

# The Economics of Anti-Corruption: Lessons from a Widespread Customs Reform

By Dean Yang<sup>1</sup>

## Introduction

Corruption is pervasive in developing countries, and is widely considered to be a major barrier to economic development.<sup>2</sup> Yet systematic empirical evidence on the effectiveness of anti-corruption efforts is scarce. The seminal theoretical work of Gary Becker and George Stigler (1974) identified a pair of generic remedies for bureaucratic corruption in government: increased monitoring and higher wages.<sup>3</sup> But for many reasons, anti-corruption reforms may fail in practice. For example, consider a reform that increases monitoring of potentially corrupt officials. Such a reform might fail if the monitors themselves were corrupt and so provide inaccurate information to higher authorities. In addition, higher-level officials may themselves be corrupt and not put the information gathered to good use. The monitoring program may simply be implemented to demonstrate the government's anti-corruption credentials. What is more, even if enforcers are honest, corrupt officials may be able to find alternative methods of continuing their corrupt dealings. Empirical work is therefore necessary to determine the effectiveness of any given anti-corruption effort.

When there is a high probability that lower-level agents monitoring corrupt activity may themselves be corrupt, it may be reasonable for higher authorities to use monitors from outside the government—in particular, private firms. Hiring private firms to monitor potentially corrupt activity may make sense if competition among the private monitors generates incentives for integrity. A widely-recognized example of government-mandated monitoring by private firms is auditing by private accounting firms of the financial statements of publicly-traded companies, an essential foundation of securities regulation.

Can "hiring integrity" from the private sector to collect information for government anti-corruption efforts be effective? This chapter analyzes a reform adopted by the customs services in many developing countries that does just that. Within a developing country government, the customs agency—the organization responsible for taxation of imported goods—is often singled out as having particularly severe problems with bureaucratic

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<sup>2</sup>For recent overviews of the relationship between corruption and development, see Bardhan (1997) and Rose-Ackerman (2004).

<sup>3</sup>More recent contributions along these lines include Besley and McLaren (1993), Mookherjee and Png (1992 and 1995), and Polinsky and Shavell (2001).

corruption. Revenue drains due to customs corruption can have important consequences because customs duties are important for public finances in the developing world. In 1990, customs duties accounted for an average of 23% of central government revenue across developing countries.<sup>4</sup>

In the past two decades, dozens of developing countries have adopted a specific approach to combating corruption in their customs services, with the ultimate goal of raising import duty collections: hiring private firms to conduct preshipment inspection of imports (known as PSI). When a government implements a PSI program, foreign inspectors verify the tariff classification and value of individual incoming shipments before they leave their countries of origin, and forward this information to the client government. Client governments seek to take advantage of the inspection firms' reputation for honesty, essentially "hiring integrity" from private firms to provide objective data on the contents of imported shipments. In nearly all cases, however, the responsibility for collecting customs duties remains in the hands of the importing country's customs officials. PSI reports simply improve the information available to higher-level enforcers, who can use the reports to hold individual customs officers accountable for collecting the correct amount of duties on shipments. In addition, the PSI reports may improve the bargaining power of importers against customs officials seeking bribes; this could facilitate trade and raise the total amount of taxable import activity.

In this chapter I survey my own research on the aggregate, country-level impact of preshipment inspection services worldwide, and also discuss empirical evidence on the microeconomics of PSI's impact within two countries. At the aggregate level, I find that countries implementing PSI programs subsequently experience large increases in the growth rate of import duties. Empirical analysis uses panel data at the country level to examine the relationship between the implementation of PSI programs and import duty collections between 1980 and 2000. After the implementation of PSI programs, import duties increase by 15-30 percentage points on average. Additional evidence suggests that reductions in corruption are the cause of the import duty improvements: PSI programs are accompanied by declines in underinvoicing and in mis-reporting of goods classifications in customs. Preshipment inspection appears to be cost-effective: improvements in import duties in the first five years after program implementation were 2-3 times larger than program costs. I summarize these findings in below; the complete analysis is presented in a separate paper, Yang (2005a).

