EXPORTING AND FIRM PERFORMANCE: CHINESE EXPORTERS AND THE ASIAN FINANCIAL CRISIS

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Abstract—We ask how export demand shocks associated with the Asian financial crisis affected Chinese exporters. We construct firm-specific exchange rate shocks based on the precrisis destinations of firms' exports. Because the shocks were unanticipated and large, they are a plausible instrument for identifying the impact of exporting on firm productivity and other outcomes. We find that firms whose export destinations experience greater currency depreciation have slower export growth and that export growth leads to increases in firm productivity and other firm performance measures. Consistent with "learning-by-exporting," the productivity impact of export growth is greater when firms export to more developed countries.

I. Introduction

PARTICIPATION in export markets is often viewed as a prerequisite for economic growth in developing countries. For example, in a report on the East Asian miracle, the World Bank (1993) pointed to export-oriented economic policies as playing a critical role in the region's rapid economic development. Cross-country studies document a positive relationship between trade and growth performance (Sachs & Warner, 1995; Edwards, 1998; Frankel & Romer, 1999), but substantial controversy persists over whether there exists a causal impact of exporting on economic growth. Growth could cause exports, or both growth and exports could be caused by other factors.[1]

A number of papers have empirically examined the relationship between exporting and economic performance using firm-level panel data. A robust finding has been that more productive firms enter export markets. For example, Bernard and Jensen (1999) document among U.S. firms that in addition to having higher productivity, exporting firms also have higher employment, shipments, wages, and capital intensity than nonexporters; and Clerides, Lach, and Tybout (1998) find that exporting firms have higher productivity levels on average than nonexporters in several developing countries. However, findings on whether exporting itself increases firm productivity have been much more mixed.[2] Two papers using firm data from China by Kraay (1999) and Zhang (2006) find positive evidence for learning by exporting.

One weakness of all of these studies is that they cannot distinguish clearly between the effects of exporting and the unobservable differences between exporting and nonexporting firms. Typically, change in firm productivity or other performance measures is regressed on initial exporter status and other initial period controls using OLS, or the level of firm performance is regressed on current or lagged export status in addition to other controls. In the latter case, further lags are sometimes used as instruments, relying on assumptions about the underlying dynamic model (Kraay, 1999; Van Biesebroeck, 2005).

Since the decisions to export and how much to export are endogenous choices of the firm, these empirical specifications fail to convincingly isolate the causal effect of exporting on firm productivity. It is easy to imagine ways in which export status could be correlated with unobserved firm characteristics that directly influence both the level and growth rate of firm productivity. For example, dynamic firm managers may be more aggressive in entering export markets and also be more adept learners or more aggressive in making productivity-enhancing investments. One way to control for selection bias is to jointly estimate an equation for participation in export markets using full information maximum likelihood (Clerides et al., 1998). However, this more structural approach does not solve the fundamental identification problem and may be sensitive to functional form assumptions about the joint error distribution (Bigsten et al., 2004). Another approach to reduce selection bias is the use of matching estimators (Girma, Greenaway, & Kneller, 2004; Fernandez & Isgut, 2005; Zhang, 2006). Matching can eliminate bias caused by selection on observables but cannot address bias associated with unobservable firm characteristics.

Conceptually, the fundamental problem is that nonexporters are an inappropriate counterfactual for exporters. One requires a benchmark for how exporters would have performed if they had not exported or if their exports had been lower. A hypothetical randomized experiment assessing the impact of exporting on firms might involve randomly as-

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[1] See Rodriguez and Rodrik (2001) and Irwin and Terviö (2002), for example.

[2] Papers that find little or no evidence of learning by exporting include Aw et al. (2000), Bernard and Jensen (1999), Clerides, Lach, and Tybout (1998), and Delgado, Farinas, and Ruano (2002). Papers that find positive evidence of learning by exporting include Alvarez and Lopez (2005), Bigsten et al. (2004), Blalock and Gertler (2004), Castellani (2002), Fafchamps, el Hamine, and Zeufack (2008), Fernandez and Isgut (2005), Girma et al. (2004), Kraay (1999), Van Biesebroeck (2005), and Zhang (2006).

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signing shocks to export demand across firms. For example, one group of firms might be assigned higher growth in the demand for their goods by foreign customers, while a second group would face lower growth in foreign demand. In this setting, the impact of exporting would then be easily identified by comparing the change in outcomes for the firms experiencing high demand growth for their exports with the corresponding change for firms experiencing low growth in demand.

This study exploits a natural experiment—Chinese exporting during the Asian financial crisis—that in key respects approximates the randomized experiment just described. In June 1997, the devaluation of the Thai baht led to speculative attacks on many other currencies worldwide. While the Chinese yuan remained pegged to the U.S. dollar, many important destinations for Chinese exports experienced currency depreciations due to the crisis (both nominal and real). For instance, between 1995 and 1998, the period investigated in this study, the Japanese, Malaysian, and Korean currencies depreciated in real terms against the U.S. dollar by 31%, 34%, and 43%, respectively. At the other extreme, the British pound and the U.S. dollar experienced real appreciations against the yuan by 14% and 7%. Because the exchange rate changes varied so widely, two observationally equivalent firms faced very different export demand shocks if one happened to export its goods to Korea and the other happened to export to the United Kingdom.

The construction of firm-specific exchange rate shocks is made possible by the availability of information on firm-specific export country destinations for foreign-invested firms in China's industrial census of 1995. These data are linked to enterprise survey data for the same firms in 1998 and 2000. We use the weighted average real depreciation experienced by a firm's precrisis trade partners as an instrument for the change in firm exports from before to after the crisis.[3]

Because the timing and pattern of devaluations due to the crisis were unforeseen, this instrumental variable approach plausibly satisfies the requirement that the instrument (an exchange rate shock index) be uncorrelated with the ultimate outcomes of interest except via the channel of interest (the change in exports). An attractive aspect of this approach is that exchange rate shocks are firm specific, so we can control for province-sector fixed effects and thus rule out bias from unobserved changes affecting specific sectors in each region. Another advantage of our study is that China did not suffer from a currency crisis itself during the Asian financial crisis, but rather experienced relatively stable economic policies and economic performance during this period.[4]

Using this identification strategy, we examine whether and how instrumented changes in exports affect measures of firm performance. We find that increases in exports are associated with improvements in total factor productivity, as well as improvements in other measures of firm performance such as total sales and return on assets. Our estimates indicate that a firm experiencing an exogenous 10% increase in exports enjoys productivity improvements of 11% to 13%, or nearly one-eighth (13%) of the sample mean productivity improvement from 1995 to 2000.

Additional results provide suggestive evidence that the association between increases in exports and productivity improvements reflects "learning by exporting," for example, by inflows of advanced technology or production techniques from overseas export customers. We find that changes in exports are more positively associated with productivity improvements in firms exporting to destinations with higher per capita GDP, which presumably have more advanced technologies.

A crucial question is whether some unobserved characteristics of firms correlated with the exchange rate shocks might be the true causal factor behind the observed productivity changes. Firms were not randomly assigned the exchange rate shocks, and so those experiencing better shocks might have experienced differential increases in productivity even in the absence of the shock. While we cannot in principle rule out all such concerns, we address this issue by gauging the stability of the regression results to accounting for changes in outcomes that are correlated with a comprehensive set of firms' preshock characteristics. The estimated impact of changes in exports (instrumented by the exchange rate shock) is little changed (and, when the outcome of interest is firm productivity, actually becomes larger in magnitude) when a comprehensive set of preshock firm characteristics is included in regressions, supporting the causal interpretation of the results.

The Chinese case is particularly interesting for studying the effect of exporting on firm outcomes because in recent years, China's export growth has been phenomenal, and China has emerged as one of the world's largest exporters. From 1990 to 2000, Chinese exports nearly quadrupled

[3] Lack of export data at the firm level for 1996 and 1997 requires us to use 1995 as our base year. This strategy of obtaining exogenous microlevel variation from overseas exchange rate shocks is analogous to the approach used in Yang (2006, 2008), which focuses on household-level variation in exchange rate shocks experienced by overseas migrants. Earlier papers using exchange rate shocks as exogenous variation include Revenga (1992) and Bertrand (2004).

[4] One previous study by Maurin, Thesmar, and Thoenig (2002) uses firm-specific exchange rates as an instrument to examine the effect of exporting on the skill intensity of French firms. The authors use the average real exchange rate with respect to two currencies (the U.S. dollar and German deutschmark) weighted by EU and non-EU export shares prior to the period of study to instrument for the ratio of exports to domestic sales. With only two exchange rates, changes in firm-specific exchange rates could easily be correlated with initial export destination shares if relative exchange movements with the U.S. dollar and deutschmark are persistent. Also, unlike the Asian financial crisis, in the French case the extent and cause of exchange rate changes is not clear. The authors do not report first-stage results and do not examine the effects of exporting on productivity.

from US$88 billion to US$330 billion.[5] China's export growth rate was the sixth highest in the world in the 1990s,[6] and by 2000, it had become the world's eighth largest exporter. There also is evidence that during the 1990s, the technological sophistication of Chinese exports increased substantially (Schott, 2006; Rodrik, 2006).

This paper is related to other work that has used sudden trade liberalizations or currency crises in specific countries as exogenous shocks to firms, comparing firm-level outcomes before and after the regime change. Increases in exporting driven by the 1994 Mexican peso crisis have been shown to lead to increases in wage premia and wage inequality that rise with initial productivity (Verhoogen, 2008; Kaplan & Verhoogen, 2005; Fung, 2008). Pavcnik (2002) finds that trade liberalization in Chile led to greater productivity improvements in plants that were import competing. Our paper differs in that we examine shocks that are heterogeneous across firms (unlike the Mexican currency crisis), are not based on potentially endogenous government actions (unlike trade liberalizations), and are not caused by major crises or regime changes that are likely to be correlated with other economic or policy changes.

The remainder of this paper is organized as follows. Section II provides a brief discussion of potential causal effects of exporting on firm performance. We provide an overview of our empirical strategy in section III. In section IV, we describe our data sources and the construction of key variables. We then turn to the first-stage regression results in section V and the IV results in section VI. Section VII describes how the effect of exporting on productivity differs according to the income level of firms' export destinations. Section VIII presents robustness checks and provides additional discussion. Section IX concludes.

II. Pathways for the Impact of Exports on Firm Productivity

The literature has identified a number of channels through which exporting may affect firm productivity. First, overseas buyers may provide technical assistance to exporters to improve production efficiency, as suggested by Grossman and Helpman (1991) and Evenson and Westphal (1995). Westphal, Rhee, and Pursell (1981) document such practices among foreign buyers from Korean exporting firms. Second, greater participation in international trade could improve firms' access to knowledge about more advanced production technologies (as in the model of Clerides et al., 1998) or the willingness of partners in foreign-invested firms to transfer technology. Third, higher-quality standards in international markets compared to domestic markets could provide greater incentives for firms to upgrade production technologies (Verhoogen, 2008). Fourth, export participation may lead to faster learning about market opportunities for new products or how to tailor products to the specific needs of individual buyers (Fafchamps, el Hamine, & Zeufack, 2008; Maurin, Thesmar, & Thoenig, 2002). Fifth, exporting can increase capacity utilization by expanding sales, which also reduces firms' vulnerability to occasional downturns in the domestic market (World Bank, 1993). This channel can affect firm productivity independent of any learning.

Most studies of the link between exporting and firm productivity focus on the extensive margin of exporting, asking whether mere participation in the export market affects firm outcomes. However, the above pathways could just as easily operate on the intensive margin, where firms continue to improve productivity as they expand their export activity. For example, investments in productivity-enhancing technologies might be lumpy, and so firms may wait until they reach a certain level of exports before making such investments. Other studies in international trade have also examined the intensive margin of exporting. Such studies have mostly focused on how productivity gains are related to the number of years that a firm has exported. A number of these studies have found evidence that learning is greater among younger firms, consistent with Arrow's learning-by-doing model (Alvarez & Lopez, 2005; Delgado, Farinas, & Ruano, 2002; Fernandez & Isgut, 2005; Girma et al., 2004), while others have found more persistent effects (Blalock & Gertler, 2004; Kraay, 1999). Other studies have examined how firm productivity gains are related to export intensity, measured by the share of sales that are exported or by the amount of exports after controlling for sales amount. Again, some have found a significantly positive effect of export intensity on productivity growth (Castellani, 2002; Girma et al., 2004; Kraay, 1999), while others have found no large or statistically significant relationship (Aw, Chung, & Roberts, 2000; Blalock & Gertler, 2004; Clerides et al., 1998).

