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Export Subsidies in a Heterogeneous Firms Framework: Evidence from Colombia*

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ABSTRACT

We evaluate the impact of firm-specific export subsidies on exports in Colombia. Using a two-stage selection correction procedure, we obtain firm-specific predicted subsidy amounts that can be explained by the characteristics that determine the firms' eligibility for government support and its amount. Drawing on the accounts of the discretionary allocation of subsidies in developing countries, we regard the discrepancy between the predicted and the observed subsidy amounts as a proxy for a firm's ties to government officials. Controlling for observable and unobservable firm characteristics and persistence in exports, we find that although, in general, subsidies exhibit a positive impact on export volumes, this impact is diminishing in subsidy size and in the degree of a firm's connectedness.

KEYWORDS: Export promotion; export subsidies

JEL Classification: F10, F13, L20, H20

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1 Introduction

> *A great part of the [...] economy is decided in the corridors of the Ministry of Economy and of the Central Bank [...]. It is more profitable to spend time in these corridors than in the manufacturing plant [...].*
>
> Quoted in Nogués (p.23: 1989)

Export subsidies are illegal under WTO regulations. They can trigger retaliatory action from trading partners, misalign prices and distort the allocation of resources. Even when effective, their impact is small and the direct fiscal cost of keeping them in place can be unjustifiably large. Yet, export subsidies remain common in the developing world.[1]

The case in favor of export subsidies is based on the argument that they can help a country to achieve export expansion and diversification of the economy towards manufacturing. They encourage a firm to undertake an activity that is costly, yet, assuming positive spillovers from exporting to other firms, socially desirable. One of the practical arguments against the use of export subsidies is that they are very easy to abuse, which renders them ineffective in achieving their original goals (Nogués, 1989).

First, there are government officials pandering to the 'connected' firms. Mobarak and Purbasari (2006) use firm-level data for Indonesia and a unique data set identifying a firm's degree of connectedness to President Suharto to investigate the impact of nepotistic relationships on the probability of obtaining import licenses for raw materials and for commodities for sale in local markets. By conservative estimates, being connected triples the likelihood of receiving a license relative to the firm's competitors, and having a member of the Suharto family on the firm's board of management quadruples the likelihood.

Second, the money is left for the discretionary use by the firm. If appropriate auditing mechanisms are absent, as in most developing countries, firms may fail to spend the additional resources on the activities fostering exports. Nogués (1989) describes a case in which a shipbuilding company drew on government export promotion funds for several years before it became known that the company had not even started its production. Rodrik (1993) cites an article from *The Economist* (August 14, 1993, pp. 37-38), in which a Kenyan firm, the sole recipient of a license to export gold and jewelry, received US$ 54 million in export subsidies (amounting to 5 percent of Kenya's total exports). Not only did the firm get a subsidy of 35 percent instead of the legally allowed

[1] See the WTO World Trade Report 2006 for an overview of the current presence and relevance of export subsidies in the developing world.

20, but the foreign buyers of its products either did not exist, or had never heard of the firm.

Third, the export promotion schemes are often complex and this leaves them open to misuse and abuse. Consider a sample list of export promotion measures operating in Argentina during the 1980s: reimbursements for exports produced with sugar (a product with important employment effects in two provinces), for exports going to new markets, for exports shipped through southern ports, for exports coming from Tierra del Fuego, for exports shipped by the customs of Salta and Jujuy, reimbursements to the enterprises that sign a contract with the government for a marginal increase of exports, reimbursements for turnkey exports, etc. Leaving aside defaulting on commitments or re-exports through a promoted port, a firm may establish barely functioning, but legal factories in promoted regions. The only production that takes place there is the sticking of labels. Tax reimbursement claims are, however, for the entire value of the output.

Such a complex system of subsidies and a questionable system to control their allocation and use go a long way toward explaining why researchers have failed to find convincing evidence in favor of export subsidies. To borrow from Rodrik (p. 10: 1993), "the received wisdom on export subsidies is that they have not been effective". We believe that this may, at least partially, be explained by the fact that most of the work on the impact of export subsidies has been done using country- and industry-level data when in reality subsidies are negotiable on a case-by-case basis. As such, these analyzes have failed to take into account the potential misallocation or misuse of export subsidies when looking at their effectiveness.

This study is motivated by the substantial variation in government support received by individual firms in Colombia during 1981-1991: while the median size of subsidies per peso of export sales was around 8-10 percent, they could be as small as two and as high as 20 percent, with a number of firms reporting subsidies in excess of a quarter of export sales.[2] We interpret such variation in subsidy rates as a sign that export subsidies may not have been designed to support the industry or the region as a whole, but to grant assistance to particular firms.

We address the following four questions: (A) how much of the variation in subsidies can be explained by firm characteristics, commonly linked to the allocation rules, (B) what type of firms obtain government support, (C) how can we measure firms' 'connectedness' influencing subsidy receipt without direct measures provided by the data

[2]The data we use are plant-level, so in the text, somewhat abusing language, the terms plant and firm are used interchangeably.

and (D) whether subsidies induce increases in export volumes and how the effectiveness of subsidies changes depending on our measure of firms' 'connectedness'.

Our empirical approach is as follows: we use a two-stage Heckman (1976, 1979) selection procedure to obtain firm-specific predicted subsidy amounts that are explained by the characteristics that determine the firms' eligibility for government support and its amount. Drawing on the accounts of the discretionary allocation of subsidies in developing countries, we regard the discrepancy between the predicted and the observed subsidy amounts as a proxy for the firm's ties to government officials. Our evidence suggests that allocation of subsidies is more complex than suggested by the literature based on industry-level analyses. Subsidies are firm-specific, with different factors affecting the allocation and the amount decisions. Many firm characteristics mentioned in the publicly available allocation rules do not seem to be important. Finally, we find that although, in general, subsidies exhibit a positive impact on export volumes, this impact is diminishing in subsidy size and in the degree of a firm's connectedness. This finding thus suggests that export subsidies are ineffective when allocated to firms based on criteria other than a firm's potential to increase exports, such as a firm's ties to government officials.

The remainder of this paper proceeds as follows. Section 2 discusses the relevant theoretical framework. Section 3 frames our analysis within the existing literature. Section 4 lays out the regulatory environment for export subsidies in Colombia. Section 5 describes the data used in the analysis and provides a preliminary descriptive look at the data. Section 6 discusses our empirical approach and Section 7 shows our results. Some concluding remarks are provided in Section 8.

2 Theoretical Framework

Our framework is based on the dynamic model of participation in export markets in the presence of sunk costs (Roberts and Tybout, 1997; Das et al., 2007), which postulates that the decision to export depends on prior exporting experience.

A subsidy in our model is linked directly to the export volumes and represents an increase in the price and, hence, the attractiveness of exports relative to domestic sales (Hoffmaister, 1991). An empirical prediction following from this relationship is that a subsidy will induce an increase in exports of the incumbent exporters.

Although a subsidy is conditional on the participation in export markets, it can, in

principle, affect the decision to start exporting. The impact can go through two channels. First, assuming firm-specific sunk costs of entry (for example, due to the different requirements for the product quality upgrading faced by individual firms), the aforementioned increase in relative prices will translate into higher expected profits in export markets for some firms and will induce the marginal non-exporters to enter the export market by loosening their financial constraints. Second, in an environment with limited capacity to monitor the disbursement and the use of subsidies, some firms will have incentives to report fictitious export sales or even start exporting, if only to obtain access to the government funds. However, it would be difficult to disentangle empirically fictitious exports from those based on the optimal allocation decision of a profit-maximizing firm.

