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# The costs of bureaucracy and corruption at customs: Evidence from the computerization of imports in Colombia

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ABSTRACT

We assess the effects of the computerization of import transactions in Colombia on import volume, port efficiency and the performance of manufacturing firms. Staggered implementation allows us to identify the causal effects of the reform. We find that computerization triggered a significant increase in reported imports in reformed ports compared to nonreformed ones, along with a sizeable increase in tax collection. Our results indicate that a combination of factors underpin the increase in declared imports: an actual increase in firms' imports, a reduction in import underreporting and a redirection of imports from nonreformed to reformed ports that reveals importers' preference for the latter. Other signs of reduced corruption include increased predictability of clearance times and a reduction in the number of customs-related corruption cases prosecuted by the authorities. Importantly, increased imports lead to better firm performance: in municipalities associated with treated customs, sales of manufacturing firms increased by 5.2% for importers and shrunk by 3.9% for nonimporters. These effects increase over time and are concentrated in small- to medium-sized firms, which appear to have been the most affected by the nontariff barriers before computerization.

## 1. Introduction

Barriers to international trade affect business activity and welfare through various channels. The literature has focused mostly on the effects of import tariffs because of their prominent role in trade policy and the ease of accessing data on tariffs.[1] However, understanding the consequences of other barriers to trade has gained increased importance. After decades of tariff reductions, many countries have little practical or political room for additional tariff cuts, while nontariff barriers "have replaced tariffs as the primary tools of trade policy" (Goldberg and Pavcnik, 2016). Because inefficiency at ports acts as a tax without generating revenue, reforming customs can be a win–win policy. In contrast to tariff reductions, these reforms can boost economic activity without sacrificing fiscal revenue.

Non-modernized customs offer customs officials significant discretion to stop cargo and a broad bureaucratic toolkit to justify delays and hurdles. This leeway creates opportunities for rent extraction, slowing international trade and imposing large costs on businesses (Sequeira, 2015). In contrast, increasingly efficient information technologies create new opportunities to reduce bureaucracy and enhance the efficiency of customs by limiting interactions between firms and customs agents.

This paper studies the effects of the computerization of customs procedures in Colombia on trade activity at ports, firm performance, and customs governance. Between 2000 and 2005, Colombia went from all import transactions being processed manually to full computerization by sequentially adding ports to the computerized system. The reform allowed importers to complete nearly all import procedures online. Costly manual reporting and manual inspection of paperwork were eliminated. The need for physical inspections was reduced, and the decision to undertake such inspections, previously at the discretion of the customs agent, was reassigned to an algorithm based on risk profiles and inconsistencies between the declarations of different actors: the foreign exporter, the importer, the transporter, the warehouse, and the bank where tariff payments were made. The calculation of fees due was automatized, as was the transmission of proof of payment and the subsequent release of cargo.

The computerization of customs procedures across the 26 ports was staggered because of the limited implementation capacity. This

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[1] For instance, see Bustos, 2011 for Argentina; Halpern et al., 2011 for Armenia; Lileeva and Trefler, 2010 for Canada; Attanasio et al., 2004, Eslava et al., 2013, and Fieler et al., 2018 for Colombia; Pavcnik, 2002 for Chile; Carballo et al., 2022, and De Loecker et al., 2016, for India; and Amiti and Konings, 2007 for Indonesia.

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progressive roll-out and the rich data on businesses and ports offer a unique opportunity to isolate the causal effects of the reform on multiple dimensions of trade and economic activity.

We investigate three main questions:

1. Did the computerization increase reported import activity at the reformed ports? Was this increase due to an increase in actual imports, a reduction in underreporting, or a diversion of transactions from unreformed to reformed ports?
2. If the reform increased actual imports, what other effects did it have on the performance of exposed firms (i.e., those that were already importing before the reform)? Was the effect heterogeneous by firm size?
3. Are there indications that the reform reduced corruption at customs?

The reform was expected to affect all these margins. According to internal reports, the reform reduced bureaucratic hurdles, increased efficiency, and reduced opportunities for smuggling, misreporting and underpayment of tariffs. From the importer's point of view, reduced bureaucracy and corruption at ports reduces the importing costs, potentially increasing imports of inputs and leading to increased input use and revenue generation. Productivity may also be affected if the imported inputs the firm starts accessing are higher quality (as in Eslava et al., 2018). However, competitors also benefit from reduced costs of imports, which may be detrimental to the performance of firms less equipped to deal with increased competition. The net effect of the reform on firms should be heterogeneous depending on whether the firm benefited from reduced transaction costs more than being hit by increased competition. The effect of reduced corruption is also theoretically ambiguous. "Coercive corruption" increases costs to firms by making them pay bribes for a service they are entitled to, but a reform that improves tax collection could also hamper the performance of firms that substituted large tariff payments for small bribes, as in "collusive corruption" (Sequeira, 2016).

To answer our three research questions, we first examine data on all import transactions, including free on board (FOB) import value, clearance time at customs, and tariffs paid by firms. We find that, as a result of computerization, reported imports increased by 70% in reformed ports compared to nonreformed ports. Using commodity trade data from UN COMTRADE, we compare imports (by type of good) as reported by the seller in the country of origin to those reported by the importer in Colombia. We find that the increase in declared imports of a good that typically passes through a reformed port is 20 log points, of which 12 points appear to be due to an actual increase in imports and the remaining 8 log points are due to a reduction in underreporting. Additionally, we observe an increase in per-dollar transportation costs for transactions passing through a reformed port, which suggests a revealed preference for ports that underwent computerization, consistent with the results of Sequeira and Djankov (2014). The increase in reported imports is concomitant with an increase in tax collection in reformed ports. Consistent with the observed reduction in underreporting, we also find a 20% increase in taxes paid per dollar imported.

We then examine the effects of the reform on firms' economic activity beyond imports. We assemble yearly panel data on all nonmicro size manufacturing plants in Colombia, spanning the pre and postreform periods (1998–2009). We find that importers in the area of influence of a reformed port experienced a gradual and substantial increase in revenue, input use, and productivity relative to plants in the area of influence of nonreformed ports. Sales expanded by 5.2% for importers and contracted by 3.9% for nonimporters in the same municipality. Hence, the negative effect of increased competition on nonimporters appears to have dominated any indirect cost-reducing effect through purchases from local importers. When matching plants to the ports most used for plants' inputs, the effect on importers' growth remains positive and significant, while the spillover effect is positive for nonimporters, indicating that nonimporters benefit from a reduction in importing costs for their inputs, even if they do not import them directly. These effects start at the first year after treatment and grow over time.

The estimated 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. This group is the top segment of plants, owned by firms that were already importing and exporting at high rates, i.e., those that had already sorted out how to address hurdles at customs before the reform.

While smoking-gun evidence of changes in corruption is always challenging to obtain, some of our results are consistent with the reform reducing corruption at ports. Among the evidence supporting this mechanism is, first, the increase in the effective tax rate that follows the reform, consistent with a reduction in misclassification and nonpayment of due taxes; second, the reduced discrepancies between imports declared in Colombia and imports declared by exporters, which indicate tax evasion at customs (Fisman and Wei, 2004); third, increased predictability in the duration of customs transactions, which is in line with reduced discretionary power of on-the-ground customs officials; and finally, a drop in the number of judiciary cases related to corruption, although the small number of such cases requires caution in interpreting this result. The large gains for firms point toward corruption that was primarily coercive, but the increased tax collection suggests that collusive corruption may have also been present (Sequeira and Djankov, 2014).

The trade literature provides evidence that tariff reductions are associated with increased revenue productivity via increased firm efficiency and quality, with more mixed results regarding markups.[2] Positive effects on firms are stronger when tariffs are reduced for their inputs compared to their outputs. Literature on the effects of administrative trade barriers is more recent and growing. Djankov et al. (2010) estimate the effect of delays at customs on trade. We contribute to this literature by providing evidence of the effect of customs computerization on business performance through a host of mechanisms, including increased efficiency and reductions in corruption, smuggling and evasion of tariff payments at ports.

