(D,0,1)} \equiv \quad = \quad \frac{1}{A}\left[1 + \frac{\delta - \frac{rK}{p_I}}{\chi(D,0,1)} + \frac{D(1+r)(1-p_I)}{(1+rD)p_I}\right] - \frac{\varphi_0}{\chi(D,0,1)}. \tag{27}$$

Although equation (27) looks messy, it implies that credit constraints are most likely to bind, $\Delta\varphi^I_{CC} > \Delta\varphi^I$, among unproductive producers that rely heavily on external finance.[48] Firms in this set would optimally like to invest in new technology but are constrained from doing so because no one will lend them the necessary funds.

C.3 Exporting and Investment Size

We assumed throughout Section 2 that firm-level investment and the likelihood of productivity growth are identical across firms. However, firms with larger potential productivity growth are plausibly willing to undertake larger investments to increase the probability of a successful investment. In this sense, credit constraints may influence *whether* firms export and invest, the *degree* of investment, and, consequently, the *need* for external finance.

To clarify the difference, suppose that firms have the option to make a large (high) investment, $F_I = F_H$, or small (low) investment, $F_I = F_L$. Large investments yield productivity growth with probability $p_I = p_H$ while the likelihood of productivity growth with a small investment is $p_I = p_L$, $p_H > p_L$. The firm's decisions are then two-fold: should it export and invest and, if so, how much should it invest? Among these possibilities, which are also compatible for the lender?

We abstract from trivial cases where one type of investment always dominates the other or only one credit constraint is relevant for all firms.[49] We focus on the case where the profitability threshold needed for low investment is less likely to bind than that for high investment. Under this assumption, equation (6) again determines the threshold for exporting and low investment. The threshold for high investment and exporting is determined by comparing the return to high investment relative to low investment:

$$\begin{aligned} &\mathbf{E}[\pi(\varphi, 1, H)] - (1 - p_{D,H})[\rho(\varphi_0, \delta, 1, H) - rK] - \\ &\mathbf{E}[\pi(\varphi, 1, L)] + (1 - p_{D,L})[\rho(\varphi_0, \delta, 1, L) - rK] = 0, \end{aligned} \tag{28}$$

where $p_{D,H}$ and $p_{D,L}$ are the probabilities of default under high and low investment, respectively. Regardless of the firm's investment decision, both high and low profitability thresholds can be described

[48] Specifically, $E[\Delta\varphi^I_{CC}] > E[\Delta\varphi^I]$ for small values of φ_0 as long as $\delta - \frac{rK}{p_I} + \frac{D(1+r)(1-p_I)F_I}{p_I} > 0$. This is very likely when p_I is small (as $p_I \to 0$), since $DF_I > rK$. It is also true when p_I is large (as $p_I \to 1$) and δ is sufficiently large, $\delta > rK$.

[49] These cases are largely subsumed in Section 2.

by a slight modification of the preceding threshold condition (7):

$$\frac{\mathbf{E}[\Delta\varphi_I^{EI}]}{\chi(D,1,I)} = \frac{1}{A+T^{-\sigma}A^*} - \left(\frac{T^{-\sigma}A^*}{A+T^{-\sigma}A^*}\right)\frac{\varphi_0}{\chi(D,1,I)}, \tag{29}$$

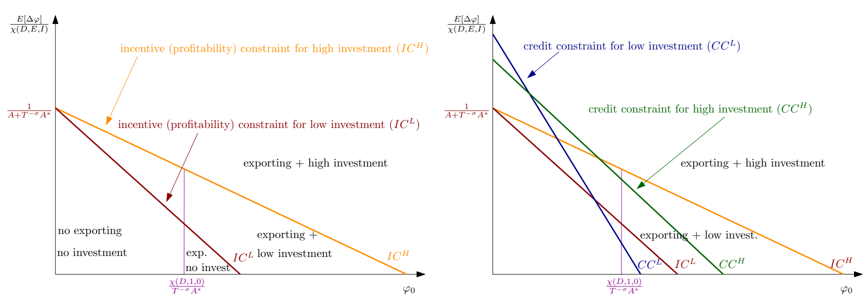
where $\chi(D,1,I) = (1+rD)(F_E+F_I)$ such that $I \in \{L,H\}$ and $F_I \in \{F_L,F_H\}$. As illustrated in panel (a) of Figure 15, the slope of equation (29) is flatter for the high investment threshold since $\chi(D,1,H) > \chi(D,1,L)$, capturing the notion that the low investment profitability threshold binds for a smaller set of firms.