Government funds are available for all exporters. This assumption, however, is based on firm homogeneity and does not agree with the empirical observation that some exporters (approximately 20 percent in our sample) do not receive government support. There are several explanations for the observed pattern. First, in case of limited funds, subsidies may be allocated based on the 'first come, first served' rule. In this case subsidies are likely to be given to firms with better access to information regarding the availability of funds. Alternatively, administrative hassle to obtain subsidies may discourage some firms from applying for government support. We suggest that both access to information and ability to deal with administrative hassles would be highly correlated with the firm's connectedness to government officials distributing the funds and with the obtained subsidy amount. Empirically this would be reflected in differences in firm-specific subsidy rates that cannot be justified by the various allocation rules. Hence, we break down the variation in firm-specific subsidy rates into two components. The first, perfectly legal, is driven by the complexity of the existing export promotion schemes. In this case, firms will receive different amounts of subsidies depending on a firm's industry, location, use of imported materials or machinery, or the destination of its exports (see Section 4 for the institutional details of the export promotion schemes in Colombia). The second component stems from the degree of the firm's connectedness to the government officials that distribute the funds. Bergström (1998), Bagella et al. (2003), and Blanes and Busom (2004) link the likelihood of a firm receiving a subsidy to various measures of the firm's political weight (for example, lobbying capacity) and find considerable discretion on the part of policy makers in the allocation decisions. Anecdotal evidence suggests that this component may be particularly important in Colombia. According to Pegurier and Salgado (2002), the policies that gave governments discretionary power to set very uneven tariffs also allowed for the arbitrary disbursing of subsidies: "There was no real quest for efficiency. Instead,

as had happened to protectionist measures for industry, support for exports became the source of rent through wasteful privilege-seeking activities." (Pegurier and Salgado, p. 4: 2002).

There are two assumptions underlying our identification strategy. First, we assume that the amount of the received support is proportional to the degree of the firm's connectedness. That connected firms are not only more likely to receive subsidies but also receive larger subsidies is true even in industrialized countries (Bertrand et al., 2004). We have also seen above that it was certainly the case in Kenya. In 1998 Colombia's TI Corruption rank was 79 (out of the participating 85 countries) with a score of 2.2 (a perfect 10 describes a totally corruption-free country), thus ranking worse than Kenya with an index of 2.5. Hence, our assumption is not entirely ungrounded. We also assume that subsidies, assigned at the discretion of the officials disbursing the funds, have no discernible positive impact on export performance. In fact, the impact may be negative if subsidies result in costly competition among firms, whereby stronger lobbying for a subsidy by one firm requires other firms in the industry to lobby harder to get a given amount of support (Mitra, 2000) or if the firm spends considerable amounts of resources on lawyers or bookkeepers that would be able to decipher the complicated rules of the export promotion schemes and concoct ways of obtaining access to the government funds. More formally, we are testing the assumption that

$$H_0 : E[Y|Subsidy^*] - E[Y|Subsidy, Connectedness] \geq 0 \tag{1}$$

where

$$Subsidy^* \begin{cases} \geq Subsidy & \text{if } Connectedness \leq 0 \\ < Subsidy & \text{if } Connectedness > 0 \end{cases}$$

Expression (1) says that the difference between the mean of exports conditional on the 'true' level of a firm's subsidy rate $Subsidy^*$ and the the mean of exports conditional on the observed level of the subsidy rate $Subsidy$ and some variable $Connectedness$ is non-negative. We assume that the 'true' subsidy rate is the rate that would be observed if subsidies were allocated purely based on the known and legal allocation rules. Hence, we assume that the observed subsidy rate exceeds the 'true' subsidy rate whenever a firm is connected. Therefore, we test the assumption that exports conditional on a firm's subsidy rate as well as a proxy for its connectedness is smaller or equal to the level of exports that would be achieved if subsidies were allocated according to the

eligibility of a firm as defined by the government's criteria. This means that subsidies above the 'true' level have no or even a negative effect on exports.

Based on this framework, we speculate that disproportionately large subsidy rates are the result of nepotistic connections between firms and authorities in charge of the allocation of subsidies and, hence, ineffective. We then test empirically whether the responsiveness of the Colombian exporters to the subsidies is consistent with our conjectures.

3 Related Literature

While theory predicts that export subsidies will increase exports, many practical issues, such as the political environment, administrative capacity to monitor their distribution and use, etc. may interfere with their impact. The search for evidence on their effectiveness has thus been left to empirical analyses.

There has been considerable empirical interest in the effectiveness of export subsidies in developing countries (Frank et al., 1975; Low, 1982; Jung and Lee, 1986; Nogués, 1989; Hoffmaister, 1991; Arslan and van Wijnbergen, 1993; Faini, 1994; Moreira and Figueiredo, 2002). The results of these studies are conflicting, with the verdict overall coming out negative. Low (1982) documents the failure of the subsidy scheme in Kenya. He attributes the disappointing effect of the program to the poor implementation by, and the significant discretionary decision-making of, the bureaucrats in charge of allocating government grants. Arslan and van Wijnbergen (1993) attribute improvements in Turkey's export performance to a depreciation of the exchange rate rather than export subsidies. Nogués (1989) concludes that export subsidies in Argentina only increased allocative inefficiency, reinforced oligopolistic market structures, and provided incentives for rent seeking. While he acknowledges some positive impact of export subsidies in the case of Brazil (later supported by Moreira and Figueiredo, 2002), he argues that the success relied crucially on accompanying macroeconomic stabilization and import liberalization. He also points out that Mexico achieved a comparable positive export performance without relying on costly subsidies. Similarly, Hoffmaister (1991) finds a positive effect of a tax credit scheme in Costa Rica on exports, but concedes that, from a cost-benefit point of view, export subsidies have been a disproportionably costly way of achieving the rise in exports.

All of the aforementioned studies are based on industry-level data. Their major shortcoming is that they do not allow any conclusion with regard to firm-specific character-

istics influencing the success of export subsidy schemes. However, firm-level analysis of export subsidies is scarce for developed and non-existent for developing countries. Bernard and Jensen (2004) test the effect of export subsidies on exports of US firms by including an 'export promotion' variable in their empirical specification analyzing firms' decision to export. Using export promotion expenditures at the state level, they find that subsidies are neither economically important nor statistically significant. Görg et al. (2006) analyze the role of firm-specific subsidies in encouraging export activity in Ireland during 1986-2002 and find that subsidies, when sufficiently large, increase firms' exports but do not influence their decision to export.

One should note that these two studies have been conducted on a set of countries with a business environment unrepresentative of a developing country and a much better administrative capacity to control the distribution of funds. We enrich the existing firm-level evidence by providing an account of the effectiveness of export subsidies in a developing country with a limited capacity to monitor the disbursements of funds and their intended uses.

4 The Regulatory Framework for Export Subsidies in Colombia

The export promotion scheme in Colombia during 1981-1991 was complex. Specifically, the range of subsidies that have been available to individual firms included a reimbursement for firms with exports exceeding the value of the imported raw materials; a reimbursement for firms importing at least 60 percent of the raw materials; a reimbursement for firms importing machinery and equipment; a reimbursement for firms exporting over 60 percent of their production; a reimbursement for firms that have been participating in the export promotion schemes for at least three years; a reimbursement for firms located in the free economic zones (Santa Marta, Barranquilla, Cartagena, Candelaria, Cúcuta, Rio Negro, Eje Cafetero, Pacífico, Bogotá and Palma Seca); an additional tax reimbursement in proportion to the total value of indirect taxes. Finally, distinct reimbursements were given to firms with exports exceeding 2, 3 or 20 million US dollars. Export promotion funds were also available to firms operating in the domestic market which produced intermediate goods for exporters. However, their number was very small in our sample (see Section 5). The bottom line is that the rules varied by industry, location and, more importantly, by firm characteristics and the exact subsidy amount was identified on a case-by-case basis by the taxation and customs officers. Whereas our research would improve dramatically if we had exact

value of the reimbursements assigned for each qualification,[3] we were unable to obtain access to this information and have to resort to approximating the features of the export promotion scheme with the available information (see Section 6 for details).