III. Empirical Approach

We estimate the impact of exporting on various firm-level outcomes. Consider the following regression equation for outcome Y_{it} for firm i observed in year t:

$$Y_{it} = \beta E_{it} + \mu_i + \gamma_t + \nu_{it}. \quad (1)$$

In equation (1), E_{it} is log of export value. μ_i is a fixed effect for firm i, γ_t is a year fixed effect, and ν_{it} is a mean-zero error term. We work with the first-differenced specification of this equation to eliminate time-invariant characteristics of firms that may be associated with both exports and the outcome variable:

$$\Delta Y_{it} = \delta + \beta \Delta E_{it} + \varepsilon_{it}. \quad (2)$$

[5] U.S. dollar figures are real, base 1995. Export data are from the World Bank's WDI 2004 data set.

[6] Only Yemen, South Korea, Ireland, Guinea-Bissau, and Mozambique had faster export growth. Chinese export performance is even more striking given that these other countries started the period from significantly lower base levels (with the exception of South Korea, whose export volumes are comparable with China's).

Here, δ is a constant equal to the change in year fixed effects ($\gamma_t - \gamma_{t-1}$) and ε_{it} is the error term, equal to $\nu_{it} - \nu_{it-1}$. Due to the characteristics of the data described below, changes place between the years 1995 and 1998 and between the years 1995 and 2000.

A problem with estimating this regression equation via ordinary least squares is that the coefficient on change in log exports, β, need not represent the causal effect of exports on the outcome variable for the reasons described earlier. It is therefore important to isolate a source of variation in firms' exports that is exogenous with respect to firm outcomes. As an instrument for firm exports, we use an exchange rate shock index defined as the weighted average real currency depreciation experienced by the firm's precrisis trade partners, derived explicitly below. We posit that firms whose trade partner countries experienced larger depreciations should see larger declines in exports. Our strategy, then, is to examine whether and how these instrumented changes in exports are associated with changes in firm performance.

A simple version of the first-stage regression equation is:

$$\Delta E_{it} = \alpha_0 + \alpha_1 SHOCKINDEX_{i98} + \psi_{it}. \tag{3}$$

Here, α_0 is a constant term and ψ_{it} is a mean zero error term. Because the impact of the exchange rate shocks on changes in firm exports may vary across firms with differing initial characteristics, we also examine a first-stage equation where the shock index is interacted with a vector of 1995 firm characteristics W_{i95}, which are also separately included as regressors:

$$\begin{aligned}\Delta E_{it} = {} & \alpha_0 + \alpha_1 SHOCKINDEX_{i98} \\ & + \beta'(SHOCKINDEX_{i98} * \mathrm{W}_{i95}) + \gamma'\mathrm{W}_{i95} + \psi_{it}.\end{aligned} \tag{4}$$

The predicted value of the change in exports from the first stage, $Pred\Delta E_{it}$, is used instead of ΔE_{it} in the second-stage regression:

$$\Delta Y_{it} = \delta + \beta Pred\Delta E_{it} + \gamma' W_{i95} + \varepsilon_{it}. \tag{5}$$

As is standard using 2SLS, coefficient standard errors are adjusted to account for the fact that the regressor is a predicted value. For β to be an unbiased estimate of the impact of the change in log exports on the change in the outcome variable, it must be true that the instrument affects only the dependent variable via the endogenous independent variable (the change in log exports), and not through any other channel. We address and provide evidence against potential violations of this exclusion restriction in section VII.

In addition, for β to be an unbiased estimate, it must also be true that the instrument for exports, the shock index, is not correlated with ongoing time trends or other shocks affecting changes in firm performance. The assumption is violated if firms exporting to countries that experienced greater depreciations were different from other firms with respect to unobserved initial (preshock) characteristics, and if changes in the outcomes would have varied according to these same characteristics even in the absence of the exchange rate shocks.

To control for this possibility, we include a vector of precrisis (1995) firm characteristics X_{i95} on the right-hand side of the estimating equation:[7]

$$\Delta Y_{it} = \delta + \beta Pred\Delta E_{it} + \omega' X_{i95} + \varepsilon_{it}. \tag{6}$$

This vector of precrisis firm characteristics includes firm variables for 1994 as well as 1995 in order to control for differences in initial levels as well as preshock trends. In order to verify whether the regression results are in fact contaminated by changes associated with precrisis firm characteristics, we examine whether the estimates are qualitatively similar when we exclude the vector of precrisis characteristics from the regressions.[8] It turns out that many of the control variables predict both the magnitude of exchange rate shocks and changes in firm performance, but the estimated effects of exports on outcome variables are relatively insensitive to the inclusion of the controls.[9]

In many contexts, positive correlation in the error terms across similar observations biases standard errors downward (Moulton, 1986). In the context of our study, there could be correlation among the shocks experienced among firms exporting to the same or similar locations. We therefore report standard errors that account for arbitrary covariance structures within clusters, where we define a cluster as all firms reporting the same primary (largest) export destination.

[7] X_{i95} includes the vector of variables interacted with the shock index, W_{i95}. The analogous first-stage equation predicting the change in log exports also necessarily includes the full set of control variables X_{i95}.

[8] The vector of precrisis control variables includes fixed effects for province-industry combinations (of which there are between 300 and 400 depending on the specification); 1995 log sales income; 1994 log sales income; 1995 share of exports to top two destinations; indicator for firm existing in 1994; indicator for firm exporting in 1994; foreign share of ownership; log of industry weighted average exports to 1995 destinations (weighted by firm's 1995 export destinations), separately for 1993 and 1996; indicator variables for firm size categories; 1995 exports as share of firm sales; 1994 exports as share of firm sales; indicator for firm exporting entire output in 1995; log exports in 1995; log exports in 1994; 1995 log capital-labor ratio; 1995 log productivity (Levinsohn-Petrin estimate); 1995 fraction of firm exports destined for Hong Kong; and log 1995 weighted average per capita GDP in firm's export destinations (weighted by firm's 1995 exports).

[9] Table A1 presents coefficient estimates from regressions of the firm's exchange rate shock on a number of preshock (1995) firm characteristics. The first regression presents coefficient estimates without including province-industry fixed effects, and the second regression includes these fixed effects. Several individual variables are statistically significantly different from 0 in both regressions, indicating that firms' export destinations experienced greater depreciations if their industry had smaller log exports to those destinations, their industry experienced greater growth (from 1993 to 1995) in exports to those destinations, the firm exported a higher share of its total exports to its top two destinations, the firm exported to higher-income destinations, and the firm had higher capital per worker.

IV. Data Sources and Key Variable Definitions

The firm-level data used in this paper come from two data sets maintained by China's National Bureau of Statistics (NBS). Data for 1995 come from China's decennial industrial census, and data for 1998 and 2000 come from NBS's annual industrial enterprise survey. The 1995 industrial census includes detailed data on all firms belonging to the township administrative level or above.[10] The annual industrial enterprise survey, on the other hand, includes firms with annual sales income above 5 million yuan, regardless of administrative level. Provision of survey information by firms is compulsory under Chinese law, and local statistical bureau offices require that firms verify or correct data suspected of being inaccurate. Unfortunately, in 1996 and 1997, data were kept for only a subsample of very large enterprises, making data from those years unsuitable for analysis.

The 1995 industrial census required firms to report a full set of firm accounting data on revenue, expenditures, exports, investment (including R&D investment), labor, capital, and intermediate inputs. In addition, foreign and joint venture firms (but not other firms) were asked to identify their top two export destination countries and the value of exports to each. In the annual industrial enterprise survey, firms report similar accounting information but provide no information on trading partners. Each firm in the two data sources has a unique identifier code, so it is possible to link observations across years to create a firm panel data set.

Because the key innovation of this paper involves constructing exchange rate shocks from information on firms' export destinations prior to the 1997 Asian financial crisis, we focus our analysis on foreign and joint venture firms (those with a positive foreign ownership share) that had positive exports in 1995.

All economic variables are expressed in real 1995 terms using province-level producer price indices obtained from the NBS. In 1997 and 1998, provincial-level producer price indices (PPIs) are used as deflators. In 1996, only a national producer price index is available, which we adjust to each province based on province-specific trends.[11] Real exchange rate data for destination countries of Chinese exports were constructed using nominal exchange rates and consumer price indices obtained from the World Bank's World Development Indicators 2004 for all countries except Taiwan. Nominal exchange rate data for Taiwan come from Bloomberg, LP, while the Taiwanese CPI was obtained from the Statistical Bureau of the Republic of China (http://eng.stat.gov.tw).

The analysis also makes use of disaggregated export data for China and reexport data for Hong Kong from the U.N. Comtrade data set.

One might worry that restricting the sample to foreign-invested firms somewhat reduces the generalizability of our results. However, FDI firms account for a large and increasing share of exports in China and throughout the rest of the world. Foreign-invested firms accounted for 31.5% of total Chinese exports in 1995, 44.1% in 1998, and 57.1% in 2004 (China Statistical Yearbook, 2005). Most Chinese exports are processed exports tied to vertical production networks; since 1995 processed exports have accounted for the majority of China's total exports (Lemoine & Unal-Kesenci, 2004). This type of trade, especially in intermediate inputs, accounts for a large share of the recent growth in world trade (Hummels, Ishii, & Yi, 2001), and much of it is controlled by multinationals. For instance, in the United States, multinationals account for over half of total exports (Slaughter, 2000).

Also, in the Chinese context, because many Chinese domestic firms were publicly owned during the period of study, restricting attention to the more market-oriented foreign-invested firms may actually make our results better reflect the effects of exporting in open market environments prevalent elsewhere and so make the results more generalizable.

Still, it is important to consider the ways in which learning by FDI firms might differ from learning by domestic firms. It could be the case that learning opportunities from exporting are fewer for foreign-invested firms because foreign investors provide state-of-the-art technology. Indeed, there is considerable evidence that FDI firms have higher productivity than domestic firms throughout East Asia, including China (Hallward-Driemeier, Iarossi, & Sokoloff, 2002). In that case, we would expect FDI firms to exhibit less learning than domestic firms, and so our estimates could be interpreted as lower bounds. However, many aspects of learning are likely to be similar for FDI and domestic firms, especially when the export destination country is not the same as the source of the FDI. It is also plausible that foreign ownership is complementary to learning by exporting if foreign partners put pressure on export partners to transfer technology to suppliers or invest in the firm's learning capacity.

A. *Defining Firm-Specific Exchange Rate Shocks*

We use the weighted average real depreciation experienced by a firm's precrisis trade partners as an instrument for the change in firm exports between 1995 and 1998. Two steps are involved in creating this variable. First, the change in the real exchange rate is constructed for each trading partner country. Let the set of all Chinese export destination

[10] Data are for firms, not establishments. All firms in China are supervised by a specific administrative level of government. China's administrative structure includes the following geographic levels, from largest to smallest: provinces, prefectures, counties, townships, and villages. Cities are divided into districts and neighborhoods. The 1995 industrial census also collected some basic information on village-level firms, but the level of detail was insufficient for analysis.

[11] We regress provincial PPIs for the years 1997 to 2003 on the national PPI, provincial consumer price indices (CPIs), and provincial retail price indices (RPIs) and include provincial fixed effects. The provincial CPIs and RPIs do not increase the fit of these regressions, so coefficients from a parsimonious specification with the national PPI and provincial fixed effects are used to estimate provincial PPIs in 1996.

countries be indexed by j (from 1 to J). For each destination j, the change in the real exchange rate vis-à-vis the Chinese yuan is

$$ERCHANGE_{j98} = [\ln(E_{j98}) - \ln(P_{j98})] - [\ln(E_{j95}) - \ln(P_{j95})], \quad (7)$$

where E_{jt} is the nominal exchange rate (currency units per yuan) and P_{jt} is the price level (consumer price index) for destination j in year t.[12]

The second step is to construct a firm-level exchange rate shock variable. Let firms be indexed by i, and let s_{i1} be the 1995 share of firm i's exports that went to its top destination country, and let s_{i2} be the share exported to the second most important destination country.[13] The firm-level real exchange rate shock measure is

$$SHOCKINDEX_{i98} = s_{i1}ERCHANGE_{1,98} + s_{i2}ERCHANGE_{2,98}. \quad (8)$$

In other words, for a firm exporting to just one country j in 1995, the shock index is simply $ERCHANGE_{1,98}$. For firms exporting to more than one foreign country in 1995, that firm's shock index is the weighted average real exchange rate change across those destination countries, with each destination's exchange rate change weighted by the share of 1995 exports going to that country. It is important that the shock index is defined solely on the basis of export destinations prior to the 1997 crisis, to eliminate concerns that export destinations might be endogenous to the shock. For instance, firms might shift the composition of their exports to destinations experiencing better exchange rate shocks.