As above, the lender only cares about meeting the minimum needed return and, as such, the credit constraint for high and low investment are analogously described as

$$\frac{\mathbf{E}[\Delta\varphi_I^{CC}]}{\chi(D,1,I)} = \frac{1}{A+T^{-\sigma}A^*}\left(1+\frac{\delta - rK/p_I}{\chi(D,1,I)} + \frac{D(1+r)(1-p_I)}{(1+rD)p_I}\right) - \frac{\varphi_0}{\chi(D,1,I)}. \tag{30}$$

As depicted in panel (b) of Figure 15, the credit constraint (30) is also flatter for high investment. The intercept of equation (30) is generally smaller for high investment relative to low:[50] among low productivity firms, higher investment increases the probability of success and disproportionately decreases the likelihood of default.

Figure 15: Exporting, low investment & high investment



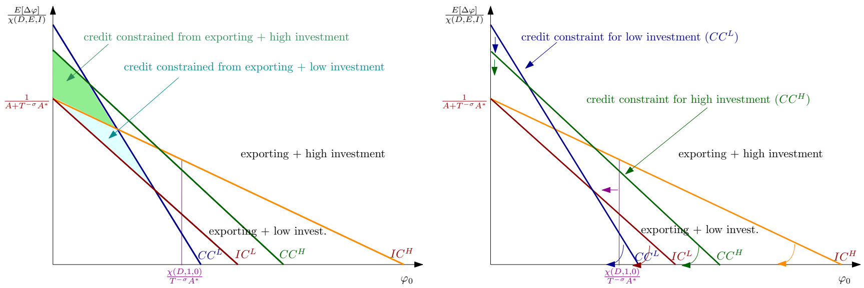
(a) Incentive Constraints (b) Credit Constraints

Notes: The IC^H and IC^L curves capture the incentive constraints for exporting with high and low investment, respectively, while the CC^H and CC^L curves represent credit constraints for high and low investment, respectively.

Similar to Figure 3a in Section 2, Figure 17a highlights two different types of constrained firms. In light blue we observe the set of firms that would optimally prefer to export and make a small investment; these firms are analogous to those described in Figure 3a. Because they are unable to secure any type of financing, they remain non-exporting and non-investing firms. The area in light green captures firms that want to both export and make a large investment. Access to credit for these firms would induce both exporting and a disproportionate increase in the expected productivity gain from exporting and investment.

[50] Sufficient conditions are that investment costs (F_L, F_H) are large. Alternatively, this conclusion also holds as long as the difference in the probabilities of success (p_H, p_L) is not too large.

Figure 16: Credit constraints, policy reform, & investment size

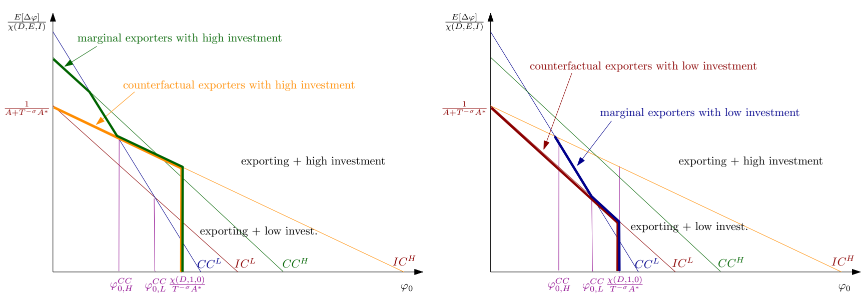


(a) Constrained Firms (b) Reform w/ Binding Constraint

Notes: The area shaded in green represents firms that are credit constrained from exporting but would optimally prefer to export and make a large investment. The IC^H and IC^L curves capture the incentive constraints for high and low investment, while the CC^H and CC^L curves represent binding credit constraints for the same investment sizes.