Computerization is a relatively low-cost reform that addresses a widespread and potentially high-cost trade barrier: bureaucracy in customs. Numerous case studies document the benefits and challenges of customs computerization (De Wulf and Sokol, 2004; Engman, 2005). The effect of customs efficiency on the trade volume is investigated through a series of evaluations of different trade facilitation programs (Martincus, 2015; Fernandes et al., 2015; Carballo et al., 2016, 2021). However, to the best of our knowledge, there is no causal evidence on how the use of information technologies at customs impacts firm growth. Compared to this literature, our study offers a broader perspective by extending the analysis to a wide range of product-level, customs-level outcomes, and firm-level evidence beyond what can be captured at the border and beyond trade itself.[3]

[2] For surveys, see Goldberg and Pavcnik, 2016; De Loecker and Goldberg (2014); Melitz and Redding (2014).

[3] See Pavcnik (2002), Lileeva and Trefler, 2010, Bustos, 2011, Eslava et al. (2013), and De Loecker et al. (2016) for the 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; Carballo et al., 2022, and De Loecker et al., 2016, for India), while Fieler et al. (2018) find positive effects on quality of input tariffs cuts in Colombia.

This paper also contributes to literature that provides firm-level evidence of the relationship between institutions and development by providing evidence of the potential of administrative reforms to impact the business sector. A stream of this literature highlights how corruption hampers development beyond simple transfers from businesses 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 of an equivalent tax increase. Sequeira and Djankov (2014) and Sequeira (2016) show that firms are willing to pay higher transport costs to import through a less corrupt port and that tariff liberalization can have limited effects on trade because of corruption in customs.

Our research also closely relates to the growing literature on the potential of information technologies to improve efficiency and reduce corruption. (Giné et al., 2012; Banerjee et al., 2004; Muralidharan et al., 2016). We complement this literature with primary evidence of the effect of a large-scale modernization of customs on outcomes at the port and firm levels. Finally, following Djankov et al. (2002), a large body of literature documents the costs of regulatory burdens on firms and the potential tradeoffs between regulation and discretion (Duflo et al., 2018; Bosio et al., 2022). We highlight that better technology can reduce the regulatory burden and discretion simultaneously.

Our results suggest that the cost of the computerization of imports (approximately 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 also more generally point to large potential gains from improving the efficiency of state agencies with which the business sector interacts. The lessons from this research are also applicable to Colombia, where customs underwent large improvements at the beginning of the century but is still less advanced than many middle-income countries and is in 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 implementation of this reform was staggered across the different ports of Colombia between 2000 and 2005 because of limited implementation capacity. Before the reform, each import transaction required physically handling paperwork at the port. Completing and handling physical paperwork impose a nonnegligible cost to the importer and create a bureaucratic hurdle, as each piece of information must be checked manually. Moreover, manual handling of paperwork introduced several layers of discretion exercised by customs officials, including decisions regarding whether physical inspections were necessary and issuance of payment forms, allowing them to affect the choice of product codes that determined the fee to be applied. Customs inspectors also received proof of payment, based on which they approved the release of cargo. The absence of automatic direct payment notification by the bank to the customs agent allegedly created room for falsification of payment proofs.

After the reform, users switched to the online declaration of imports. The platform allows instant recording and comparison across declarations by the transporter, the importer, the warehouse, and the bank, speeding the inspection of paperwork and reducing the number of administrative steps needed. Direct inspection processes in the new system are triggered by risk profiles and inconsistencies in declarations, identified by the computer system rather than based on the arbitrary decisions of customs officers. This change reduced physical inspections by customs from over 50% to approximately 9%. Automatic cross checks and traceability also reduce the risk that a customs inspector declares conformity between the cargo manifest and the physical cargo when there are inconsistencies. Under the new system, the customs agency receives electronic payment confirmation. Once a payment is made, the release request is executed without intervention by customs authorities. Approximately 86% of the merchandise is automatically released compared with 30% to 40% before the reform.

## 3. Effects of the reform on imports

This section establishes that the reform led to increased import activity and tax collection at reformed ports while reducing underreporting. The next section analyzes how these changes reflect the performance of businesses in a port's affected plants.

### 3.1. Data

To analyze the effect of the reform on imports, we use administrative records from the Colombian tax administration, DIAN. The database covers all import transactions, identifying the importer through a firm tax code. It gathers approximately 1,000,000 import transactions per year from approximately 56,000 firms. For each transaction, it provides information about the port of entry, the good that is imported (at a level equivalent to the six digits of the Harmonized system), its quantity, value, destination, origin, and 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. With the DIAN's help, we were able to recover whether each transaction was processed manually or by computer. Appendix B shows a detailed list of the variables included in the customs database that are relevant for our analysis.

We label a port as computerized in a given year if over 80% of total imports passing through that port during the year were declared by computer rather than manually.[5] Fig. 1 shows that, in each port, the switch from manual to digital imports almost always implied a rapid jump from 0 to nearly 100% of imports computerized over the course of one year, remaining close to 100% for subsequent years. We thus consider reform at the port level as a discontinuous treatment that is not sensitive to setting the cutoff at 80%.[6]

### 3.2. Effects on transactions at the port level

As described in the Introduction, there are several possible channels through which the reform should operate, including a facilitation of imports and a reduction in smuggling, underdeclarations, and misreporting. Given these factors, one would expect importing activity and tax collection to increase in treated ports. We assess these potential effects through port-level regressions. We collapse the transaction database to the port-year level and resort to a double difference strategy that takes advantage of the sequencing of computerization across ports to

[4] It can discourage investment (Samphantharak and Malesky, 2008) and human capital accumulation (Ferraz et al., 2012; Reinikka and Svensson, 2004) and can lead to the misallocation of capital (Khwaja Mian, 2005) or talent (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).

[5] 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.

[6] Bogota, which was the first customs to implement the computerization, is the one exception, showing a rapid increase to approximately 86% of transactions but reaching 100% only by the second year since the beginning of the computerization. Nevertheless, the initial rapid increase is enough to consider Bogota as treated in year 2001. In Appendix D, we show that the results are robust to the exclusion of municipalities one by one and that they are not sensitive to the use of the continuous treatment equal to the share of computerized imports, rather than a dummy.

