The Costs of Bureaucracy and Corruption at Customs: Evidence from the Computerization of Imports in Colombia

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Abstract

How deep are the economic effects of e-government at the public agencies that interact most intensely with the business sector? We assess the effects of the computerization of import transactions in Colombia, relying on its sequential implementation for identification. Compared with non-exposed producers, importers exposed to reformed ports experienced an increase in sales, input use, imports and exports, as well as revenue productivity. Reformed ports increased the number and size of import transactions, and tax collections per dollar of imports. The reform also increased the predictability of customs clearance times and reduced smuggling and corruption cases at ports.

JEL codes D73, F61,O12, K42

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1 Introduction

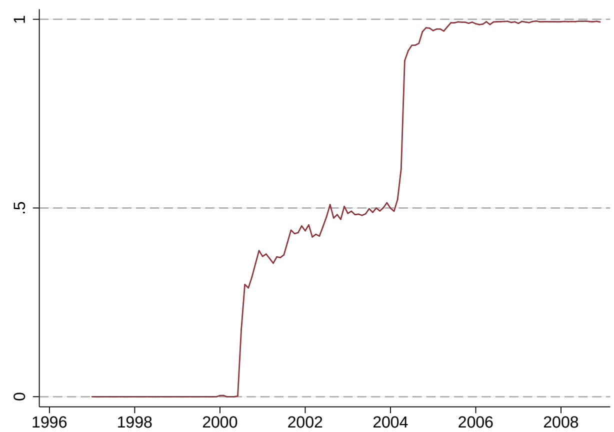
Barriers to international trade affect business activity and welfare through various channels. The literature has mostly focused on the effects of import tariffs, because of their prominent role in trade policy and the ease of accessing data on tariffs.[1] Understanding the consequences of other barriers to trade has become equally important: after decades of tariff reductions, many countries have little practical or political room for additional tariff cuts, while non-tariff barriers "have replaced tariffs as the primary tools of trade policy" (Goldberg and Pavcnik, 2016). Reforms to customs are a particularly promising area for policy improvement and research: not only are inefficiency and corruption at ports common complaints of the business sector in many economies, but also, in stark contrast to tariff reductions, these reforms need not sacrifice fiscal revenue in order to boost trade. Moreover, increasingly efficient information technologies open new windows of opportunity for relatively low-cost interventions that enhance the efficiency of customs and other government agencies. They also limit the room for corruption by reducing contact between customs officials and businesses trading with other countries, or more generally between public agents and businesses.

We study the effects of the computerization of customs procedures in Colombia on different dimensions of business performance, and on indicators of the effectiveness and transparency of customs, to shed light on the mechanisms by which customs affect economic outcomes. Between 2000 and 2005, Colombia went from all import transactions being processed manually to essentially 100% computerization of those transactions (Figure 1). The reform allowed importers to complete almost every step of the importing procedure online rather than in person, thus limiting the interaction with customs agents. The need for physical inspections was reduced, and the decision to undertake such inspection, previously at the discretion of the customs agent, was delegated to an algorithm on the basis of risk profiles and inconsistencies between the declarations of the foreign exporting firm, the importing firm, the transporter, the warehouse, and the bank where tariff payments were made. These changes reduced bureaucratic hurdles and increased efficiency. Customs are often mired in bureaucracy and corruption

[1] For instance, see Bustos, 2011 for Argentina; Halpern et al., 2011 for Armenia; Lileeva and Trefler, 2010for Canada; Attanasio et al., 2004, Eslava et al., 2013, and Fieler et al., 2018 for Colombia; Pavcnik, 2002 for Chile; Topalova and Khandelwal, 2011, and De Loecker et al., 2016, for India; and Amiti and Konings, 2007 for Indonesia.

in developing countries, potentially imposing large costs on businesses and the economy as a whole.[2] Non-modernized customs offer customs officials significant discretion to stop cargo, and a broad bureaucratic toolkit from which to draw on to justify delays and hurdles, creating opportunities for rent extraction Sequeira (2015). The use of information technologies at customs can address these barriers by reducing bureaucracy and limiting interactions between firms and customs agents.

Figure 1: Proportion of imports in Colombia declared by computer

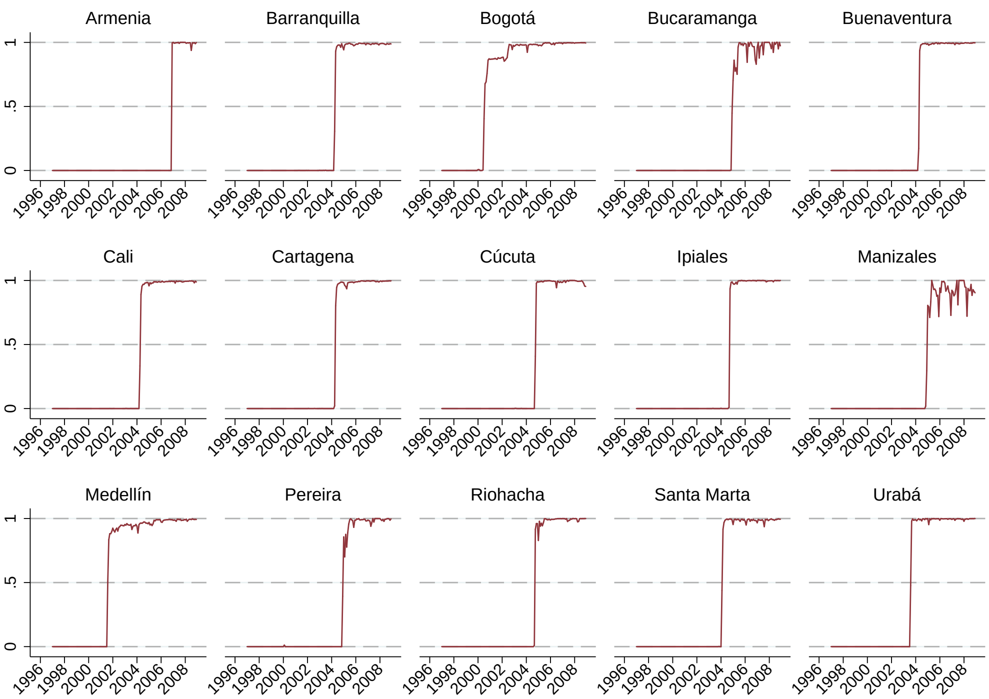


Note: Figure 1 presents the proportion of import transactions in Colombia that were declared by computer. Using data from the DIAN (Dirección de Impuestos y Aduanas Nacionales of Colombia) from 1994 to 2014, we calculated by month, the average across all transactions of a dummy equal to one if the transaction was declared by computer.

The limited capacity of the team that put the computerization in place in Colombia led to a sequential implementation of the reform across the 26 customs posts (which we refer to as "ports" regardless of the transportation mode that each handles: air, sea or land). From the beginning of 2000 to the end of 2005, customs moved from zero to over 99% of transactions computerized nationally, sequentially adding ports to the computerized system (Figure 2). This sequential implementation, together with rich data on businesses and ports, offers an opportunity to isolate the causal effects of the reform.

[2]Using data for Uruguay, Martincus et al. (2015) estimate that a 10% delay in processing times at customs reduces exports at the mean exporting firm by close to 4%. In most of the developing world, firms responding to the World Business Environment Surveys identify customs and trade regulations as the second most serious constraint on private sector development (Batra et al., 2003).

Figure 2: Proportion of imports declared by computer in the 15 largest customs



Note: Figure 2 presents the proportion of imports declared by computer for each of the 15 largest ports. Using data from the DIAN from 1994 to 2014, we calculated by port the monthly average across all transactions of a dummy equal to one if the transaction was declared by computer.

We begin by analyzing the impact of the reform on businesses taking advantage of rich yearly panel data, spanning the pre- and post-reform periods (i.e. 1998-2009). We combine data on plant performance that covers all non-micro manufacturing plants in Colombia, with data on import transactions, including FOB import value, clearance time at customs, and tariffs paid by all firms that own such manufacturing plants. We subsequently use other sources and levels of data to investigate whether the reform increased the effectiveness of customs at collecting taxes, at reducing smuggling and import underdeclaration, and at reducing corruption cases at customs. We also investigate whether, as a result of the computerization, the time to clear customs became more predictable for importers, and whether they seem to be willing to have their cargo travel further just to go through a computerized port.

In our plant level analysis, we conduct a triple difference estimation of the effect that computerization at ports relevant to the plants had on the performance of importing plants compared with the effect on non-importing plants. This provides a test of whether the reform facilitated imports for firms that were more likely to go through the reformed ports. We also estimate double differences that capture changes in the outcome variables induced by computerization at the associated port for a plant that was either previously a non-importer or an importer. This allows us to test how each group was impacted by the reform. We control for differential trends by plant and port size to address potential concerns that the sequencing of the reform may have been correlated with the efficiency of ports, and this efficiency may in turn correlate with the dynamics of plants' performance in the port's area of influence, potentially biasing the double difference coefficients. Pre-reform parallel trends hold for both double and triple differences.

Our plant-level analysis explores two different approaches to matching plants with ports. In our geographic specification, a port is assigned to each plant based on the frequency with which, prior to the reform, all plants from the plant's same municipality imported through a given port. In the second specification, we match plants to ports based on the frequency with which the inputs that the plant used before the reform were imported through a given port. We find that, compared with producers associated with unreformed ports, the computerization of customs led to a gradual and substantial increase in the revenue, productivity and economic activity of importers. Sales expanded for importers by 5.2%, and contracted by 3.9% for non-importers in the same municipality, consistent with non-importers being more exposed to the pro-competitive effects of increased imports than its cost-reducing effect. When matching ports by inputs, the effect on importers' growth remains positive and significant, while the spillover effect is positive for non-importers that use inputs that are more likely to go through reformed ports.

Qualitatively similar effects are found for the number of employees, expenditures in material inputs, exports, and measures of revenue productivity. The effects are observed starting at the first year after treatment and grow in size as time elapses. In line with the findings of Beck et al. (2005), the effects on importing firms are markedly heterogeneous by plant size; they are concentrated in the bottom size quartiles and are negligible and not statistically significant for the top quartile, likely composed of plants owned by the firms that were already best equipped to deal with the hurdles at customs before the reform.

Additional estimations allow us to investigate other outcomes of interest and shed light on the channels behind these results. The picture that emerges not only points to actual improvements in the efficiency and predictability for importers, but also suggests enhanced capabilities to collect tariff payments, a reduction in smuggling by importers and a reduction in corruption by customs officials.

We use a double difference identification strategy to estimate the effects on outcomes related to the import transactions by the firm that owns the plant, for importers at reformed vs. importers at unreformed ports. The results indicate that, as a consequence of computerization, plants in the area of influence of a port pay higher tariff amounts to the customs administration, both in relation to the FOB value of the transaction and the amount due in theory. Comparing changes in the characteristics of transactions at reformed vs. un-reformed ports, we also find that the computerization led to a large increase in the number of import transactions going through the treated ports, and in the total annual value of imports declared. Total taxes collected at customs increased significantly as a result of an increase in both the effective tax rate on imports and the tax base. The ratio of taxes paid to taxes due also increased, suggesting a reduction of fraudulent non-payments. Using UN Comtrade data to compare imports declared by Colombian importers with exports to Colombia declared by foreign producers, at a disaggregated product level, we conclude that 60% of the increase in the tax base is due to an actual increase in Colombian imports and 40% to a reduction in smuggling and under-declaration of imports. We find an increase in the transportation costs of merchandise reaching treated customs, suggesting that firms are willing to travel further in order to go through a computerized port. Sequeira and Djankov (2014) similarly find that firms in southern Africa are willing to travel significantly longer distances in order to go through a less corrupt port. This redirection of imports reveals a preference for reformed customs and can explain why the time to clear customs didn't decrease with the treatment.

While finding clear evidence of changes in corruption is always challenging, we find suggestive evidence that the reform reduced corruption at ports. Tackling corruption was indeed one of the objectives of the reform, as indicated in DIAN's internal documents that we survey (DIAN is the Colombian customs authority, by its acronym in Spanish). These documents discuss how computerization would sever lines that made import transactions susceptible to corruption, in particular the discretionary power of on-the-ground customs officials. Among the evidence supporting this mechanism is, first, the increase in the effective tax rate that follows the reform, consistent with a reduction in miscategorization and non-payment of due taxes. Second, the reform reduced discrepancies between imports declared in Colombia and the ones declared by exporters, which are an indicator of tax evasion at customs (Fisman and Wei, 2004). Third, in line with a reduction of the discretionary power of on-the-ground customs officials, we find that the reform significantly increased the predictability of the duration (number of days) of customs transactions. Finally, using data from the *Procurador General de la Nación* (the General Prosecutor), we find that the computerization of a port is followed by a drop in the number of judiciary cases related to corruption at DIAN in the port's municipality, although the small number of such cases suggests that this result should be interpreted with caution.

The combination of benefits for firms and increased tax collection shows that the reform can be a win-win for the private sector and the government. The large gains for the firms point towards corruption that was primarily coercive and harmful for the economy, but the increase in tax collection reveals that it was also partly collusive Sequeira and Djankov (2014). It appears that the largest firms were less likely to entirely pay their taxes before the reform, which may partly explain why they benefited the least from the reform.