The subsidies were paid in one installment in the period concurrent with the rewarded exporting activity. A small portion of the sample also reports export taxes to be reimbursed by the government. We include those as part of the overall export incentive package.

Before proceeding to the data analysis, we would like to emphasize the scheme's potential for abuse. Díaz and Escudero (2002) report that the introduction of the Tax Reimbursement Certificate CERT in 1983 was motivated mainly by the fraud opportunities (reimbursement on fictive exports) provided by the former certificate. Although we do not have similar reports on other export promotion measures, it is nevertheless plausible that they were just as prone to abuse. Moreover, it is unlikely that the new tax reimbursement certificate was immune to fraud.

5 Data, Sample Selection and a Preliminary Look at the Data

Our data come from the 1981-1991 panel of the Annual Manufacturing Surveys (AMS). The AMS data covers all manufacturing plants with ten or more workers.[4] Among other things, the AMS reports values of production, domestic and foreign sales, imported and domestically purchased intermediate inputs, wage bills by skill category, capital stocks, ownership, location, and subsidies. Roberts and Tybout (1996) provide a more comprehensive description of the data.

A look at the raw data reveals several patterns regarding the provision of subsidies in Colombia. With the exception of food production, each of the export-oriented industries

[3] For example, instead of just knowing that a reimbursement was granted for imports of machinery, it would be valuable to know whether a fixed amount was granted irrespective of how much was imported or whether it was a percentage of the value of the imported goods, whether it was based on the amount of imports or on the ratio of imports to the output, etc.

[4] The manufacturing industries used in this study and their respective ISIC codes are: 311 (food products), 312 (other food products), 313 (beverages), 314 (tobacco), 321 (textiles), 322 (clothing and apparel), 323 (leather products, excluding clothing and shoes), 324 (leather shoes), 331 (lumber, wood and cork products, excluding furniture), 332 (furniture), 341 (paper), 342 (printing and publishing), 351 (industrial chemicals), 352 (other chemicals), 353 (petroleum refining), 354 (petroleum and coal products), 355 (rubber products), 356 (plastic products), 361 (pottery), 362 (glass products), 369 (other products of non-metallic minerals), 372 (non-ferrous metals), 381 (metal products), 382 (machinery), 383 (electronic machinery and equipment), 384 (transportation equipment), 385 (professional and scientific equipments), 390 (miscellaneous manufacturing, such as jewelry, musical instruments, sporting goods, etc.).

is heavily subsidized - 70 to 80 percent of all exporters receive government assistance and the percentage of exporters receiving subsidies remains fairly stable across years. Graphs 1-4 suggest substantial variation in government support received by individual plants: while the median size of subsidies per peso of export sales is around 8-10 percent, it can be as small as two and as high as 20 percent, with many firms reporting subsidies amounting to more than a quarter of export sales.[5] The increase in subsidy rates in 1984 and 1985 (Graph 4) is due to a significant change in the composition of government support during these years. Tax rebates and export pre-financing became unimportant, whereas direct subsidies received greater weight (Ocampo and Villar, 1995). Note that it is also during these two years that the subsidy rates exhibited the largest variability, probably reflecting different rates at which the firms complied with the new rules.

The large number of outliers is striking. Although we clean the data to exclude erroneous observations and extreme outliers, we use a rather relaxed rule to define outliers and choose to keep the information on the plants receiving unusually generous amounts of subsidies relative to their export sales as depicted in Graphs 1-4.[6] The main reason behind this decision is reports on unclear and often discretionary rules to obtain export subsidies in Colombia discussed above. We want to know whether such, potentially inappropriate, discretionary handouts from the government affect the recipient's exporting behavior (and we anticipate the answer to be negative).

A closer look at Graph 3 suggests that location may play a role in whether a plant is likely to obtain a disproportionately high subsidy - Bogotá and Medellin seem to have the highest number of 'outliers'. Considering that Bogotá is the capital city of the country and Medellin is the second largest city, geographically very close to Bogotá, one may not discard the possibility that it is in these cities where the decisions on public funding are of most politicized and discretionary nature. Both areas, of course, are the epicenter of economic activity in the country and have the largest number of plants both exporting and non-exporting. The ratio of exporters to non-exporters, or percentage of exporters receiving subsidies, however, does not make these cities stand out from the rest of the country.

The data set contains 1,423 plants which have exported at some point during the

[5] In this part we present the data on subsidies in terms of subsidy rates, i.e., the amount of government support per peso of export sales. Since numerous factors determine absolute subsidy amounts, looking at their variation without taking those factors into account, would be pointless.

[6] We define an outlier according to the rule $Q1 - (2 \times IQR)$ or $Q3 + (2 \times IQR)$.

sample period. Only 146 of these have never received any subsidies. The rest have obtained subsidies at some point, although not necessarily during each year of foreign market participation - 301 plants report zero export subsidies along with positive export volumes. Summary characteristics of the key variables are provided in Table 1. Apart from the variation in the subsidy rates and slightly higher average export volumes, there appears to be little difference between the plants in our sample.

6 Empirical Strategy

In this paper we strive to answer the following questions: (A) how much variation in subsidies can be explained by firm characteristics that are commonly linked to the allocation rules, (B) what type of firms obtain government support, (C) how we can measure firms' 'connectedness' influencing subsidy receipt without direct measures provided by the data and (D) whether subsidies induce increases in export volumes and how the effectiveness of subsidies changes depending on our measure of firms' 'connectedness'. We explain below how we tackle each of the questions.

A. How Much Variation in Subsidies is Explained by Firm Characteristics that are Commonly Linked to the Allocation Rules?

We use analysis of variance (ANOVA) to determine the proportion of the total variation that can be explained by the various allocation criteria. We apply ANOVA stepwise: we start by the most obvious characteristic, exporting status; we continue by considering other common criteria such as industry, location and year; we then add firm-specific characteristics described in Section 4 on the subsidies allocation rules in Colombia. Finally, we add firm-specific fixed effects to see how much of additional variation can be explained by time invariant firm-specific unobservable factors. Each step tells us how much of the total variation in subsidies can be attributed to the addition of the relevant allocation rule. The goal of this exercise is to illustrate the degree of the explanatory power contained in the various criteria.

B. Allocation of Export Subsidies

To determine which firms obtain subsidies we model the allocation rule as follows:

$$\ln Subsidy_{ijt} = \beta_0 + \beta_1 \ln Exports_{ijt} + \beta_2 \ln Exports_{ijt}^2 + \gamma Z_{ijt} + \epsilon_{ijt} \qquad (2)$$

where $Subsidy_{ijt}$ denotes subsidy amount of firm i in sector j at time t. Vector Z contains information on the extent to which each firm satisfies the eligibility requirements for the various reimbursements. Thus, to account for the features of the export promotion scheme described earlier, we use a ratio of export volumes over the amount of the imported raw materials; the amounts of purchased and used foreign raw materials and the share of foreign raw materials in total (since different allocation rules apply to these three characteristics); the amount of imported machinery and equipment and export intensity of the firm. Although we have information on the exact threshold export amounts, beyond which the firms qualify for different subsidy amounts, we find that including a linear and a quadratic term on export volumes is a better alternative to a set of dummy variables identifying various thresholds. We use industry dummies to account for the possibility that the government may have differential stimuli for some goods (for example, 'non-traditional exports'), as well as for the possibility that the subsidies in some industries were introduced to neutralize the effects of tariffs. Moreover, we use year dummies to control for the occasional modifications in the existing export promotion measures.