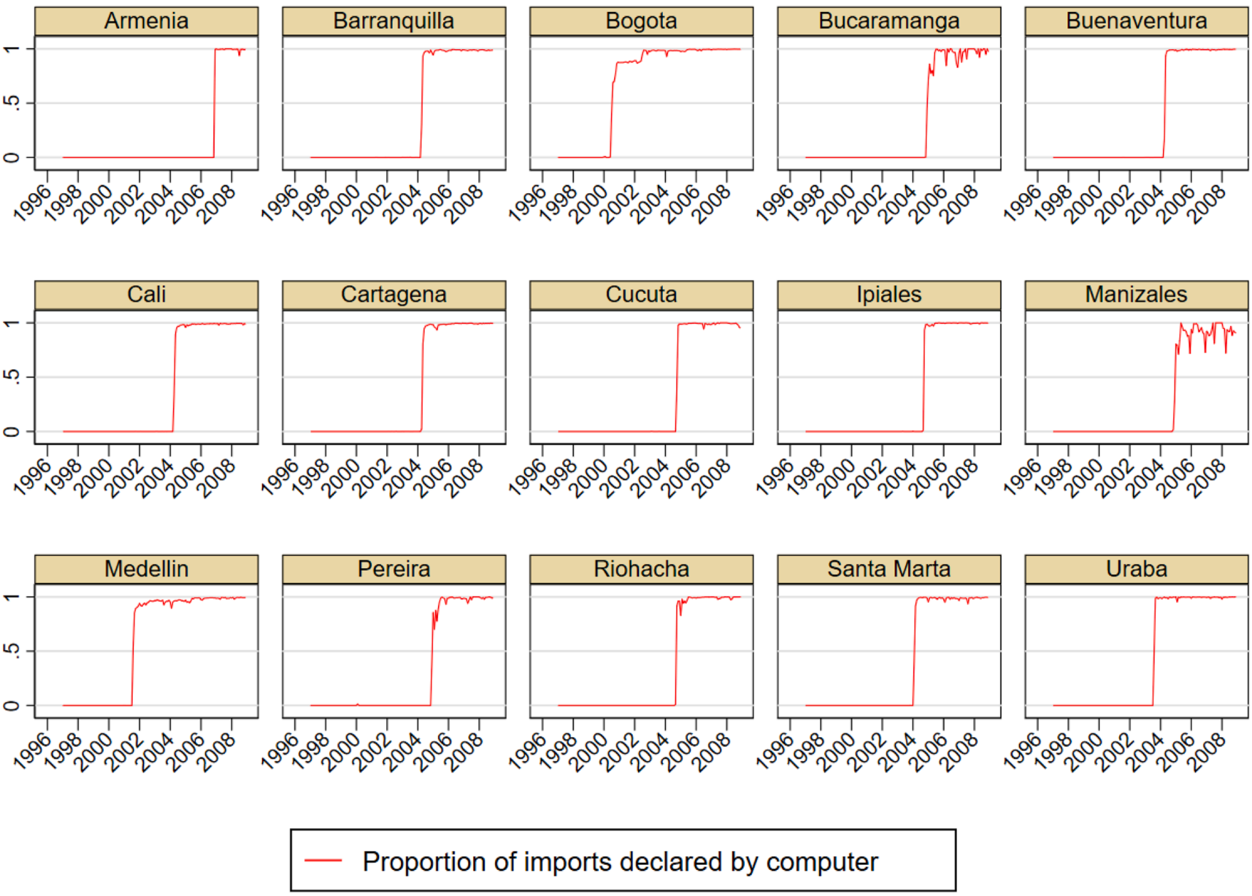


**Fig. 1.** Proportion of imports declared by computer for the 15 largest ports. The figure presents the proportion of import transactions in Colombia that were declared by computer by port. Using data from DIAN (Dirección de Impuestos y Aduanas Nacionales of Colombia) from 1994 to 2014, for each month, we calculated for each port, the monthly average across all transactions of a dummy equal to one if the transaction was declared by computer. The data comprise approximately 1 million observations per year.

uncover the effects of the reform on the volume and value of transactions:

$$Y_{ct} = \propto + \delta T_{ct} + \lambda X_{ct} + \theta_c + \gamma_t + \varepsilon_{pct} \tag{1}$$

where $T_{ct}$ is equal to one if over 80% of total imports going through port $c$ during year $t$ were declared by computer; $Y_{ct}$ is the outcome variable of interest measured at port $c$ at time t; $\theta_c$ are customs-level dummies; and $X_{ct}$ is the value of transactions in the associated port in 1999 (before the reform) interacted with year dummies to control for eventual changes over time that vary by the initial size of customs. $\delta$ is the difference-in-differences estimator of the effect of computerization. Standard errors are clustered at the customs level.

Table 1 reports the double difference estimations of the effects of the reform, corresponding to coefficient $\delta$ in Eq. (1). Each column presents results for a different outcome variable. The first two columns show that the reform was followed by drastic increases in the number of transactions and in the total FOB value of imports at reformed ports compared to unreformed ports. Compared to the pre-reform mean value of the corresponding variable, the estimated coefficients indicate increases of approximately 67% and 70%, respectively. These large estimated increases are consistent with the fact that the total value of Colombian imports nearly doubled between 2000 and 2008 (Appendix A). Revenue collection increased by 72% vs. the pre-reform mean value, hence slightly more than proportionally with respect to the value of imports. The increase in tax revenue points to one main advantage of trade facilitation via reforms on the administrative margin compared to trade liberalizations: the potential to increase trade without sacrificing tariff collections.

The increase in registered imports at reformed ports, relative to unreformed ones, may be due to a combination of the following three channels: (1) an increase in the value of actual imports at reformed ports; (2) a redirection of imports from untreated ports to those already computerized (i.e., a combination of increased imports in reformed ports with reduced imports at unreformed ones); and (3) a reduction in smuggling and underdeclaration at treated ports. Any of these scenarios indicates successful computerization, either because of improved tax collection or the facilitation of imports. The following set of results dig further to better distinguish between these three possible impact channels.

Table 1 also shows that total transportation costs increased by 113% of its mean 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% of its mean (Column 5). An increase in per-transaction transportation costs to reach treated ports suggests a revealed preference for more distant ports that underwent computerization. This apparent redirection toward reformed ports is reminiscent of the findings of Sequeira and Djankov (2014) that firms in Southern Africa are willing to increase travel costs to travel through a less corrupt port. This choice suggests that part of the relative increase in imports at reformed ports reflects redirection from unreformed ports.

We now run an event study specification to check the parallel trend assumption and assess the dynamics of the estimated effects. In particular, we run the following regression:

**Table 1**
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/Value of imports |
|---|---|---|---|---|---|
| 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] | 0.019 | 0.022 | 0.036 | 0.045 | 0.012 |
| Mean of outcome var. | 47,239 | 1,276 | 31.4 | 9.54 | 0.0076 |

This table reports estimation results from estimating Equation (1). 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 with the plant. The dependent variable in Column 1 is the number of transactions that involved 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 the imported value, and observations are weighted by the initial value of total imports that entered the country via the corresponding port. The monetary values are expressed in billions of pesos per year and are deflated by PPI (1988). Controls include customs port and year fixed effects and initial size of the customs port interacted with year dummies.
Standard errors clustered at the port level are in parenthesis.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
[1] Wild bootstrap p value of the coefficient "Reform at port", with errors clustered at the customs level (9,999 repetitions), including a correction for small number of clusters.

$$Y_{ct} = \propto + \sum_{\substack{y=-7 \\ y \neq 0}}^{y=6} \left[\delta_y T_{ct6}\right] + \theta_c \lambda X_{ct} + \gamma_t + \varepsilon_{ct} \tag{2}$$

The dynamic treatment variables $T_{cty}$ are leads or lags of the treatment variable. Hence, $\delta_y$ assesses the effect of computerization on $Y_{ct}$ after $y$ years when $y$ is positive and is used to check the parallel trend when $y$ is negative. The two exceptions are year $T_{ct-7}$, which we define as a dummy equal to one for any year that is seven years or more prior to the baseline year for port $c$, and $T^p_{ct6}$, which is equal to one if customs $c$ at time $t$ had been computerized for six years or more. They are included to ensure that the only omitted year is $y = 0$, the period before the treatment starts. Hence, all coefficients $\beta_y$ can be interpreted as the double difference effect of the year $y$ of treatment compared with the baseline value. Again, standard errors are clustered at the customs level.

Fig. 2 displays the leads and lags estimated coefficients ($\delta_y$ in Equation (2). None of the coefficients corresponding to pre-reform periods are significant. This finding implies that there is no significant difference in the trend of treated versus nontreated ports before each treatment starts shows that the assumption of parallel trends prior to the reform holds for import values and tax collection. The figure also shows that the impacts of the reform tend to grow progressively over time. In particular, the positive effects are only marginally significant in the first and second year after the reform and grow progressively in size and significance for subsequent years. For instance, the average estimated effect on the value of imports of 895 billion pesos (on tax collections of 22.6 billion pesos), reaches a magnitude above 2000 (40) by year five after the reform. In summary, and as expected from the qualitative review of Section 2, Fig. 2 shows that the effects of the reform on the number of imports and their total value increased gradually. Appendix C presents and discusses the results of the event study for additional outcome variables, confirming parallel trends prior to the beginning of treatments and a similarly progressive growth in impacts for those variables. Additionally, as a robustness check in Appendix D, we apply the methodology of De Chaisemartin and d'Haultfoeuille (2020) to allow for a heterogeneous treatment effect across years and find that the results remain consistent.

### 3.3. Effects of the reform on declared and undeclared imports

The previous section provides evidence that computerization increased declared imports and tax collection, raising the question of whether this increase results from an actual increase in transactions or an improved declaration of imports that would have been underdeclared in absence of the reform. To disentangle these two channels, we examine the UN COMTRADE data, which includes Colombian imports as reported in Colombia and in their countries of origin, for a given product defined at the HS4 level, in a given year, and from a given country of origin. These data allow us to estimate the value of import transactions reported by the exporter and that reported by the importer. If properly reported, the two sides of the report should differ in terms of transport costs only, which are included only in the importer's declaration. Import values as reported by importers should thus be larger than those reported by exporters at their origin. However, exporters have little incentive to underreport, whereas importers may underreport to lower their tax payments at the border. A negative difference between the declarations made by the importer and the exporter indicates smuggling or other forms of tax evasion (Fisman and Wei, 2004).[7] Using the same type of data, Kellenberg and Levinson (2019) show that less corruption, stronger auditing, and stronger accounting standards all reduce underreporting of trade transactions.