This paper contributes to the literature on the effects of administrative trade barriers by providing evidence of the effect of customs computerization on business performance through a host of mechanisms, including increased efficiency and reduced corruption at ports, and the reduction of smuggling and evasion of tariff payments. Computerization is a relatively low cost reform that addresses a widespread and potentially high cost barrier to trade: bureaucracy at customs. While numerous case studies document the benefits and challenges of customs computerization (De Wulf and Sokol, 2004; Engman, 2005), to the best of our knowledge there is no causal evidence of the impact of the use of information technologies at customs on firm-level outcomes. The effect of customs efficiency on the volume of trade is addressed in a series of evaluations of different trade facilitation programs (Martincus et al., 2015; Fernandes et al., 2015). Our study offers a broader perspective by extending the analysis to a wide range of firm-level, product-level, and customs-level outcomes.

We complement the abundant body of literature on the effect of import tariffs on business performance with evidence on the potential effectiveness of reforms on the administrative margin of trade policy, with a particular focus on e-government interventions. The trade literature has found evidence that tariff reductions are associated with increases in revenue productivity, via increased firm efficiency and quality, with more mixed results regarding markups (for surveys, see Goldberg and Pavcnik, 2016; De Loecker and Goldberg, 2014; Melitz and Redding, 2014).[3] The positive effects of tariff reductions are much stronger for input tariffs compared with output tariffs (De Loecker et al., 2016). Consistent with our results, this points to the clear-cut benefits for firms of trade-related input cost reductions and the more ambiguous pro-competitive effects of trade, clearly positive in terms of stimulating efficiency and quality gains, but potentially negative for size and revenue productivity if markups decrease sufficiently.

This paper also contributes to the literature that provides firm-level evidence on the relationship between institutions and development, providing direct evidence of the potential of administrative reforms to impact the business sector. A particularly interesting stream of that literature focuses on the effects of corruption, which has been found to take a toll on development that goes beyond simple transfers from the business sector to corrupt officials. [4] Using firm-level surveys in Uganda, Svensson (2003) finds that a one percentage point increase in bribes reduces annual firm growth by three percentage points, which is three times the negative impact as the one of equivalent formal taxes. Sequeira and Djankov (2014) and Sequeira (2016) are among the papers that look most closely at firms and corruption at customs, showing that firms are willing to increase their transport costs in order to go through a less corrupt port, and that tariff liberalization can have limited effects on trade because of corruption at customs.

[3] See Pavcnik (2002), Lileeva and Trefler, 2010, Bustos, 2011, Eslava et al. (2013), and De Loecker et al. (2016)for positive effects of output tariffs reductions on revenue productivity, efficiency (quantity productivity), technology adoption, and markups. Even larger positive effects on revenue productivity have been found for input tariff reductions (Amiti and Konings, 2007, for Indonesia; Topalova and Khandelwal, 2011, and De Loecker et al., 2016, for India), while Fieler et al. (2018) find positive effects on quality of input tariffs cuts in Colombia.

[4] It can discourage investment (Samphantharak and Malesky, 2008) and human capital accumulation (Ferraz et al., 2012; Reinikka and Svensson, 2004), lead to the misallocation of capital (Khwaja and Mian, 2005), or talents (Ebeke et al., 2015). It also affects public expenditures Olken (2006), and harms the government's ability to correct externalities (Olken and Barron, 2009). For an overview of the empirical literature on corruption in developing countries see Olken and Pande (2012).

However, to the best of our knowledge, there is no causal evidence of the consequences of a bureaucratic and corrupt customs on firm-level outcomes other than imports and exports. Our research also closely relates to the growing literature on the potential of information technologies to improve efficiency and reduce corruption, which has mostly focused on e-government interventions in the delivery of transfers (Giné et al., 2012; Lewis-Faupel et al., 2016; Banerjee et al., 2016; Muralidharan et al., 2016). We complement this literature with primary evidence of the effect of a large-scale intervention at customs on outcomes at the firm and the port.

The cost of the computerization of imports (about USD 9 million) is dwarfed by the observed benefits to the economy and for tax revenues. The policy lessons from our research not only apply to many low income countries that have not yet computerized their customs procedures, but more generally point to large potential gains from improving the efficiency of the state agencies with which the business sector interacts, and limiting the scope for corrupt practices at these agencies. However, the applicability of this customs reform in different contexts is not granted, given the need for political will and institutional capacity to adopt and successfully implement it (De Wulf and Sokol, 2004). In fact the lessons from this research are also applicable to Colombia, where customs underwent large improvements at the beginning of the century, but today, still lag behind many middle income countries, and are in urgent need of a major upgrade.

2 Context and the Computerization of Customs in Colombia

At the beginning of the 21st century, Colombia adopted a program to fully computerize import transactions at customs. The program, named Siglo XXI, was regarded as a game changer by DIAN's management and operation staff in 2016, when we interviewed them.[5] Siglo XXI started in 2000. By the end of 2005 all transactions at ports were computerized (Figure 1). As a consequence of the limited capacity of the implementing team, the computerization occurred in different ports at different times, as illustrated in Figure 2.

The new system facilitated the import process by allowing users to declare their imports online rather than in paper at the time of arrival at customs. It

[5]DIAN started the development of *Siglo XXI* in 1997 with the project "*Sistema Global de Información y Mejoramiento de la Gestión Aduanera Siglo XXI*"

was expected to reduce costs associated with bureaucracy and immobilization of merchandise at customs. DIAN's internal documentation stresses that the reform goes beyond a digitization of the declaration. It emphasizes a broad "need to bring the technological strategy in line with developments in regulation and procedures." For this reason, our study must be interpreted as an evaluation of the entire reform that was made possible by the computerization.

DIAN shared internal documents describing the reform. The title of the most comprehensive document, "Advances in the Control of Corruption through the Customs System Siglo XXI"[6] places tackling corruption as a central goal of the reform. It provides detailed accounts of the flaws of the existing system and how the reform aims to address them. We present key features of the reform, as described by the internal documentation, emphasizing the significance of DIAN staff's opinions by keeping them in their own words when possible:

- Before the reform, "The only criteria to inspect the charge was the judgment of the customs inspector", but after the reform, "the possibility of corruption acts has been diminished since the transporter interacts with the customs officer only when the computer system calls for such interaction"

- Prior to the reform there was a risk of a cargo not being presented to the customs office, or of the quantities or values being under-reported. After the reform, customs are automatically informed of deliveries.

- "In the previous scheme, there was no transmission of the message with payment notification by the bank, which generated multiple problems that were difficult to detect before the release of the cargo (falsifications, adulteration, etc.)", whereas in the new scheme, the DIAN receives the online confirmation of payments and "once the payment is made, the release request is executed without any intervention by customs authorities". "About 86% of the merchandise is automatically released" compared with 30 to 40% before the reform.

- Siglo XXI allowed for instant recording and comparison across declarations by the transporter, the importer, the warehouse, and the bank. "Discretion by the officials for the selection of documents for inspection purposes is completely eliminated".

[6] All citations are translated from Spanish

- The physical or documentary inspection processes in the new system are triggered by inconsistencies in the declarations and risk profiles rather than arbitrary decisions of customs officers. "In the new scheme, customs executes physical inspections on no more than 9% of the documents. This is an improvement, since before, the rate was over 50%."

- The "risk that the customs inspector declares conformity between the cargo manifest and the physical cargo when there is a difference that should cause its apprehension" was reduced by the "full control and identification of the actors who perform each one of the checks". That is, the new system generated traceability and better accountability from the customs officers.

The report, written in 2001, after implementation had started in Bogota, analyzes short-term effects of the program. A first set of benefits presented in the report relates to an increase in convenience for importers due to the reduction in bureaucracy. "With the exception of the physical inspection and payment, all the processes related to the declaration's obligations can be developed from its management center through the Internet." The documentation argues that the time to clear customs was reduced and the reform "reduce[d] the possibility of errors that imply indirect costs to the user such as onerous sanctions or delays in the procedures."

The report also suggests that the reform in Bogota led to a reduction in corruption. It highlights a "decrease of discretion in the exercise of the customs function, through the automation of a large part of the controls and the exercise of physical reviews based on risk profiles, and with the support of the computer application, providing greater transparency in the procedures" as well as an "improvement in the control of the fulfillment of the obligations by the customs authority, and specifically those of declaration and payment of taxes." Mistrust in the activities performed by some customs agents before the reform is evident in these words, as is the desire both to limit their direct interactions with the importers and, when interactions are necessary, to make them more transparent and traceable. The documentation states that the reform resulted in a "decrease in the degree of corruption, evidenced by the reduction of complaints by customs users and their associations." According to the document, the program may have contributed to the 17.75% increase in the collection of customs taxes in Bogota in its first year of implementation. Outright smuggling, underreporting, miscategorization of products (towards products with a lower tax rate) and not paying the amount due were all concerns raised by the DIAN.

These qualitative insights from DIAN's internal reports have implications that can be tested in the data. They suggest that we should expect an increase in the number of transactions (if the reform reduced outright smuggling and facilitated imports) and of the value of imports (if it reduced under-declaration). For any given product, reduced smuggling should have also led to a smaller gap between the value of imports reported in Colombia and the value of exports to Colombia reported in other countries. A reduction in miscategorization should translate into an increase in the tax rate. Following DIAN's recommendation, we will also compare the amounts actually paid by the importers to the amount due according to the data. As mentioned above, discrepancies between amounts due and paid are a sign that may reflect fraud made easier by a lack of communication with the banks. Import facilitation may also be reflected by a reduction in the time taken to clear customs. This facilitation and reduction of corruption should lead to a preference for reformed customs, likely reflected in an increase in imports through reformed customs. The switch from discretionary power to a stricter application of the rules may imply a change in actual sanctions applied to faulty transactions. Finally, another insight from discussions and documentation is that risk profiling went from using relatively crude to complex algorithms, as the administration improved its ability to make use of the available data to identify risky transactions. Hence, the benefits of the reform should be progressive over the years following its implementation.

3 Data and Variables Definitions

This section describes the four databases used for the analysis and how we combined them. We use data on manufacturing plants combined with data on import transactions to assess the effects of the reform on businesses and ports. The data from the International Trade Statistics Database allow us to study the effects of the reform on smuggling, as revealed by gaps between the values declared in Colombia and those declared in the origin countries. We also use data on judiciary cases related to corruption. Our period of study goes from 1998 to 2008, covering three years prior to the first computerization to three years after the last port was computerized.

3.1 Data Description

This paper combines four rich annual databases. First, we use the imports transaction database from DIAN, which covers all import transactions, identifying the importer. This database gathers about 1,000,000 import transactions per year from about 56,000 firms. For each transaction, it provides information about the good that is imported (at a level close to the six digits of the Harmonized system), its quantity, value, destination, origin, and the taxes to be paid in association with the transaction. It also lists the dates of arrival and clearance, allowing us to calculate the clearance time for each transaction. We were able to recover whether each transaction was processed manually or by computer. Panel B of Table A1 lists the variables from the customs database that we use for our analysis.

Most of the outcome variables for manufacturing plants come from the Annual Manufacturing Survey (EAM) provided by the official Colombian statistical agency (DANE), rich yearly panel data on about 6,000 manufacturing plants. The database covers all manufacturing plants in the country with at least 10 employees , or with a level of annual production above a given limit (about USD 150,000 in 2017). We restrict the data to the balanced panel of plants for 1998 to 2008, leaving 3,651 plants.[7] The EAM data contains annual information on value added, sales, inputs, labor, capital and other standard indicators of the economic activity of the plants. We also compute, based on these indicators, measures of productivity, in particular, sales per worker and Revenue Total Factor Productivity. Table 1A provides a description of the variables from the EAM data. We also use uniquely rich EAM data listing all inputs used by a plant, at a level of aggregation that is close to HS6.

We use firm identifiers in the EAM database to merge it with the DIAN database.[8] This also allows us to assign, to each plant in the EAM, outcomes registered by DIAN related to importing by its owning firm: value of imports, taxes due and paid, etc. The DIAN data also allows us to examine port-level outcomes.

We also use data from the UN Comtrade database, which is a repository of

[7]Restricting the initial sample avoids selection biases in the estimated effects due to dropout. The results remain highly consistent if we include plants that exit the survey.

[8]The Colombian statistical office DANE established protocols to make this merge possible under their strict confidentiality rules.

official international trade statistics. It provides the total value of trade by importing country, exporting country and category of good (using the Harmonized System code). This data is used to investigate the extent to which the increase in imports declared in Colombia is also reflected in an increase in exports to Colombia reported in other countries.

Finally, we use the Attorney General's Office's records of disciplinary and legal violations in the public sector.[9] Using this information, we build a dataset that counts, by municipality and year, the number of corruption-related cases against officials affiliated with DIAN and those against officials affiliated to other agencies.