Although the country-level evidence indicates that preshipment inspection programs are generally effective, success is not guaranteed, and examining situations where PSI failed to produce the desired results can shed further light on the conditions under which such programs are likely to succeed. My microeconomic empirical studies focus on the experience of two countries: the Philippines and Colombia. These within-country analyses find that when the increase in enforcement (enabled by PSI) is only partial—in that it only addresses a subset of potential methods of avoiding import duties—then there can be substantial displacement to alternative methods of avoiding import duties.

The first of these micro-level studies examines efforts to evade PSI in the context of

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<sup>4</sup>The sample used to calculate this statistic is described in Section below.

a preshipment inspection program in the Philippines. In 1990, the Philippine government reformed its PSI program to close a loophole that had previously been exploited by importers seeking to avoid paying import duties. The reform constituted a quasi-experiment because the increased enforcement applied only to shipments from a subset of countries, so that corresponding shipments from all other countries serve as a comparison group. Increased enforcement reduced the targeted method of duty avoidance but led to substantial displacement to an alternative duty-avoidance method (shipping via duty-exempt export processing zones), amounting to 2.7 percent of total imports from treatment countries. The hypothesis that the reform led to zero change in total duty avoidance cannot be rejected. Displacement was greater for products with higher tariff rates and import volumes, consistent with the existence of fixed costs of switching to alternative duty-avoidance methods. I summarize these findings below; the complete analysis is presented in a separate paper, Yang (2005b).

The second within-country study examines the implementation of PSI in Colombia. Here, the measure of the extent of duty avoidance is the 'import capture ratio', that is, the ratio between Colombia's reported imports of a product, and other countries' reported exports to Colombia of the same product.<sup>5</sup> Identification of the impact of enforcement on displacement exploits the fact that PSI was required for only a subset of product groups; other import categories serve as comparison groups. For importers of products requiring PSI, potential methods of duty avoidance included misclassifying their shipments as products not requiring PSI, as well as outright smuggling (avoidance of formal customs channels). Displacement to either duty-avoidance method should lead to lower import capture ratios for displaced products. Import capture ratios for products requiring PSI decline more when the products have higher tariffs, and when enforcement was lower against product misclassification. These findings have not been published elsewhere, and the complete analysis is presented below.

Aside from shedding light on the effectiveness of a widely-used anti-corruption reform in customs, these findings also suggest lessons for anti-corruption efforts more broadly. In PSI programs, foreign inspectors simply provide additional information to higher levels of government while keeping duty collection and enforcement in the hands of government employees. These studies indicate that information is a key constraint facing anti-corruption enforcers, and policies that find innovative ways to alleviate information constraints can have large returns in terms of reducing corruption. In addition, the evidence demonstrates that private firms can successfully be used to generate information for anti-corruption efforts. Finally, the PSI experience in the Philippines and Colombia suggests that to be successful, anti-corruption reforms should be "broad" in the sense of encompassing a wide range of possible alternative methods of committing the illegal activity of interest; otherwise, displacement to alternative methods can negate the original goals of the reform.

This research is part of an emerging empirical literature on the impact of monitoring on bureaucratic corruption worldwide. Rafael Di Tella and Ernesto Schargrodsky (2003) examine the impact of increased enforcement on corruption in

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<sup>5</sup>Studies that use a similar measure include Morgenstern (1950), Bhagwati (1964), Naya and Morgan (1969), De Wulf (1981), and, most recently, Fisman and Wei (2004).

hospital procurement in Argentina. Benjamin Olken (2005) developed field experimental evidence on how different types of monitoring affect corruption in Indonesian road projects. In Uganda, Ritva Reinikka and Jakob Svensson (2004) find that diversion of government funds intended for education is reduced when intended funding levels are publicized in newspapers. In a U.S. private-sector context, Daniel Nagin, James Rebitzer, Seth Sanders, and Lowell Taylor (2002) use a field experiment to document the impact of increased monitoring on opportunistic behavior by telephone call center employees.