The UN COMTRADE data do not identify the port through which the merchandize goes. However, we can define a level of treatment by product by imputing, for each HS4 code, the probability that imports of that group passed through a reformed port in each year. To calculate that treatment variable, we multiply the share of the HS4 imports passing through a port (from DIAN data in 1999) times the treatment dummy for this port in the corresponding year. We then run a regression of the inverse hyperbolic sine transformation (IHST) of the value of imports reported by different sides of the transaction on this treatment variable. The IHST values can be interpreted in the same way as a logarithmic transformation but have the advantage of being more robust to zero and near zero values.[8]

Table 2 displays estimated coefficients from running regressions of the value of imports of a certain product from a certain origin, as reported in Colombia or the origin country, on the probability that the product enters the country through a port that has been computerized, controlling for fixed effects for every combination of product category and country of origin, as well as year fixed effects. Each coefficient can be interpreted as the change in the outcome variable that occurs when the probability of a product passing through a reformed port changes from zero to 100% probability. As displayed in Column 1, such a change in status leads to a 12 log point increase in imports declared by exporters

[7] Smuggling refers to transactions that are not declared, with no tax paid on these transactions. Other forms of tax evasion include underdeclaration of amounts or quantities, or miscategorization of goods.

[8] In this setting, the results are nearly identical with the log and IHST, given that there are no zero values by construction because product categories that are not traded were not included, and the values of imports are rarely near zero.

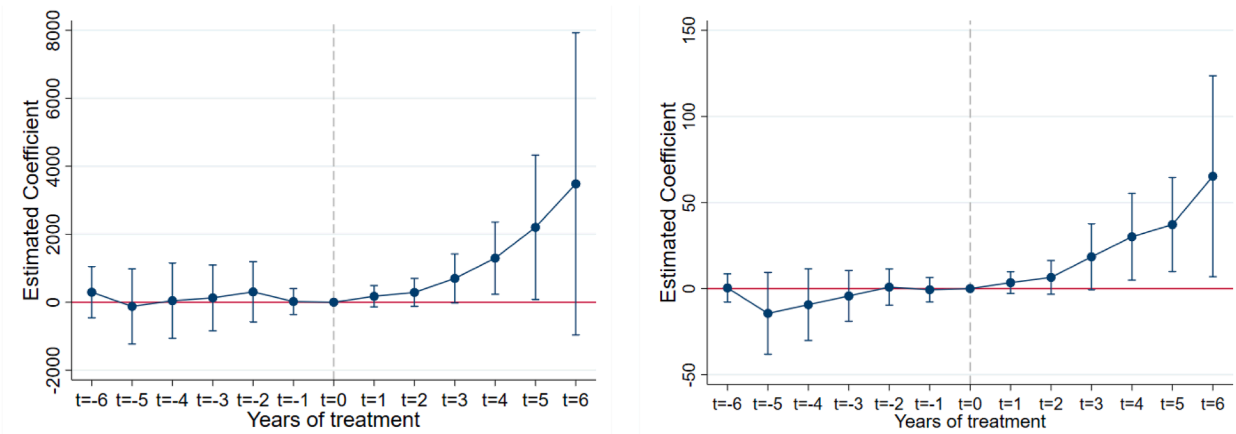


**Fig. 2.** Event study of the effects of the reform on port-level outcomes. The figure displays the coefficients and their 95% confidence intervals, obtained when running Eq. (2) with the outcome variables Import value (Free on Board) and Duties. The data include 286 port year-level observations. In Appendix C, we present and discuss the values obtained in the corresponding estimations for all outcome variables.

**Table 2**
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[1] | Log value of imports as declared in Colombia[1] | Import Capture Ratio = Value declared in (Colombia/Origin) |
|---|---|---|---|
| Probability that the product passes through reformed port | 0.12*** (0.029) | 0.20*** (0.028) | 0.024*** (0.0062) |
| Observations | 35,190 | 35,190 | 35,190 |
| Wild Bootstrap p value[2] | 0.0002 | 0.0000 | 0.0002 |
| Mean (in level, not log) | 11,229 | 12,481 | 0.814 |

Each observation corresponds to a product category (HS4) and country of origin. The table includes all product-origin combinations with at least one transaction between 1998 and 2008. Controls include fixed effects for every combination of product category and country of origin as well as year fixed effects. The “probability that the product passes through a reformed port” is a weighted average of the port treatment variable for the corresponding year, where the weights reflect the likelihood that the inputs used by the firm pass through the corresponding port in 1999.
The Import Capture Ratio in the last column is the ratio of the value of imports as declared in Colombia divided by the value of imports declared in the country of origin. When the value is below 1, lower values are interpreted as more missing declarations in Colombia.
Robust standard errors are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
[1] The log values of the explained variables in Columns 1 and 2 correspond to an inverse hyperbolic sine transformation (IHST(x) = log(x + sqrt(x^2 + 1)), which has been shown to be more robust to zeros and near zero values than log transformation. 2 Wild bootstrap p value of the coefficient “Plant’s inputs expected treatment”, with errors clustered at the type of good level (9,999 repetitions).

in the countries of origin (approximately 11 percent). Because exports of other countries are typically not subject to underreporting, we interpret this coefficient as the true increase in imports by Colombia when a product goes from not treated to fully treated. The differential impact on imports reported by importers vs. exporters is consistent with a reduction in underreporting. Furthermore, as shown in Column 2, a change from zero to one probability of passing through a computerized port also increased imports declared in Colombia by 20 log points (approximately 18 percent). Taking the ratio of coefficients in Columns (1) vs. (2), we conclude that approximately 60% of the increase in the declarations of imports is due to an actual increase in imports, while the remaining 40% results from reduced underdeclarations. The last column directly tests the effect of the reform on the import capture ratio, given by the 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)). We find that the reform led to a 0.024 increase in this index.[9] To interpret the magnitude of this effect, 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. Hence, the 0.024 increase in the import capture ratio implies a 12.9% reduction in underreporting.

Taken together, our results point toward the three previously mentioned mechanisms for increased registered imports in reformed customs, all occurring simultaneously. This subsection shows that approximately 60% of the increase in imports declared in Colombia result from an actual increase in imports, while the remaining 40% results from a decrease in underreporting. The prior subsection showed that part of this increase is due to redirected imports among customs, revealing a preference for treated customs and pointing at the fact that part of the large relative increase in imports in reforms vs. unreformed ports is due to reduced imports at the latter. The difference in the estimated effect of the reform on reported imports between Table 1 and Table 2 highlights the role of the redirection of trade between ports of Colombia in explaining a fraction of the increase in reported imports in reformed vs. unreformed ports. Table 1 shows a 70% increase in reformed ports vs. unreformed ones, while Table 2 estimates a 20% total increase in declared imports when they pass through a reformed port with probability one versus with probability zero. Although the 2 regressions are not fully comparable, the large difference between the two effects indicates that the redirection of trade between Colombian customs appears to play a major role.

## 4. Effects on plant-level economic outcomes

The documented increase in imports suggests that producers may experience other changes. We now investigate those potential effects using data at the manufacturing establishment level.

[9] 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 those from exporters. In calculating the import capture rate, a maximum value of one is imposed for the case when reports by importers exceed those by exporters.

### 4.1. Data and variable definitions

Most of our outcome variables for manufacturing plants come from the Annual Manufacturing Survey (EAM) provided by the official Colombian statistical agency (DANE), rich yearly panel dataset on approximately 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 (approximately USD 150,000 in 2017). We restrict the data to the balanced panel of plants for 1998 to 2008, leaving 3,651 plants.[10] The EAM data contain annual information on value added, sales, inputs, labor, capital and other standard indicators of economic activity of the plants. Based on these indicators, we also compute measures of productivity, in particular, sales per worker and (revenue) total factor productivity. Appendix A provides a description of the variables from the EAM data. A plant identifier allows following plants over time, and a firm identifier links each plant with the firm that owns it. We work with data at the plant (rather than firm) level because one of our producer-level treatment definitions is based on the location of the plant and the port most used for producers at that location.[11] We also use uniquely rich EAM data listing all inputs used by a plant at a level of aggregation close to HS6 to create one of our definitions of treatment at the plant level based on the port through which a plant's inputs are usually imported.

We use firm identifiers in the EAM database to merge it with the DIAN database.[12] This step 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.

We determine the extent to which a plant is treated based on the extent of the connection of a plant to a given port, using two different criteria to connect plants to ports. Our definitions rely on a driver of the decision to import through a given port: geography and the usual origin of a plant's inputs.