We modify the shock index when firms report Hong Kong as one of their export destinations, which is the case for 47.4% of firms. Nearly all Chinese exports to Hong Kong are reexported (Feenstra & Hanson, 2004), so that the relevant exchange rate change is not with respect to the Hong Kong dollar but rather with respect to the ultimate export destination. However, firms do not report the ultimate destination of their shipments to Hong Kong.[14] We therefore assume that any shipments to Hong Kong are distributed to third countries in proportions equivalent to the distribution of Hong Kong reexports of products in the firm's industrial sector.[15] We then use Hong Kong reexport destination shares by sector to construct weighted average real exchange rate shocks by sector and assign the sector-specific shock index to the portion of each firm's exports that go to Hong Kong.

Formally, the real exchange rate change for Hong Kong reexports in sector m is taken to be

$$ERCHANGE_{m98}^{HongKong} = \sum_{j \neq HongKong} k_{mj95} ERCHANGE_{j98}, \quad (9)$$

where k_{mj95} is the share of reexports destined for country j in Hong Kong's total reexports of sector m in 1995. $ERCHANGE_{j98}$ is as defined before. This sector-specific real exchange rate change for Hong Kong is then used for firms in sector m in calculating $SHOCKINDEX_{i98}$.

B. Productivity Measurement

Firm-level productivity is a primary outcome of interest in our analysis. We consider two types of productivity measures: an OLS estimator and the estimator proposed by Levinsohn and Petrin (2003) that corrects for bias due to the endogeneity of inputs with respect to productivity.

The OLS estimator assumes that the production technology is Cobb-Douglas and is based on estimation of the following OLS regression equation:

$$y_{it} = \beta_0 + \beta_l l_{it} + \beta_k k_{it} + \varepsilon_{it}, \quad (10)$$

where y_{it} is log value added,[16] l_{it} is log number of employees, k_{it} is log fixed assets, and ε_{it} is a mean zero error term. The residual from this regression is the log of productivity, which we denote θ_{it}^{OLS} for firm i in year t. We use the pooled sample data for 1995, 1998, and 2000.

A problem with the OLS productivity estimator is that it is based on coefficient estimates on capital and labor, which are likely to be biased. Of particular concern is the possibility that firms with higher productivity will have different input use than firms with lower productivity (Olley & Pakes, 1996; Levinsohn & Petrin, 2003). This will lead to biased estimates of the coefficients on capital and labor that cannot be definitively signed in advance. Thus, the OLS productivity estimator will be biased as well. Levinsohn and Petrin (2003) (henceforth LP) propose an estimator that uses intermediate inputs as proxies for productivity, in contrast to the Olley and Pakes (1996) estimator, which uses investment

[12] The calculation does not take into account the change in the Chinese domestic price level because this will not vary across firms and so will be accounted for by the constant term in the empirical analysis.

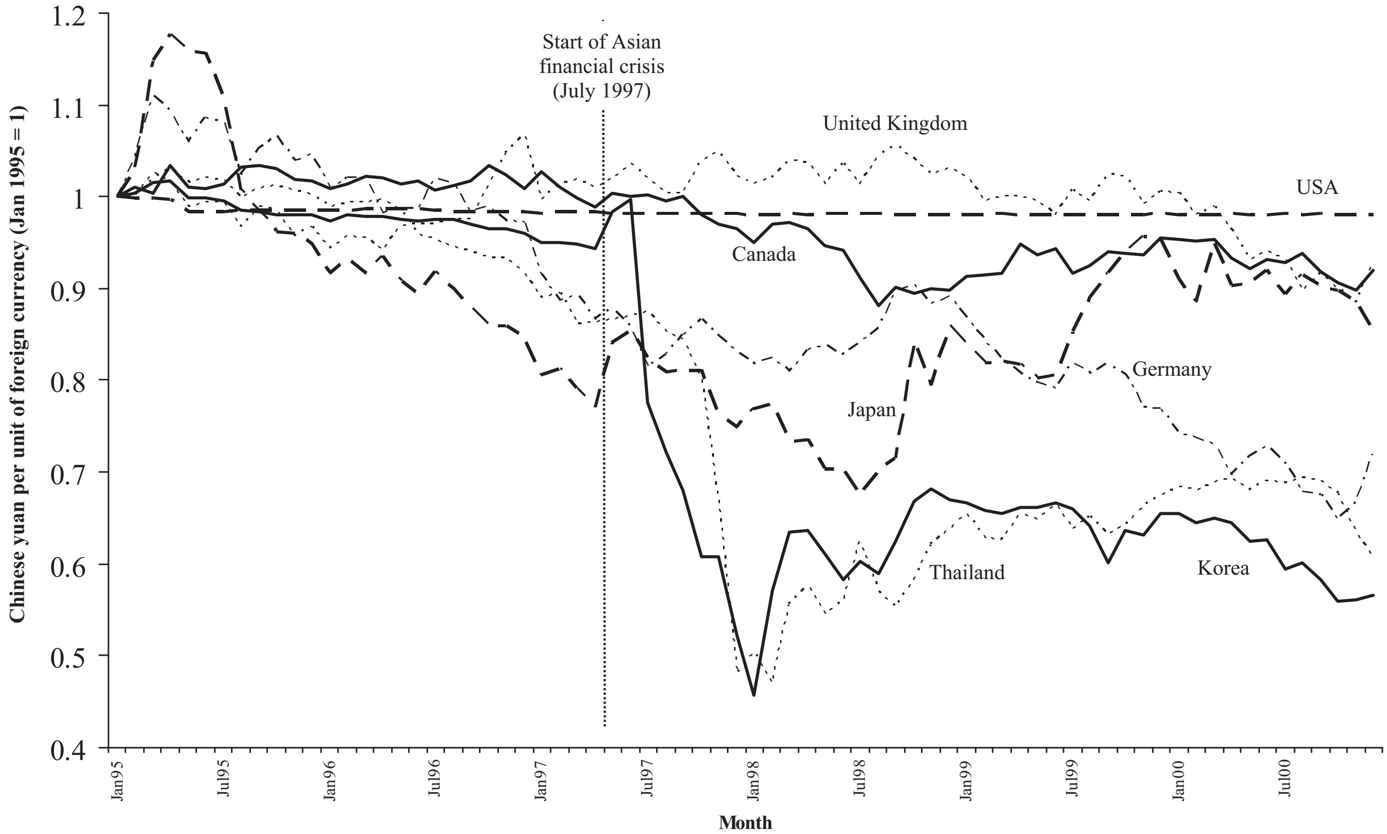
[13] Because the survey asks only about firms' top two export destinations, we construct these shares ignoring any exports going to countries beyond the top two. In practice, this is not a very important assumption because firms' exports turn out to be highly concentrated by destination. In 1995, 77.4% of firms export to only a single country, 83.7% export to no more than two, and in 91.6% of firms, exports to the top two destinations make up three-quarters or more of total exports.

[14] Indeed, they may not even know exactly the ultimate destination of their shipments to Hong Kong if their products are sold to trading companies that later decide where shipments are reexported.

[15] We define 24 sectors that are groupings of HS (1992) two-digit industries into the sector categories used in the Chinese industry classification system.

[16] Value added is explicitly reported in the annual industrial enterprise survey data. In the 1995 industrial census, value added is calculated as current revenue minus intermediate inputs plus value-added tax. For both the OLS and LP productivity estimators, we replace 0 and negative values of value added with 1 before taking logs. This adjustment is necessary for roughly 10% of firms. Regression results are robust to excluding firms with 0 or negative value added.

FIGURE 1.—EXCHANGE RATES IN SELECTED DESTINATIONS OF CHINESE EXPORTS, JANUARY 1995–DECEMBER 2000
CHINESE YUAN PER UNIT OF FOREIGN CURRENCY, NORMALIZED TO 1 IN JANUARY 1995



Note: Exchange rates are as of last day of each month.
Source: Bloomberg LP.

as a proxy. The LP estimator has the advantage that intermediate inputs are typically reported for most firms, while investment is often 0 in data sets of developing country firms. Intermediate inputs also may respond more smoothly to productivity shocks, while adjustment costs may keep investment from responding fully to such shocks. We calculate the LP log productivity estimate, θ_{it}^{LP}, using intermediate inputs as the proxy variable.[17] In the regressions, we examine the total change in productivity from 1995 to either 1998 or 2000 rather than an annualized productivity measure.

V. The Impact of Exchange Rate Shocks on Exports

Figure 1 displays monthly exchange rates for selected major Chinese export destinations expressed in Chinese yuan per unit of foreign currency (normalized to 1 in January 1995).[18] A decline in a particular country's exchange rate should be considered a negative shock to firms exporting to that location: each unit of foreign currency would be convertible to fewer Chinese yuan, making Chinese goods more expensive in real terms.

In the mid-1990s, Chinese exchange rates with other currencies were for the most part quite stable. The largest changes occurred after the start of the Asian financial crisis in July 1997. In particular, real exchange rates in Thailand and Korea plummeted dramatically in that month. In other countries, the changes were less dramatic and sometimes followed slightly different time patterns. Japan, for example, experienced more modest real depreciation through 1998 and then recovered. The German exchange rate actually dipped prior to the crisis, in January 1997. Exchange rate changes in several other major European destinations of Chinese exports (such as France, Belgium, and the Netherlands) closely track Germany's and so are not shown on the graph.

In table 1, we describe the magnitude of real exchange rate changes and export growth between 1995 and 1998 for China's top twenty export partner countries using Chinese export data as reported in the U.N. Comtrade data set. Exports to each country include the value of both direct exports to the country and reexports from Hong Kong.

Among the top twenty trading partners, the four countries whose real exchange rates with respect to the Chinese yuan

[17] We use the estimator implemented as a Stata command and described by Petrin, Levinsohn, and Poi (2004).

[18] The exchange rates in the figure are as of the end of each month, and were obtained from Bloomberg LP.

TABLE 1.—EXPORTS, EXCHANGE RATE SHOCKS, AND CHANGE IN EXPORTS FOR CHINA, 1995–1998
Top Twenty Chinese Export Destinations, 1995

Destination	Shock Index[a]	Change in Ln(exports), 1995–1998[b]	1995 Exports (US$Billions)	% of Total Exports in 1995
United Kingdom	−0.14	0.23	6.9	3.6
United States	−0.07	0.36	54.4	28.0
Panama	−0.03	0.19	1.7	0.9
Russian Federation	−0.02	0.04	1.8	0.9
Italy	−0.01	0.29	3.6	1.8
Brazil	−0.01	0.42	1.8	0.9
Canada	0.04	0.24	3.6	1.8
Spain	0.11	0.32	2.2	1.1
France	0.13	0.30	4.1	2.1
Australia	0.13	0.18	3.6	1.9
Singapore	0.14	−0.30	6.6	3.4
Netherlands	0.15	0.25	5.3	2.7
Belgium-Luxembourg	0.16	0.26	2.0	1.0
Germany	0.16	0.13	11.5	5.9
Philippines	0.23	−0.18	2.5	1.3
Japan	0.31	0.06	37.1	19.1
Thailand	0.32	−0.40	2.7	1.4
Malaysia	0.34	−0.32	2.3	1.2
Republic of Korea	0.43	−0.30	8.5	4.4
Indonesia	0.90	−0.90	2.0	1.0

Source: U.N. Comtrade data set.
Note: Exports to Hong Kong are dropped from the data set, and Hong Kong's reported reexports are considered exports of China to respective destinations. Destinations in table account for 84% of total Chinese exports in 1995.
[a] Change in Log Real Exchange Rate, 1995–1998, expressed as a fraction of the 1995 value (10% depreciation is 0.1, 10% appreciation is −0.1).
[b] From 1995–1998.

experienced the largest depreciations were Indonesia (90%), Korea (43%), Malaysia (34%), and Thailand (32%). These were also the four country destinations with the largest reductions in Chinese exports from 1995 to 1998. Exports to Indonesia declined by 90%, to Korea by 30%, to Malaysia by 32%, and to Thailand by 40%. In contrast, exports increased to all countries whose currencies with respect to the yuan appreciated. The fastest export growth rates were to Brazil (42%), the United States (36%), Spain (32%), and Italy (29%). Of these countries, only Spain's currency depreciated, slightly by 11%.

Figure 2 provides a graphical view of export changes for the same twenty countries, in ascending order of 1995–1998 real exchange rate devaluation (from left to right, top to bottom). Each graph displays log exports from 1990 to 2004, where exports are normalized so that the first year is 100 before taking logs, and all graphs have the same vertical scale. Changes in Chinese exports from 1995 to 1998 are indeed more negative in countries experiencing real exchange rate devaluations (in the bottom row) than in those experiencing real exchange rate appreciations (top row). These graphs are also useful to confirm that post-1997 declines in exports in the countries experiencing the largest depreciations are not just continuations of preexisting negative export trends. In fact, the opposite appears to be true: precrisis exports were actually growing robustly prior to 1997 in Japan, Thailand, Malaysia, Korea, and Indonesia, and then took sharp downward dips thereafter.