We use the Heckman two-stage selection model to estimate Equation (2) (Heckman, 1976, 1979). The receipt of subsidies hinges on two factors: participation in exports and decision to apply for subsidies. The decisions to export or not to export and to apply or not to apply for subsidies were made by individual firms. Thus, firms that do not apply for subsidies constitute a self-selected sample. It is likely, for example, that it is the firms which would receive very small subsidy amounts that choose not to bother applying for them. A zero amount of subsidies in this case would not be a fair reflection of what firms would have received if they had chosen to apply for subsidies. An additional, and for this study more relevant, selection effect would arise if some firms receive subsidies because they have large error terms. That is, some firms may receive a large subsidy not because they qualify for this amount, but because they have a high value on some unmeasurable variable which is captured in the error term. In our case, it is ties to the officials distributing the funds that go unmeasured into the error term (of course, we do not exclude the possibility of other unmeasured or unmeasurable factors that influence the amount received). Finally, we would like to allow for the possibility that different factors influence the decision to apply for subsidies and the amount of the subsidies received. For these reasons we believe that the Heckman model is an appropriate procedure to obtain the predicted value of the subsidy amount. We posit that it would be unreasonable to assume that (1) the unmeasured factor does not influence the amount received, (2) every variable influencing the decision to apply for subsidies is controlled for, or (3) the same variables determine the decision to ap-

ply for subsidies (or the probability of receiving them) and the amount of the subsidy received. So, we follow the Heckman model, where in the first stage we estimate an equation determining the decision to apply for subsidies and in the second stage we model an expected amount of the subsidy.

Technically, the Heckman model could be identified when the same variables are used in the selection and in the outcome equations. In this case, identification is only based on the distributional assumptions about the residuals and is possible only due to the non-linearity of the inverse Mill's Ratio. The problem with relying on non-linearity as the only source of identification is that it may lead to imprecise estimates in the outcome equation, especially if the selection equation is not very good at determining the selection rule. Although, this is not the case in this study (the independent variables in our selection equation have a very high explanatory power with the R-squared of over 77 percent), we include an additional variable that appears in the selection equation but does not appear in the outcome equation: last period's exporting status of the firm. We motivate this choice theoretically: given high persistence in exporting behavior, firms who exported a year ago are very likely to export in the current period, i.e., to satisfy the main qualification criterion of obtaining subsides. The main determinants of the subsidy amount, however, are the export volume and the export intensity and not the exporting status of the firm in the past.[7]

C. Estimating Connectedness

Ideally, we would like to have exact information on the nepotistic and political connections between the firms' managers and the government officials distributing the funds. The second best option would be to compute the exact amount of the subsidy for which a firm is eligible and compare it to the obtained amount. To do this, we would need precise allocation rules. That is, we would need to know the exact numeric subsidy rate for each qualification criterion (for example, three percent for exports of goods, additional two percent for importing machinery, etc.) and the exact subsidy rate for each threshold (for example, three percent for exports of goods below 10,000 pesos, four percent for exports of goods amounting to more than 10,000, but less than 100,000 pesos, etc.) Unfortunately, such information is unavailable to us and we resort to approximating connectedness with the information we have at hand.[8] Different proxies

[7]Hysteresis models suggest that the persistence in exporting behavior is due to the existance of sunk costs (Roberts and Tybout, 1997). These costs affect the entry and exit patterns and not necessarily the size of the trade flows.

[8]Whereas we have no way of verifying by how much the amount of subsidies to which a firm is eligible that we obtained empirically differs from the amount computed numerically based on the

of the firm's connectedness have been proposed in the literature for data sets similar to ours, i.e., void of an exact identifier for the firm's connectedness. Bergström (1998) and Bagella et al. (2003), for example, use a firm's labor force to proxy for the firm's lobbying capacity. The results from Chaney et al. (2007) suggest that the variation in a firm's reported sales can act as a good proxy for the firm's political connectedness as politically connected firms provide significantly lower quality sales data than their unconnected peers.

We do the following: From the Heckman model described in part B above, we obtain predicted values for export subsidies and compare these to the observed subsidy amounts. We do so by taking the ratio of the observed to the predicted subsidy amounts. This is our proxy for 'connectedness' to be used in the rest of the analysis. Formally, we obtain $\widehat{Subsidy}$ from (2) and assume that $\widehat{Subsidy} = Subsidy^*$. Therefore, our measure for 'connectedness' is $Connectedness = \frac{Subsidy}{Subsidy^*} = \frac{Subsidy}{\widehat{Subsidy}}$ where[9]

$$Connectedness \begin{cases} \leq 1 & \text{if } \widehat{Subsidy} \geq Subsidy \\ > 1 & \text{if } \widehat{Subsidy} < Subsidy \end{cases}$$

where *Connectedness* is bounded at zero.

It may appear intuitively unreasonable why some firms would obtain less than what should be granted to them based on their characteristics and the allocation rules. We explain it by incomplete information regarding the allocation rules and it is more than plausible that lack of information is linked to a firm's connectedness: the better connected a firm, the more it is aware of its eligibility for government support and the best connected firms are able to receive more than what they qualify for.[10]

D. Estimating Impact of Subsidies on Export Performance

To answer the question of how the receipt of disproportionately high subsidies affects their effectiveness in stimulating exports, we estimate exporting behavior as a function of government support in form of an unrestricted Autoregressive Distributed Lag (ADL) model (in obvious notation):

precise allocation rules, we believe these measures to be highly correlated.

[9] We choose the ratio of received over predicted values because this presentation allows for a straightforward interpretation of our results. Obviously, we could reverse the ratio and preserve our findings.

[10] To check this conjecture, we also report results obtained from relaxing this assumption in Section 7.

$$\begin{aligned}\ln Y_{ijt} = \alpha_0 + \pi_0 \ln Y_{ij,t-1} + \pi_1 \ln Subsidy_{ijt} + \pi_2 \ln Subsidy_{ij,t-1} + \\ + \pi_3 \ln Subsidy^2_{ijt} + \pi_4 \ln Subsidy^2_{ij,t-1} + \beta_5 Connectedness_{ijt} + \\ + \pi_6 Connectedness_{ij,t-1} + \pi_7 \ln Subsidy_{ijt} \times Connectedness_{ijt} + \\ + \pi_8 \ln Subsidy_{ij,t-1} \times Connectedness_{ij,t-1} + \pi_9 X_{ijt} + \pi_{10} X_{ij,t-1} + \\ + \eta_j + \phi_t + \mu_{ijt} \qquad (3)\end{aligned}$$

The volume of exports for firm i in industry j and year t is regressed on the subsidy amount, the interaction term identifying connected firms and firm characteristics, where all terms enter contemporaneously as well as lagged one period. We draw on the extensive literature on firms' export supply to select additional controls for equation (3). X_{ijt} is a vector of variables that have been suggested as potentially important determinants of exporting behavior and includes measures of plant size (logarithm of employment), labor productivity (real output per worker), market share (firm's sales as a percentage of total industry sales), and time dummies. We also used the unrestricted ADL estimates to test for the dynamic common factor representation of (3) imposing the corresponding nonlinear restrictions on the parameters, but these restrictions were rejected by the data (see Table 5).

Restricting ourselves to a short-run interpretation of the coefficients obtained from the dynamic model, the coefficients π_1, π_2, π_3 and π_4 measure incremental returns to attracting government support, whereas π_7 and π_8 measure how the impact of the subsidies on the export promotion depends on a firm's connectedness. The overall impact of subsidies on exports is $\pi_1 + \pi_2 + 2 \times \pi_3 \ln Mean(Subsidy_{ijt}) + 2 \times \pi_4 \ln Mean(Subsidy_{ij,t-1}) + \pi_7 Mean(Connectedness_{ijt}) + \pi_8 Mean(Connectedness_{ij,t-1})$. In case connectedness reduces the effectiveness of subsidies in promoting exports, the sum of the terms $\pi_7 \times Mean(Connectedness_{ijt})$ and $\pi_8 \times Mean(Connectedness_{ij,t-1})$ will be negative.