Our first treatment variable is the "reform at the geographically assigned port", 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 with the plant. The port associated with the plant is the port representing the highest share of imports by all producers in the plant's municipality in 1999.[13]This variable can be interpreted as the most likely entry port given the geographical location of the plant. To minimize endogeneity, the rule of assignment uses imports prior to the reform, 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 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 pass through each port in 1999. Thus, we use the composition of inputs of each plant to determine the expected share of inputs that is likely to pass 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:

$$T_{pt} = \sum_c [W_{pc99} * T_{ct}]$$

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

$$W_{pc99} = \sum_i [S_{pi99} * s_{ic99}]$$

where $S_{pi99}$ is the share that plant $p$'s expenditure in input $i$ represents out of $p$'s total expenditure in material inputs in 1999, and $S_{ic99}$ is the share represented by imports of $i$ through port $c$ out of total national imports of $i$ in 1999. This treatment index takes a value of 0 for a plant for which imports of its 1999 inputs went entirely through ports that by year $t$ were not yet computerized. It varies continuously from this extreme to 1, which is the case for a plant for which imports of its 1999 inputs went entirely through ports that started being treated before or during the year $t$.

The two treatment variables capture different channels for the effect of computerization and thus shed light on different mechanisms. The "reform at the geographically assigned port" captures the effect of facilitating imports for producers in municipality c. This situation may positively affect plant $p$ by making its inputs easier to access but may also face p with fiercer competition by other producers in the same location. The positive effect is more likely for importers, while the negative competition effect may hold for all plants. In turn, the "plant's inputs expected treatment" captures mainly the cost-reducing effect of the reform on plants whose inputs are traditionally imported through reformed ports, either by the plant directly or by other importers who then sell those inputs to plants.

### 4.2. Methodology

For plant-level data, we implement a triple-difference analysis that estimates the differential effect of the reform on plant outcomes between importers, directly affected by import facilitation, and nonimporters, indirectly affected. We define a plant as importing if the firm to which the plant belongs carried out any import transaction between 1997 and 1999 (i.e., prereform). Our estimations aim to determine three sets of effects of the computerization of customs on firms' economic activity: (1) effects on importing firms, (2) effects on nonimporting firms and (3) the difference between the first two sets of effects.

Our main regression can be written as follows:

$$Y_{pt} = \beta T_{pt} * I_p + \delta T_{pt} + X_{pt}\lambda + \theta_p + \gamma_t + \varepsilon_{pt} \quad (3)$$

where $Y_{pt}$ is the outcome of interest at plant $p$ in year $t$. $T_{pt}$ indicates whether plant $p$ at year $t$ is treated, using one of the treatment variables defined above: the "reform at the geographically assigned port" dummy or the continuous "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 has started. $\theta_p$ are plant-level fixed effects, and $\gamma_t$ are year fixed effects. $X_{pt}$ is a set of additional controls, composed of the log of the plant's value added in 1999 interacted with year dummies. In the case of the "reform at the geographically assigned port" specification, this vector of controls also includes the value of transactions in the associated port in 1999 interacted with year dummies. These controls purge our coefficient of interest from the fact that initial differences in port and firm characteristics may drive differences in changes over time. We do not control for $I_p$ because it is already captured by the plant dummies. $\varepsilon_{pt}$ is

[10] Restricting the initial sample avoids selection biases in the estimated effects due to dropout. Additionally, for this reason, our results do not examine effects stemming from changes in plant entry or exit, but rather, within plant's changes among plants that remain active throughout the study period.

[11] Over 90% of firms are uni-plant.

[12] The Colombian statistical office DANE established protocols to make this merge possible under their strict confidentiality rules and only for the period up to 2008.

[13] More precisely, 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, using imports transactions data. 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.

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 serial correlation within clusters and over time can cause dramatically high rejection rates. Hence, we also present the wild bootstrap p values of the $\beta$ coefficients, following Cameron et al. (2008), who showed that this strategy addresses serial correlation even when the number of clusters is small.

Regression (3) identifies three effects of interest, including the treatment effects on importing firms and the treatment effect on nonimporting firms (through double differences), and the difference between these two effects, through a triple difference that tests the hypothesis of positive effects on importers relative to nonimporters. In particular, $\delta$ estimates the double difference effect on nonimporters, i.e., the change in outcome $Y_{pt}$ that occurred among nonimporting plants when imports became computerized in the corresponding port relative to nonimporters associated with nonreformed ports:

$$\delta = (\Delta Y_{pt} \mid \Delta T_{pt} = 1, I_p = 0, \Delta X_{pt}) - (\Delta Y_{pt} \mid \Delta T_{pt} = 0, I_p = 0, \Delta X_{pt}) \tag{4}$$

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

$$\delta + \beta = (\Delta Y_{pt} \mid \Delta T_{pt} = 1, I_p = 1, \Delta X_{pt}) - (\Delta Y_{pt} \mid \Delta T_{pt} = 0, I_p = 1, \Delta X_{pt}) \tag{5}$$

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

The identifying assumption for the $\delta + \beta$ coefficients ($\delta$ coefficient) is that, in the absence of the reform, there would be no systematically different trend between the outcomes of the importing (nonimporting) plants associated with reformed ports and those that are not. The identifying assumption for the triple difference $\beta$ is that the difference between the trend of importing and nonimporting firms in the 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 reform sequencing was prioritized in geographic areas where businesses had greater potential and expected growth. It would fail for the triple difference if importing plants at reformed ports had greater growth potential than nonimporters at the same ports. Even though the early reformers were the larger ports, where plants may have been more productive or larger to start with, there is no reason to expect that the associated plants were evolving at a systematically different pace compared with other ports. This situation becomes even less likely in the case of the triple difference coefficient.

Being classified as an importer or nonimporter does not fully coincide with actually being an importer or nonimporter during the entire study period. Firms that imported in the prereform period are on average 80% likely to import during any of the following years of the study, whereas prereform nonimporters have an 8% probability of importing during the same period, in which case they would benefit directly from the reform. Hence, $\beta$ can be interpreted as an intention to treat coefficient for the effect of computerization on importers vs. nonimporters, with a 72% compliance rate.

Finally, the estimation strategy does not require that nonimporters are not affected by the reform, i.e., nonimporters 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.

### 4.3. Effects on the economic activity of manufacturing plants

The results from estimating Equation (3) are presented in Table 3. The results defining treatment through the "reform at plant's assigned port" dummy and the "plant inputs expected treatment" are shown in Panels A and B, respectively. In both panels, the first line shows coefficient $\beta$, the second line displays $\delta$, and the last row presents the p value of the sum of the two coefficients. 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.

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 in input expenditures by importers compared to nonimporters at reformed ports of approximately 6.4 log points. This triple-difference increase in input use is also reflected in sales and value added, with effects of 9.1 log points and 14 log points, respectively, for importers compared to nonimporters in reformed ports. After adding coefficients $\beta$ and $\delta$ (the double difference on the importers), the value added of importing plants increased by a significant 6.7 log points. 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 increase significantly. Additionally, consistent with increased size and productivity, the likelihood of exporting increases for importers. Clearly, the reform facilitated imports and benefited importing plants, which were most likely to benefit from computerized customs.

The large effects for importers vis-a-vis nonimporters in reformed ports partly reflect an estimated negative effect on treated nonimporters (compared with nonimporters associated with nonreformed ports) of approximately 7.3 log points in the case of value added. This result indicates that the indirect benefit due to the reduced cost of inputs and the availability of higher quality inputs is more than offset by the direct negative competition effect. Nonimporters close to reformed ports are plausibly most affected by the import-competition channel to the extent that they do not *directly* benefit from importing cheaper inputs. The fact that they face increased competition may explain why they are forced to decrease their use of inputs and their revenue. Competition may also lead to price reductions, possibly explaining the negative revenue TFP effect that we observe for this group. In Appendix D, we show that the results remain consistent when we allow for heterogeneous effects depending on the year of the beginning of the treatment, following De Chaisemartin and d'Haultfoeuille (2020).

In Panel B, where treatment is the continuous "plant's inputs expected treatment" variable, the results are similar to those of Panel A for the triple difference effect $\beta$, which is significant for all outcomes, except investments in capital (first line of coefficients). According to these results, being fully treated increases the log of value added of importing firms by 19 log points compared to nonimporting firms. The order of magnitude appears to be higher than in Panel B. However, because this treatment is continuous, coefficients in Panel B should be interpreted as the effect of a switch from 0% to 100% of inputs passing through a port that is treated. This treatment is more intense than that in Panel A. Moreover, the treatment in Panel B captures mainly the positive cost-reduction effect on plant $p$ rather than the increased competition effect, which could benefit even plants that do not import their inputs directly if the cost reduction is passed-through by traders of the imported inputs.