Regression-based estimates of the impact of 1995–1998 real exchange rate changes on changes in exports over the same time period are presented in table 2. In the first column, the unit of observation is exports to one of 153 Chinese export destinations. (Data are from the U.N. Comtrade data set.) Hong Kong reexports are treated as exports from China to their respective destinations. We regress the change in log total export value on the shock index for the destination and weight each observation by 1995 total exports so that the estimated relationship is not heavily influenced by exports to relatively unimportant destinations. The coefficient on the shock index (−0.632) is negative and highly statistically significant. The R^2 of the regression (0.45) is quite high as well, indicating that real exchange rate changes account for a substantial fraction of the variation in Chinese exports by destination over this time period.

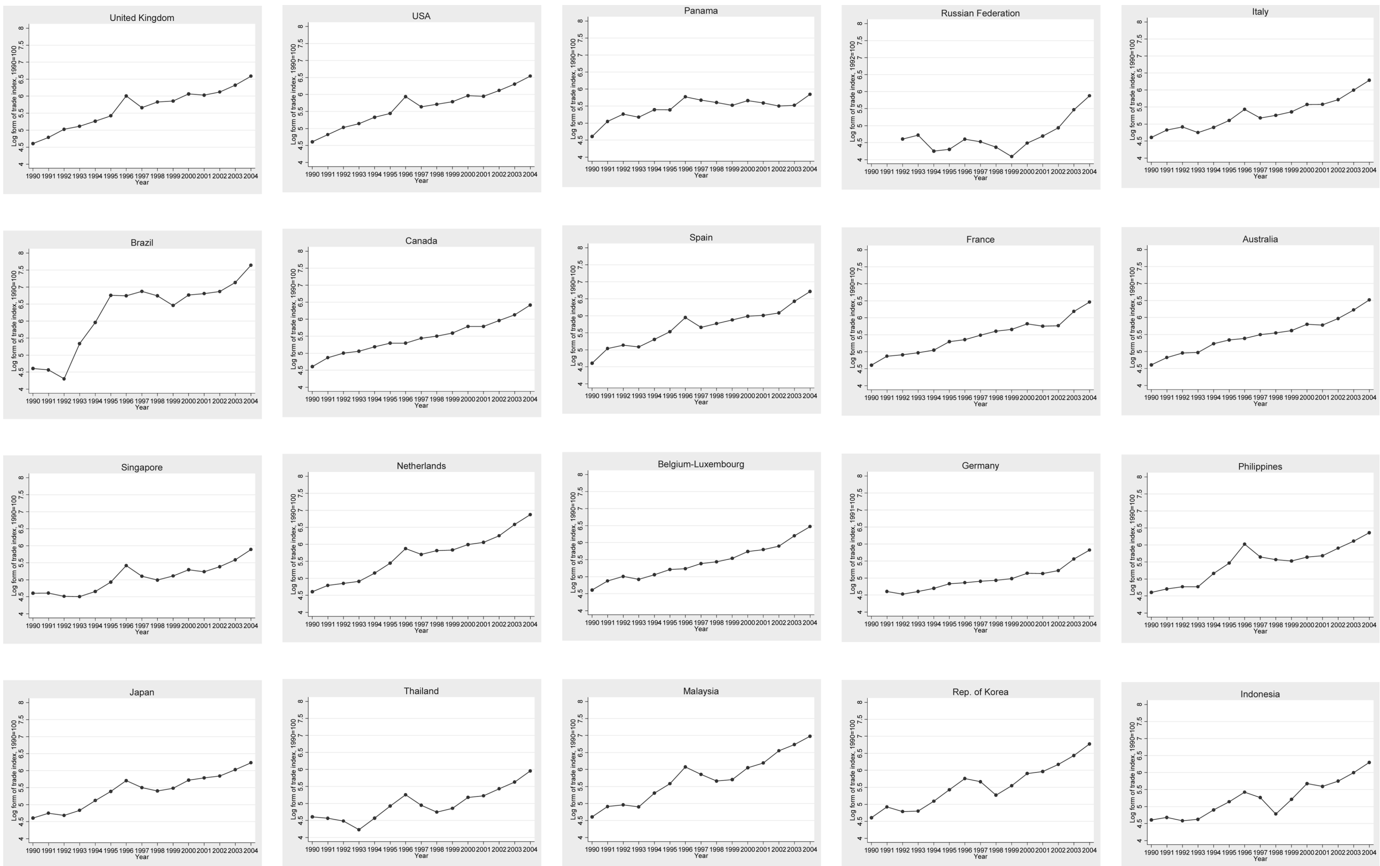
The Comtrade data also provide information on quantities, enabling us to look separately at the effect of exchange rate shocks on changes in quantities and changes in unit values. Unit values could adjust if firms price to market by cutting prices and reducing markups when the Chinese yuan appreciates with respect to the currencies of their export destinations. Such behavior has been found in other studies (Katayama, Lu, & Tybout, 2005; Atkeson & Burstein, 2008) and could lead us to overstate the impact of exports on productivity, if more favorable exchange rate shocks raise exporters' markups, and thus measured productivity, without increasing the ability of the firm to produce a greater quantity of goods with the same amount of inputs. Changes in unit values also could reflect changes in product quality (Hallak, 2006).[19]

We therefore run regressions at the level of the product-destination (exports of HS six-digit products to specific destinations), of which there are close to 88,000 in the Comtrade data for Chinese exports. In the second column of the table, the dependent variable is the change in log total value of exports (analogous to the dependent variable in the first regression, except at a much higher level of disaggregation). As in the first column, the coefficient on the shock index is negative and highly statistically significant. The coefficient (−1.042) indicates that a 10% depreciation of a foreign currency versus the Chinese yuan reduces exports to that country by 10.4%. While the coefficient in the second column is roughly two-thirds larger in magnitude than the coefficient in the first column, the standard error on the second column's estimate is large enough that the null hypothesis that the two coefficients are identical cannot be rejected.[20]

The third and fourth columns of the table examine the impact of the exchange rate shock on the change in log export unit value and change in log export quantity, respectively.

[19] Earlier studies (e.g., Pavcnik, 2002) do not deal with the markup issue.
[20] The R^2 in the second column has also dropped dramatically in relation to the first column, which is likely due to the fact that more factors must come into play to explain variation in exports at the detailed product destination level than are relevant for aggregate exports to countries as a whole.

FIGURE 2.—CHINESE EXPORTS TO TOP TWENTY DESTINATIONS, 1990–2004



Source: U.N. Comtrade.
Note: Destinations in increasing order of post-1997 real exchange rate depreciation, from left to right, top to bottom.

TABLE 2.—IMPACT OF REAL EXCHANGE RATE SHOCKS ON CHINESE EXPORTS, 1995–1998 (OLS REGRESSIONS)

Dependent Variable: Unit of Observation:	ΔLn(Total Value of Exports) Destination	ΔLn(Total Value of Exports) Product-Destination	ΔLn(Export Unit Value) Product-Destination	ΔLn(Export Quantity) Product-Destination
Shock index	−0.632	−1.042	−0.161	−0.881
	(0.057)***	(0.293)***	(0.088)*	(0.262)***
R^2	0.45	0.04	0.00	0.02
Number of observations	153	87,934	87,934	87,934

Note: Standard errors in parentheses. Data source is U.N. Comtrade data set. Unit of observation in first regression is an export destination country. Unit of observation in other regressions is a destination-product combination, where product is HS (1992) six-digit category. Observations weighted by first-period (1995) total exports. Changes are from 1995 to 1998. "Shock index" is export destination's change in real exchange rate from 1995 to 1998 expressed as a fraction of 1995 value (10% depreciation is 1.1, 10% appreciation is 0.9). "Total value" is total value of exports. "Unit value" is total value divided by quantity. Exports to Hong Kong are dropped from the data set, and Hong Kong's reported reexports are considered exports of China to their respective destinations. Significant at *10%, ***1%.

We find that nearly all of the change in export value in response to exchange rate shocks results from changes in quantities rather than changes in unit values. In the export unit value regression, the coefficient on the shock index is negative, but is relatively small in magnitude (−0.161) and is statistically significantly different only from 0 at the 10% level. In the export quantity regression, by contrast, the coefficient on the shock index is relatively large in magnitude (−0.881) and is statistically significantly different from 0 at the 1% level. These results suggest that 15.5% (0.161 divided by 1.042) of the total change in export value caused by exchange rate shocks can be attributed to changes in unit values.

We conduct a similar analysis using the firm data. In this case, we are unable to distinguish between quantities and unit values. However, with firm data, we are able to control for a large number of additional control variables, and we are able to examine interactions between the shock index and various firm characteristics.

Summary statistics for the firm data are provided in table 3. In the main results tables, we focus on results for a balanced sample of 3,339 firms that are observed continuously across the 1995, 1998, and 2000 surveys.[21] The mean firm exhibited substantial export growth: the mean changes in log exports across firms are 0.45 and 0.49 over the

[21] Results are qualitatively very similar for unbalanced samples of firms (when the 1995–1998 sample is allowed to differ from the 1995–2000 sample), as will be discussed in more detail below.

TABLE 3.—SUMMARY STATISTICS FOR CHINESE FIRMS

Shock Index:	Mean	s.d.	Number of Observations			
	0.13	0.15	3,339			
	1995–1998			1995–2000		
	Mean	s.d.	Number of Observations	Mean	s.d.	Number of Observations
Dependent variables						
ΔLn(exports)	0.45	1.26	3,339	0.49	1.44	3,339
ΔLn(productivity, OLS)	0.57	2.92	3,339	0.83	2.77	3,339
ΔLn(productivity, LP)	0.67	2.95	3,339	0.95	2.81	3,339
ΔLn(workers)	0.17	0.58	3,339	0.19	0.69	3,339
ΔLn(capital)	0.16	0.68	3,339	0.13	0.85	3,339
ΔLn(capital/worker)	−0.02	0.77	3,339	−0.07	0.88	3,339
ΔLn(wages/worker)	0.25	0.72	3,312	0.37	0.68	3,312
Δreturn on assets	−0.01	0.15	3,339	0.01	0.18	3,339
ΔLn(sales)	0.35	0.91	3,338	0.45	1.06	3,338
ΔLn(sales/worker)	0.18	0.81	3,338	0.26	0.87	3,338
ΔLn(intermediate inputs)	0.29	0.94	3,339	0.37	1.07	3,339
Δforeign ownership share	0.01	0.18	3,323	0.01	0.20	3,323
Precrisis (1995) characteristics						
Sales (US$)	9,538,428	36,988,462	3,339	9,538,428	36,988,462	3,339
Exports (US$)	5,684,255	19,162,600	3,339	5,684,255	19,162,600	3,339
Export share of sales	0.75	0.34	3,339	0.75	0.34	3,339
Export share of top two destinations	0.95	0.16	3,339	0.95	0.16	3,339
Per capita GDP in export destination (US$)	25,763	11,159	3,339	25,774	11,151	3,339
Foreign ownership share	0.69	0.30	3,339	0.69	0.30	3,339
Capital/worker (US$)	10,156	20,560	3,339	10,156	20,560	3,339

Note: Data are from Chinese Industrial Census 1995 and Annual Firm Survey 1998. Sample is balanced across 1995, 1998, and 2000. "Shock index" is real exchange rate index based on firm's precrisis export composition, normalized to 1 in 1995 (10% depreciation is 0.1, 10% appreciation is −0.1). Productivity measures are OLS (from regression of log value added on log fixed assets and log employment) and Levinsohn-Petrin (LP).

1995–1998 and 1995–2000 periods, respectively. In addition to these mean changes, it is also worth noting that most firms experienced increases in exports from before to after the crisis. Between 1995 and 1998, 65.5% of firms had positive export growth, and the corresponding figure for 1995 to 2000 is very similar: 65.0%. We emphasize this to note that the natural experiment in this paper occurred in a period of overall export growth, so that the exogenous fluctuations in exporting we identify mostly lead to lower-than-expected positive growth instead of driving firms into negative growth.[22]

Regressions examining the impact of the shock index (and associated interaction terms) on the change in firm-level log exports are presented in table 4. To ease the interpretation of regression coefficients, the shock index and all variables interacted with it are standardized to have mean 0 and standard deviation 1.

All regressions include province-industry fixed effects and the full set of preshock control variables described above. The first two columns present results for changes between 1995 and 1998, and the last two columns present results for changes between 1995 and 2000.

When the shock index is entered into the regression without interaction terms (columns 1 and 3), its coefficient estimate is negative, but it is statistically significant only in the first column for 1995–1998 changes. In both regressions, the F-test of the statistical significance of the shock index yields relatively low F-statistics (of 4.73 and 1.12, respectively), indicating that the shock index by itself would be a somewhat weak instrument.