The presence of the lagged dependent variable on the right-hand-side of the estimating equation and the fact that we have a panel data set with large N and relatively short T determines our choice of the estimation procedure. We estimate the dynamic model with a generalized method of moments (GMM) estimator, based on Holtz-Eakin et al. (1988) and Arellano and Bond (1991) to obtain consistent estimators of Equation (3). We use the difference GMM estimator, which uses first-differences to instrument for

levels in the estimation. This estimator has the advantage of purging all plant-level effects through the use of first differences. It also allows to instrument for right-hand-side variables that are endogenous. Given that subsidies are a direct function of exports, this is particularly important in our case. Hence, all right-hand-side variables in Equation (3) are assumed to be endogenous, with the exception of the year dummies, which are treated as strictly exogenous. For the lagged dependent variable on the right-hand-side, all available moment conditions are exploited, whereas less moment conditions are used for all other regressors (for subsidies and connectedness we use lagged levels dated $t-4$ as instruments while for the control variables, we use lagged levels dated $t-2$). To increase efficiency, we use the two-step estimator and employ the small sample correction for the asymptotic variance proposed by Windmeijer (2005).[11]

It should be noted that by construction, the term Connectedness indirectly contains information on the volume of exports, the dependent variable of the equation above. It enters through the predicted subsidy amount which appears in the denominator of the Connectedness measure, i.e., the ratio of the observed over the predicted subsidy amounts. Note that this does not imply that the estimated impact of connectedness on the effectiveness of subsidies has to be either negative or positive by construction.

7 Results

Table 2 reports the findings from the ANOVA analysis and breaks down the total variation in subsidy rates in separate components. Thus, 46 percent of total variation can be explained by the exporting status of a firm. Once we concentrate on exporters only, exports are very bad at explaining the variation in the subsidy rates (adjusted R-squared of 0.00). Industry and region are not much better. It is when we include the year effects that the adjusted R-squared increases to 13 percent. The next biggest contributor to the explanation of the total variation in subsidy rates is a plant-specific fixed effect which increases the total amount of explained variation to 31 percent. The point of this exercise is to show that much of the variation in subsidy rates remains a mystery and cannot be accounted for by the linear relationship with information on the degree to which a firm fulfills the official criteria set by the export promotion scheme, such as the use of foreign raw materials, or with other observable firm characteristics.

Table 4 reports our more detailed findings regarding the factors that affect the allocation process. In column (1) we report the findings from the OLS regression that ignores the censored nature of the dependent variable and the potential selection bias.

[11] To implement the estimator, we used Roodman's (2005) xtabond2.

In column (2) we report the findings from the probit regression in which we model the decision of the firm to apply for export subsidies, with an additional regressor - previous period's exporting behavior - that we believe to affect the receipt of the subsidies, but not their amount. In columns (3) and (4) we report the results of the Heckman Maximum Likelihood (ML) estimation. For comparison, in columns (5) and (6) we report the findings from the Heckman two-step procedure. The two-step Heckman estimates a probit in the selection stage and OLS in the regression stage. The two-step model provides a straightforward test for the presence of the selectivity bias. If the null hypothesis of no selectivity bias is rejected, estimating both stages simultaneously by Maximum Likelihood achieves more efficient estimates than the two-stage procedure and is therefore preferable. In our case, the Wald Test strongly rejects the null hypothesis that the coefficent of the Inverse Mills Ratio equals zero, which suggests the presence of selection bias. Looking at the estimate of ρ, the correlation coefficient, in column (3), shows that there exists statistically significant correlation between unobservables affecting the amount of subsidies obtained and the allocation of subsidies.

For the sake of completeness, we also conduct estimations using Tobit because subsidies are either zero or positive and find that, with few exceptions, the significance and sign of coefficients are not qualitatively different from those obtained by the Heckman Selection models. We estimate both a simple Tobit and a Random Effects Tobit. A comparison of those results indicates that the panel-level variance component is important and that the panel estimator is different from the pooled estimator.

The variables that consistently increase the propensity to receive export subsidies are past exporting experience, current volume of exports (although the return to export volumes is decreasing, as indicated by a negative and significant coefficient on the squared term), amount of indirect taxes (which may be indicative of a tax refund element of the export subsidy scheme), ownership status (limited partnerships, and in some specifications corporations, are more likely to obtain subsidies than other ownership forms), and firm's location in the two of the four largest cities of the country - Medellin and Bucaramanga - or in the rest of the country where some of the free economic zones are located (we cannot identify more precisely the location of firms in the free economic zones listed in the Section 4). The impact of the aforementioned factors is distinct in the two stages of the allocation decision. Firms with a smaller market share, for example, are less likely to receive subsidies, however, those that do, receive larger amounts than bigger firms. Firms paying higher indirect taxes are more likely to receive subsidies, but the amounts obtained are smaller. A firm's location is important for the propensity to receive subsidies, but not for the obtained amounts. In years

1983-1985 firms are no more likely to obtain subsidies than in 1981, but an average recipient gets substantially higher amounts. Starting in 1987, there is some evidence of decreasing subsidy amounts, which corresponds to the government attempts to reduce, if not eliminate, export subsidies. By 1990, the amounts decrease below the 1981 level and, by 1991, fewer firms apply and/or qualify for subsidies.

Curiously, none of the factors that we know to be part of the allocation rules, apart from the volume of exports, affects the propensity to obtain subsidies and only few affect the obtained amounts. The amount of foreign raw materials is positively correlated with the obtained amounts, but the share of foreign raw materials in total is negatively correlated with the obtained amounts. This finding is in sync with the allocation of subsidies to the firms that satisfy the minimal requirements on the use of imported raw materials, but still favor domestic suppliers. This rule is common in developing countries, where governments attempt to reinforce backward and forward linkages between the industries.

Overall, we find that although firm characteristics explain variation in subsidy amounts (Adjusted R-squared from the OLS regression is 0.78), many of the anticipated effects are insignificant. In Graph 5 we show a box plot of the ratio of observed over predicted subsidies and in Graph 6 we plot the predicted subsidy amounts from the Heckman ML estimation against the observed amounts. By construction, the ratio of observed over predicted subsidies is centered around 1, with a long positive tail. The positive outliers indicate the observations where firms obtain unusually high subsidy amounts compared to what they should be getting based on their characteristics.

As explained earlier, we use this difference between the observed and predicted subsidy amounts as a proxy for a firm's connectedness to government officials. An implicit assumption that we are making here is that all firms receiving subsidies are connected to government officials, although to a varying degree. That is, the observations indicated as blobs in the lower part of Graph 6 are firms that, although sufficiently connected to get some support, are not connected enough to obtain full information about the availability of funds and allocation requirements, and hence, do not receive the amounts that are due to them based on their characteristics.

Having constructed the ratio of the observed over predicted subsidies, we use it as a proxy for connectedness to estimate Equation (3). The results of this analysis are reported in Table 5, Column (1). First, we notice the relatively weakly persistent nature of exporting behavior - the coefficient on the lagged dependent variable is positive and

statistically significant. It implies that an increase in exports in $t-1$ by 10 percent increases exports in t by around 2.3 percent.

We estimate the contemporaneous and lagged effects of subsidies on exports. Table (5) shows that we do not reject for columns (1)-(4) the Arellano-Bond test (Arellano and Bond, 1991) for the absence of second-order serial correlation in the first-differenced residuals, which means that the estimator is consistent. We find that subsidies have a positive impact on exporting behavior across all specifications (1)-(4), which is given by the sum of the coefficients for t and $t-1$. This impact is decreasing in the amount of the subsidy as implied by the statistically significantly negative coefficient of the contemporaneous squared term of subsidies. In line with our hypothesis in Section 6, in Column (1), the coefficient on the interaction term for t is negative and statistically significant, whereas the coefficient for $t-1$ is not statistically different from zero. That is, the effectiveness of subsidies decreases as the gap between the observed and the predicted subsidy amounts increases. Overall, the estimated effect of a 10 percent increase in subsidies is to increase exports by 13 percent holding connectedness and everything else constant, which is a sizeable effect.[12] At the same time, increasing connectedness by 10 percent would lower exports by 50 percent, which is an even larger effect. For the control variables, we find larger firms, measured by their total employment to export more; a finding inline with theory (Melitz, 2003).