Consistent with this focus on the positive cost reduction effect in Panel B, the results of the double difference on nonimporters stand in sharp contrast to those of Panel A: Panel B shows that inputs, imports and sales increased significantly for nonimporters in treated ports vs. those in untreated ports. Only the TFP double difference maintains the same sign as the one 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).

**Table 3**
Effects of the reform on the activity of manufacturing plants.

| **3.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 the geographically 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 the geographically 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] | 0.041 | 0.025 | 0.057 | 0.088 | 0.010 | 0.006 | 0.045 | 0.072 | 0.076 |
| 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 |
| **3.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 Eq. (3). Each observation corresponds to a plant and year. "Reform at the geographically 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 with the plant. The port associated with the plant is the port representing the 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 pass 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 (0 values are highly uncommon for these variables and become missing observations when it is the case). 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 the port level are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. 1 Wild bootstrap p value of the coefficient "Importer plant * Reform at plant's assigned port", with errors clustered at the 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.

The results of Table 3 suggest that the pro-competitive effect of the reform dominates among nonimporting firms that are geographically close to reformed ports and negatively affects their economic activity (Panel A). They also indicate that a plant that uses inputs made more accessible by the reform is more likely to benefit from the cost-reducing effect, even if it does not directly import those inputs (Panel B).[14]

### *4.4. Parallel trends and dynamic effects of computerization on Plants' economic activity*

We extend the analysis to an event study to check that the required parallel trend hypothesis holds during the years that preceded the treatment and to shed light on the dynamic effects of the treatment by presenting year-by-year changes in the outcome variables following the implementation of the reform. This 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:

$$Y_{pt} = \propto + \sum_{\substack{y=-6 \\ y \neq 0}}^{y=7} \left[\beta_y T^p_{cty} * I_p + \delta_y T^p_{cty}\right] + \lambda X_{pt} + \theta_t + \gamma_t + \varepsilon_{pt} \tag{6}$$

where $y$ indicates the number of years after the reform if positive or before the reform if negative, with $y = 0$ representing the last year before the beginning of the reform. We refer to t when $y = 0$ is the baseline year (last year before the computerization). Hence, if $y$ is positive, $T^p_{cty}$ is a dummy equal to 1 if port $c$ associated with plant $p$ at time $t$ had been using computers for exactly $y$ years. If $y$ is negative, $T^p_{cty}$ is a dummy equal to 1 if customs $c$ at time $t$ is $|y|$ years before baseline. Again, the two exceptions are year $T^p_{ct-6}$, equal to one for any $y \leq 6$, and $T^p_{ct7}$, which is equal to one if $y \geq 7$, to ensure that the only omitted year is the baseline year for that customs port.[15] Hence, $\beta_y$ represents our estimation of the difference in the change in $Y_{pt}$ between importing plants and nonimporting plants at reformed ports after $y$ years of computerization of the assigned port.

Fig. 3 presents the results for $\beta_y$, the triple difference effect of the reform on importers compared with nonimporters.[16] The coefficients on the left of $t = 0$ display the estimated coefficients and confidence intervals of the leads, as presented in Equation (6). We find that none of the pre-trend coefficients is significant at the 10% level, despite substantial variation,[17] which confirms that the parallel trend hypothesis generally holds in this context. As the estimated coefficients displayed on the right side of $t = 0$ are the lags, their increasing trend indicates that the effects of the reform on importers relative to exporters tend to increase over time. From an increase in the plant's value added of 7 log points in the first year, the estimated increase grows to 15 log points by year 6 after the reform started. This finding is consistent with our expectation given that 1) the port should adapt to the new technology

[14] We refrain from inferring aggregate effects from these results since the nature of the exercise allows us to reach conclusions by comparison with the control group of plants associated with unreformed ports. As shown here and 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 are not confined to the set of plants most exposed to the reform.

[15] Our presentation of the results does not include $\beta_y \leq -6$ and $\beta_y \geq 7$ 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.

[16] The values of $\beta_y$, $\delta_y$ and $\beta_y + \delta_y$ are all presented in Appendix C.

[17] Substantial, though non-significant variations are observed in pre-trend periods. This is plausibly due to the economic instability that hit Colombia in the late 1990′s, which could have affected importing and non-importing firms differentially, but this effect appears to be stabilized before t=0.

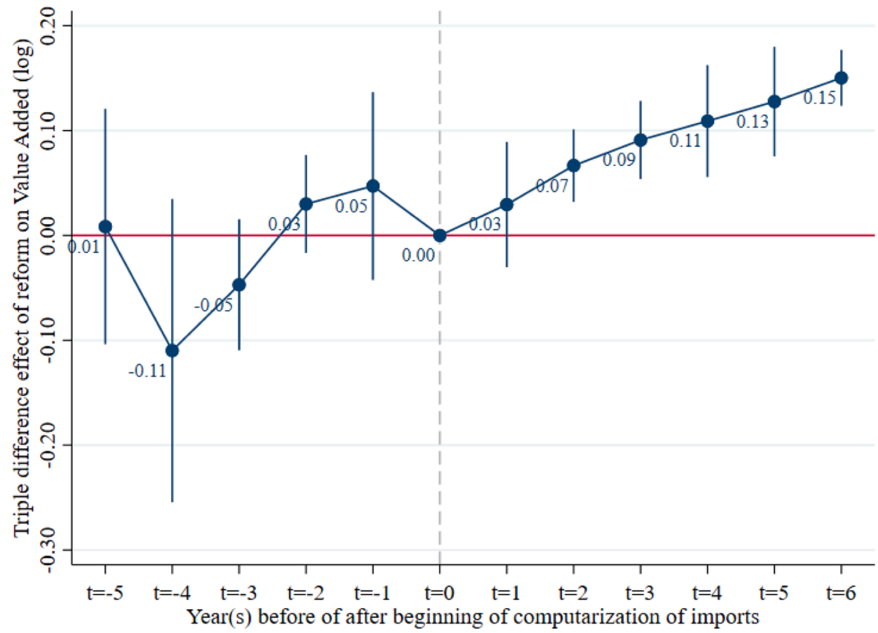


**Fig. 3.** Event study of the triple difference effects on plant value added. The figure provides a visual representation of the triple difference effects on value added before and after the reform. The values that appear in the figures correspond to the leads (to the left of $t = 0$) and lags (to the right of $t = 0$), as displayed in Model (6), with their 95% confidence intervals. They can be interpreted as the effect of the treatment on value added, $t$ years after (or before if negative) the reform was initiated. We control for the Colombian annual GDP per capita growth interacted with the importer dummy to take into account the macroeconomic variability and its heterogeneous effects on importers versus non-importers. The data for this estimation include 39,891 plant year-level observations. Full results for all outcome variables are presented in Appendix C.

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. As shown in Appendix C, these conclusions hold for most outcomes that reflect economic activity and are qualitatively similar in terms of double difference effects on importers.

### 4.5. Heterogeneous effects by plant size

As a final step in our plant-level analysis, we replicate the regression of Model (3), splitting the sample by quartile of value added in 1999. This estimation serves two purposes. First, one could be concerned about comparing importers to nonimporters given that importers tend to be substantially larger than nonimporters. To illustrate this fact, the quartile of the smallest plants has only 21% of plants that are importers, compared with 95% for the fourth quartile. We are also interested in learning about how businesses of different sizes are affected by computerization. The findings are presented in Table 4 by quartile of value added in 1999, from lowest (quartile 1) to highest (quartile 4). These results use the geographic assignment of ports. Appendix E shows and discusses the same estimations applied to the input assignment.