In Figure 16 we observe that allowing for multiple investment options reinforces the notion that policy reform changes both the opportunities and constraints faced by the firm. Regardless of the structure for the firm's investment decision, we continue to expect meaningful correlation between policy reform and exporting regardless of whether any particular credit constraint binds.

Figure 17: Heterogeneous returns & identification



(a) High Investment (b) Low Investment

Notes: In panel (a) (panel (b)) the green (red) and orange (blue) lines document analogous loci for marginal exporters with high (low) investment. The intersection points $\varphi^{CC}_{0,H}$ and $\varphi^{CC}_{0,L}$ distinguish sets of firms credit constrained from exporting with high investment and those constrained from exporting with low investment, respectively.

Figure 17 documents exporting and investment productivity thresholds in a model with multiple investment options. In Figures 18a and 18b we identify the threshold $\varphi^{CC}_{0,L}$, which distinguishes firms for which the credit constraint is binding. The threshold $\varphi^{CC}_{0,H}$ further subdivides constrained firms into those that may benefit from disproportionate productivity growth (the region to the left of $\varphi^{CC}_{0,H}$) and those that do not (between $\varphi^{CC}_{0,H}$ and $\varphi^{CC}_{0,L}$).

D Further robustness on the instrument

In the main part of the paper we estimate a positive and significant relationship between the probability of a Canadian manufacturing plant exporting and the change in U.S. tariffs with the rest of the world, between 2010 and 2000. This section provides further supportive evidence by estimating the impact of changes in U.S. tariffs with the rest of the world on the change in overall Canadian exports. First, we use U.S. import data at the 10-digit HS level from the U.S. Census and aggregate them to the 5-digit NAICS level. This is the level of aggregation we use in the main part of the paper. The second step is to estimate the following relationship:

$$\Delta_{2010,2000} \log(M_i^{CA}) = \beta \Delta_{2010,2000} \log(1 + t_i^{ROW}) + X_i + u_i, \tag{31}$$

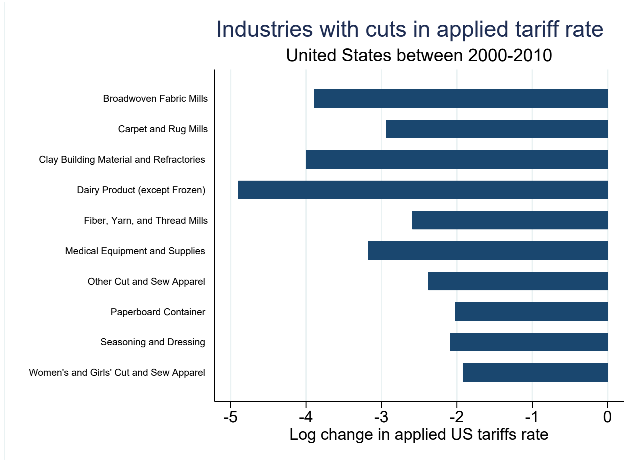
where $\log(M_i^{CA})$ is the log change of Canadian exports to the U.S. between 2010 and 2000 in a 5-digit NAICS industry i, $\Delta_{2010,2000} \log(1 + t_i^{ROW})$ is the log change of U.S. tariffs with the rest of the world between 2010 and 2000 in industry i and X_i are additional control variables. Note that we define U.S. tariff changes with the rest of the world as follows:

$$\Delta_{2010,2000} \log(1 + t_i^{ROW}) = \log(1 + \sum_j^N w_{2000,ij} t_{2010,ij}^{ROW}) - \log(1 + \sum_j^N w_{2000,ij} t_{2000,ij}^{ROW}), \tag{32}$$

where $t_{t,ij}$ is the average U.S. tariff rate applied to country j in industry i in year t and $w_{2000,ij}$ is the share of country j's imports in overall U.S. imports in industry i in the year 2000. Figure 18 plots the largest changes (increases and decreases) in applied tariff rates with the rest of the world per 5-digit NAICS industry.