Since we are predominantly interested in the unusually high subsidy rates, we have repeated the analysis in which we truncated the ratio of the observed over predicted subsidies at 1. That is, we have allowed 'connectedness' to exist only for firms for which observed amounts are higher than predicted. There is a serious issue with this procedure, however. We need to make an arbitrary decision as to how to treat firms with lower than predicted subsidy amounts. If we assign the value of zero for every firm that is reporting a ratio below 1 (Column (2)), we assume that there is no difference between the firms not receiving any subsidies or receiving less than what they should. Making a distinction between them requires that we chose a different value. We could pull the value of the ratio down to the minimum observed ratio, 0.01, for all firms with the ratio less or equal to one (Column (3)). Alternatively, we could pull the ratio up to 1 (Column (4)). Experimenting with various truncation points supports overall our findings from Column (1) that subsidies have a positive impact on exports and that the impact becomes smaller as subsidies increase. The coefficient on the interaction term, however, loses its significance (except for Column (3) in which the coefficient of

[12]Obviously, in order to be able to 'hold constant' our measure of connectedness, we have to assume that also the 'true' subsidy rate increases by 10 percent.

the interaction term is positive and statistically significant in time t and negative and statistically significant in time $t-1$).

Using the same Colombian manufacturing data for a subset of firms used here,[13] Das et al. (2007) assess within a structural model the effect of different forms of exports subsidies on exports. They find that allocating higher subsidies per-unit of exports results in much larger increases of exports per peso spent than alternative subsidization policies which are targeted at the extensive margin through lump-sum transfers.[14] While we also find that subsidies have a positive impact on the intensive margin, we find as well evidence that the effect is decreasing in the amount of the assistance received and depends negatively on the degree of the firm's connectedness to government officials.

8 Conclusions

Economists are mostly opposed to export subsidies because they are (1) not well targeted, (2) not easy to administer, and (3) open to rent-seeking. In this paper we have explored the allocation of export subsidies in Colombia. Our evidence suggests that one or all the conjectures put forward by economists could be in action. First, allocation of subsidies is more complex than suggested by the literature based on industry-level analyses. Subsidies are firm-specific, with different factors affecting the allocation and the amount decisions. Many firm characteristics mentioned in the publicly available sources regarding the allocation rules do not seem to be important. Finally, there is a lot of unexplained variation in the subsidy rates obtained by various firms. Therefore, generally positive effects of export subsidies as a function of export volumes as found for example in Das et al. (2007) may be undermined by rent-seeking activities of recipient firms.

[13] Das et al. (2007) use data on firms belonging to ISIC codes 32 and 35.

[14] For their policy experiments, these authors use subsidy rates of 2, 5 and 10 percent.

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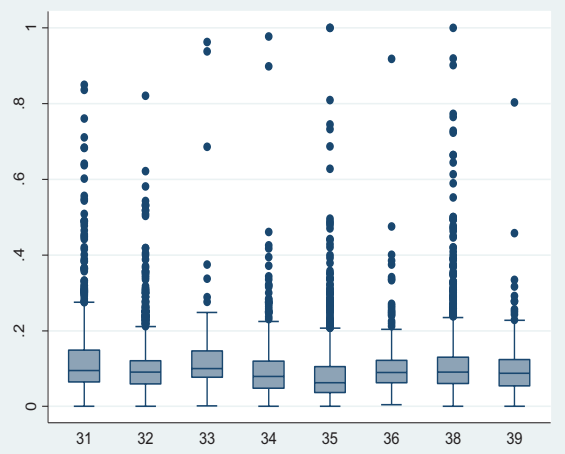
Figure 1: Box plot of export subsidy rates by 2-digit SIC industry



Definition of ISIC codes: **31** Manufacture of Food, Beverages and Tobacco; **32** Textile, Wearing Apparel and Leather Industries; **33** Manufacture of Wood and Wood Products, Including Furniture; **34** Manufacture of Paper and Paper Products, Printing and Publishing; **35** Manufacture of Chemicals and Chemical, Petroleum, Coal, Rubber and Plastic Products; **36** Manufacture of Non-Metallic Mineral Products, except Products of Petroleum and Coal; **37** Basic Metal Industries; **38** Manufacture of Fabricated Metal Products, Machinery and Equipment; **39** Other Manufacturing Industries.

Figure 2: Box plot of export subsidy rates by ownership (1 - limited partnership, 2 - corporation, 0 - other)

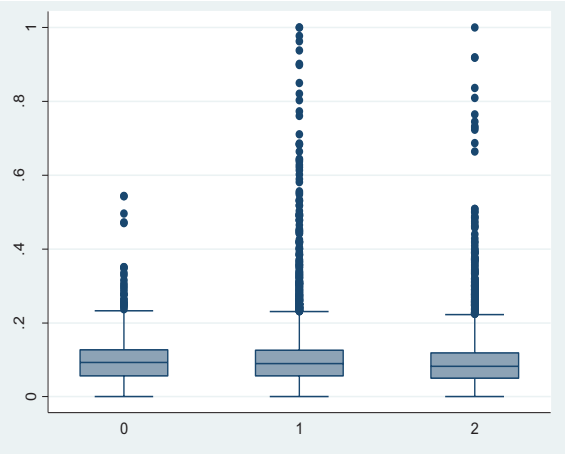


Figure 3: Box plot of export subsidy rates by metropolitan area

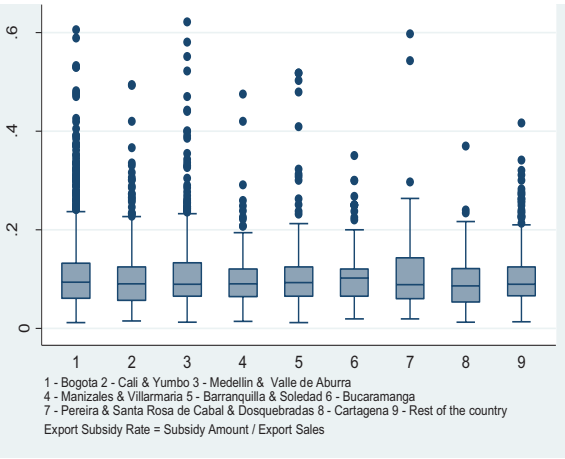


Figure 4: Box plot of export subsidy rates by year

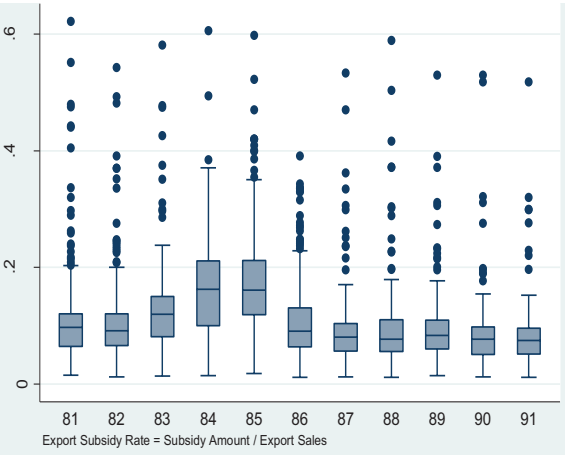


Figure 5: Box plot of the ratio of observed over predicted subsidies

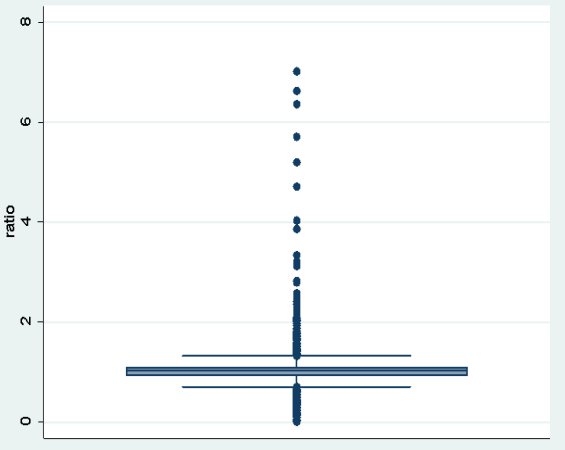


Figure 6: Scatter plot of predicted subsidy amounts from Heckman ML estimation against observed amounts