We discuss the magnitudes of the triple difference effect on importers compared with nonimporters ($\beta$), presented in the first row for each quartile. The positive effects on importers (compared to nonimporters) are most frequent in 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 decrease to 9 log points in the third quartile and are not significant among the quartile of largest firms. The effects of the reform on imports, input expenditures, sales and exports are also strongest in the second quartile, while the effects on job creation and TFP are strongest in the

**Table 4**
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 plant*Reform | 0.047 | 0.091 | 0.18*** | 0.084** | 0.060 | 0.094** | 0.059* | 0.040* | 0.036* |
| **(Importer** | at geogr. assigned port | (0.034) | (0.051) | (0.051) | (0.026) | (0.038) | (0.038) | (0.031) | (0.019) | (0.018) |
| **dummy =** | Reform at geogr. | 0.037 | −0.035 | −0.081** | 0.017 | −0.049* | −0.097*** | −0.024 | −0.026*** | −0.0097** |
| **21%)** | assigned port | (0.029) | (0.030) | (0.033) | (0.021) | (0.022) | (0.015) | (0.015) | (0.0073) | (0.0033) |
| **Quartile 2** | Importer plant*Reform | 0.093* | 0.13*** | 0.20*** | 0.076* | −0.0027 | 0.13** | 0.065 | 0.036** | 0.039* |
| **(Importer** | at geogr. assigned port | (0.043) | (0.038) | (0.047) | (0.034) | (0.031) | (0.050) | (0.047) | (0.014) | (0.018) |
| **dummy =** | Reform at geogr. | 0.0035 | −0.028 | −0.038* | −0.0055 | −0.0100 | −0.034 | −0.0040 | −0.00056 | −0.027*** |
| **42%)** | assigned port | (0.029) | (0.028) | (0.020) | (0.045) | (0.049) | (0.034) | (0.024) | (0.010) | (0.0070) |
| **Quartile 3** | Importer plant*Reform | 0.058* | 0.090*** | 0.13** | 0.041 | 0.015 | 0.085** | 0.068 | 0.044* | 0.0070 |
| **(Importer** | at geogr. assigned port | (0.028) | (0.021) | (0.036) | (0.031) | (0.037) | (0.031) | (0.048) | (0.019) | (0.018) |
| **dummy =** | Reform at geogr. | −0.0084 | −0.047 | −0.080 | −0.021 | −0.040 | −0.059 | −0.017 | −0.027 | 0.0033 |
| **75%)** | assigned port | (0.038) | (0.028) | (0.046) | (0.039) | (0.034) | (0.038) | (0.045) | (0.015) | (0.013) |
| **Quartile 4** | Importer plant*Reform | 0.061 | 0.0034 | −0.025 | −0.077 | −0.18** | 0.052 | 0.15** | 0.0076 | −0.012 |
| **(Importer** | at geogr. assigned port | (0.053) | (0.034) | (0.025) | (0.076) | (0.070) | (0.076) | (0.054) | (0.015) | (0.073) |
| **dummy =** | Reform at geogr. | −0.032 | 0.021 | 0.045 | 0.066 | 0.16** | −0.021 | −0.14** | 0.0076 | −0.0030 |
| **95%)** | assigned port | (0.053) | (0.040) | (0.034) | (0.075) | (0.062) | (0.077) | (0.054) | (0.019) | (0.069) |

Table 4 reports estimation results from estimating Equation (3), replicating the results of Table 3A, 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). See the legend of Table 3 for more details on variables and specification.
Standard errors clustered at the port level are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

lowest quartile, closely followed by the second quartile. Capital remains largely unaffected by computerization in all groups.

In standard models of trade with fixed costs of trading and heterogeneous production units, a 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, and the smallest plants were likely too far from the productivity cutoff for importing and exporting to be affected by the reform. Medium-sized plants, meanwhile, were more likely close to being able to engage (or engage more intensively) in international trade only if the costs of doing so were lower. The distribution across quartiles of the positive effect of the reform on importing firms is consistent with this interpretation, as the effect of computerization on the likelihood of exporting and other outcomes is highest among firms in the second quartile. It is also possible that the cost reduction resulting from the reform varies by plant size. This situation 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.

## 5. Further evidence of the effects on efficiency and corruption at ports

### 5.1. Effect on tax payments and time at customs

This subsection analyzes the effects of the reform on plants' payments of customs fees and time to clear customs. Since information on import transactions applies only for importing plants, comparison with nonimporters is no longer possible. Thus, we estimate a double difference version of Equation (3) for outcomes from the DIAN database on import transactions. In particular, we estimate:

$$Y_{pt} = \propto + \rho T_{pt} + \lambda X_{pt} + \theta_p + \gamma_t + \varepsilon_{pt} \tag{7}$$

where the notation is identical to that of Equation (3). The coefficient $\rho$ is the double difference effect of the reform on plants at computerized ports compared with those at noncomputerized ports.[18] Appendix C shows that the required prereform parallel trend between both groups holds.

Table 5 reports the equation (7)'s estimated $\rho$ coefficient. As mentioned in Section 2, misclassification of products and nonpayment of due taxes were common concerns at the DIAN before the reform. If the computerization succeeded at reducing misclassification, then it should increase the effective tax rate, which is what we find in Column 1 of Table 5. The ratio of all taxes collected divided by declared imports increased by 0.005, which represents 18% of the average effective tax rate. While we cannot fully rule out that this effect may be driven by a shift in the composition of imports toward goods with higher tax rates, we do not find compelling evidence that the reform would have induced a systematic shift toward such imports.

The second column of Table 5 presents the effect on average time to clear customs (averaged across a plant's transactions), a standard indicator of convenience and (absence of) bureaucracy. 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, which is puzzling given our initial expectation that faster service would be a mechanism of the large positive effects on firms' economic activity documented in this paper. This nonresult implies that the program triggered other forms of facilitation, such as reduced bureaucratic costs (i.e., the need to physically go to the customs multiple times) or reduced costs related to corruption (i.e., paying bribes, uncertainty or moral cost). While we can provide only limited evidence of these mechanisms, they are consistent with evidence that plants are redirecting imports from nontreated customs to treated customs, which (1) shows a revealed preference for treated customs (even though it increases transportation costs) and (2) may have increased delays in treated customs with respect to nontreated customs. Thus, this redirection of trade toward treated customs provides a possible explanation for why, in equilibrium, we do not see a large and significant effect in time to clear customs. Additionally, during the study period, the average time to clear customs across Colombian ports decreased from approximately 15 to approximately 12 days, which is consistent with a scenario where the reduction in time occurred together with a reallocation toward reformed customs.[19]

**Table 5**
Double difference estimation of the effects of the reform on characteristics of import transactions by importing plants (observations at plant- year level).

| VARIABLES | Taxes Paid/ Import Value | Customs Clearance Time | Taxes paid/ Taxes due | Sanctions/ Import value |
|---|---|---|---|---|
| Reform at plant's assigned port | 0.0050*** | −0.45 | 0.059** | 0.000021*** |
| | (0.00059) | (0.30) | (0.021) | (5.46e-06) |
| Observations | 18,622 | 18,635 | 18,414 | 18,622 |
| Wild Bootstrap p value[1] | 0.016 | 0.273 | 0.168 | 0.156 |
| Mean of outcome var. | 0.028 | 13.6 | 0.86 | 0.000029 |

Table 5 reports estimation results from estimating Equation (7). 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 are 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 with the plant. The port associated with 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, the initial log of value added of the plant interacted with year dummies and the initial log of the customs port size interacted with year dummies. Observations are weighted by the value added of the plant in 1999, before any reform started. Standard errors clustered at the port level are in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. 1 Wild bootstrap p value of the coefficient "Reform at plant's assigned port", with errors clustered at the customs level (9,999 repetitions).

We also investigate discrepancies between taxes paid and taxes due in Column 3. The reform was expected to improve communication with the bank and ensure that the payment was made before releasing merchandise, one of the objectives that motivated the reform according to the DIAN. The outcome variable is the ratio of the tariff that was actually paid by the plant to the tariff due (after potential deductions). Computerization increased this ratio by 5.9 percentage points, thus reducing the nonpayment of due taxes by more than a third. Finally, Column 4 shows a significant increase in sanctions after the reform, which may reveal a switch from informal bribes to formal sanctions (though this result appears to be small in magnitude and frequency). The last two results are not robust to the use of the wild-bootstrap p value, implying that they may be spurious or driven by a small number of clusters.

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 presented in Table 6, splitting the sample by quartiles of 1999 value added, as in Table 4. Interestingly, the increased effective tax

[18] Since information in the DIAN database is reported at the firm rather than the plant level, plants are assigned outcomes corresponding to their owning firms. They are still matched to ports based on the plant's location, relying on our baseline definition of $T_{pt}$ in the geographic specification. Over 90% of plants in the manufacturing survey belong to single-plant firms, so the firm-plant distinction is not crucial in this context.

[19] The time to clear customs in Colombia is reported in Appendix A.