This research also relates to research on avoidance of taxes on international trade. Lant Pritchett and Geeta Sethi (1994) find that collected import duties as a share of import value rise less than one-for-one with the tariff rate, and interpret this as evidence of tax evasion or avoidance. Raymond Fisman and Shang-Jin Wei (2004) find that the extent of import underinvoicing rises as the tariff rate rises for Chinese imports from Hong Kong. A number of authors examine tax-induced transfer pricing within multinational firms (for example, Jean-Thomas Bernard and Robert Weiner 1990, James Hines and Eric Rice 1994 and Kimberly Clausing 1998). In the related realm of income tax evasion, Steven Klepper and Daniel Nagin (1989) examine cross-sectional correlates of income underreporting on specific line items of US tax returns, and Joel Slemrod, Marsha Blumenthal, and Charles Christian (2001) examine the impact of closer monitoring of income tax returns on tax payments in a randomized experiment in Minnesota.

The remainder of this chapter is organized as follows. I begin by providing background on preshipment inspection programs worldwide, and discuss the potential positive effects of PSI as well as the potential for unintended negative consequences (in particular, displacement to alternative duty avoidance methods). The next section summarizes the cross-country evidence on the effectiveness of PSI from 1980-2000. I then turn to my detailed analyses of individual countries. First, I outline the microeconomic empirical evidence on displacement in the case of the Philippines, and then turn to detailed evidence on the correlates of displacement of duty avoidance in the Colombian PSI program. The chapter concludes with a discussion of the implications of these findings for anti-corruption efforts more broadly.

## **Background on preshipment inspection**

Corruption in customs takes two generic forms. The first is simply theft of government resources. A corrupt customs bureaucracy may turn over to the government treasury only a fraction of monies collected from importers, simultaneously falsifying import documentation to mask the revenue theft. The second form of corruption is the extraction of bribes from importers. Customs may delay incoming shipments (often under the pretext of problems in import documentation) to extract bribes, potentially discouraging import trade. The net result may be less import duty revenue than would have been collected in the absence of corruption.<sup>6</sup> Countries implement preshipment inspection programs to combat both types of corruption in customs.

A handful of multinational inspection firms—all headquartered in

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<sup>6</sup>However, it is also possible that importers may end up paying less than the legislated tariffs on their imports due to corruption, in which case corruption could *encourage* imports.

Europe—provide PSI services. Implementing a PSI program involves hiring one or more of these firms to inspect incoming shipments, using their established worldwide network of inspection agents. PSI programs are typically initiated and supervised by a country's finance ministry (or occasionally its central bank), often upon the recommendation of multilateral funding institutions. When governments institute PSI programs, importers are required to have their incoming shipments inspected by a certified firm's agents before they leave the country of origin. Importers inform the PSI firm's local office of the pending shipment, and the PSI firm arranges for its own or affiliated agents in the origin country to inspect the shipment before departure.

Shipments are typically inspected at the premises of the exporting firm or at the port of departure. PSI firms assess the tariff classification, quantity, and total value of individual shipments, and send their assessments to the client government. Many programs require that tamper-resistant seals be placed on shipping containers after inspection. In nearly all PSI programs, the PSI firm does not collect the import duties; rather, actual duty collection remains the responsibility of customs officials in the shipment's destination country. Upon the shipment's arrival in the destination country, the client government can use the PSI firm's assessment to identify customs officials who may be complicit in allowing misreporting of shipment contents and underpayment of import duties. PSI contracts specify the specific product categories and types of shipment that are subject to the inspection requirement. Often, shipments below a minimum value threshold (ranging from \$500 to \$5,000) are exempted from PSI. Data on the share of imports for which PSI is required are not generally available, but when it has been reported the percentage is usually in the 80%-90% range (see Vinod Rege 2001).