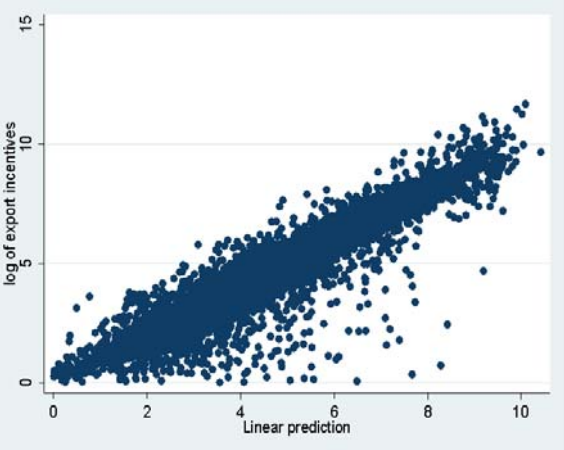


Table 1: Summary Statistics

	Exporters with Subsidies			Exporters Without Subsidies		
	N	Mean	SD	N	Mean	SD
Ln(Subsidies)	6033	4.695	2.126	831	0	0
Ln(Exports)	6033	7.206	2.122	831	6.435	2.714
Ln(Total Labor)	6033	4.609	1.225	831	4.440	1.204
Ln(Indirect Taxes)	6033	7.194	2.324	831	6.195	3.132
Market Share	6033	0.022	0.053	831	0.025	0.076
Ln(Purchases of New Machinery)	6033	4.763	3.072	831	4.413	3.103
Ln(Purchases of Foreign Raw Materials)	6033	4.969	4.366	831	3.929	4.340
Ln(Use of Foreign Raw Materials)	6033	9.047	1.759	831	9.106	1.935
Ratio of Exports Over Imports	6033	2.721	2.919	831	1.368	1.126
Share of Foreign Raw Materials	6033	0.242	0.299	831	0.189	0.287
Export Intensity	6033	0.212	0.273	831	0.195	0.315
Ln(Labor Productivity)	6033	5.276	1.080	831	5.461	1.312

Table 2: Proportion of total variation in subsidy rates explained by the criteria set in the allocation rules

Total Variation in Ln(Subsidy Rate)	110.09	58.95	58.95	58.95	58.95	58.95	58.95	58.95	58.95	58.95	58.95
Residual Variation	58.95	58.93	57.61	57.00	56.67	56.09	50.36	47.91	46.19	44.95	29.48
Adjusted R-squared	0.46	0.00	0.02	0.03	0.03	0.04	0.13	0.14	0.15	0.17	0.31
Number of Obs.	55415	6864	6864	6864	6864	6864	6864	6864	6864	6864	6864
Coefficients from ANOVA:											
Exporting Status	51.14										
Export Intensity		0.02	*0.01*	0.03	0.03	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*
3-digit Industry			1.32	0.81	0.82	0.80	0.91	0.95	0.97	0.74	0.16
Export Intensity × 3-digit Industry				0.60	0.54	0.46	0.50	0.50	0.49	0.49	0.26
Region					0.33	0.64	0.66	0.64	0.44	0.34	0.22
Export Intensity × Region						0.58	0.66	0.61	0.34	0.34	0.32
Period							5.73	1.92	0.59	0.44	0.36
3-digit Industry × Period								2.45	2.26	2.29	2.23
Region × Period									1.73	1.82	1.60
Purchases of New Machinery										*0.01*	*0.01*
Use of Foreign Raw Materials										0.05	0.00
Ratio of Exports to Imports										*0.00*	*0.00*
Share of Foreign Raw Materials in Production										0.10	*0.00*
Recipient of Subsidies for 3 or more years										1.15	0.13
Plant											15.50

Notes:
Factors **not** significant at 10% level are indicated in *italic*.

Table 3: The Determinants of the Allocation and Amount of Export Subsidies

	OLS	Probit	Heckman (ML)		Heckman (two-step)		Tobit	RE Tobit
	ln(Subsidies)	Recipient of ln(Subsidies)	ln(Subsidies)	Recipient of ln(Subsidies)	ln(Subsidies)	Recipient of ln(Subsidies)	ln(Subsidies)	ln(Subsidies)
Ln(Exports)	0.37***	0.96***	0.34***	0.91***	0.52***	0.96***	1.61***	1.44***
	(0.04)	(0.03)	(0.05)	(0.03)	(0.05)	(0.02)	(0.03)	(0.03)
Ln(Exports Squared)	0.03***	-0.06***	0.03***	-0.05***	0.02***	-0.06***	-0.05***	-0.04***
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Ln(Total Labor)	0.03	-0.01	0.01	-0.01	0.01	-0.01	-0.00	-0.18***
	(0.04)	(0.05)	(0.02)	(0.04)	(0.02)	(0.03)	(0.03)	(0.04)
Ln(Indirect Taxes)	0.12***	0.11***	-0.05***	0.07**	-0.03**	0.11***	0.21***	0.20***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.01)	(0.02)	(0.02)	(0.02)
Ln(Market Share)	-0.05	-0.13**	0.08***	-0.12***	0.07***	-0.13***	-0.12***	-0.10**
	(0.03)	(0.05)	(0.02)	(0.04)	(0.02)	(0.03)	(0.03)	(0.05)
Ln(Purchases of New Machinery and Equipment)	-0.00	-0.01	-0.00	-0.01	-0.00	-0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Ln(Imports of Raw Materials)	0.02**	0.02	-0.01	0.02	-0.00	0.02**	0.04***	0.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Ln(Use of Imported Raw Materials)	-0.11***	-0.09*	0.04*	-0.04	0.02	-0.09***	-0.17***	-0.03
	(0.03)	(0.05)	(0.02)	(0.04)	(0.02)	(0.03)	(0.03)	(0.04)
Ratio of Exports Over Imports	-0.00	0.00	-0.00	0.00	-0.00***	0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Share of Imported Raw Materials in Production	-0.21*	0.04	-0.26***	0.12	-0.26***	0.04	-0.30***	-0.21
	(0.11)	(0.18)	(0.08)	(0.16)	(0.05)	(0.11)	(0.12)	(0.15)
Export Intensity	0.50**	0.02	0.40***	0.00	0.39***	0.02	0.46***	0.30*
	(0.24)	(0.22)	(0.10)	(0.22)	(0.07)	(0.15)	(0.14)	(0.18)
Limited Partnership	0.32***	0.25**	0.04	0.21**	0.07*	0.25***	0.67***	0.44***
	(0.11)	(0.11)	(0.06)	(0.10)	(0.04)	(0.08)	(0.08)	(0.12)
Corporation	0.39**	0.20	0.09	0.09	0.12***	0.20**	0.68***	0.55***
	(0.17)	(0.13)	(0.08)	(0.12)	(0.05)	(0.08)	(0.09)	(0.14)
Observations	12081	10655	10655		10655		12081	12081
R-squared	0.784							

Notes:
1. Standard Errors in parentheses.
2. * indicates significance at 10%; * at 5%; ** at 1%.