3.2 Treatment Variables

We label a port as computerized in a given year if over 80% of total imports going through that port during the year were declared by computer rather than manually.[10] Notice from Figure 2 that in each port the switch from manual to digital imports almost always implied a rapid jump from 0 to almost 100% of imports computerized over the course of one year, and remained around 100% for subsequent years. We thus consider reform at the port level as a discontinuous treatment not sensitive to setting the cutoff at 80%. [11]We subsequently determine the extent to which a plant is treated based on the extent of the connection of a plant to a given port. The match between plants and customs must 1) reflect a higher likelihood that the plant imports through its associated customs, and 2) not be driven by the endogenous choice of the plant to import through a specific port. To achieve these objectives, we use two different criteria to connect plants to ports, drawing on two main drivers of the decision to import from a given port: geography and the types of inputs, and therefore their origins. We show in section 4.2 that results are quite consistent across these different specifications of the treatment.

[9]The database's name is the Sistema de Información de Registro de sanciones y causas de Inhabilidad

[10]A code in the DIAN database allows us to identify whether the declaration is manual or computerized. We thank DIAN's staff for informing us about this code.

[11]Bogota, which was the first customs to implement the computerization, is the one exception with a rapid increase to about 86% of transactions, but reaching 100% only 2 years after the beginning of the computerization. Still, the initial rapid increase is enough to consider Bogota as treated in year 2001. Table A2 shows that the results are robust to the exclusion of municipalities one by one, and Table A4 shows that the results are not sensitive to the use of the continuous treatment equal to the share of computerized imports, rather than a dummy.

First, we calculate the "reform at the geographically assigned port", which can be interpreted as the most likely entry port given the geographical location of the plant. To obtain it, we match each plant to the port through which the highest share of imports by producers in the plant's municipality went in 1999.[12] Notice that, to minimize endogeneity, the rule of assignment uses imports prior to the reform, and averaged at the municipality level. This definition matches each plant to one port, resulting in a binary treatment where a plant-year combination is "treated" if the port assigned to the plant had undergone computerization at that point in time.

Second, we define the "plant's inputs expected treatment", which uses the composition of inputs of each plant to determine the expected share of inputs that is likely to go through a computerized port. This alternative treatment definition recognizes that the port of entry is affected by the country of origin of the imported input. Expenditure in each material input is separately recorded for each plant in the EAM data, at a level of disaggregation similar to HS6. The "plant's inputs expected treatment" for plant p and year t is calculated as follows:

$$T_{pt} = \sum_{c} \left[W_{pc99} \; * \; T_{ct} \right]$$

where T_{pt} is a weighted average of the ports' treatments T_{ct}, which is equal to one if over 80% of total imports going through port c during year t were declared by computer. The weights W_{pc99} are given by:

$$W_{pc99} = \sum_{i} \left[S_{pi99} \; * \; S_{ic99} \right]$$

where S_{pi99} is the share of input i used by plant p over all inputs used by plant p in 1999, and S_{ic99} is the share of imports of input i that went through port c over all inputs i imported nationally in 1999.

This continuous treatment index associated with a given plant and year varies from 0 in the case of a plant for which national-level imports of its 1999 inputs went entirely through ports that by year t were not yet computerized, to 1 in the

[12] We first calculate, for each firm in the DIAN database, the share of the value of its imports that came through each port in the pre-reform period. We then average this share across firms in the same municipality, rank the resulting average for each municipality over ports, and assign to each municipality the port to which the highest share is associated. Finally, we associate each plant in the EAM database to the port assigned to its municipality.

case of a plant for which national-level imports of its 1999 inputs went entirely through ports that started being treated before or during year t.

Additionally, section 4.4 shows the robustness of the results when using a continuous version of the "reform at the geographically assigned port" which uses weights proportional to the share of imports of the municipality going through each port. In this "Geographically defined plant's expected treatment", the weights are multiplied by the share of transactions declared by computer in the corresponding port (also using a continuous port-level measure rather than a dummy equal to 1 if the share exceeds 80%). Finally, our strategy identifies the differentiated effect of the treatment on exposed compared with non-exposed plants. A plant is considered as exposed to the treatment if the firm to which the plant belongs carried out any import transaction in 1998-1999.

4 Effects on Plant-Level Economic Outcomes

We begin by analyzing the effects of the reform on plant-level economic outcomes, including output, input use, productivity, imports and exports. A subsequent section studies how the reform affected tax collection and time to clear customs at the level of ports, underdeclaration of imports in Colombia relative to the country of origin, and the number of corruption complaints at reformed ports.

4.1 Methodology

Our methodology builds on the fact that computerization occurred in different ports at different points in time, because of the limited capacity of the team that trained the customs agents to use the new program. Under the reasonable assumption that the cost of going through a specific port is affected by the importer's geographical distance to the port as well as the distance from the origin of imports to the port, an importing plant will be considered as treated when its "closest" port or ports are reformed. This "closeness" will be defined either by the "reform at the geographically assigned port" dummy or by the "plant's inputs expected treatment" variable defined above.

Import facilitation reduces input costs for importers, potentially improving their revenues, increasing use of material inputs, and even increasing revenue productivity both via cost reduction and input quality increase (see, e.g. Goldberg and Pavcnik 2016). Non-importing firms that are also close to treated ports should not be seen as a perfect counterfactual, though. The positive effects on importers may subsequently translate into quality increases and cost reductions at downstream plants via input-output linkages (Fieler et al. 2018). At the same time, import facilitation may expose import-competing producers to fiercer competition, potentially forcing them to downsize and affecting their productivity Fieler et al. 2018; De Loecker et al. 2016). Our key hypothesis is that if the treatment facilitated imports, then we should observe that among plants associated with a reformed port, the treatment benefited importing firms compared with non-importing firms more than among plants associated with a non-reformed port. Our triple difference strategy described below provides a test of this hypothesis. The same estimations also provide double differences, informative of the treatment effects on importing firms and non-importing firms, separately.

Our main regression can be written as:

$$Y_{pt} = \beta T_{pt} * I_p + \delta T_{pt} + X_{pt}\lambda + \theta_p + \gamma_t + \varepsilon_{pt} \qquad (1)$$

where Y_{pt} is the outcome of interest at plant p in year t; T_{pt} is, for plant p at year t, one of the treatment variables defined above: the "reform at the geographically assigned port" dummy or the "plant's inputs expected treatment" variable. I_p is a plant-level dummy, equal to one if the plant imported at least once between 1997 and 1999 before any treatment. θ_p are plant-level fixed effects and γ_t are year fixed effects. X_{pt} is a set of additional controls, composed of the log of the value added in 1999 interacted with year dummies, and, in the case of the "reform at the geographically assigned port" specification, it also includes the value of transactions in the associated port in 1999 interacted with year dummies. These controls take into account that initial differences in port and firm characteristics may drive differences in changes over time.[13] Note that we do not control for I_p alone because it is already captured by the plant dummies. ε_{pt} is the error term, clustered at the customs level to account for possible customs-level shocks.

The relatively small number of clusters is a potential source of concern. (Bertrand et al., 2004) show that, in difference in differences, serial correlation within clusters and over time can cause dramatically higher rejection rates.

[13] To address these potential differences we: 1) add the controls described above, 2) express all outcomes in logs, ratios or dummies, and 3) in Table 3, we present the estimations separately by quartile of the initial value added of the plants.

Hence, we also present the wild bootstrap p-values of the β coefficients, following Cameron et al. (2008), who showed that this strategy adresses the issue of serial correlation even when the number of clusters is small.

Regression 1 identifies three effects of interest, including the double differences, informative of the treatment effects on importing firms and non-importing firms, separately, and the triple difference that tests our core hypothesis. In particular, δ estimates the double difference effect, i.e. the change in outcome Y_{pt} that occurred among non-importing plants at the time when imports became computerized in the corresponding port, relative to non-importers associated with non-reformed ports:

$$\delta = (\Delta Y_{pt} \mid \Delta T_{pt} = 1, I_p = 0, \Delta X_{pt}) - (\Delta Y_{pt} \mid \Delta T_{pt} = 0, I_p = 0, \Delta X_{pt}) \qquad (2)$$

Meanwhile, $\beta + \delta$ is our double difference estimation of the effect of computerization on importing plants:

$$\delta + \beta = (\Delta Y_{pt} \mid \Delta T_{pt} = 1, I_p = 1, \Delta X_{pt}) - (\Delta Y_{pt} \mid \Delta T_{pt} = 0, I_p = 1, \Delta X_{pt}) \quad (3)$$

Hence, β is the triple difference estimator of the effect of the treatment, i.e., the change at importing plants compared with non-importing plants, as a result of the reform. β provides our cleanest test that the computerization was successful in facilitating imports at reformed ports.

The identifying assumption of the double difference is that, in absence of the reform, there would be no systematically different trend between the outcomes of the plants associated with reformed ports and those that are not. The identifying assumption of the triple difference is that the difference between the trend of importing and non-importing firms in absence of the reform is not systematically related to the timing of the reform. The parallel trend hypothesis for the double difference could fail if the sequencing of the reform was prioritized in geographic areas where businesses had a greater potential and expected growth. It would fail for the triple difference if importing plants at reformed ports had greater growth potential that non-importers at the same ports. Even though the early reformers were the larger ports, there is no reason to expect that the associated plants were evolving at a systematically different pace compared with other ports. This be-comes even less likely in the case of the triple difference coefficient. Furthermore, the dynamic controls for initial size of plants and ports, each one interacted with time dummies, help remove concerns related to different paths that may be related to the initial heterogeneity. In Figure 3 and Table A2, we test the absence of parallel trend in the years preceding the reform, for both the double and triple difference effects, using a dynamic version of our main specification which breaks down the estimation of the treatment effect by year.

Importantly, being classified as an importer or non-importer is not completely synonymous with actually being an importer or non-importer during the entire study period. Firms that imported in the pre-reform period are 80% likely to import on average during the whole period, whereas pre-reform non-importers have an 8% probability of importing during the whole period, and thus to benefit directly from the reform. Hence the coefficient β can be interpreted as an Intention To Treat coefficient for the effect of the computerization on importers vs. non-importers, with a 72% compliance rate.

Finally the estimation strategy does not require that non-importers are not affected by the reform; non-importers should not be perceived as a pure control group. All firms may be affected through competition and indirect access to better and cheaper inputs or other markets.

4.2 Effects on the Economic Activity of Manufacturing Plants

We run the regression described in equation 1 to estimate the effect of the computerization on plant-level activity and present the results in Table 1. The "reform at the geographically assigned port" and "plant inputs expected treatment" are shown in Panels A and B, respectively. In both Panels, the first line shows coefficient β in equation 1: the triple difference estimate of the effect of the program for importing plants compared with non-importing plants. The second line displays δ, the double difference effect for non-importers. The last row of the panel presents the p-value of the sum of the two coefficients, which is the double difference estimate of the effect on importers. In Panel A, because the standard errors are clustered at the level of the 26 customs, we also show the wild bootstrap p-value of the triple difference estimates.

Table 1: Effects of the reform on the activity of manufacturing plants

1.A Effects when each plant is matched with a port based on its geographical location

VARIABLES	Log Inputs	Log Sales	Log Value Added	Log Number of workers	Log Capital	Export dummy	Log Value Added per Worker	TFP	Imports
Importer plant* Reform at plant's assigned port	0.064	0.091	0.14	0.054	0.030	0.068	0.087	0.027	0.022
	(0.017)	(0.013)	(0.016)	(0.015)	(0.024)	(0.038)	(0.013)	(0.0078)	(0.011)
Reform at plant's assigned port	-0.0019	-0.039	-0.073	-0.017	-0.042	-0.022	-0.056	-0.011	-0.0084
	(0.021)	(0.016)	(0.0097)	(0.019)	(0.021)	(0.027)	(0.014)	(0.0054)	(0.0079)
Observations	39,952	39,952	39,891	39,952	39,952	39,952	39,891	39,952	39,952
Wild Bootstrap p-value[1]	0.0410	0.0254	0.0566	0.0879	0.0098	0.0059	0.0449	0.0723	0.0762
p-val of sum of both coef.	0.001	0.001	0.000	0.002	0.495	0.077	0.006	0.027	0.214

1.B Effects when each plant is matched with ports based on fraction of the plant's inputs that is treated

VARIABLES	Log Inputs	Log Sales	Log Value Added	Log Number of workers	Log Capital	Export dummy	Log Value Added per Worker	TFP	Imports
Importer Plant * Plant's inputs expected treatment	0.10	0.15	0.19	0.11	0.038	0.021	0.085	0.036	0.056
	(0.030)	(0.026)	(0.029)	(0.021)	(0.029)	(0.011)	(0.023)	(0.0098)	(0.010)
Plant's inputs expected treatment	0.16	0.059	-0.014	0.026	-0.037	0.024	-0.039	-0.024	0.062
	(0.039)	(0.033)	(0.038)	(0.027)	(0.038)	(0.017)	(0.031)	(0.012)	(0.012)
Observations	39,765	39,765	39,704	39,765	39,765	39,765	39,704	39,765	39,765
Wild Bootstrap p-value[1]	0.000	0.000	0.000	0.000	0.1922	0.0661	0.000	0.000	0.000
p-val of sum of both coef.	0.000	0.000	0.000	0.000	0.997	0.010	0.091	0.241	0.000

This Table reports estimation results from estimating model (1). Each observation corresponds to a plant and year. "Reform at plant's assigned port" is a treatment dummy equal to 1 starting in the first year in which more than 80% of import transactions were computerized in the port associated to the plant. The port associated to the plant is the port representing a highest share of imports by all producers in the plant's municipality in 1999. The "Plant's inputs expected treatment" is a weighted average of the port treatments of the corresponding year, where the weights reflect the likelihood that the inputs used by the firm go through each port in 1999. "Importer Plant" is a dummy equal to 1 if the corresponding firm exported at least once between 1997 and 1999. Except dummies, all values are expressed in logs. Controls include plant and year fixed effects, initial log of value added of the plant interacted with year dummies and initial log of size of the customs port interacted with year dummies. Standard errors clustered at port level are in parenthesis.