To gain a graphical sense of the relationship between the shock index and the changes on log exports, we examine the nonparametric relationship between the two variables after partialing out the influence of other covariates. In figure 3, we display the relationship along with confidence interval bands, using a locally weighted regression estimator. The figure reveals a negative relationship between the two variables over both the 1995–1998 and 1995–2000 periods. The relationship appears somewhat flatter for the 1995–2000 period, particularly in the middle range of exchange rate shock values (with a higher density of observations in the firm data), helping to explain the lack of statistical significance on the shock index in the 1995–2000 regression of column 3 in table 4.

In columns 2 and 4 of the table, the shock index is interacted with several 1995 firm characteristics: the log of weighted per capita GDP in the firm's export destinations (with export shares as weights), the fraction of firm exports

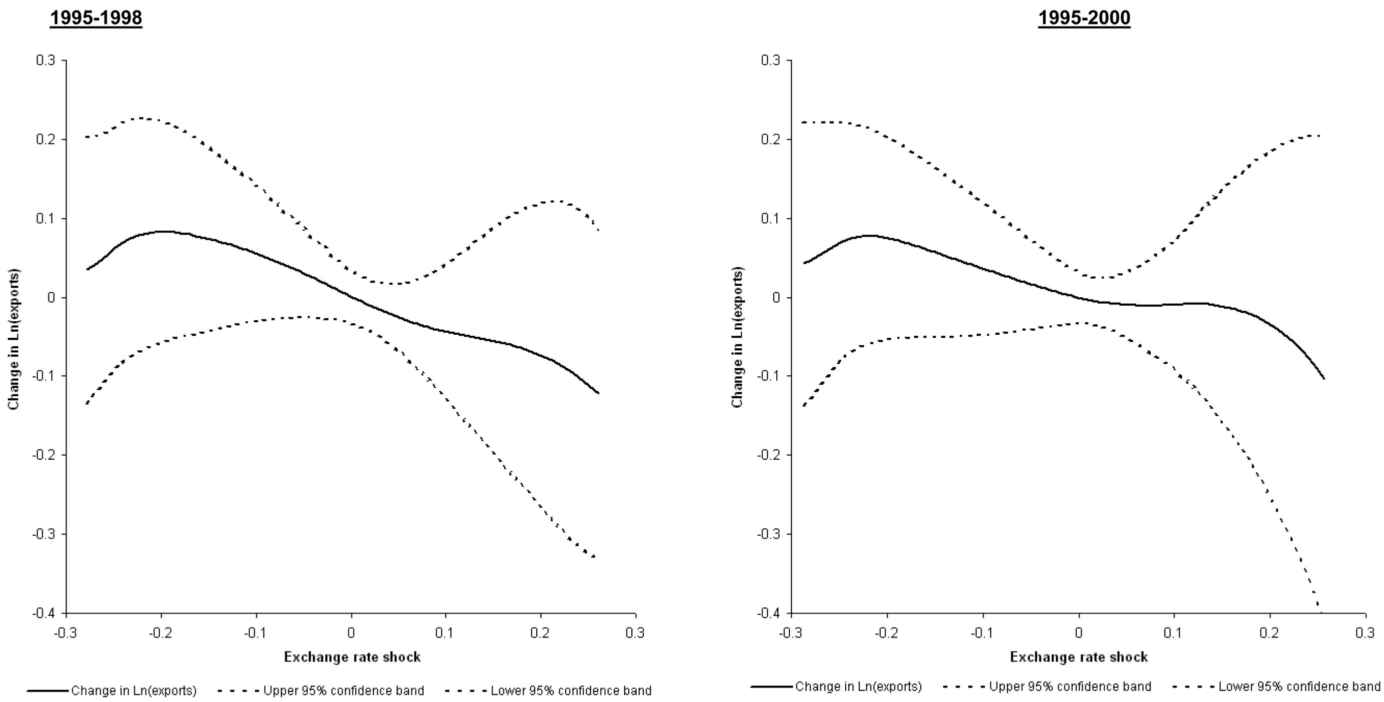
[22] We tested whether the effect of the instrumented change in log exports is different when that change is negative (results available on request). The results suggest that for firms with negative export growth, the effect of exports on productivity is more muted or nonexistent (coefficients are closer to 0 and not significant). However, standard errors are large due to the relatively small number of firms with negative export growth (we also cannot reject the hypothesis that the effect of export changes on productivity is symmetric for positive and negative changes), and so strong conclusions cannot be made on this front.

TABLE 4.—IMPACT OF EXCHANGE RATE SHOCKS ON EXPORTS OF CHINESE FIRMS (OLS ESTIMATES)

Dependent Variable: ΔLn(exports) Time Period for ΔLn(exports):	1995–1998 (1)	1995–1998 (2)	1995–2000 (3)	1995–2000 (4)
Shock Index	−0.053	−0.006	−0.035	0
	(0.024)**	(0.059)	(0.033)	(0.091)
Shock Index × Ln(Per Capita GDP in Destinations), 1995		−0.019		−0.007
		(0.012)		(0.018)
Shock Index × % of Exports to Hong Kong, 1995		0.059		0.045
		(0.065)		(0.101)
Shock Index × Foreign Ownership Share, 1995		0.028		0.058
		(0.013)**		(0.016)***
Shock Index × Ln(Capital/Worker), 1995		0.029		0.01
		(0.018)		(0.018)
Shock Index × Ln(Sales), 1995		−0.029		−0.024
		(0.034)		(0.031)
Shock Index × Ln(Productivity, LP), 1995		0.01		−0.02
		(0.013)		(0.025)
Province-industry fixed effects	Y	Y	Y	Y
Precrisis control variables	Y	Y	Y	Y
Number of observations	3,339	3,339	3,339	3,339
R^2	0.44	0.44	0.41	0.42
F-test: Joint significance of instrument(s)	4.73	10.09	1.12	6.88
P-value	0.03	0.00	0.29	0.00

Note: Standard errors in parentheses, clustered by first export destination. Unit of observation is a firm. Changes are from 1995 to 1998 or 1995 to 2000. Sample is balanced across 1995, 1998, and 2000. Firms included in sample all had nonzero exports in 1995. See table 3 for variable definitions and other notes. Shock index and variables interacted with shock index all are normalized to have mean 0 and standard deviation 1. Province-industry fixed effects are interactions between indicator variables for 26 provinces and 24 industries. Precrisis control variables are: 1995 log sales income; 1994 log sales income; 1995 share of exports to top two destinations; indicator for firm existing in 1994; indicator for firm exporting in 1994; foreign share of ownership; log of industry weighted average exports to 1995 destinations (weighted by firm's 1995 export destinations), separately for 1993 and 1996; indicator variables for firm size categories; 1995 exports as share of firm sales; 1994 exports as share of firm sales; indicator for firm exporting entire output in 1995; log exports in 1995; log exports in 1994; 1995 log capital-labor ratio; 1995 log productivity (Levinsohn-Petrin estimate); 1995 fraction of firm exports destined for Hong Kong; and log 1995 weighted average per capita GDP in firm's export destinations (weighted by firm's 1995 exports). Significant at **5%, ***1%.

FIGURE 3.—EXCHANGE RATE SHOCK AND CHANGE IN EXPORTS
NONPARAMETRIC FAN REGRESSION, CONDITIONAL ON PROVINCE-INDUSTRY FIXED EFFECTS AND PRECRISIS CONTROL VARIABLES



Note: Precrisis control variables are: 1995 log sales income; 1994 log sales income; 1995 share of exports to top two destinations; indicator for firm existing in 1994; indicator for firm exporting in 1994; foreign share of ownership; log of industry weighted average exports to 1995 destinations (weighted by firm's 1995 export destinations), separately for 1993 and 1996; indicator variables for firm size categories; 1995 exports as share of firm sales; 1994 exports as share of firm sales; indicator for firm exporting entire output in 1995; log exports in 1995; log exports in 1994; 1995 log capital-labor ratio; 1995 log productivity (Levinsohn-Petrin estimate); 1995 fraction of firm exports destined for Hong Kong; and log 1995 weighted average per capita GDP in firm's export destinations (weighted by firm's 1995 exports).

destined for Hong Kong, the foreign ownership share, log capital per worker, log sales, and log productivity (Levinsohn-Petrin). Justification for the exogeneity of the interaction terms stems from the unanticipated nature of the exchange rate shocks and the predetermined nature of firm characteristics measured in 1995.

Across both regressions, coefficients on the interaction term with foreign ownership share are positive in sign and are statistically significantly different from 0 at conventional levels. When firms' export partners experience exchange rate devaluations, exports decline less in firms with greater foreign ownership shares. This may reflect the fact that exports in such firms are more likely to be destined for overseas owners or firms otherwise linked in some way to the Chinese exporters so that exports are less price elastic. For example, exports of firms with higher foreign ownership may frequently be part of global within-firm production processes, so that their export demand may be insensitive to relatively large exchange rate fluctuations.[23] Multinationals also may use financial instruments to hedge against exchange rate risk. In the 1995–1998 regression, the shock index also has a less negative effect on firms that have higher capital-labor ratios.

F-statistics for the test of the joint significance of the shock index and associated interaction terms in columns 2 and 4 (10.09 and 6.88, respectively) are substantially larger than the corresponding F-statistics in columns 1 and 3, suggesting that including the interaction terms in the set of instruments is desirable to reduce weak-instrument problems (Bound, Jaeger, & Baker, 1995).[24]

The coefficients on the shock index and associated interaction terms, combined with the shock index and 1995 characteristics of each firm, can be used to calculate the firm-specific predicted changes in log exports associated with the exchange rate shocks. In the 1995–1998 period, the mean predicted impact of the exchange rate shock is −0.019, with a standard deviation of 0.077. In the 1995–2000 period, the mean is −0.011 and the standard deviation is 0.082. This amount of variation is nonnegligible: a two-standard-deviation difference in the predicted change in log exports is roughly 16 percentage points over either time period.[25] In the regression results to follow, we discuss the magnitude of the IV estimates by describing the estimated impact of a 10% increase in exports, which is roughly 1.25 standard deviations of the predicted change in exports driven by the exchange rate shocks.

VI. The Impact of Exporting on Firm Performance

To analyze the effect of exporting on firm performance, we regress the change in various firm performance measures on the change in log exports. Table 5 presents OLS and IV

Table 5.—Impact of Change in Exports on Change in Firm Outcomes
Coefficients on ΔLn(exports) in OLS and IV Regressions

	Dependent Variable: Change in . . .										
	Ln (productivity, OLS) (1)	Ln (productivity, LP) (2)	Ln (workers) (3)	Ln (capital) (4)	Ln (capital/ worker) (5)	Ln (wages/ worker) (6)	Return on Assets (7)	Ln (sales) (8)	Ln (sales/ worker) (9)	Ln (intermediate inputs) (10)	Foreign Ownership Share (11)
1995–1998											
Ordinary least squares	0.258	0.355	0.16	0.14	−0.02	0.06	0.022	0.458	0.298	0.451	0.008
	(0.034)***	(0.040)***	(0.023)***	(0.017)***	(0.009)**	(0.012)***	(0.002)***	(0.035)***	(0.018)***	(0.030)***	(0.003)**
	[3,339]	[3,339]	[3,339]	[3,339]	[3,339]	[3,312]	[3,339]	[3,338]	[3,338]	[3,339]	[3,323]
Instrumental variables	1.191	1.127	−0.107	−0.076	0.03	0.456	0.096	0.65	0.752	0.624	0.031
	(0.438)***	(0.419)***	(0.117)	(0.112)	(0.091)	(0.125)***	(0.032)***	(0.139)***	(0.114)***	(0.126)***	(0.041)
	[3,339]	[3,339]	[3,339]	[3,339]	[3,339]	[3,312]	[3,339]	[3,338]	[3,338]	[3,339]	[3,323]
1995–2000											
Ordinary least squares	0.257	0.382	0.206	0.196	−0.011	0.061	0.028	0.499	0.293	0.489	0.005
	(0.016)***	(0.013)***	(0.011)***	(0.013)***	(0.013)	(0.012)***	(0.002)***	(0.018)***	(0.018)***	(0.025)***	(0.004)
	[3,339]	[3,339]	[3,339]	[3,339]	[3,339]	[3,312]	[3,339]	[3,338]	[3,338]	[3,339]	[3,323]
Instrumental variables	1.273	1.485	0.345	0.392	0.047	0.294	0.068	0.697	0.349	0.653	0.117
	(0.336)***	(0.321)***	(0.179)*	(0.189)**	(0.103)	(0.135)**	(0.033)**	(0.215)***	(0.200)*	(0.206)***	(0.054)**
	[3,339]	[3,339]	[3,339]	[3,339]	[3,339]	[3,312]	[3,339]	[3,338]	[3,338]	[3,339]	[3,323]

Note: Each coefficient (standard error) is from a separate regression of the change in firm outcome on ΔLn(exports). Standard errors clustered by first export destination. Sample size in brackets. Samples are balanced across 1995, 1998, and 2000 surveys. In IV regressions, first-stage regressions are in even-numbered columns. All regressions include fixed effects for province-industry and all precrisis control variables listed in table 4. See tables 3 and 4 for variable definitions and other notes. Significant at *10%, **5%, ***1%.