Figure 18: Main 5-digit NAICS industries with applied tariff changes

(a) Tariff increasing industries



(b) Tariff decreasing industries

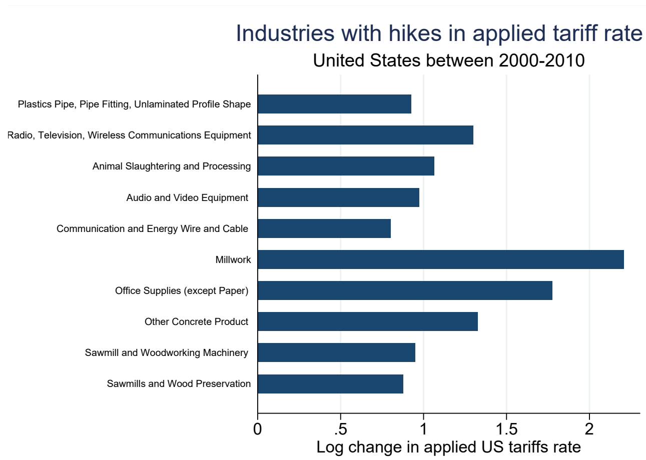


Table 13: U.S. imports from Canada and tariff changes vis-á-vis the Rest of the World

Dependent variable:	Change log (US imports from Canada)			
	(1)	(2)	(3)	(4)
Change log(U.S. tariff with Rest of the World)	0.208***	0.120*	0.165***	0.157**
	(0.060)	(0.069)	(0.057)	(0.069)
log(initial labour productivity)		0.154**		0.051
		(0.072)		(0.077)
log(initial size)		0.151**		0.153***
		(0.059)		(0.057)
Change log(import from China)			-2.141***	-1.851***
			(0.419)	(0.554)
Number of observations	162	142	159	139
R2	0.070	0.131	0.203	0.198

Notes: Standard errors are in parentheses.

Table 13 plots the results of equation 31. Column 1 shows that a 10 percentage point increase in the U.S. applied tariff rate with the rest of the world increases Canadian exports by 2 percent. This estimated increase is robust to the addition of several control variables. In column 2 we add the log of initial labour productivity (defined as real value added divided by employment) and the log of initial size (defined as total sales) in a specific industry in the year 2000 as additional controls. In column 3 we add the log change in imports from China between 2010 and 2000. The results show that an increase in Chinese exports to the U.S. reduced imports from Canada but the coefficient of the change in the applied tariff rate with the rest of the world remains significant. This result holds in column 4 when we add all three controls to the regression. Overall, these results suggests that changes in the U.S. applied tariff rate with the rest of the world significantly affect Canadian exports to the U.S. and supports our findings in the first stage probit regression on the entry decision of Canadian manufacturing plants.

Finally, we provide additional evidence that U.S. tariff changes 2010 and 2000 significantly affected U.S. import flows. To do so, we estimate the impact of changes in applied bilateral tariff rates at the 5-digit NAICS level on changes in bilateral trade flows. The regression specification looks as follows:

$$\Delta_{2010,2000}\log(M_{ij}) = \beta\Delta_{2010,2000}\log(1 + t_{ij}) + f_i + f_j + u_{ij}, \tag{33}$$

where $\log(M_{ij})$ is the log change of exports of country j to the U.S. between 2010 and 2000 in a 5-digit NAICS industry i, $\Delta_{2010,2000}\log(1 + t_i^{ROW})$ is the associated log change of U.S. applied tariffs to country j between 2010 and 2000 in industry i, and f_i and f_j are country-specific and industry-specific fixed-effects, respectively. The results in Table 14 show that the implied tariff elasticity is -6.2 without fixed-effects (see column 1) and -4.2 with fixed-effects (see column 2). These estimates are in the range of the recent literature on the tariff elasticity (see, e.g., Head and Mayer (2014)) and show that the change in the applied tariff rate at the 5-digit NAICS level leads to consistent estimates on trade flows.