[1] Wild bootstrap p-value of the coefficient "Importer plant * Reform at plant's assigned port", with errors clustered at customs level in panel A and plant level in panel B (9,999 reps). In panel A it also includes a correction for small number of clusters.

We first describe the results of Panel A, where the treatment is a dummy equal to one if the reform was implemented at the geographically assigned port, which implies that all producers in plant p's municipality were treated. We find that the reform led to a substantial increase of input expenditures by importers at reformed ports, of about 6.4 log points in comparison with non-importers at the same ports. Additionally, we find that, in comparison to non-importing plants, sales value increased by 9.1 log points and the value added grew by 14 log points. Summing coefficients β and δ, we find that the value added of importing plants increased by a significant 6.7 log points (p-value of 0.000). The expansion of treated importing plants appears to be driven by an increase in the use of labor and material inputs (including imported ones), and in productivity, but not by an increase in capital investments. Both the value added per worker and revenue productivity increases significantly. Also, consistent with increased size and productivity, the likelihood of exporting increases for importers. Clearly, the reform facilitated imports and benefited the economic activity of the plants that were most likely to use computerized customs.

The large effects vis-a-vis non-importers in reformed ports partly reflect an estimated negative effect on treated non-importers (compared with non-importers associated with non-reformed ports), of about 7.3 log points in the case of value added. This indicates that the indirect benefit due to the reduction in the cost of inputs and the availability of higher quality inputs is more than offset by the direct negative competition effect. Non-importers "close" to reformed ports are plausibly most affected by the reform through the import-competition channel, to the extent that they are located in the areas where both the cost of imports is most likely to be reduced, and importing competitors are most benefitted.

In Panel B of Table 1, the association of plants with customs uses weights that reflect the likely ports of transit of the inputs used by the plant. Because non-importers can use inputs imported by importers, in this estimation the treatment variables should mainly capture the cost-reducing effect of the reform of input even for non-importers, though less so than for direct importers. The main conclusions are broadly similar to those of Panel A for the triple difference effect β, which is significant for all outcomes, except investments in capital. The order of magnitude is generally higher. However, because this treatment is continuous, coefficients in Panel B should be interpreted as the effect of a switch from 0% of inputs going through a customs that became treated to 100%. This is a more intense treatment than in Panel A, which may explain the larger magnitude of the coefficients. By contrast, the results of the double difference on non-importers are substantially different to those of Panel A: inputs, imports and sales increased significantly as a result of the treatment. Only the TFP double difference maintains the same sign as in Panel A. It is likely that these firms, which were not importing before the reform, were able to either start importing the inputs themselves (last column) or indirectly increase their inputs and benefit from the lower price and higher quality of inputs imported by other plants (first column). The effects on imports are substantially higher with this specification, both for non-importing and importing plants, consistent with the positive effects being driven by a facilitation of imports. These results also re-enforce our interpretation that the negative estimated effects for non-importers in Panel A reflect the pro-competitive effect of the reform, which dominates among firms that are geographically close to reformed ports. On the other hand, a plant that uses inputs that are made more accessible by the reform, which is the treatment in Panel B, is more likely to benefit from the cost-reducing effect even if it does not directly import those inputs.

We refrain from inferring aggregate effects from these results, since the nature of the exercise only allows us to reach conclusions by comparison with the control group of plants associated with un-reformed ports. As is frequently the case with reforms that affect international trade, control producers are likely to also be affected by the reform, since both the pro-competitive and cost-reducing effects of trade increases are not confined to the set of plants most exposed to the reform (see Goldberg and Pavcnik, 2016, for a discussion of the limitations of empirical analyses of the effect of trade reform). Moreover, these figures hide large heterogeneity by plant size, which we analyze in section 4.5.

4.3 Parallel Trends and Dynamic Effects of Computerization

To check that the required parallel trend hypothesis holds during the years that preceded the treatment, we extend the analysis to a dynamic specification. This approach also sheds light on the dynamic effects of the treatment, by presenting year-by year changes in the outcome variables following the implementation of the reform. However, the dynamic specification can only be implemented with the "reform at the geographically assigned port" treatment T because it requires a dichotomous treatment variable.

The specification below breaks up the estimation of the treatment effect by year:

$$Y_{pt} = \alpha + \sum_{y=-3}^{y=-1} \left[\beta_y T^p_{cty} * I_p + \delta_y T^p_{cty}\right] + \sum_{y=1}^{y=4} \left[\beta_y T^p_{cty} * I_p + \delta_y T^p_{cty}\right] + \lambda X_{pt} + \theta_p + \gamma_t + \varepsilon_{pt} \quad (4)$$

where y is equal to the number of years after the reform (if positive) or (or $|y|$ year before the reform if negative) with $y = 0$ on the last year before the reform, which we will refer to as the baseline year. Hence, T^p_{cty} is a dummy equal to 1 if port c associated with plant p at time t has used computers for exactly y years if y is positive. And if it is negative, T^p_{cty} is a dummy equal to 1 if customs c

at time t is $|y|$ years before $y = 0$. The two exceptions are year T^p_{ct-3}, which we define as a dummy equal to one for any year that is three years or more prior to the baseline year for port c, and T^p_{ct4}, which is equal to one if customs c at time t had been computerized for four years or more. They are included to ensure that the only omitted year is the baseline year, so that all coefficients β_y can be interpreted as the triple difference effect of the year y of treatment, in comparison with the baseline value. [14] Hence β_y is our estimation of the difference in the change in Y_{pt} between importing plants and non-importing plants at reformed ports after y years of computerization of the assigned port, and δ_y is a measure of the change in the outcome Y_{pt} on non-importer plants after y years of customs computerization (compared with a non-computerized port). Finally $\beta_y + \delta_y$ is the double difference effect for importers. The values of β_y, δ_y and $\beta_y + \delta_y$are all presented in Appendix Table A2 and Figure 3 presents the results of β_y, the dummies associated with the triple difference.

The results for the triple difference effect of the reform on importers compared with non-importers, summarized in Figure 3, show that the parallel trend hypothesis holds for every outcome. Furthermore, the effects of the reform on importers increase over time. This is consistent with our expectation given that 1) the port itself should adapt to the new technology and progressively learn to make better use of the data to improve its risk profiling (see section 2), and 2) inputs from abroad should have effects through innovation and competitiveness that may build up over time. The conclusions are qualitatively similar when looking at double difference effects on importers, shown in Table A2.

4.4 Robustness Checks on the Economic Activity of Manufacturing Plants

We provide two robustness checks of the results of this plant-level analysis. First, one may be concerned by the limited number of ports (26), and the fact that a few of them may concentrate most of the importing activity, so that even a single port may drive our results.[15] The fact that standard errors are clustered by port, and that the results remain significant when using wild bootstrap, should limit

[14] Our presentation of the results does not include $\beta_{y\leq-3}$ and $\beta_{y\geq4}$ since the years for which some observations are available vary by customs depending on the year of its reform, hence their coefficients are a mix of selection and time effects, not easy to interpret and not necessary for our analysis.

[15] For example, Bogotá alone handles 46% of imports transactions and was the first to implement the reform.

inference based on a small number of customs. To further address this concern, we show in Appendix Table A3 that the results are highly consistent in magnitude and statistical significance when excluding, one by one, the 10 largest customs.[16]

Figure 3: Dynamic triple difference effect of the customs reform on key outcomes

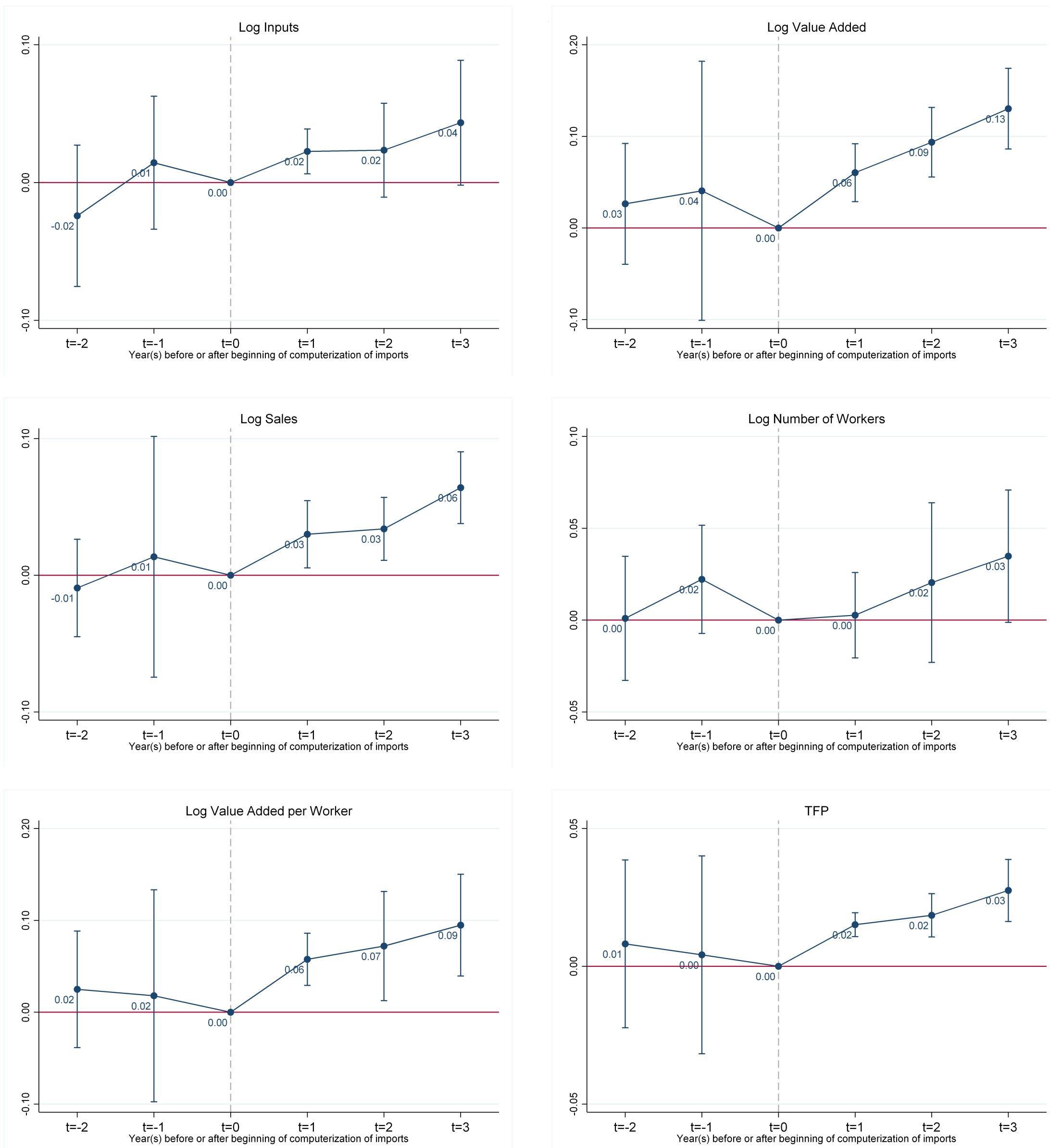


Figure 3 provides a visual representation of the triple difference effects of changes at plant level before and after the reform, corresponding to model (2) and presented in Table A2.

[16] In fact, for each one of the 10 largest customs separately, the estimation excludes all plants matched with that port

Second, we test the robustness of our results to a continuous definition of plant exposure to a port using the geographic criterion. Appendix Table A4 reproduces our results for equation 1, defining the treatment variable T_{ct} as a weighted average of a port's share of computerized transaction in the corresponding year, where the weight of each port is the share of the imports by all producers in the plant's municipality that went through that port in 1999. This results in a continuous treatment variable that can be interpreted as the share of imports that are expected to be computerized based on imports of the municipality in 1999. Appendix Table A4 shows that using this continuous treatment leads to qualitatively similar conclusions with, if anything, coefficients of a larger order of magnitude, which again must be interpreted as the effect of switching from 0% to 100% treated. This specification also reduces concerns about having only 26 clusters, since the treatment is continuous and not tied to a small number of clusters.

4.5 Heterogeneous Effects by Plant Size

As a final step in our plant-level analysis, we replicate the regression of model (1), splitting the sample by quartile of value added in year 1999. This serves two purposes. First, one can be concerned about comparing importers and non-importers given that, on average, the former are larger than the latter. Indeed, the quartile of the smallest plants is comprised of only 21% of plants that are initially exporters, compared with 95% for the fourth quartile. Most importantly, we are also interested in learning about how businesses of different sizes are affected by the computerization. The findings are presented in Table 2, by quartile of value added in 1999, from lowest (quartile 1) to highest (quartile 4). These results use the geographic assignment of ports, while Appendix Table A5 shows the same results with the inputs assignment.