[23] We regard exploring these hypotheses (and others) explaining heterogeneity in the impact of exchange rate shocks on firm exports as important avenues for future research.

[24] However, these instruments are still relatively weak given the F-statistic thresholds recommended by Stock and Yogo (2005) for avoiding size distortions in IV estimation. As a robustness check, we present below instrumental variable estimates with standard errors and 5% significance tests based on Andrews, Moreira, and Stock (2006), who provide a method for adjusting critical values of test statistics in the presence of weak instruments so that significance tests have the correct size.

[25] That said, this variation is relatively small in comparison with the mean and standard deviation of the change in ln(exports) from table 3: mean 0.45 (standard deviation 1.26) in the 1995–1998 period and mean 0.49 (standard deviation 1.44) over the 1995–2000 period.

regression estimates of the coefficient on the change in log exports, separately for the 1995–1998 and 1995–2000 periods. The dependent variables are all in first differences and are listed across the top of the table. As in the first-stage regressions, we control for province-sector fixed effects and a vector of precrisis control variables in all regressions. In the IV regressions, the first-stage equations are those in table 4, columns 2 and 4 (for the 1995–1998 and 1995–2000 periods, respectively).

Overall, we find strong evidence that increases in exporting lead to increases in firm productivity. For both the OLS and LP productivity measures, the IV estimate of the impact of the change in exports is positive and statistically significantly different from 0 over both the 1995–1998 and the 1995–2000 time periods. The coefficient estimates in regressions using the 1995–2000 period are slightly larger in magnitude than those for the 1995–1998 period. However, standard errors are large enough that one cannot reject the hypothesis that the coefficients are the same across the two time periods.

In interpreting the coefficient on the change in export value when using the exchange rate shock as an instrument, the estimated effect of exports on productivity may partially reflect the small fraction of the change in export value attributable to changes in unit values (or markups). The previous analysis of Chinese trade data (see columns 2 to 4 of table 2) indicates that 15.5% of the increase in export values associated with the exchange rate shocks is due to changes in unit values. Therefore, we consider 84.5% of the estimated change in productivity caused by changes in export value to be a true change in productivity.

These productivity effects are not extremely large, but neither are they negligible. The regression results in table 5 indicate that a 10% increase in exports (0.1 increase in log exports) leads to a 0.108 (84.5% of 0.1273) increase in log OLS productivity and a 0.126 (84.5% of 0.1485) increase in log LP productivity over the 1995–2000 period. These numbers can be compared to mean productivity improvements over 1995–2000, which were 0.83 for OLS productivity and 0.95 for LP productivity (table 3). So a 10% increase in exports leads to OLS and LP productivity improvements equal to roughly one-eighth (13%) of the mean productivity improvement over the time period.

Consistent with the positive productivity effects of exporting, we also find statistically significant positive effects of exporting on sales and on return on assets over both time periods. There is also a positive and significant effect on sales per worker in the 1995–1998 sample, but this effect declines in magnitude (but remains statistically significantly different from 0 at the 10% level) in the 1995–2000 sample. According to the IV coefficient estimates, a 10% increase in exports increases sales by 6% to 7% (over both time periods), and return on assets by 0.96 percentage point from 1995 to 1998 and by 0.68 percentage point from 1995 to 2000.

In regressions for workers and capital, IV coefficient estimates are not statistically significantly different from 0 in the 1995–1998 sample (and are actually negative in sign). In the 1995–2000 sample, the coefficients in the workers and capital regressions are positive and statistically significant at the 10% and 5% levels, respectively. Firms hire more capital and labor in response to increases in exports over the 1995–2000 period. Capital and labor respond quite similarly (coefficient estimates in the capital and worker regressions are very similar), so the capital-worker ratio exhibits little relationship with the change in exports in the IV regressions. The difference in results for the change in capital and labor over the two time periods suggests that firms have difficulty adjusting capital and labor stocks in the short run (1995–1998) but not in the longer run (1995–2000). The lack of short-term response supports the assumption that exchange rate changes were unanticipated.

The instrumented change in exports has a positive and statistically significant effect on total wages per worker in the 1995–1998 period, while in the 1995–2000 period, this effect is smaller in magnitude but still statistically significant. Although the difference in the effects for the two periods is not statistically significant, it is consistent with the need for firms to pay workers more in the shorter term to compensate them for greater effort or productivity when output increases in response to export demand but the number of workers and the amount of capital is unchanged. The lack of significant changes in labor and capital in the short term suggests that initial productivity effects are associated with process innovations rather than technology embodied in new capital. Finally, increases in exports are associated with statistically significant increases in the share of firm ownership that is held by foreigners in the 1995–2000 period but not in the 1995–1998 period. Firms may become more attractive to potential foreign investors when they experience exogenous improvements in exports (and thus improvements in other firm performance measures). As with the worker and capital outcomes, it is reasonable that this effect appears with some lag.

For nearly all dependent variables in table 5, IV estimates of the coefficient on log exports are larger in magnitude than the OLS estimates. The difference is proportionately greatest in the productivity regressions. For example, in the regression for LP productivity over 1995 to 2000, the OLS coefficient on the change in log exports is 0.382, but in the IV regression, the coefficient is 1.485. The large difference between IV and OLS is also evident in the regressions for wages per worker, which is sometimes used as an alternative measure of firm productivity.

What might explain larger coefficient magnitudes in the IV results? One possibility is that the OLS estimates in the productivity regressions are biased by omitted variables that lead to increases in firm scale (including increases in exports) but have minimal or negative productivity effects. For example, firms undergoing mergers with other firms or

TABLE 6.—HETEROGENEITY IN IMPACT OF EXCHANGE RATE SHOCKS ON EXPORTS OF CHINESE FIRMS, 1995–2000 (IV ESTIMATES)

	Dependent Variable: Change in . . .			
	Ln (productivity, OLS)	Ln (productivity, LP)	Ln (productivity, OLS)	Ln (productivity, LP)
Δln(exports)	0.609	0.806	0.507	0.613
	(0.303)**	(0.300)***	(0.362)	(0.370)*
Δln(Exports) × Ln(Per Capita GDP in Destinations), 1995	0.172	0.18	0.363	0.376
	(0.097)*	(0.096)*	(0.143)**	(0.146)***
Δln(Exports) × % of Exports to Hong Kong, 1995			−0.105	−0.124
			(0.119)	(0.122)
Δln(Exports) × Foreign Ownership Share, 1995			0.094	0.243
			(0.318)	(0.325)
Δln(Exports) × Ln(Capital/Worker), 1995			−0.467	−0.504
			(0.214)**	(0.219)**
Δln(Exports) × Ln(Sales), 1995			0.378	0.45
			(0.186)**	(0.191)**
Δln(Exports) × Ln(Productivity, LP), 1995			−0.425	−0.459
			(0.260)	(0.266)*
Province-industry fixed effects	Y	Y	Y	Y
Precrisis control variables	Y	Y	Y	Y
Number of observations	3,339	3,339	3,339	3,339

Note: Standard errors in parentheses, clustered by first export destination. Unit of observation is a firm. Changes are from 1995 to 2000. Variables interacted with change in log exports all are normalized to have mean 0 and standard deviation 1. Instrumental variables for interaction terms with Δln(exports) are original instruments (listed in table 4) interacted with the corresponding interaction term. See tables 3 and 4 for variable definitions and other notes. Significant at *10%, **5%, ***1%.

rapidly expanding their production facilities would exhibit simultaneous increases in various indicators of firm scale, such as sales, workers, and capital, as well as exports. Mergers or expansion activity may have a temporary negative effect on productivity (due to, say, inefficiencies during reorganization of production lines), biasing downward the OLS coefficient on the change in exports in the productivity regressions.

Classical measurement error in the export variable is not likely to be an important explanation for the differences between the OLS and IV results. To test for measurement error bias, we estimate 1995–2000 IV regressions using the 1995–1998 change in log exports as the instrument for the 1995–2000 change in log exports. If attenuation bias due to classical measurement error is important, the IV coefficient estimates should be larger than the OLS estimates. As it turns out, however, these alternative IV estimates yield results very similar to OLS.[26]

VII. Do the Results Reflect Learning by Exporting?

Exporting may raise firm productivity by raising firms' exposure to technological or institutional advances in their export destinations, perhaps by communication with foreign buyers.[27] If this were the case, then we should expect the impact of exporting to be larger when firms export to more developed countries.

To test this hypothesis, we use the per capita GDP of an export destination as a proxy for the destination's level of technological and institutional development. We estimate IV regressions of the change in productivity on the change in exports, where we include an interaction term between the change in log exports and log per capita GDP of the firm's export destinations. Here, as before, the change in log exports is instrumented with the shock index and associated interaction terms (as in table 4). For brevity, let $\boldsymbol{Z}_i$ be the vector that includes the shock index and the set of associated interaction terms in table 4. In addition, the interaction term Δln(Exports) × (Initial Period Log per Capita GDP in Destinations) is itself instrumented with the interactions of the original instruments with destination log per capita GDP, or $\boldsymbol{Z}_i \times$ (Log per capita GDP in destinations).

IV regression results for the two productivity measures are presented in the first two columns of table 6. For ease of interpretation, destination log per capita GDP is normalized to have mean 0 and standard deviation 1. The coefficient on Δln(Exports) × (Log per Capita GDP in Destinations) is positive and statistically significantly different from 0 at the 10% level in regressions for both productivity outcomes. Both regressions imply that a 1 standard deviation increase in log per capita income level of a firm's export destinations leads to an increase of about 0.18 in the impact of Δln(exports) on the change in firm productivity.

While these regression results support the hypothesis that exporting to more developed countries leads to higher productivity gains, one might raise omitted variable concerns: it could be that per capita GDP in a firm's export destinations is simply proxying for other correlated firm characteristics that are the true sources of heterogeneity in the productivity impact of exporting.

To test whether such omitted variable concerns are important, we run additional regressions that include several additional interactions between Δln(exports) and initial

[26] For example, the coefficients in the OLS and IV regressions for productivity (LP) are 0.456 and 0.512, respectively. Regression results for other dependent variables are available from the authors on request.

[27] In a study of French firms, MacGarvie (2006) finds that exporters tend to obtain new technology from abroad by analyzing competing products and through communication with foreign buyers.

1995 firm characteristics: the share of exports destined for Hong Kong, the share of firm ownership that is foreign, ln(capital/worker), ln(sales), and ln(productivity, LP). Each of these interaction terms is instrumented by a set of variables analogous to those in the first two columns of the table. For example, Δln(Exports) × (% of Exports to Hong Kong) is instrumented with $\boldsymbol{Z}_i$ × (% of Exports to Hong Kong).

Results are presented in the last two columns of the table. As it turns out, inclusion of the additional interaction terms leads the coefficient on Δln(Exports) × (Log per Capita GDP in Destinations) to more than double in magnitude. The new regressions imply that a 1 standard deviation increase in the income level of a firm's export destinations leads to a nearly 0.4 increase in the impact of Δln(exports) on the change in firm productivity. The fact that the coefficient rises substantially in magnitude with the inclusion of the additional interaction terms suggests that, if anything, omitted variables bias leads the coefficient on Δln(Exports) × (Log per Capita GDP in Destinations) to be understated. Overall, then, the results are consistent with exporting leading to productivity improvements by inflows of advanced technological or institutional knowledge from more developed countries.

Coefficients on some of the other statistically significant interaction terms are also worth noting. In response to exogenous increases in exports, firms experience greater productivity growth when they have lower initial capital per worker, greater initial sales, and lower initial productivity. Scale may matter if small firms are unable to make R&D investments or if there are other scale economies to productivity improvement, while firms with lower initial productivity and less capital intensity may simply have more room for improvement in productivity.

In both regressions, the coefficient on the interaction term with share of exports to Hong Kong is negative although not statistically significant. This may be due to the fact that technological inflows may be attenuated when a Hong Kong-based trading company mediates trade flows between a Chinese firm and its ultimate destination.

VIII. Robustness Checks

In this section, we seek to shed further light on the nature of the impact of exporting on firm outcomes. In doing so, we refer to previous tables and also provide additional regression-based evidence.