**Table 6**
Effect of computerization on characteristics of import transactions by importing plants, separating plants by quartile of value added in 1999.

| Quartile (of VA in 1999) | VARIABLE: | Customs Clearance Time | Taxes Paid/Import Value | Taxes paid/Taxes due | Sanctions/Import value |
|---|---|---|---|---|---|
| Quartile 1(Exposure = 21%) | Reform at plant's assigned port | 0.024 | −0.0021 | 0.049 | 0.000020*** |
| | | (0.79) | (0.0025) | (0.030) | (4.84e-06) |
| | Mean of outcome var | 12.6 | 0.024 | 0.96 | 0.000020 |
| Quartile 2(Exposure = 42%) | Reform at plant's assigned port | −0.48 | 0.0025 | −0.000083 | 0.000035 |
| | | (0.58) | (0.0030) | (0.013) | (0.000026) |
| | Mean of outcome var | 13.0 | 0.025 | 0.97 | 0.000028 |
| Quartile 3(Exposure = 75%) | Reform at plant's assigned port | 0.23 | 0.011*** | 0.014 | 0.000039 |
| | | (0.36) | (0.0018) | (0.015) | (0.000042) |
| | Mean of outcome var | 13.3 | 0.030 | 0.94 | 0.000020 |
| Quartile 4(Exposure = 95%) | Reform at plant's assigned port | −0.51 | 0.0042*** | 0.067** | 0.000019** |
| | | (0.31) | (0.00078) | (0.022) | (6.73e-06) |
| | Mean of outcome var | 14.2 | 0.028 | 0.75 | 0.000035 |

This table reports estimation results from estimating Equation (7), thus replicating the results of Table 5 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. The share of exposure in each quartile corresponds to the share of importing firms at the beginning of the study period. For more details about the specification and variables, see the legend of Table 5.
Standard errors clustered at the port level are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

rate is concentrated in the two quartiles of the largest firms. Furthermore, all quartiles display a prereform average ratio of taxes paid over taxes due between 94% and 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 approximately 6.7 percentage points with computerization. These patterns are consistent with large firms being more able than others to deal with costly customs procedures and to collude with customs officials to receive special treatment, especially in the prereform environment. This finding may also partly explain why we observed in Table 2 that the top quartile benefited the least from computerization.

### 5.2. *Predictability of clearance times*

Our previous results suggest that reformed ports are more attractive for importers. Why? Having already reported a nonstatistically significant and very minor reduction in average clearance times (Table 5), we now assess whether the reform affected the predictability of clearance times. There are two reasons that we are interested in this particular outcome. First, being able to predict clearance times matters to importers and thus indicates the facilitation of imports. Second, arbitrary delays are used by customs agents to extract bribes from importers. The reform aimed at reducing such practices through the reduction and standardization of interactions between importers and customs agents. If the reform was effective, then this fact should be reflected in an increased predictability of clearance times.

Table 7 reports the R2 of a transaction-level regression of time to clear customs on transaction characteristics observable to the firm, including weight, value, product, and port, as well as firm fixed effects. The regressions include more than a million transactions. We separate the results by year and treatment status of the ports to compare the R2 for treated customs to that for nontreated customs for each year from 2001 to 2004, which are the years with a mixture of treated and nontreated ports. An additional line pools together all years from 2001 to 2004. The R2 is significantly higher for computerized ports than for noncomputerized ports in the same year, with a difference that ranges from 3.3 to 5.6 percentage points. The results could be affected by preexisting differences in the predictability of customs clearance time in customs that were first computerized. However, they suggest a facilitation through better predictability and reduced discretionary power of customs agents.

**Table 7**
Effects of computerization on predictability of time to clear customs and tax rate.

| R-squared of Customs Clearance Time by treatment group | | | | |
|---|---|---|---|---|
| Year | Noncomputerized 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 (the number of observations is close to 1 million per year). For each year, we present the difference between the R2 of treated and nontreated group and the p value of the significance of their difference estimated by bootstrapping (1,000 repetitions).

### 5.3. *Corruption cases at DIAN*

We further investigate whether the reform can be associated with a decrease in the number of 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 concerning other state agencies. The variables are described in Appendix B. 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 number of cases (specified by municipality) is most frequently zero in the analysis. Hence, the results should be interpreted cautiously and as part of a set of evidence that points toward corruption as one of the mechanisms at play rather than as standalone results.[20]

Table 8 shows the results of regressions that follow the double difference specification described in Equation (1). The outcome variables are 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 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). Cases *not* related to

[20] Additionally, 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 and hence reflect authorities' intention 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.

**Table 8**
Estimation of the effects of the reform on investigations related to corruption at DIAN at the port-year level.

| VARIABLES | Number of DIAN Corruption Related Cases | Factor of DIAN Corruption Related Cases |
|---|---|---|
| Reform at port | −0.180** | −0.343** |
| | (0.0735) | (0.166) |
| Observations | 286 | 286 |
| Wild Bootstrap p values | 0.022 | 0.066 |
| Mean of outcome var. | 0.122 | 0 |

This table reports estimation results from estimating Equation (1). 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 nonrelated 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 the port level in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
[1]Wild bootstrap p value of the coefficient "Reform at port", with errors clustered at the customs level (9,999 repetitions), including a correction for small number of clusters.

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 and changes over time in the effectiveness of the judiciary system. Appendix C displays the corresponding event study and concludes that none of the four pretrend coefficients is significant. Table 8 shows 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 an economically and statistically significant 0.34 standard deviation. This estimate is consistent with reduced corruption being concurrent with the reform and helping to explain the observed increase in firm-level activity and tax collection, with the limitations previously discussed for this exercise.

## 6. Conclusion

This paper analyzes the economic consequences of the computerization of import declarations in Colombia, together with the reorganization that it allowed. The results show that import activity and tax collection at customs increased substantially at treated ports because of an increase in both the tax base and the effective tax rate. The analysis of international trade data reveals that the increase in import declarations is due to a mix of an actual increase in imports and a reduction in smuggling and underdeclaration. Our finding that importers are willing to travel longer distances to pass through automatized and less corrupt customs is reminiscent of the results of Sequeira and Djankov (2014), who interpret it as a revealed preference for noncorrupt customs. Time to clear customs did not decrease but became more predictable, which would be expected if customs agents' discretion is reduced. Finally, we find direct evidence of reduced corruption: the reform led to reduced smuggling and a significant drop in the number of corruption investigations related to DIAN officials (though this evidence is only suggestive, given the limited number of DIAN-related corruption cases).

As a result of this trade facilitation, the reform triggered progressive and significant growth in the value added, employment, productivity, and propensity to export of importing producers, with small and medium importing producers benefiting the most from the reform. Our results on importing firms are robust to assigning customs based on proximity or on the plant's composition of inputs. However, the effects on nonimporting plants vary across the two specifications, suggesting that negative competitive pressure is dominant for nonimporting plants located close to a reformed port. However, the improved (indirect) access to imported inputs is dominant for plants that used inputs more likely to pass through a treated port.

A strength of this paper is its effort to triangulate evidence from various databases and approaches, finding that data from surveys of manufacturing firms, customs transactions, international trade statistics and corruption cases all point toward significant and economically large improvements for importing plants when their assigned ports underwent the reform. This study adds to the scarce evidence on the costs of corruption at customs, highlighting the potential of e-government interventions to increase efficiency while limiting interactions prone to corruption. The large impacts we find are in line with previous literature that shows that import taxes can harm firm productivity and growth (Trefler, 2004; Amiti and Konings, 2007; Kugler and Verhoogen, 2009; Halpern et al., 2011; Eslava et al., 2013; Fieler et al., 2018) and with other literature showing that the effects of "corruption tax" can be multiple times larger than those of an equivalent formal tax (Wei, 2000; Svensson, 2003). The computerization had an estimated total cost of approximately nine million dollars, which is dwarfed by its estimated benefits.[21] Interestingly, DIAN engineers reported how challenging it was to obtain funding to support project development. This paper shows that when properly implemented, such investment can have a high return for the economy. This paper adds to growing evidence on the significant benefits of the proper use of information and communication technologies to improve institutions. When a typical tradeoff between regulation and facilitation is typically expected (Djankov et al., 2002), we find that technological progress can lead to simultaneous improvements in both dimensions. Rigorous evidence of successful attempts to tackle corruption in the interaction between government agencies and the business sector is very limited, particularly at the customs level. Hence, it is important to draw lessons from this case.

The conditions that allowed this reform to occur and to be successful remain to be explained further. 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)." Finally, the reasons that an intervention expected to reduce rents from corruption to such an extent overcame the natural opposition of rent-holders remain to be explored.

## Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

## Data availability

The authors do not have permission to share data.

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

## Appendix A. Supplementary material

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jpubeco.2023.104969.

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