We discuss the magnitudes of the triple difference effect on importers compared with non-importers (β), but the qualitative conclusions about heterogeneity across sizes are very similar for the double difference on importers ($\beta+\delta$). Results in general show that the positive effects on importers are concentrated on the first three quartiles, with effects on the upper quartile being in general not statistically significant and much smaller in magnitude than those for smaller plants. In fact, it is the second quartile where effects are generally strongest. In terms of value added, for instance, the first and second quartiles were the most affected by the reform with triple difference effects of 18 and 20 log points, which go down to 9 log points in the third quartile, and are not significant among the quartile of largest firms. Effects on imports, input expenditures, sales and exports are again strongest in the second quartile. The effects on job creation and TFP are in fact strongest in the lowest quartile, closely followed by the second quartile. Capital remains largely unaffected by the computerization in all groups.

Table 2: Effect of the reform on the activity, of manufacturing plants, by quartile of value added in 1999, using geographic assignment to ports

Quartile (of VA in 1999)	VARIABLES (in log):	Inputs	Sales	Value Added	Number of workers	Capital	Export dummy	Value Added per Worker	TFP	Imports
Quartile 1 (Importer dummy = 21%)	Importer plant* Reform at plant's assigned port	0.047 (0.034)	0.091 (0.051)	0.18 (0.051)	0.084 (0.026)	0.060 (0.038)	0.094 (0.038)	0.059 (0.031)	0.040 (0.019)	0.036 (0.018)
	Reform at plant's assigned port	0.037 (0.029)	-0.035 (0.030)	-0.081 (0.033)	0.017 (0.021)	-0.049 (0.022)	-0.097 (0.015)	-0.024 (0.015)	-0.026 (0.0073)	-0.0097 (0.0033)
Quartile 2 (Importer dummy = 42%)	Importer plant* Reform at plant's assigned port	0.093 (0.043)	0.13 (0.038)	0.20 (0.047)	0.076 (0.034)	-0.0027 (0.031)	0.13 (0.050)	0.065 (0.047)	0.036 (0.014)	0.039 (0.018)
	Reform at plant's assigned port	0.0035 (0.029)	-0.028 (0.028)	-0.038 (0.020)	-0.0055 (0.045)	-0.0100 (0.049)	-0.034 (0.034)	-0.0040 (0.024)	-0.00056 (0.010)	-0.027 (0.0070)
Quartile 3 (Importer dummy = 75%)	Importer plant* Reform at plant's assigned port	0.058 (0.028)	0.090 (0.021)	0.13 (0.036)	0.041 (0.031)	0.015 (0.037)	0.085 (0.031)	0.068 (0.048)	0.044 (0.019)	0.0070 (0.018)
	Reform at plant's assigned port	-0.0084 (0.038)	-0.047 (0.028)	-0.080 (0.046)	-0.021 (0.039)	-0.040 (0.034)	-0.059 (0.038)	-0.017 (0.045)	-0.027 (0.015)	0.0033 (0.013)
Quartile 4 (Importer dummy = 95%)	Importer plant* Reform at plant's assigned port	0.061 (0.053)	0.0034 (0.034)	-0.025 (0.025)	-0.077 (0.076)	-0.18 (0.070)	0.052 (0.076)	0.15 (0.054)	0.0076 (0.015)	-0.012 (0.073)
	Reform at plant's assigned port	-0.032 (0.053)	0.021 (0.040)	0.045 (0.034)	0.066 (0.075)	0.16 (0.062)	-0.021 (0.077)	-0.14 (0.054)	0.0076 (0.019)	-0.0030 (0.069)

Table 2 reports estimation results from estimating model (1), replicating the results of Table 1A, separately for each quartile of value added of plants in 1999 (where quartile 1 corresponds to the smallest firms and quartile 4 to the largest firms). Standard errors clustered at port level are in parenthesis.

This heterogeneity in the effect of the reform on economic activity of importers could be driven by two factors. First, in standard models of trade with fixed costs of trading and heterogeneous production units, a given reduction in the cost of trade should lead to heterogeneous effects (Melitz, 2003; Fieler et al., 2018). The largest and most productive businesses were likely already importing and exporting even at relatively high cost, the smallest plants were likely too far from the productivity cutoff for importing and exporting, whereas medium size plants were most likely to extend their trade activity as a result of the cost reduction. The distribution across quartiles of the positive effect of the reform on importing firms seems to support this interpretation, since the effect of the computerization on the likelihood to export and other outcomes is highest among firms in the second quartile. Second, it is possible that the cost reduction brought by the reform varies by plant size. This would occur if large firms were more able to manage or circumvent the bureaucracy at customs, or if corruption was more collusive for producers of a certain size, and coercive for others. This is consistent with the findings of Beck et al. (2005) who, using a firm-level database covering 54 countries, find that institutional development systematically benefits small firms more than large firms.

5 Effects on Customs

This section presents complementary evidence on effects of the reform on the functioning of ports and their interaction with manufacturing importers, which is suggestive of the mechanisms at play.

5.1 Effect of the Reform on Import Transactions: Plant-Level Estimations

This subsection analyzes the effects of the reforms on outcomes related to import transactions, still using plant and year as the observation. Since information on import transactions applies only for importing plants, comparison with non-importers is no longer possible. Thus, we estimate a double difference version of equation 1 for outcomes from the DIAN database on import transactions. In particular, we estimate:

$$Y_{pt} = \alpha + \rho T_{pt} + \lambda X_{pt} + \theta_p + \gamma_t + \varepsilon_{pt} \tag{5}$$

where the notation is identical to the one of equation 1. The coefficient ρ is the double difference effect of the reform on plants at computerized ports compared with those at non-computerized ports. Since information in the DIAN database is at the firm rather than the plant level, plants are assigned the outcomes corresponding to their owning firms. They are still matched to ports based on the plant's location, resorting back to our baseline definition of T_{pt} in the geographic specification. Over 90% of plants in the manufacturing survey are single-plant firms, so that the firm-plant distinction is not crucial in this context. The parallel trend test for this estimation is presented in Appendix Table A6, showing only one significant coefficient out of 8, which is within what may be expected by chance.

As mentioned in section 2, miscategorization of products and non-payment of due taxes were common concerns at the DIAN before the reform. If the computerization succeeded at addressing them, then it should increase the effective tax rate, which is what we find in column 2 of Table 3. The ratio of all taxes collected divided by declared imports increased by 0.005, which represents 18% of the average effective tax rate. This is consistent with a reduction in the miscategorization of inputs in order to reduce taxes, although it can also result from an actual shift in the composition of imports towards imports with higher taxes.

Table 3: Double difference estimation of the effects of the reform on characteristics of import transactions by importing plants (observations at plant- year level)

VARIABLES	Customs Clearence Time	Paid/FOB Import Value	Paid/Due	Sanctions/FOB import value
Reform at plant's assigned port	-0.45	0.005	0.059	0.000021
	(0.30)	(0.00059)	(0.021)	(5.46e-06)
Observations	18,635	18,622	18,414	18,622
Wild Bootstrap p-value[1]	0.2734	0.0156	0.1680	0.1563
Mean of outcome var.	13.6	0.028	0.86	0.000029

Table 3 reports estimation results from estimating model (5). Each observation corresponds to a plant and year. It includes all (the 2,128) manufacturing plants in Colombia that could be matched to the customs data, and available from 1998 to 2008. Outcome variables correspond to the average, across transactions by the firm that owns the plant, of the corresponding variable. "Reform at plant's assigned port" is a treatment dummy equal to 1 starting in the first year in which more than 80% of import transactions were computerized in the customs post associated to the plant. The port associated to the plant is the port with the highest share of imports by all producers in the plant's municipality in 1999. Controls include plant and year fixed effects, initial log of value added of the plant interacted with year dummies and initial log of size of the customs port interacted with year dummies. Observations are weighted by the value added of the plant in 1999, before any reform started. Standard errors clustered at port level are in parenthesis.
[1] Wild bootstrap p-value of the coefficient "Reform at plant's assigned port", with errors clustered at customs level (9,999 repetitions)

We also investigate discrepancies between taxes paid and taxes due. The reform was expected to improve the communication with the bank and make sure that payment was made before releasing merchandise, which is what we investigate in column 3. The outcome is the ratio of the tariff that was actually paid by the plant to the tariff due (after potential deductions). The computerization increased this ratio by 5.9 percentage points, thus reducing by more than a third non-payment of due taxes. Finally, column 4 shows a significant increase in sanctions after the reform, which may reveal a switch from informal bribes to formal sanctions. The last two results though, are not robust to the use of the wild-bootstrap p-value, hence it may be spurious or driven by a small number of clusters.

We also examine the effect on average time to clear customs (averaged across a plant's transactions), a standard indicator of convenience and (absence of) bureaucracy. Perhaps surprisingly, we find that the reform had no significant effect on the number of days needed to clear customs. Given the standard error, we can rule out any impact of more than one day on customs clearance duration. As discussed in the following subsection, we find evidence that plants are redirecting imports from non-treated customs to treated customs, which would have increased delays in treated customs with respect to non-treated customs. Appendix Figure A1, however, shows that in Colombia, during the period of the study, the time to clear customs went down from about 15 to 11 days, which is consistent with this scenario. Also, the time to clear customs fails to capture changes in other aspects of convenience such as removing the need to physically go to the customs multiple times or being asked to pay bribes.

We also replicate these estimations by quartile of firm size to better understand the heterogeneity of impact that is evidenced in section 4.5. The results are reported in Appendix Table A7, splitting the sample by quartiles of 1999 value added, as in Table 2.[17] Interestingly, the increase in the effective tax rate is concentrated in the two quartiles of largest firms. Furthermore, all quartiles display pre-reform average ratio of taxes paid over due between 94% to 97%, except for the top quartile, which paid only 75% of the amount due. The top quartile is also the only group with a significant change in this variable, which increased by about 6.7 percentage points with the computerization. These patterns are consistent with large firms being more able than others to deal with costly customs procedures, and also to collude with customs officials to receive special treatment, especially in the pre-reform environment. This provides an additional explanation for why we observed in Table 2 that the top quartile is the one that benefited the least from the computerization.

5.2 Effect of the Reform on Ports

We also use the DIAN data to analyze outcomes at the port level. We focus on

[17]Notice first that because only importing firms remain in this regression, this limits the sample, and generates a difference in the size of the samples left in each quartile, with the lowest quartiles having fewer observations, and thus being more noisy. Appropriate caution is required for interpretation.

changes in total number and value of import transactions going through the port, as well as taxes collected.

As in section 5.1, we resort to a double difference strategy, analyzing changes at reformed ports. In contrast with previous results, an observation corresponds to a port and year:

$$Y_{ct} = \alpha + \delta T_{ct} + \theta_c + \lambda X_{ct} + \gamma_t + \varepsilon_{pct} \tag{6}$$

where δ is the coefficient of interest, which captures the change in Y_{ct} at the time of the computerization of customs c, and θ_c are customs-level dummies. X_{ct} is the value of transactions in the associated port in 1999 interacted with year dummies, to control for changes over time that vary by initial size of customs. Standard errors are clustered at the customs level.

We also run an accompanying dynamic specification to check the parallel trend assumption and assess the dynamics of effects. In particular, we run the following regression:

$$Y_{ct} = \alpha + \sum_{y=-3}^{y=-1} [\delta_y T_{cty}] + \sum_{y=1}^{y=4} [\delta_y T_{cty}] + \theta_c + \lambda X_{ct} + \gamma_t + \varepsilon_{ct} \tag{7}$$

The definition of the dynamic treatment variables T_{cty} is analogous to that for the dynamic treatment variables in the plant-level demand specification 4. Hence, δ_y assesses the effect of the computerization on Y_{ct} after y years when y is positive, and it is used to check the parallel trend when y is negative. Again, standard errors are clustered at the customs level. The results, reported in Appendix Table A8, show that none of the 10 pre-treatment effects is significant at the ten percent level, which increases our trust that the parallel trend assumption holds in this context.

The description of the intervention in section 2 draws attention to several possible channels through which the reform should operate, including a facilitation of imports as well as a reduction of smuggling, under-declarations and misreporting. As a consequence of these factors, one would expect importing activity and tax collection to increase in treated ports. These port-level regressions allow us to assess these channels.

The first two columns of Table 4 show that the reform was followed by a drastic increase in the number of transactions (about 67% of its mean value), and in the total FOB value of imports (70%), at reformed ports. Interestingly, Appendix Table A8 shows that the effects of the reform on the number of imports and their total value also increase gradually, following the same pattern as the one observed in input use.

We turn our interest to taxes collected at the port, which was one of the a primary objectives of the reform. Revenue collection is a particularly interesting outcome, as customs reforms have the potential to increase trade without sacrificing tariff collections, in contrast with trade liberalizations based on tariff reductions. Column 3 of Table 4 shows that tax collection increased by 72% with the reform. The magnitude is very similar to the one of the value of imports, which suggests an increase in tax collection essentially driven by the increase in the tax base. This conclusion differs from the one in the previous subsection and can be due to the lower precision in customs-level estimations, resulting in large confidence intervals.