In return for their services, PSI firms typically charge a fee of about 1% of the value of imports inspected, usually with a minimum charge per shipment in the vicinity of \$250. The client government pays the fee in most PSI programs, but in some countries importers pay the fee. Across all PSI-using countries between 1990 and 2000, estimated PSI fees amounted to an average of 1.3% of central government tax revenues. Total fees paid worldwide to PSI firms were on the order of US\$500 million annually during the same years.<sup>7</sup>

In 1985, Indonesia became the first country to require preshipment inspection of imports for customs purposes. The Philippine program followed soon afterwards, and was active from April 1987 to March 2000. In total, over 50 developing countries have implemented customs PSI programs for some period of time.<sup>8</sup> As of mid-2002, such

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<sup>7</sup>For these fee calculations, I use data from the IMF's Direction of Trade Statistics and a historical database of PSI programs I collected. The estimate of PSI fees paid in year  $t$  by country  $j$  is  $Fees_{jt} = (0.01) \cdot (0.8) \cdot M_{jt}PSIfrac_{jt}$ , where  $M_{jt}$  is the total value of shipments recorded as destined for country  $j$  in year  $t$  by trade partner countries, and  $PSIfrac_{jt}$  is the fraction of year  $t$  that country  $j$  had an active PSI program. I assume that PSI is only required for a fraction 0.8 of imports, and that the PSI fee is a fraction 0.01 of total imports inspected. The annual worldwide total of  $Fees_{jt}$  averages \$547 million per year from 1990-2000.

<sup>8</sup>A small number of countries retain preshipment inspection firms to verify national quality or safety standards, to help enforce foreign exchange restrictions, or for other non-customs purposes.

programs remained active in nearly 40 countries.

## Potential positive and negative effects of PSI

There are various channels through which preshipment inspections can reduce the incentives for customs corruption, and eventually lead to higher import duty collections. First, PSI improves the monitoring ability of higher-level enforcers. It generates an independent source of information that higher levels of government can use to discover and prosecute corrupt practices by customs officers and importers. In the absence of PSI, uncovering corruption in customs requires time-consuming investigative work, and is made particularly difficult by the large number of import transactions. PSI helps investigators identify import transactions where duties calculated from the PSI report diverge substantially from duties actually collected by customs officials, suggesting that investigations should be targeted at such transactions.

Second, the existence of PSI-generated information may encourage imports by reducing importers' costs (in terms of bribes and delays). A primary tactic used by corrupt customs officials to extract bribes from importers is to delay the clearance of shipments from customs, often on the pretext that there is some discrepancy between the importer's customs declaration and the shipment's actual contents. A preshipment inspection generates independent information on the contents of a shipment that could increase an honest importer's bargaining power vis-à-vis a corrupt customs officer, potentially reducing customs clearance times. Patrick Low (1995) and Glenn Jenkins (1992) cite survey evidence that PSI was accompanied by dramatic reductions in customs clearance times in Indonesia.

However, the success of preshipment inspection programs is far from guaranteed. Success requires client governments to use the PSI-generated information to seek out and prosecute corrupt actors. Governments may simply hire PSI firms under pressure from multilateral funding institutions and may not actually use the data generated. Higher-level enforcers who receive the PSI reports may not have the expertise to use the information effectively, or they may themselves be corrupt. It is also possible that customs corruption may be cost-reducing for importers, if importers' bribe-inclusive payments to customs are lower than legally-required duties on shipments. PSI may raise importers' costs, reduce import volumes, and, depending on supply elasticities, ultimately reduce duty collections. Furthermore, importers whose costs are raised by PSI may seek out alternative methods of avoiding import duties.

## International evidence, 1980-2000

How effective has PSI been in helping countries raise their import duty collections? Is there evidence that PSI helps reduce corruption in customs and stimulate trade? I summarize here evidence on the impact of preshipment inspection across many countries over two decades (for details see Yang 2005a).