A. *Are Productivity Improvements Simply due to Increases in Capacity Utilization?*

An important question when interpreting the estimated effect of exporting on firm productivity is whether the relationship simply reflects changes in capacity utilization. This concern arises if firms are unable to change their capital stocks and labor forces in response to a reduction in export demand. Then reductions in exports (and thus total firm sales and value added), keeping labor and capital constant, would lead to reductions in measured productivity, even though such productivity declines would not reflect technological changes or efficiency improvements.

While the absence of measures of firm capacity utilization in our data makes it impossible to address this issue directly, we find this interpretation of the results unlikely because of the time pattern of the results. If changes in capacity utilization were the primary explanation for the export-driven changes in measured productivity, we would expect that the impact of export changes on measured productivity would be lower in the 1995–2000 period than in the 1995–1998 period, as firms were able to adjust their capital and labor over time.

As it turns out, however, the productivity impact of exports follows exactly the opposite pattern across the two samples. The impact of export changes on firm productivity is actually slightly larger in the latter period than in the former. This pattern suggests that observed improvements in measured productivity are due to efficiency improvements or other technological progress, and not just increased capacity utilization. A capacity utilization story also cannot explain the greater effect of exporting on productivity when the export destination country is more developed.

B. *Potential Violations of the IV Exclusion Restriction*

The analysis so far assumes that the exchange rate shocks affect only the various firm-level outcomes via their effect on the firm's exports. However, it is possible that the exchange rate shocks directly affect firm outcomes independent of their effect on firm exports. Here we address two potential alternative channels for the exchange rate shocks' effects on firm productivity: via increases in foreign investment and intermediate input prices.

We documented in table 5 that instrumented changes in exports lead to increases in foreign investment over the 1995–2000 period and argued that this may be due to firms' increased attractiveness in the wake of export-driven performance improvements. But another possibility is that because existing foreign ownership tends to differentially come from the same countries to which firms export, the exchange rate shocks directly affect the cost of acquiring additional ownership shares by existing foreign owners. An exchange rate appreciation in a firm's export partners would raise exports but would also reduce the cost for investors in the same overseas locations to acquire additional ownership shares in the firm.[28]

If such an effect is important in practice, some fraction of the productivity improvements that accompany increased exports may be due to increased foreign investment rather than increased exports. For example, increased foreign own-

[28] When capital markets are imperfect, wealth shocks can enable greater investment, so currency appreciations in investor countries may lead to greater FDI outflows (Froot & Stein, 1991).

ership may lead to increased technology transfer from the overseas investors. In this case, the IV estimates of the impact of exports on productivity would be overstated.

To gauge the extent to which increased foreign ownership shares in and of themselves might be biasing the results, we include a control for the change in foreign ownership share in the regressions. The results of this exercise are presented in table 7 for the 1995–2000 period. In the top row, the original IV estimates from table 5 are presented for comparison. It turns out that the IV estimates for all of the outcome variables are very similar to the original estimates with the inclusion of this control (second row of table 5). The coefficient on the change in foreign ownership variable itself (not shown) is consistently small and statistically insignificant. There is therefore no indication that improvements in firm performance driven by correlated changes in foreign ownership are imparting substantial bias to the results.[29]

A second potential violation of the exclusion restriction occurs if firms tend to import intermediate inputs from the same countries to which they export. For example, Chinese firms may import intermediate inputs from parent companies overseas, assemble these inputs into finished products, and then send them back to their parent companies in the same locations. For such firms, exchange rate appreciation in a firm's overseas export locations also makes intermediate inputs more expensive. The firm's exports should rise, while the prices of intermediate inputs (in Chinese yuan) should also rise. Any increase in firm productivity due to the increase in exports would be offset by increases in intermediate input costs (the yuan value of intermediate inputs should increase, which in itself decreases measured productivity). This logic suggests that effects of the exchange rate shocks on the yuan value of intermediate inputs should lead to IV estimates of exports on productivity that are biased toward 0.

To gauge the extent to which this bias is important in practice, we regress the change in log intermediate inputs (valued in Chinese yuan) on the change in exports. The results are presented in the second-to-last column of table 5. Assuming a constant ratio of intermediate inputs to output, if intermediate inputs are not imported from the export destinations, the proportional effect of the change in exports on sales should be similar to its effect on intermediate inputs—in other words, the coefficient on the change in exports should be similar in the sales and intermediate inputs regressions. However, if firms import intermediate inputs from their export destinations, then increased exports caused by exchange rate shocks are also associated with higher intermediate input costs, so the coefficient on the instrumented change in log exports should be larger in the

[29] For changes in outcome variables over the 1995–1998 period, robustness checks analogous to those in table 7 also lead to results very similar to the baseline specification (not shown due to space considerations but available from the authors on request).

TABLE 7.—OTHER ROBUSTNESS CHECKS FOR IMPACT OF CHANGE IN EXPORTS ON CHANGE IN FIRM OUTCOMES, 1995–2000
(COEFFICIENTS ON ΔLN(EXPORTS) IN IV REGRESSIONS)

	Dependent Variable: Change in . . .										
	Ln (productivity, OLS)	Ln (productivity, LP)	Ln (workers)	Ln (capital)	Ln (capital/worker)	Ln (wages/worker)	Return on Assets	Ln (sales)	Ln (sales/worker)	Ln (intermediate inputs)	Foreign Ownership Share
Baseline IV estimates	1.273	1.485	0.345	0.392	0.047	0.294	0.068	0.697	0.349	0.653	0.117
	(0.336)***	(0.321)***	(0.179)*	(0.189)**	(0.103)	(0.135)**	(0.033)**	(0.215)***	(0.200)*	(0.206)***	(0.054)**
	[3,339]	[3,339]	[3,339]	[3,339]	[3,339]	[3,312]	[3,339]	[3,338]	[3,338]	[3,339]	[3,323]
Controlling for change in foreign share	1.256	1.466	0.345	0.374	0.028	0.262	0.069	0.675	0.326	0.635	NA
	(0.362)***	(0.346)***	(0.190)*	(0.182)**	(0.105)	(0.121)**	(0.033)**	(0.223)***	(0.214)	(0.221)***	
	[3,323]	[3,323]	[3,323]	[3,323]	[3,323]	[3,296]	[3,323]	[3,322]	[3,322]	[3,323]	
No control variables	0.93	1.18	0.415	0.354	−0.056	0.296	0.095	0.932	0.515	0.839	0.11
	(0.392)**	(0.318)***	(0.175)**	(0.176)**	(0.077)	(0.157)*	(0.027)***	(0.168)***	(0.201)**	(0.146)***	(0.045)**
	[3,339]	[3,339]	[3,339]	[3,339]	[3,339]	[3,312]	[3,339]	[3,338]	[3,338]	[3,339]	[3,323]
Andrews, Moreira, Stock significance tests	1.273	1.485	0.345	0.392	0.047	0.294	0.068	0.697	0.349	0.653	0.117
	(0.468)	(0.476)	(0.146)	(0.195)	(0.191)	(0.171)	(0.038)	(0.156)	(0.162)	(0.179)	(0.054)
	[3,339]	[3,339]	[3,339]	[3,339]	[3,339]	[3,312]	[3,339]	[3,338]	[3,338]	[3,339]	[3,323]
5% Wald critical value	4.29	4.49	5.58	4.37	3.38	3.12	3.76	18.58	7.12	13.3	2.78
Wald statistic	8.22	10.82	6.26	4.48	0.07	3.31	3.57	22.36	5.16	14.8	5.14

Note: Each coefficient (standard error) is from a separate regression of the change in firm outcome on ΔLn(exports). Sample size in brackets. In bottom row of results, IV estimates, 5% Wald critical values, and Wald statistics based on Andrews, Moreira, and Stock (2006). All regressions include fixed effects for province-industry and all precrisis control variables listed in table 4. See tables 3 and 4 for variable definitions and other notes. Significant at *10%, **5%, ***1%.

intermediate inputs regression than in the sales regression. It turns out that for both the 1995–1998 and 1995–2000 periods, the IV coefficient on the change in log exports in the intermediate inputs regression is actually slightly smaller in magnitude than the corresponding coefficient in the sales regression. Thus, there is no indication of substantial bias due to changes in intermediate inputs prices.

B. *Importance of Control Variables*

Inclusion in the regressions of province-industry fixed effects as well as a wide variety of control variables for preshock firm characteristics was motivated by the desire to account as much as possible for firm heterogeneity and any ongoing time trends in firm outcomes that may be correlated with firms' initial characteristics. Here, we test the sensitivity of the empirical results to inclusion of these covariates.

In the next-to-last row of table 7, we present IV regression results where no right-hand-side controls are included in the regression, with the exception of the main effects of the variables that are interacted with the shock index. As it turns out, dropping the control variables leads to coefficient estimates qualitatively very similar to the original estimates. The coefficient estimates in the OLS and LP productivity regressions are somewhat smaller in magnitude but remain statistically significant at conventional levels. This also lends credence to the assumption that the distribution of unexpected exchange range shocks was relatively random and not systematically correlated with specific firm characteristics.

D. *Weak Instruments*

Due to concerns that the set of instruments may be relatively weak, the bottom row of table 7 presents regression results where standard errors and 5% significance tests follow the methodology to calculate conditional IV estimates proposed by Andrews et al. (2006). Their method adjusts critical values of test statistics in the presence of weak instruments so that significance tests have the correct size.[30] Below each IV coefficient estimate, we display the Andrews, Moreira, and Stock 5% Wald critical value and the Wald statistic for the test of the null hypothesis that the IV estimate is equal to 0.

Whenever the Wald statistic takes on values greater than the (regression-specific) 5% Wald critical value, the null hypothesis is rejected at the 5% level. The inferences based on the AMS Wald statistics turn out to be very similar to the original results. The coefficients on the change in exports in the productivity regressions are statistically significantly different from 0 at the 5% level. The same is true in the regressions for workers, capital, wages per worker, sales, intermediate inputs, and foreign ownership share. The only two cases where the baseline IV results indicate rejection of the null at the 5% level but the AMS results do not are in the regressions for return on assets and sales per worker, where Wald statistics are slightly lower than AMS 5% Wald critical values. Overall, there do not seem to be strong indications of substantial size distortions due to weak instruments.

E. *Transfer Pricing*

Because our sample consists of FDI firms, one possible concern is that transfer pricing in response to exchange rate changes could complicate the interpretation of our results. However, if transfer pricing by multinationals seeks to move profits to countries where taxation is lower, then optimal transfer pricing should not be affected by exchange rate changes, which do not affect relative taxation rates. However, exchange rate devaluation in an export destination country will reduce the profits of Chinese exporters, assuming that the effect on profits of a lower export sales price dominates declines in the price of any imported inputs from the same country. If multinationals respond by moving profits to Chinese affiliates via transfer pricing in order to cushion the effect of the shock and take advantage of low Chinese tax rates, this would create a negative association between exports and profits, which would lead us to underestimate the effect of exports on firm performance.

F. *Sample Selection Issues*

Foreign-invested 1995 exporters that also appear in the 1998 or 2000 annual surveys make up the sample for analysis. There are 13,605 foreign-invested firms in 1995 with positive exports and complete data for all variables of interest. Our primary sample for analysis includes firms that could be followed through 1998 and 2000, had complete data on all variables used in the analyses, and had exports in all three years. This balanced 1995–1998–2000 sample consisted of 3,339 firms (a 25% matching rate).

With such a high rate of noninclusion in the sample, it is important to consider whether our results are likely to be contaminated by sample selection biases. First, it should be kept in mind that the 13,606 firms in 1995 include firms of all sizes, while (due to survey design) the firms in the 1998 and 2000 surveys include only firms above 5 million yuan in sales revenues. In 1995, 4,992 of the 13,606 firms (36.7%) had sales below 5 million yuan. Even in the complete absence of sample selection, there would be a high rate of noninclusion because many firms would remain below the 5 million yuan threshold.

It would be problematic, however, if the likelihood of remaining below the 5 million sales threshold, of falling below that threshold from above, or of having 0 exports in any of the years were related to the exchange rate shocks of interest. In addition, the shocks of interest could in principle also affect the likelihood that firms exit the sample by

[30] The test has been implemented as the "condivreg" command in Stata by Moreira and Poi (2001).

shutdown or merger with another firm. For example, if the firms experiencing the most negative shocks were also less likely to be observed in 1998 or 2000, then effects of export shocks on firm outcomes would be understated because the set of firms experiencing the worst shocks would be relatively depopulated of the firms whose outcomes deteriorated the most.