Table 4: Double difference estimation of the effects of the reform on the characteristics of import transactions going through a port (observations at the port-year level)

VARIABLES	Number of import transactions	Total value of imports (FOB)	Total Taxes (VAT + Tariff)	Transportation Costs	Transportation Costs / FOB
Reform at port	31,956	895	22.6	10.8	0.0029
	(14,265)	(405)	(11.0)	(5.28)	(0.0011)
Observations	286	286	286	286	286
Wild Bootstrap p-value[1]	0.0194	0.0217	0.0360	0.0450	0.0116
Mean of outcome var.	47,239	1,276	31.4	9.54	0.0076

This Table reports estimation results from estimating model (6). Each observation corresponds to a customs post and year from 1998 to 2008. "Reform at port" is a treatment dummy equal to 1 starting in the first year in which more than 80% of import transactions were computerized in the customs post associated to the plant. The dependent variable in column 1 is the number of transactions that went through the port. In columns 2 to 4, the dependent variables correspond to the sum across those transactions, in the last column, the dependent variable is the ratio of transportation costs over value of imports, and the observation is weighted by the initial size of the customs port. Controls include customs port and year fixed effects and initial size of the customs port interacted with year dummies. Standard errors clustered at port level are in parenthesis.

[1] Wild bootstrap p-value of the coefficient "Reform at port", with errors clustered at customs level (9,999 repetitions), including a correction for small number of clusters.

The increase in registered imports at reformed ports can be due to any of the following three channels, or a combination of them: 1) an actual increase in the value of aggregate imports 2) a redirection of imports from plants who prefer going through treated ports instead of non-treated ports 3) a reduction in smuggling and under-declaration. Any of these scenarios is a sign of success for the reform, either because it improves tax collection or reveals a facilitation of imports, but our next results will dig further to better distinguish between these three possible reasons.

The plant-level evidence previously presented suggests that, indeed, importers affected by the reform increased their use of imports and inputs, which is suggestive that the actual increase is at play.[18] Besides this, total transportation costs increased by 113% with the treatment (column 4), hence, in greater proportion than the total value of imports, and transportation costs as a share of imports increased by 38% (column 5). An increase in per-transaction transportation costs to reach treated ports is suggestive of a revealed preference for ports that underwent the computerization, and thus, that the redirection of transactions is also at play. This redirection towards reformed ports is reminiscent of the findings of Sequeira and Djankov (2014) that firms in Southern Africa are willing to increase travel costs in order to go through a less corrupt port. The following section uses data from UN Comtrade that allow us to disentangle an actual increase in imports from an increase in the declaration of imports from a reduction of smuggling.

5.3 Effects of the Reform on Declared Imports

The gap between the amount reported for a given international transaction by the exporter and that reported by the importer is an indicator of smuggling and other forms of tax evasion.[19] (Fisman and Wei, 2004). If properly reported, the two sides of the report should only differ by transport costs, included only in the importer's declaration. Import values as reported by importers should thus be larger than those reported by the exporters at origin. However, exporters have little incentive to under-report, whereas importers may under-report in order to avoid paying taxes. Hence suspicion is raised when the total value of declarations by exporters exceed the one by importers. We use UN Comtrade data, where we can see Colombian imports as reported in Colombia and their countries of origin, for a given product, year, country of origin. This allows us to assess the extent to which the reform is associated with, on one side, an increase in the declarations by exporters towards Colombia, and on the other side, a decrease in the gap

[18] Also, from a merely descriptive look at the data, total revenues collected at Colombian customs increased by 32% from 1999 to 2008.

[19] Smuggling refers to transactions that are not declared at all, with no tax paid on these transactions. Other forms of tax evasion include under-declaration of amounts or quantities, or miscategorization of goods.

between reports by Colombian importers and their counterparts. Using the same type of data, Kellenberg and Levinson (2019) show that less corruption, stronger auditing and stronger accounting standards all reduce the under-reporting of trade transactions.

We use UN Comtrade data on Colombian imports at the level of product code (HS4), country of origin, and year. The UN Comtrade data does not identify the port through which the merchandise goes. However, we can define a level of treatment by product that follows the same logic as the "plant's inputs expected treatment" specification. We impute, for each HS4 code, the probability that imports of that group went through a reformed port in each year. To calculate the treatment variable, we multiply the share of the HS4 imports going through a port (from DIAN data in 1999), by the treatment dummy of the port in the corresponding year. We then run a regression of the log of the value of imports reported by different sides of the transaction, on this treatment variable, which measures the probability that the given HS4 product category is treated by the reform.

Table 5: Effect of computerization on imports declared in Colombia by importers vs. declared in origin country by exporters (observations at level of HS4 product level per origin)

VARIABLES	Log value of imports as declared in country of origin	Log value of imports as declared in Colombia	Import Capture Ratio = Value declared in (Colombia/Origin)
Plant's inputs expected treatment	0.12 (0.030)	0.2 (0.029)	0.024 (0.0062)
Observations	35,190	35,190	35,190
Wild Bootstrap p-value[1]	0.0008	0.0000	0.0006
Mean (in level, not log)	11,229	12,481	0.814

Each observation corresponds to a product category (HS4) and country of origin. It includes all product-origin combinations with at least one transaction between 1998 to 2008. Controls include fixed effects for every combination of product category and country of origin as well as year fixed effects. The "Plant's inputs expected treatment" is a weighted average of the port treatments of the corresponding year, where the weights reflect the likelihood that the inputs used by the firm go through each port in 1999. Robust standard errors are in parenthesis.
[1] Wild bootstrap p-value of the coefficient "Plant's inputs expected treatment", with errors clustered at type of good level (9,999 repetitions)

Table 5 displays our results. The computerization led to a significant increase in imports declared by exporters in the origin countries, revealing a true increase in imports to Colombia (column 1). This increase reaches 12 log points when a product goes from not treated to fully treated. Furthermore, the computerization also increased imports declared in Colombia, by 20 log points (column 2). The differential impact on reports by importers vs exporters is evidence consistent with a reduction in under reporting. Indeed, the last column shows that the reform led to an improvement in the import capture ratio, given by ratio of values declared in Colombia vs in origin countries, which can be interpreted as the share of actual imports that is declared (Yang (2006)).[20] The average value of the import capture ratio in our data is 0.814, indicating that in Colombia, over our study period, at least 18.6% of imports are not reported. The result in column 5 shows that the treatment significantly increased the Import Capture Ratio by 0.024, which represents a 12.9% reduction in under reporting.

To conclude, our results taken together point towards the three previously mentioned mechanisms for increased registered imports in reformed customs, all occurring at the same time. The prior subsection showed that part of this increase is due to a redirection of imports among customs, which reveals a preference for treated customs. This subsection shows that there is also an increased in imports declared in Colombia, of which 60% comes from an actual increase in imports and 40% is due to a decrease in under-reporting.

5.4 Further Evidence of Effects on Efficiency and Corruption at Ports

Our previous results suggest that reformed ports are more attractive for importers. Why? The reform was expected to make customs more efficient and less corrupt. We have already reported pieces of evidence consistent with these types of improvement: increases in the collection of taxes and the enforcement of sanctions, and reduction in smuggling. This section shows complementary evidence on changes at the customs.

Having already reported a not statistically significant and very minor reduction in average clearance times (Table 3), we now assess whether the reform affected the predictability of clearance times. We are interested in predictability first because it matters to importers and thus is an indicator of the facilitation of

[20] The import capture ratio is in fact a lower bound of the share declared by importers since, with transport costs, declarations in Colombia should exceed the ones from exporters. In calculating the import capture rate, a maximum value of one is imposed, for the case when the reports by importers exceed those by exporters.

imports. Second, unnecessary delays have been described as a tool for customs agents to extract bribes from importers. The reform aimed at reducing such practices through a reduction and standardization of the interactions between importers and customs agents. If it was effective, then it should be reflected in an increase in predictability of clearance times.

Table 6 reports the R2 of a transaction-level regression of time to clear customs on transaction characteristics observable to the firm, including weight, value and firm fixed effects, product and port. The regressions include more than a million transactions. We show separate results by year and treatment status of the ports, to compare the R2 of treated customs with the ones of non-treated customs for each year from 2001 to 2004, which are the years with a mixture of treated and non-treated ports. An additional line pools together all years from 2001 to 2004. The R2 is significantly higher for computerized ports compared with the non-computerized ones in the same year, with a difference that ranges from 3.3 to 5.6 percentage points. The results are thus suggestive of a facilitation through better predictability and a reduction of the discretionary power of customs agents.

Table 6: Effects of computerization on predictability of time to clear customs and tax rate

R-squared of Customs Clearance Time by treatment group

Year	Non-Computerized Customs	Computerized Customs	Difference	p-value of difference
2001	35.6%	38.9%	3.3%	0.000
2002	31.7%	37.3%	5.6%	0.000
2003	35.0%	38.4%	3.4%	0.000
2004	38.0%	42.9%	4.9%	0.000
2001-04	27.8%	33.1%	5.4%	0.000

This table presents, separately for each year and treatment group, the R2 of a regression at the transaction level of the time to clear customs on net weight, fob value in pesos, port dummies, firm dummies and dummies for category of product imported (HS2). Each import transaction is an observation. For each year, we present the difference between the R2 of treated and non treated group and the p-value of the significance of their difference estimated by bootstrap (1,000 repetitions).

To further investigate whether the reform can be associated with a decrease of the number of official corruption investigations against customs officials, we use the number of corruption cases registered by the *Procuraduría General de la Nación* (the General Prosecutor). We split cases into those related to DIAN and those of other state agencies. The variables are described in Panel D of Appendix Table A1. This approach has the advantage of using a direct measure of corruption, but is limited by the fact that there are only 37 cases of corruption related to DIAN during the study period, so that the dependent variable (specified by municipality) is most frequently zero in the analysis. Hence the results should be interpreted cautiously, as part of a set of evidence that point towards corruption as being one of the mechanisms at play, rather than as stand-alone results. [21]

Table 7: Estimation of the effects of the reform on investigations related to corruption at DIAN at the port-year level

VARIABLES	DIAN Corruption Related Cases	Factor of DIAN Corruption Related Cases
Reform at port	-0.18	-0.343
	(0.0735)	(0.166)
Observations	286	286
Wild Bootstrap p-values	0.0222	0.0659
Mean of outcome var.	0.122	0

This table reports estimation results from estimating model (4). Outcome variables are computed using the archive of investigations from the *Procuraduría General de la Nación* (General Prosecutor). Each observation corresponds to the municipality where a customs post is located and year from 1998 to 2008. Controls include municipality fixed effects, year fixed effects, and the number of corruption related cases non related to DIAN (in column 2 the factor of non DIAN corruption related cases). "Reform at port" is a treatment dummy equal to 1 starting in the first year in which more than 80% of import transactions were computerized in the customs of the municipality. Standard errors clustered at port level in parenthesis.
[1] Wild bootstrap p-value of the coefficient "Reform at port", with errors clustered at customs level (9,999 repetitions), including a correction for small number of clusters.

We present in Table 7 the results of regressions that follow the double difference specification described in equation 6. The outcome variable is the total number of corruption cases related to DIAN (column 1) and a factor estimated by Principal Component Analysis of the 4 types of violation, to check whether the results are sensitive to the form of aggregation of the 4 types of violation considered as corruption (column 2). All regressions include port and year fixed effects, as well as the total number of violations in the port's municipality that are *not* related to DIAN (or the corresponding factor in column 2). Together,

[21] Also an increase in the number of judiciary cases is an ambiguous indicator of corruption, since it may indicate that there is more corruption, or that corruption is more likely to be detected, hence reflect an intention by the authorities to tackle corruption. In this context, because the General Prosecutor is a national institution, we consider that, at a given period, its intention to prosecute should not vary across customs.

this accounts for variations between customs and changes in corruption and the judiciary system over time. Cases *not* related to DIAN have a strong predictive power of DIAN related cases and purge the estimation from any determinant of corruption cases that is not specific to DIAN. Appendix Table A9 displays the parallel trend estimate showing that none of the four pre-trend coefficients is significant. The results of Table 7 show a statistically significant drop in the number of violations related to DIAN, concurrent with the reform. Using the factor specification, we estimate that the drop in corruption cases represents 0.34 of a standard deviation. This is consistent with a reduction in corruption being concurrent with the reform and being part of the explanation for the observed increase in firm-level activity and tax collection.

6 Conclusion

This paper analyzes the economic consequences of the computerization of import declarations in Colombia, accompanied by a reorganization that automatized many decisions and limited the discretionary power of customs officers. Together, the results show that the reform triggered a progressive and significant growth of the importing firms that are most likely to access customs. Small and medium importing firms benefited the most from the reform. Increased growth is observed in terms of value added, employment, productivity and the propensity to export. Our results on importing firms are robust to assigning customs based on proximity or on the plant's composition of inputs, while those on non-importing plants, for which a potentially negative competitive pressure effect is likely more important, vary depending on the specification. Plants linked to reformed ports that were not importing were negatively affected, potentially as a consequence of their loss in competitiveness compared with importing firms, while firms that were not importing but used inputs more likely to go through a treated customs indirectly benefited from the computerization. Tax collection at customs also increased substantially, because of an increase in both the tax base and the effective tax rate. The trade statistics confirm that the increase in import declarations is due to a mix of an actual increase in imports and a reduction of smuggling and under-declaration.