Table 14: Bilateral U.S. Imports and Tariff Changes

Dependent variable:	Change log (bilateral imports) (2010–2000)	
	(1)	(2)
Change log(bilateral tariff), 2010–2000	-6.177***	-4.145**
	(1.262)	(1.758)
Country fixed-effects	no	yes
Industry fixed-effects	no	yes
Number of observations	8,301	8,301
Adjusted R2	0.012	0.271

Notes: Standard errors are in parentheses.

E Measurement Robustness: Labour Productivity

Table 15 repeats the benchmark exercises but employs labour productivity in place of total factor productivity. The qualitative impact of individual co-variates and the statistical significance of each treatment effect are largely unchanged relative to our benchmark results for TFP in Table 5. However, there is a substantial increase in the magnitude of the measured treatment effect. Annualized, average labour productivity among Canadian manufacturers that entered export markets is estimated to more than double each year. In contrast, the estimated treatment effects for both investment and leverage change very little when we employ labour productivity as a control variable in columns 2 and 3. This in turn suggests that the large increase in the productivity treatment effect is unlikely to be driven by the substitution of initial labour productivity for initial TFP as a co-variate.

Instead, the stark quantitative differences across productivity outcome variables highlight the importance of productivity measurement in this context. Although labour productivity is a transparent measure of efficiency, it is complicated by the fact that it captures numerous underlying changes. In addition to TFP growth, labour productivity changes reflect differences in input use across firms, biased technological upgrading or increasing returns to scale. Increasing returns to scale are of particular consequence in this setting, since U.S. export markets are often much larger than domestic Canadian markets. To the degree that standard measures of TFP adjust for these differences, we expect that they capture a more accurate estimate of changes in firm efficiency.

Table 15: Productivity Measurement Robustness

Dependent variable:	Log-change labour productivity	Change in leverage ratio	Log-change in tangible assets
Non-exporters			
|Initial leverage - av. initial leverage|	0.6065	0.1752	0.1217
	(0.1320)	(0.1109)	(0.1369)
Initial labour productivity	-0.3579	-0.0058	0.0117
	(0.1389)	(0.0190)	(0.0233)
Average initial leverage ratio	0.6087	0.4072	-0.6189
	(0.2772)	(0.1866)	(0.1870)
log(age)	-5.8918	6.3966	-19.6035
	(3.8448)	(2.8005)	(3.5902)
log(age) squared	1.1519	-1.1074	3.2778
	(0.6867)	(0.4978)	(0.6418)
Initial net size	-0.5608	-0.0367	-0.0278
	0.0942	0.0571	0.0576
Difference: exporters vs. non-exporters			
|Initial leverage - av. initial leverage|	-0.8132	1.1570	-1.3262
	(0.5460)	(0.4660)	(0.4996)
Initial labour productivity	-0.0091	0.0023	0.0301
	(0.3788)	(0.0619)	(0.0665)
Average initial leverage ratio	-1.6493	-1.6456	2.1259
	(0.7460)	(0.5596)	(0.7139)
log(age)	16.8925	-9.1108	39.3125
	(10.4040)	(9.0950)	(12.7908)
log(age) squared	-3.1512	1.6335	-6.8811
	(1.8321)	(1.6140)	(2.2952)
Initial net size	1.0575	0.0393	0.2993
	(0.4832)	(0.2320)	(0.1944)
Control function			
Polynomial 1	-19.2343	-1.4879	-6.9772
	(7.5858)	(4.1016)	(3.4247)
Marginal effects			
ATE	0.1538	0.5158	-0.8800
	(1.9359)	(0.9828)	(0.9170)
ATT	6.3376	0.9799	1.3951
	(1.0324)	(0.5906)	(0.6394)
ATUT	-2.2695	0.3318	-1.7762
	(2.8498)	(1.4651)	(1.3092)
LATE	3.2714	0.7661	0.3312
	(0.7654)	(0.4565)	(0.5494)
Observable heterogeneity (p-value)	(0.0000)	(0.0000)	(0.0000)
Essential heterogeneity (p-value)	(0.0000)	(0.0000)	(0.0000)
Number of observations	10,367	10,367	10,367

Notes: This table reports the results from estimating equation (16) using labour productivity, the leverage ratio and angible assets as outcome variables. We employ the net-size specification in each column. Standard errors are in parentheses.