The regressions of table A2 test whether the exogenous shocks of interest are in fact correlated with noninclusion in the sample between 1995 and the latter survey years. For each of the 13,606 firms observed in 1995 and that have complete data on all variables, we construct the predicted change in log exports from the first-stage regressions of table 4. In both columns of the table, the dependent variable is an indicator variable for a firm being included in the balanced 1995–1998–2000 sample of 3,339 firms. In column 1, the right-hand-side variable of interest is the predicted change in exports from 1995 to 1998 (predicted using the regression of table 4, column 2), and in column 2, the predicted change in exports is from 1995 to 2000 (calculated using coefficients from table 4, column 4). The regressions include all province-industry fixed effects and preshock control variables included in other regressions. Because the regressions include a generated regressor (the predicted change in log exports), bootstrapped standard errors are reported.

As it turns out, the coefficients on the predicted change in log exports in both regressions are very small in magnitude and are not statistically significantly different from 0. There is no evidence that predicted exports over either 1995–1998 or 1995–2000 are correlated with inclusion in the sample, and therefore no indication that bias due to sample selection is a cause for concern.[31]

G. *Unbalanced Sample Results*

All results presented thus far have been for the balanced 1995–1998–2000 firm sample, and so one might wonder whether the results are different when examining an expanded sample of firms that are not restricted to be common across 1995, 1998, and 2000. Over the 1995–1998 time period, 4,605 firms can be matched, have complete data on all 1995 control variables, and have exports in both years. Over the 1995–2000 period, the corresponding number is 3,930 firms.

In table A3 and A4, we present regression results for these unbalanced samples. Table A3 presents the first-stage results (and is analogous to table 4), and table A4 presents the OLS and IV results (analogous to table 5). The first-stage, OLS, and IV results are in most respects very similar in the balanced sample in comparison to the unbalanced results. The main difference of note is that the IV estimates of the impact of the change in log exports on productivity from 1995 to 1998 are substantially smaller in magnitude than the corresponding estimates for the period 1995 to 2000 and are not statistically significantly different from 0.

[31] Regression results are very similar if the sample is restricted to firms with at least 5 million yuan in sales in 1995. Furthermore, attrition from the sample that is due only to having no exports in either 1998 or 2000 (which affects 835 firms) is also not correlated with the predicted change in exports. Results are available from the authors on request.

IX. Conclusion

This paper has examined the impact of exogenous shocks to export demand on the performance of Chinese firms. In 1997, the Asian financial crisis led to large real exchange rate shocks in several important destinations of Chinese exports. Because most firms were not well diversified in their countries of export, changes in export demand showed great heterogeneity across firms. We find that greater real depreciation in a firm's export partner's currency leads to slower growth in firm exports from before to after the Asian crisis. Using exogenous exchange rate shocks and their interactions with preshock firm characteristics as instruments, we find that exporting increases firms' total factor productivity, total sales, and return on assets. These results are highly robust to relaxing many of the estimation assumptions, providing support for the learning-by-exporting hypothesis. Consistent with this hypothesis, we also find that the export-productivity relationship is stronger in firms exporting to richer countries.

These results suggest that a number of additional analyses would be worth undertaking. For example, it is of interest to examine productivity spillovers to firms that were not exporting prior to the Asian crisis: when firms' exports fluctuate in response to exchange rate changes in their export destinations, does the performance of other nearby firms change? The search for evidence of such spillovers could take place within geographic areas (provinces) and within industrial sectors. It is also interesting to ask about the impact of entry into exporting, which may be different from the impact of increases in exporting among firms that were already exporting in an initial period. An approach for examining this question that builds on this analysis would be to use the average exchange rate shock in one's province and industry as an instrument for export entry. This strategy could work if informational spillovers from other exporters or economies of scale on the part of firms that service exporters (transport providers, pure trading firms) lead the costs of export entry to decline when total exports from a locality rise.

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APPENDIX

TABLE A1.—PREDICTING FIRM'S EXCHANGE RATE SHOCK WITH PRECRISIS VARIABLES, CHINESE EXPORTING FIRMS, 1995 (OLS ESTIMATES)

	Dependent Variable: Shock Index, 1995–1998	
	(1)	(2)
Ln(sales), 1995	−0.004	−0.001
	(0.0050)	(0.0040)
Ln(sales), 1994–1995	0.003	0.006
	(0.0040)	(0.003)*
Firm has 1994 sales (indicator), 1995	−0.006	0.014
	(0.0270)	(0.0240)
Ln(exports), 1995	0.003	0.001
	(0.0050)	(0.0040)
Ln(exports), 1994–1995	0	−0.003
	(0.0030)	(0.0030)
Firm has 1994 exports (indicator), 1995	−0.031	−0.016
	(0.015)**	(0.0140)
Ln(total exports in firm's industry to same destinations), 1995	−0.006	−0.018
	(0.002)***	(0.002)***
Ln(total exports in firm's industry to same destinations), 1993–1995	0.14	0.211
	(0.008)***	(0.009)***
Export share of sales, 1995	0.02	0.002
	(0.0180)	(0.0160)
Export share of sales, 1994–1995	−0.02	−0.003
	(0.0170)	(0.0150)
Firm exports 100% of sales (indicator), 1995	−0.023	−0.004
	(0.007)***	(0.0060)
Exports to top two destinations as share of total exports, 1995	0.052	0.054
	(0.015)***	(0.014)***
Ln(per capita GDP in export destinations), 1995	0.031	0.046
	(0.004)***	(0.004)***
Foreign ownership share, 1995	−0.023	0.014
	(0.009)***	(0.0080)
Ln(capital/worker), 1995	0.008	0.008
	(0.002)***	(0.002)***
Province-industry fixed effects	—	Y
Number of observations	3,339	3,339
R^2	0.13	0.46

Note: Unit of observation is a firm. All changes are from 1995 to 1998. Firms included in sample all had nonzero exports in 1995. Province-industry fixed effects are interactions between indicator variables for 26 provinces and for 24 industries. Significant at *10%, **5%, ***1%.

TABLE A2.—IMPACT OF PREDICTED CHANGE IN EXPORTS ON SAMPLE SELECTION (OLS REGRESSIONS)

	Dependent Variable: Included in Balanced 1995–1998–2000 Sample (Indicator)	
Time Period for Predicted Change in Exports:	1995–1998	1995–2000
Predicted change in exports	−0.02	−0.024
	(0.491)	(0.253)
Province-industry fixed effects	Y	Y
Precrisis control variables	Y	Y
Number of observations	13,605	13,605
R^2	0.21	0.21
Addendum		
Number of firms included in final sample	3,339	3,339
Rate of inclusion in sample	25%	25%
Predicted change in exports is from:	Table 4, column 2	Table 4, column 4

Note: Bootstrapped standard errors in parentheses. Unit of observation is a firm. Sample is all firms with complete data on right-hand-side variables in the 1995 Chinese industrial census. Precrisis control variables are: 1995 log sales income; 1994 log sales income; 1995 share of exports to top two destinations; indicator for firm existing in 1994; indicator for firm exporting in 1994; foreign share of ownership; log of industry weighted average exports to 1995 destinations (weighted by firm's 1995 export destinations), separately for 1993 and 1996; indicator variables for firm size categories; 1995 exports as share of firm sales; 1994 exports as share of firm sales; indicator for firm exporting entire output in 1995; log exports in 1995; log exports in 1994; 1995 log capital-labor ratio; 1995 log productivity (Levinsohn-Petrin estimate); 1995 fraction of firm exports destined for Hong Kong; and log 1995 weighted average per capita GDP in firm's export destinations (weighted by firm's 1995 exports).

TABLE A3.—IMPACT OF EXCHANGE RATE SHOCKS ON EXPORTS OF CHINESE FIRMS, UNBALANCED SAMPLE (OLS ESTIMATES)

	Dependent Variable: ΔLn(exports)			
Time Period for D Ln(exports):	1995–1998 (1)	1995–1998 (2)	1995–2000 (3)	1995–2000 (4)
Shock index	−0.049	0.01	−0.042	0.016
	(0.027)*	(0.090)	(0.031)	(0.060)
Shock Index × Ln(Per Capita Income in Destinations), 1995		−0.025		−0.007
		(0.011)**		(0.017)
Shock Index × % of Exports to Hong Kong, 1995		0.079		0.084
		(0.084)		(0.072)
Shock Index × Foreign Ownership Share, 1995		0.023		0.065
		(0.012)*		(0.018)***
Shock Index × Ln(Capital/Worker), 1995		0.048		0.014
		(0.015)***		(0.026)
Shock Index × Ln(Sales), 1995		−0.014		−0.011
		(0.029)		(0.019)
Shock Index × Ln(Productivity, LP), 1995		−0.005		−0.014
		(0.016)		(0.020)
Province-industry fixed effects	Y	Y	Y	Y
Precrisis control variables	Y	Y	Y	Y
Number of observations	4,605	4,605	3,930	3,930
R^2	0.38	0.38	0.4	0.41
F-test: Joint significance of instrument(s)	3.22	10.12	1.77	8.58
P-value	0.08	0.00	0.19	0.00

Note: Standard errors in parentheses. Unit of observation is a firm. Changes are from 1995–1998 or 1995–2000. Firms included in sample all had nonzero exports in 1995. See table 3 for variable definitions and other notes. Shock index and variables interacted with shock index all are normalized to have mean 0 and standard deviation 1. Province-industry fixed effects are interactions between indicator variables for 26 provinces and for 24 industries. Precrisis control variables are: 1995 log sales income; 1994 log sales income; 1995 share of exports to top two destinations; indicator for firm existing in 1994; indicator for firm exporting in 1994; foreign share of ownership; log of industry weighted average exports to 1995 destinations (weighted by firm's 1995 export destinations), separately for 1993 and 1996; indicator variables for firm size categories; 1995 exports as share of firm sales; 1994 exports as share of firm sales; indicator for firm exporting entire output in 1995; log exports in 1995; log exports in 1994; 1995 log capital-labor ratio; 1995 log productivity (Levinsohn-Petrin estimate); 1995 fraction of firm exports destined for Hong Kong; and log 1995 weighted average per capita GDP in firm's export destinations (weighted by firm's 1995 exports). Significant at *10%, **5%, ***1%.

TABLE A4.—IMPACT OF CHANGE IN EXPORTS ON CHANGE IN FIRM OUTCOMES, UNBALANCED SAMPLE ACROSS 1998, 2000 (OLS AND INSTRUMENTAL VARIABLES ESTIMATES)

	Dependent Variable: Change in . . .										
	Ln (productivity, OLS)	Ln (productivity, LP)	Ln (workers)	Ln (capital)	Ln (capital/ worker)	Ln (wages/ worker)	Return on Assets	Ln (sales)	Ln (sales/ worker)	Ln (intermediate inputs)	Foreign Ownership Share
1995–1998											
Ordinary least squares	0.279	0.374	0.158	0.126	−0.032	0.041	0.023	0.427	0.269	0.415	0.005
	(0.028)***	(0.027)***	(0.021)***	(0.017)***	(0.006)***	(0.011)***	(0.003)***	(0.034)***	(0.016)***	(0.031)***	(0.003)*
	[4,605]	[4,605]	[4,605]	[4,605]	[4,605]	[4,565]	[4,605]	[4,604]	[4,604]	[4,605]	[4,596]
Instrumental variables	0.562	0.556	−0.004	−0.118	−0.114	0.258	0.079	0.553	0.555	0.491	0.005
	(0.341)	(0.343)	(0.111)	(0.112)	(0.106)	(0.133)*	(0.025)***	(0.105)***	(0.125)***	(0.108)***	(0.022)
	[4,605]	[4,605]	[4,605]	[4,605]	[4,605]	[4,565]	[4,605]	[4,604]	[4,604]	[4,605]	[4,596]
1995–2000											
Ordinary least squares	0.242	0.358	0.19	0.19	0	0.068	0.028	0.485	0.295	0.479	0.003
	(0.016)***	(0.015)***	(0.008)***	(0.007)***	(0.010)	(0.013)***	(0.005)***	(0.016)***	(0.013)***	(0.020)***	(0.002)
	[3,930]	[3,930]	[3,930]	[3,930]	[3,930]	[3,897]	[3,930]	[3,929]	[3,929]	[3,930]	[3,918]
Instrumental variables	1.164	1.363	0.321	0.413	0.092	0.166	0.088	0.649	0.324	0.599	0.107
	(0.451)**	(0.418)***	(0.195)	(0.171)**	(0.101)	(0.108)	(0.039)**	(0.207)***	(0.202)	(0.209)***	(0.052)**
	[3,930]	[3,930]	[3,930]	[3,930]	[3,930]	[3,897]	[3,930]	[3,929]	[3,929]	[3,930]	[3,918]

Note: Each coefficient (standard error) is from a separate regression of the change in firm outcome on ΔLn(exports). Sample size in brackets. Sample unbalanced across 1995–1998 and 1995–2000. All regressions include fixed effects for province-industry and all precrisis control variables listed in table 4. See tables 3 and 4 for variable definitions and other notes. Significant at *10%, **5%, ***1%.