These large impacts are in line with previous literature that shows that import taxes can be harmful to firm productivity and growth (Trefler, 2004; Amiti and Konings, 2007; Kugler and Verhoogen, 2009; Halpern et al., 2011), and with other literature showing that the "corruption tax" can have effects that are multiple times larger than the one of an equivalent formal tax (Wei, 2000; Svensson, 2003). Quite strikingly, data from surveys of manufacturing firms, customs transactions, international trade statistics and corruption cases all point towards significant improvements for importing plants when their assigned ports underwent the reform.

Rich internal documentation from the DIAN expands on how the reform would tackle the many opportunities for corruption in the previous system. These documents describe how the reform was expected to reduce smuggling, lead to better communication with transporters, banks and other stakeholders, making fraud more difficult, and reduce the discretionary power of customs agents, leading to fewer complaints about bureaucracy and corruption at customs. These insights guide our empirical approach to the channels, and ultimately turn out to be quite consistent with our findings. Evidence of facilitation at customs includes an increase in imports, and the finding that importers are willing to travel longer distances to go through automatized and less corrupt customs (similar to the results of Sequeira and Djankov, 2014). Time do clear customs did not decrease but became more predictable. Customs also became more effective at collecting taxes due and enforcing sanctions. Moreover, we find evidence of reduced corruption: the reform led to a reduction in smuggling and a significant drop in the number of corruption investigations related to DIAN officials.

This study provides new evidence of the great benefits of computerization of imports for the economy. It also adds to the scarce evidence on the costs of corruption at customs, highlighting the potential of e-government interventions to better manage information, increasing efficiency while limiting interactions that are prone to corruption. The computerization had an estimated total cost of about nine million dollars, which is dwarfed by its estimated benefits. [22] Interestingly, DIAN engineers reported how challenging it was to obtain funding to support the development of the project. This paper shows that, when properly implemented, such investment can have a high return for the economy.

This paper adds to growing evidence for the significant benefits of proper use of information and communication technologies to improve institutions, reduce

[22]For comparison, a 1% increase in value added in the manufacturing sector in 2000 was worth about 163 million dollars, and a 1% increase in taxes of manufacturing imports was worth 30 million dollars.

bureaucracy and tackle corruption. Rigorous evidence of successful attempts to tackle corruption in the interaction between government agencies and the business sector is rare to come by, in particular at customs level. Hence the importance of drawing lessons from this case. The conditions that allowed this reform to be successful remain to be explained further. One can highlight that, in the case of Colombia, the program was developed internally for three years prior to its first implementation. The DIAN's internal documentation mentions that "it has been of singular importance for technology-process integration, that the Siglo XXI project has not been conceived exclusively by a software engineering team, but as a working group, with the inclusion of customs experts and engineers who developed the application, in perfect collaboration with external users (customs users and unions)." However, the reasons why an intervention expected to reduce rents from corruption to such an extent overcame the natural opposition of rent-holders remain to be explored.

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Figure A1: Time to clear customs when importing to Colombia

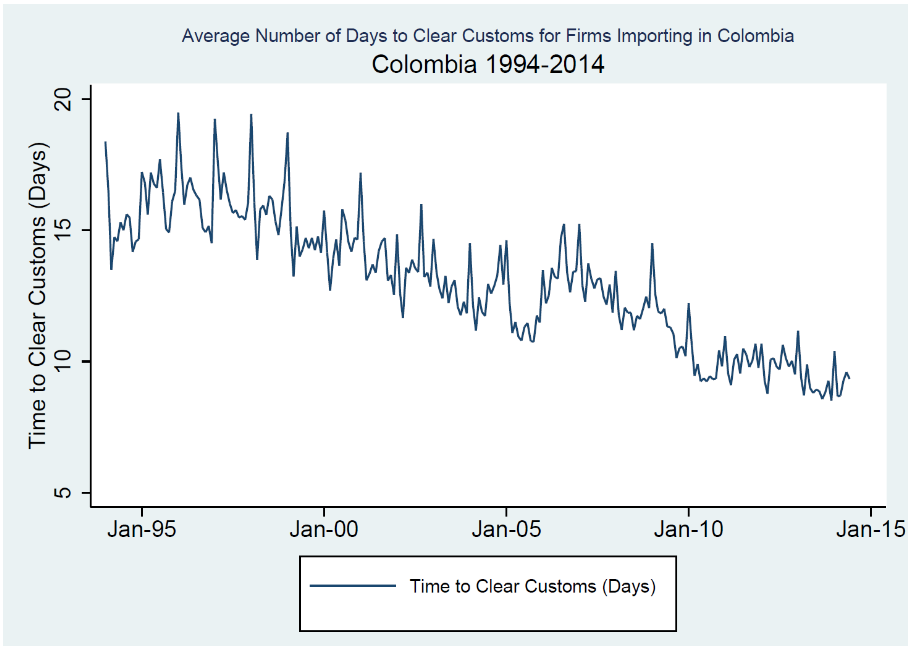

Figure A1 presents the average time to clear customs during an import transaction in Colombia. Using data from the DIAN from 1994 to 2014, we calculated by month, the average time of all import transactions.

Table A1: Definition of Variables

A1.A definition of variables from the Annual Manufacturing Surveys

Variable	Description
Reform at plant's assigned port	A dummy equal to one when the customs post assigned to the plant underwent the computerization, i.e. after the first year with at least one month in which the proportion of import declarations made by computer is bigger than 80%
Plant's inputs expected treatment	Weighted average of the port treatments in the corresponding year. The weights come from the sum of the share of use of each input type (HS6) multiplied by the share each port from which the input type went through in 1999.
Importer Plant	Dummy equal to 1 if the plant imported a positive value in any year between 1997 and 1999
Inputs*	Expenditure in material inputs used by the plant
Sales*	Total revenue from sales, measured in millions of real pesos
Value Added *	Sales from which the value of inputs is substracted
Number of workers	Number of employees in the plant
Capital*	Capital stock, including machines, equipment and transport equipment
Value Added per Worker*	Value Added divided by the number of workers in the plant
Export Dummy	Takes the value 1 if the plant exported a positive value that year
TFP	Total Factor Productivity, calculated as a residual, with elasticity of inputs calculated by sector
Imports	Value of imports (Free On Board) declared at the DIAN by the firm (sum of all transactions during the year)
Customs Initial Import Value	Total value of imports (Free On Board) that went through customs post assigned to the plant in 1999

A1.B definition of variables from customs analyzed at plant level

Variable	Description
Customs Clearence Time	Average across transactions of number of days that the merchandise takes to clear customs. At the transaction level, clearance time is calculated as the difference between date of authorization to leave and date of arrival
Paid/FOB	Average across transactions of the ratio between the amount that has been paid and the value of the merchandise
Paid/Due	Average across transactions of the ratio between the amount that was paid and the amount calculated by DIAN as due
Sanctions/FOB import value	Average across transactions of the value of sanctions due to delays and violations, divided by the FOB value

A1.C definition of variables from customs analyzed at customs level

Variable	Description
Number of Imports	Number of imports transactions through the custom post during the year.
Total value of imports (FOB)*	Value of the merchandise imported (Free on Board, i.e. not including neither transport costs nor insurance)
Total Taxes (VAT+Tariff)*	Total amount of Tariff Taxes and Value Added Taxes collected
Total Transportation Costs*	Transportation costs from departure abroad to the customs

A1.D definition of variables from UNCOMTRADE

Variable	Description
Value of imports as declared in Colombia	Total value of Colombian imports from a given country, by year and HS04 product code, as declared in Colombia by the importer. Inclusive of the cost of transportation to the Colombian border.
Value of imports as declared in country of origin	Total value of exports towards Colombia from a given country, by year and HS04 product code, as declared in the country of origin by the exporter. Does not include the cost of transporation to the Colombian border.
Import Capture Ratio	Ratio of value of imports as declared in Colombia to value of those imports as declared in the country of origin, as defined above, by year and HS04 product category. An Import Capture Ratio below 1 is an indicator of subdeclaration (or miscategorization) of imports from the importer.

A1.E definition of variables on cases related to corruption at DIAN

Variable	Description
DIAN Corruption Related Cases	Number of cases involving DIAN (Dirección de Impuestos y Aduanas Nacionales) of one of the following types: administrative violations, contractual violations, budget violations, and violations with criminal connotations. The listing of violations related to corruption comes from the *Procuraduria General de la Nacion* .
Number of corruption cases other than DIAN	Number of cases among the four types of cases listed above, but NOT involving the DIAN.
Factor of DIAN Corruption Related Cases	First factor of thefour types of cases listed above (involving DIAN)
Factor of corruption cases other than DIAN	First Factor of the four types of cases listed above, but NOT involving the DIAN.

*To limit the influence of extreme values, values beyond the 99th percentile were replaced by its 99th percentile.
All monetary values are expressed in billions of pesos per year and are deflated by PPI (1988).

Table A2: Parallel trends analysis and dynamic effects of the reform on the activity of manufacturing plants: each plant is matched with a port based on its geographical location

VARIABLES	Log Inputs	Log Sales	Log Value Added	Log Number of workers	Log Capital	Export dummy	Log Value Added per Worker	TFP	Imports (FOB)
2 years before treatment at assigned port	-0.056	-0.061	-0.058	-0.056	0.05	-0.015	-0.0027	-0.0054	-0.011
	(0.041)	(0.026)	(0.024)	(0.019)	(0.019)	(0.047)	(0.028)	(0.011)	(0.016)
1 year before treatment at assigned port	-0.046	-0.040	-0.039	-0.038	0.0098	0.0045	-0.0012	-0.00038	0.011
	(0.027)	(0.028)	(0.035)	(0.011)	(0.0090)	(0.017)	(0.028)	(0.0086)	(0.013)
1 year after treatment at assigned port	0.010	-0.029	-0.053	-0.024	-0.036	-0.035	-0.029	-0.0035	0.0035
	(0.034)	(0.023)	(0.017)	(0.017)	(0.029)	(0.049)	(0.015)	(0.0078)	(0.0044)
2 years after treatment at assigned port	0.0093	-0.029	-0.058	-0.018	-0.060	-0.015	-0.04	0.00037	0.0021
	(0.052)	(0.032)	(0.018)	(0.026)	(0.037)	(0.045)	(0.018)	(0.0093)	(0.0087)
3 years after treatment at assigned port	0.035	-0.013	-0.053	-0.0071	-0.067	-0.0065	-0.046	0.0026	0.0072
	(0.050)	(0.030)	(0.018)	(0.028)	(0.041)	(0.044)	(0.020)	(0.0091)	(0.012)
Importer Plant * 2 years before treatment at assigned port	-0.024	-0.0093	0.026	0.00097	-0.035	-0.016	0.025	0.0081	-0.0030
	(0.022)	(0.015)	(0.029)	(0.015)	(0.026)	(0.073)	(0.028)	(0.013)	(0.0092)
Importer Plant * 1 year before treatment at assigned port	0.014	0.014	0.041	0.022	-0.0024	-0.039	0.018	0.0042	-0.0084
	(0.021)	(0.038)	(0.061)	(0.013)	(0.013)	(0.036)	(0.050)	(0.016)	(0.0099)
Importer Plant * 1 year after treatment at assigned port	0.023	0.03	0.06	0.0027	0.0047	0.061	0.058	0.015	0.0025
	(0.0071)	(0.011)	(0.014)	(0.010)	(0.012)	(0.067)	(0.012)	(0.0019)	(0.0057)
Importer Plant * 2 years after treatment at assigned port	0.024	0.034	0.094	0.020	0.013	0.049	0.072	0.019	0.0074
	(0.015)	(0.0100)	(0.016)	(0.019)	(0.013)	(0.069)	(0.026)	(0.0034)	(0.0078)
Importer Plant * 3 years after treatment at assigned port	0.043	0.064	0.13	0.035	0.0071	0.045	0.095	0.028	0.013
	(0.020)	(0.011)	(0.019)	(0.016)	(0.020)	(0.061)	(0.024)	(0.0049)	(0.011)
2 years before treatment (alpha+beta)	-0.08	-0.070	-0.032	-0.055	0.015	-0.031	0.022	0.0027	-0.014
	(0.034)	(0.028)	(0.025)	(0.020)	(0.016)	(0.038)	(0.018)	(0.0099)	(0.014)
1 year before treatment (alpha+beta)	-0.032	-0.027	0.0021	-0.01	0.0073	-0.035	0.017	0.0038	0.0027
	(0.025)	(0.020)	(0.027)	(0.0075)	(0.0067)	(0.025)	(0.026)	(0.0095)	(0.0091)
1 year after treatment (alpha+beta)	0.033	0.0015	0.0078	-0.021	-0.031	0.026	0.029	0.012	0.0061
	(0.030)	(0.021)	(0.012)	(0.014)	(0.026)	(0.036)	(0.013)	(0.0071)	(0.0073)
2 years after treatment (alpha+beta)	0.033	0.0051	0.035	0.0029	-0.047	0.035	0.032	0.019	0.0095
	(0.051)	(0.030)	(0.015)	(0.018)	(0.033)	(0.037)	(0.021)	(0.0079)	(0.013)
3 years after treatment (alpha+beta)	0.078	0.051	0.077	0.028	-0.060	0.039	0.049	0.03	0.020
	(0.048)	(0.029)	(0.013)	(0.017)	(0.033)	(0.036)	(0.015)	(0.0065)	(0.014)

This table reports estimation results from estimating model (2). This replicates the specification of Table 1 Panel A but separating the treatment effects by year (before and after the beginning of the treatment). The only omitted treatment year is the year prior to the "Reform at plant's assigned port" so that all coefficients can be interpreted as differences with respect to that year. The five coefficients in the bottom of the table estimate the sum of the first to set of coefficients (for the reform and its interaction with Importer plant) and its standard error. Standard errors clustered at port level are in parenthesis.