Table 4: The Determinants of the Allocation and Amount of Export Subsidies - continued

	OLS	Probit	Heckman (ML)		Heckman (two-step)		Tobit	RE Tobit
	ln(Subsidies)	Recipient of ln(Subsidies)	ln(Subsidies)	Recipient of ln(Subsidies)	ln(Subsidies)	Recipient of ln(Subsidies)	ln(Subsidies)	ln(Subsidies)
Location - Cali	0.06	0.19	0.01	0.14	0.03	0.19***	0.17**	0.24*
	(0.07)	(0.13)	(0.05)	(0.11)	(0.04)	(0.07)	(0.08)	(0.14)
Location - Medellin	0.15***	0.45***	-0.07	0.35***	-0.02	0.45***	0.38***	0.40***
	(0.05)	(0.09)	(0.04)	(0.08)	(0.03)	(0.07)	(0.06)	(0.12)
Location - Manizales	0.10	-0.00	0.10	-0.11	0.12*	-0.00	0.24*	0.06
	(0.18)	(0.23)	(0.10)	(0.20)	(0.07)	(0.13)	(0.14)	(0.27)
Location - Barranquilla	0.18**	0.13	0.00	0.11	0.01	0.13	0.24***	0.35**
	(0.09)	(0.17)	(0.06)	(0.14)	(0.04)	(0.09)	(0.09)	(0.16)
Location -Bucaramanga	0.24***	0.50***	-0.01	0.33***	0.03	0.50***	0.43***	0.55***
	(0.06)	(0.14)	(0.05)	(0.12)	(0.05)	(0.11)	(0.11)	(0.18)
Location - Pereira	0.21	0.26	0.10	0.16	0.12	0.26*	0.28*	0.51*
	(0.17)	(0.18)	(0.09)	(0.15)	(0.07)	(0.15)	(0.15)	(0.28)
Location - Cartagena	0.18	0.27	0.06	0.09	0.10	0.27*	0.42***	0.29
	(0.32)	(0.22)	(0.09)	(0.17)	(0.07)	(0.15)	(0.14)	(0.27)
Location - Other	0.18**	0.37***	-0.05	0.30***	-0.01	0.37***	0.41***	0.46***
	(0.09)	(0.13)	(0.06)	(0.11)	(0.04)	(0.08)	(0.08)	(0.15)
Year 82	-0.10**	-0.23***		-0.09		-0.23**	-0.17	-0.13
	(0.04)	(0.09)		(0.09)		(0.11)	(0.12)	(0.09)
Year 83	0.16***	0.00	0.27***	0.00	0.31***		0.39***	0.38***
	(0.05)	(0.00)	(0.06)	(0.00)	(0.05)		(0.12)	(0.09)
Year 84	0.26***	-0.02	0.54***	-0.05	0.57***	-0.02	0.60***	0.65***
	(0.05)	(0.09)	(0.06)	(0.08)	(0.05)	(0.11)	(0.12)	(0.09)
Year 85	0.28***	-0.03	0.63***	-0.02	0.66***	-0.03	0.63***	0.65***
	(0.06)	(0.09)	(0.06)	(0.08)	(0.05)	(0.11)	(0.12)	(0.09)
Year 86	0.06	0.08	0.08	0.16*	0.11**	0.08	0.25**	0.25***
	(0.06)	(0.09)	(0.07)	(0.09)	(0.05)	(0.11)	(0.11)	(0.09)
Year 87	-0.01	-0.00	-0.05	0.02	-0.02	-0.00	0.07	0.12
	(0.06)	(0.10)	(0.06)	(0.09)	(0.05)	(0.11)	(0.11)	(0.09)
Year 88	0.01	0.02	-0.04	0.10	-0.02	0.02	0.11	0.17*
	(0.06)	(0.09)	(0.06)	(0.09)	(0.05)	(0.11)	(0.11)	(0.09)
Year 89	-0.06	0.04	-0.07	0.05	-0.04	0.04	0.00	0.07
	(0.06)	(0.09)	(0.06)	(0.09)	(0.05)	(0.10)	(0.11)	(0.09)
Year 90	-0.23***	0.13	-0.24***	0.17*	-0.21***	0.13	-0.15	0.02
	(0.06)	(0.10)	(0.06)	(0.09)	(0.05)	(0.10)	(0.11)	(0.09)
Year 91	-0.27***	-0.32***	-0.20***	-0.26***	-0.19***	-0.32***	-0.31***	-0.09
	(0.06)	(0.09)	(0.06)	(0.09)	(0.05)	(0.10)	(0.11)	(0.09)
Exporter in Previous Year		0.58***		0.56***		0.58***		
		(0.06)		(0.05)		(0.05)		
ρ			-0.840					
			(0.013)					
LR ($\rho = 0$)			205.06					
			p=0.000					
Inverse Mills Ratio					-0.37***			
					(0.12)			
Constant	-0.90*	-4.15***	0.92***	-3.95***	-4.15***	-4.034***	-6.52***	-6.52***
	(0.47)	(0.75)	(0.34)	(0.63)	(0.46)	(0.428)	(0.49)	(0.70)
SD Individual Effect								1.30***
								(0.04)
SD Error Term								1.40***
								(0.01)
Observations	12081	10655	10655		10655		12081	12081
R-squared	0.784							

Notes:
1. Standard Errors in parentheses.
2. * indicates significance at 10%; * at 5%; ** at 1%.

Table 5: The Effect of Export Subsidies on Exports - Difference GMM Results

		Connectedness truncated		
		at 0 if	at 0.01 if	at 1 if
		Predicted Subs.≤Observed Subs.		
Dependent Variable: log(Exports)	(1)	(2)	(3)	(4)
Ln(Exports) $t-1$	0.225**	0.026	0.275***	0.070
	(0.093)	(0.093)	(0.104)	(0.088)
Ln(Subsidies) t	3.725***	2.912***	2.073***	3.497***
	(0.324)	(0.159)	(0.145)	(0.903)
Ln(Subsidies) $t-1$	-0.861*	-0.295	-0.409*	0.071
	(0.455)	(0.353)	(0.221)	(0.734)
Ln(Subsidies) Squared t	-0.141***	-0.233***	-0.136***	-0.200***
	(0.019)	(0.030)	(0.021)	(0.041)
Ln(Subsidies) Squared $t-1$	0.026	0.031	0.006	0.005
	(0.019)	(0.036)	(0.022)	(0.036)
Connectedness t	-3.701***	-0.553	-2.523***	0.918
	(0.457)	(1.231)	(0.368)	(0.874)
Connectedness $t-1$	1.313***	0.952	1.511***	-0.509
	(0.498)	(1.177)	(0.531)	(0.653)
Connectedness×Ln(Subsidies) t	-1.066***	0.011	0.269***	-0.981
	(0.349)	(0.205)	(0.080)	(0.811)
Connectedness×Ln(Subsidies) $t-1$	0.185	-0.198	-0.161**	-0.186
	(0.299)	(0.170)	(0.076)	(0.563)
Ln(Total Labor) t	1.369***	2.105***	1.752***	1.840**
	(0.542)	(0.748)	(0.652)	(0.765)
Ln(Total Labor) $t-1$	-0.126	-0.925	-0.706	-0.939
	(0.464)	(0.659)	(0.544)	(0.573)
Ln(Market Share) t	0.165	1.277*	0.750	0.937
	(0.442)	(0.658)	(0.558)	(0.591)
Ln(Market Share) $t-1$	-0.211	0.480	-0.279	-0.258
	(0.388)	(0.590)	(0.459)	(0.552)
Ln(Labor Productivity) t	0.369	0.294	0.647	0.562
	(0.449)	(0.626)	(0564)	(0.618)
Ln(Labor Productivity) $t-1$	-0.107	-0.679	-0.633	-0.737
	(0.458)	(0.685)	(0.539)	(0.633)
Observations	9232	9232	9232	9232
Number of plants	1417	1417	1417	1417
AR(1) P-value	0.000	0.000	0.000	0.000
AR(2) P-value	0.482	0.953	0.162	0.583
Hansen J P-value	0.245	0.423	0.680	0.313
COMFAC	0.001	0.000	0.000	0.000

Notes:

1. Windmeijer-corrected standard errors in parentheses.
2. * indicates significance at 10%; * at 5%; ** at 1%.
3. Regressions include constant and year dummies.
4. COMFAC is a minimum distance test of the common factor restrictions.