Table A3: Triple-difference treatment effects on firms outcomes excluding ports one by one

Excluded ports	Inputs	Sales	Value Added	Number of workers	Capital	Export dummy	Value Added per Worker	TFP	Imports (FOB)	Nb of obs.
Bogotá	0.058	0.12	0.18	0.079	0.057	0.097	0.01	0.036	0.04	17,589
	(0.026)	(0.02)	(0.031)	(0.024)	(0.046)	(0.025)	(0.022)	(0.014)	(0.015)	
Medellin	0.069	0.09	0.14	0.051	0.046	0.094	0.072	0.021	0.021	32,505
	(0.018)	(0.015)	(0.019)	(0.018)	(0.030)	(0.015)	(0.046)	(0.0068)	(0.015)	
Cali	0.061	0.088	0.14	0.052	0.027	0.088	0.074	0.027	0.02	38,478
	(0.018)	(0.013)	(0.015)	(0.015)	(0.024)	(0.014)	(0.037)	(0.0081)	(0.012)	
Barranquilla	0.056	0.084	0.13	0.046	0.021	0.89	0.079	0.031	0.016	37,477
	(0.017)	(0.011)	(0.013)	(0.012)	(0.021)	(0.015)	(0.036)	(0.0076)	(0.0088)	
Cartagena	0.076	0.097	0.14	0.062	0.031	0.082	0.075	0.025	0.019	37,763
	(0.013)	(0.015)	(0.017)	(0.016)	(0.026)	(0.013)	(0.038)	(0.0082)	(0.011)	
Buenaventura	0.067	0.090	0.14	0.061	0.021	0.076	0.075	0.025	0.024	37,944
	(0.021)	(0.015)	(0.014)	(0.018)	(0.025)	(0.090)	(0.041)	(0.0082)	(0.013)	
Ipiales	0.061	0.088	0.14	0.051	0.023	0.088	0.068	0.028	0.022	39,655
	(0.017)	(0.012)	(0.014)	(0.014)	(0.022)	(0.013)	(0.038)	(0.0079)	(0.012)	
Santa Marta	0.064	0.088	0.14	0.052	0.027	0.085	0.067	0.027	0.022	39,688
	(0.018)	(0.013)	(0.014)	(0.015)	(0.023)	(0.013)	(0.038)	(0.0080)	(0.012)	
Cúcuta	0.065	0.092	0.14	0.055	0.029	0.088	0.068	0.028	0.021	39,919
	(0.017)	(0.014)	(0.016)	(0.015)	(0.024)	(0.014)	(0.038)	(0.0079)	(0.011)	
Pereira	0.064	0.091	0.14	0.054	0.030	0.087	0.068	0.027	0.022	39,952
	(0.017)	(0.013)	(0.015)	(0.015)	(0.024)	(0.013)	(0.038)	(0.0078)	(0.011)	

Table A3 reports estimation of the triple difference results from estimating model (1), replicating the results of Table 1A, but excluding, one by one the port reported in column 1. Standard errors clustered at port level are in parenthesis.

Table A4: Effect of computerization on plants' economic activity using a continuous treatment based on relevant fraction of ports treated

VARIABLES	Log Inputs	Log Sales	Log Value Added	Log Number of workers	Log Capital	Export dummy	Log Value Added per Worker	TFP	Imports
Importer Plant * Plant's location expected treatment	0.12	0.16	0.22	0.11	0.041	0.11	0.041	0.038	0.047
	(0.031)	(0.026)	(0.029)	(0.022)	(0.030)	(0.023)	(0.011)	(0.0099)	(0.010)
Plant's Treatment Probability	0.0084	-0.035	-0.055	-0.032	-0.051	-0.023	-0.016	0.00091	-0.018
	(0.036)	(0.031)	(0.037)	(0.026)	(0.034)	(0.034)	(0.021)	(0.014)	(0.015)
Observations	39,699	39,699	39,640	39,699	39,699	39,640	39,699	39,699	39,699
Wild Bootstrap p-value[1]	0.000	0.000	0.000	0.000	0.167	0.000	0.000	0.000	0.000
p-val of sum of both coef.	0.000	0.000	0.000	0.002	0.740	0.011	0.244	0.003	0.067

This table reports estimation results from estimating model (1), using a continuous measure of treatment based on its location. "Plant's location expected treatment" is the weighted average of port's share of imports that were computerized , where the weight of each port is given by the share of the imports by all producers in the plant's municipality that went through that port in 1999. Each observation corresponds to a plant and year. All values (except dummies) are expressed in logs. Controls include plant and year Fixed effects, initial log of value added of the plant interacted with year dummies and initial log of size of the port interacted with year dummies. Standard errors clustered at port level are in parenthesis.

[1] Wild bootstrap p-value of the coefficient "Importer Plant * Plant's location expected treatment", with errors clustered at plant level (9,999 reps).

Table A5: Effect of the reform on the activity, of manufacturing plants, by quartile of value added in 1999, when each plant is matched with ports based on fraction of the plant's inputs that is treated

Quartile (of VA in 1999)	VARIABLES (in log):	Inputs	Sales	Value Added	Number of workers	Capital	Export dummy	Value Added per Worker	TFP	Imports
Quartile 1 (Importer dummy = 21%)	Importer Plant * Plant's inputs expected treatment	0.12 (0.060)	0.19 (0.061)	0.30 (0.054)	0.14 (0.033)	0.089 (0.036)	0.16 (0.031)	0.016 (0.013)	0.066 (0.015)	0.071 (0.0073)
	Plant's inputs expected treatment	0.26 (0.029)	0.18 (0.040)	0.12 (0.065)	0.10 (0.032)	0.056 (0.040)	0.019 (0.056)	0.039 (0.021)	-0.0058 (0.022)	-0.019 (0.0057)
Quartile 2 (Importer dummy = 42%)	Importer Plant * Plant's inputs expected treatment	0.16 (0.053)	0.24 (0.043)	0.28 (0.054)	0.13 (0.026)	-0.029 (0.038)	0.16 (0.048)	0.035 (0.013)	0.056 (0.015)	0.075 (0.019)
	Plant's inputs expected treatment	0.27 (0.059)	0.14 (0.071)	0.060 (0.092)	0.1300 (0.067)	0.020 (0.072)	-0.069 (0.057)	0.068 (0.022)	-0.046 (0.016)	-0.020 (0.0070)
Quartile 3 (Importer dummy = 75%)	Importer Plant * Plant's inputs expected treatment	0.055 (0.022)	0.096 (0.032)	0.12 (0.044)	0.086 (0.027)	0.070 (0.039)	0.030 (0.041)	0.022 (0.018)	0.034 (0.030)	0.054 (0.0071)
	Plant's inputs expected treatment	0.14 (0.034)	0.031 (0.021)	0.0028 (0.031)	-0.025 (0.039)	-0.087 (0.030)	0.028 (0.053)	0.048 (0.029)	-0.038 (0.017)	0.048 (0.018)
Quartile 4 (Importer dummy = 95%)	Importer Plant * Plant's inputs expected treatment	0.077 (0.065)	0.015 (0.036)	-0.030 (0.038)	-0.10 (0.082)	-0.27 (0.088)	0.074 (0.10)	0.14 (0.031)	0.011 (0.022)	-0.016 (0.087)
	Plant's inputs expected treatment	0.069 (0.080)	0.079 (0.034)	0.057 (0.055)	0.16 (0.082)	0.21 (0.100)	-0.11 (0.11)	-0.17 (0.031)	0.0026 (0.039)	0.21 (0.090)

Table A5 reports estimation results from estimating model (1), replicating the results of Table 1 Panel B, separately for each quartile of value added of plants in 1999 (where quartile 1 corresponds to the smallest firms and quartile 4 to the largest firms). Standard errors clustered at port level are in parenthesis.

Table A6: Dynamic effects of the reform on characteristics of imports transactions (observations at plant-year level)

VARIABLES	Customs Clearence Time	Paid/FOB	Paid/Due	Sanctions/FOB import value
2 years before treatment at assigned port	0.35 (0.47)	0.00034 (0.0023)	-0.079 (0.030)	0.000005 (0.000023)
1 year before treatment at assigned port	0.88 (0.52)	0.0021 (0.0019)	-0.016 (0.032)	0.000016 (0.000024)
1 year after treatment at assigned port	-0.25 (0.30)	0.0072 (0.0012)	0.080 (0.034)	0.000005 (0.000017)
2 years after treatment at assigned port	0.37 (0.31)	0.0080 (0.0010)	0.15 (0.057)	0.000018 (0.000011)
3 years after treatment at assigned port	0.082 (0.30)	0.0084 (0.00090)	0.15 (0.024)	0.000030 (9.85e-06)

This table reports the results of estimating a dynamic version of model (5). This replicates the equation of Table 3 but separating the treatment effects by year (before and after the beginning of the treatment). The only omitted treatment year is the year prior to the "Reform at plant's assigned port" so that all coefficients can be interpreted as differences with respect to that year. Standard errors clustered at port level are in parenthesis.

Table A7: Effect of computerization on characteristics of imports transactions by importing plants, separating plants by quartile of value added in 1999

Quartile (of VA in 1999)	VARIABLE (log):	Custom Clearence Time	Paid/FOB	Paid/Due	Sanctions/FOB import value
Quartile 1	Reform at plant's	0.024	-0.0021	0.049	0.00002
(Exposure =	assigned port	(0.79)	(0.0025)	(0.030)	(4.84e-06)
21%)	Mean of outcome var	12.6	0.024	0.96	0.000020
Quartile 2	Reform at plant's	-0.48	0.0025	-0.000083	0.000035
(Exposure =	assigned port	(0.58)	(0.0030)	(0.013)	(0.000026)
42%)	Mean of outcome var	13.0	0.025	0.97	0.000028
Quartile 3	Reform at plant's	0.23	0.011	0.014	0.000039
(Exposure =	assigned port	(0.36)	(0.0018)	(0.015)	(0.000042)
75%)	Mean of outcome var	13.3	0.030	0.94	0.000020
Quartile 4	Reform at plant's	-0.51	0.0042	0.067	0.000019
(Exposure =	assigned port	(0.31)	(0.00078)	(0.022)	0.000007
95%)	Mean of outcome var	14.2	0.028	0.75	0.000035

Table A7 reports estimation results from estimating model (3), thus replicating the results of Table 3, but separately for each quartile of value added of plants in 1999. Observations are weighted by the value added of the firm in 1999, before any reform started. Standard errors clustered at port level are in parenthesis.

Table A8: Dynamic effects of the reform on the characteristics of imports transactions at the level of a customs' post (observations at port-year level)

VARIABLES	Nb of imports	Total value of imports (FOB)	Total Taxes (VAT + Tariff)	Transportation Costs	Transportation Costs / FOB
2 years before treatment	-10,097	-231.0	-4.039	-3.882	0.000345
	(9,451)	(255.9)	(5.665)	(3.091)	(0.00178)
1 year before treatment	-10,825	-245.9	-3.388	-8.929	-0.000480
	(7,127)	(154.7)	(2.620)	(5.761)	(0.00121)
1 year after treatment	5,746	156.1	3.727	2.571	0.00309
	(4,023)	(130.1)	(2.058)	(1.118)	(0.00161)
2 years after treatment	16,286	479.7	15	4.105	0.00425
	(8,520)	(223.3)	(6.800)	(2.090)	(0.00295)
3 years after treatment	20,987	796.1	23.45	4.045	0.00702
	(9,368)	(303.6)	(9.251)	(4.206)	(0.00353)

This Table reports estimation results from estimating model (7). This replicates the equation of Table 4 but separating the treatment effects by year (before and after the beginning of the treatment). The only omitted treatment year is the year prior to the "Reform at plant's assigned port" so that all coefficients can be interpreted as differences with respect to that year. Standard errors clustered at port level are in parenthesis.

Table A9: Dynamic effects of the reform on investigations related to corruption at DIAN

VARIABLES	DIAN Corruption Related Cases	Factor of DIAN Corruption Related Cases
2 years before treatment	0.0222	0.0465
	(0.0394)	(0.0668)
1 year before treatment	0.0388	0.0605
	(0.0311)	(0.0491)
1 year after treatment	-0.00437	-0.0336
	(0.0578)	(0.0813)
2 years after treatment	-0.0636	-0.493
	(0.111)	(0.307)
3 years after treatment	-0.313	-0.485
	(0.150)	(0.222)

This table reports estimation results from estimating model (7). This replicates the equation of Table 7 but separating the treatment effects by year (before and after the beginning of the treatment). The only omitted treatment year is the year prior to the "Reform at plant's assigned port" so that all coefficients can be interpreted as differences with respect to that year. Standard errors clustered at port level in parenthesis.