The most important element of this analysis is information on the existence of PSI programs across countries, and the dates those programs operated. I assembled these

program dates via phone interviews and documentation provided by the four largest multinational firms that offer PSI services, for all programs through the end of the year 2000. These firms are Bureau Veritas, Cotecna, Inchcape Testing Services (ITS), and Societe Generale de Surveillance (SGS). The handful of remaining PSI firms had contracts that entirely overlapped with those of the four largest firms, so that these four firms' contracts provide a complete accounting of past programs.

Because the ultimate goal of preshipment inspection programs is to raise customs revenue, the primary outcome of interest in this analysis is annual import duty collections at the national level. Subsidiary outcomes include total imports and measures of misreporting in customs. These data come from publicly-available sources, including World Development Indicators 2004 and the World Bank's Trade and Production dataset.

The analysis focuses on 19 countries for which data on import duties are available before and after the start of their PSI programs.<sup>9</sup> These countries and their program dates are listed in Table 1. The remaining countries serve as controls, and primarily contribute to the estimates by helping to pin down year effects and the coefficients on various control variables (such as other tax revenues and tariff rates). I include no developed countries in the sample for empirical analysis because PSI is purely a developing-country phenomenon. The first PSI contract started in 1985, so I limit the analyses to the years 1980 through 2000.

## Effect of PSI on import duties

In estimating the impact of preshipment inspection on country-level variables (such as import duties), a central methodological concern is that countries implementing PSI programs are likely to be quite different from countries that do not. For example, countries that implemented PSI programs at some point between 1985 and 2000 were poorer and more corrupt on average (as measured in 1980-1984). Thus it would be invalid to simply compare an outcome such as import duty collections for countries that do and do not have PSI programs at a single point in time, and to infer that any differences reflect the causal impact of PSI programs.

Instead of relying on cross-country comparisons at a single point in time, the analysis instead estimates the impact of PSI based on changes over time within PSI-implementing countries. Specifically, the estimated impact of PSI focuses on the change in outcomes (e.g., import duties) from before to after the start of a PSI program.

The results indicate that the import duties increase by 15-30 percentage points on average in the five years after the start of a PSI program. Preshipment inspection appears to have been quite cost-effective, with improvements in import duty collections in the first five years of the program equal to 2.6 times program costs.

A graphical view of the relationship between import duties and PSI programs provides a summary of the main finding. In Figure 1, the solid line plots the conditional mean of log import duties in a range of years before and after the start of a country's PSI program. The conditional mean is normalized to zero in year -1. (Year -1 is the year

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<sup>9</sup> Because countries with better data availability may also have more competent governments, allowing data availability to define the sample may suggest that the results should apply mainly to countries with comparable public institutions. That said, the list of countries in Table 1 includes both relatively rich developing countries (such as Argentina) and some of the very poorest ones (Paraguay, Burkina Faso).

immediately prior to the starting year of the program, year 0 is the starting year, etc.)<sup>10</sup> Figure 1 reveals that the conditional mean of log import duties for countries using PSI shows a marked positive change immediately after the PSI program is put in place. By contrast, there is no such change prior to the beginning of the PSI contract. This fact is helpful, as it provides evidence that the later increase in import duties is unlikely to be driven by mean reversion. Each coefficient on indicators for years after the start of PSI is statistically significantly different from zero at the 95% confidence level, while none of the coefficients for years prior to PSI start are statistically significant.

Even though the focus on this analysis is on within-country changes that result from PSI programs (rather than on differences across countries that do and do not have such programs), it is still important to ask whether the association between PSI programs and growth import duties indeed reflects the causal impact of PSI. For instance, if countries implement PSI programs at the same time as they make substantial public finance reforms, the observed increase in import duty growth may not be due to PSI, but rather to other actions the country takes at the same time. Two main approaches address such concerns.

First, one might be worried that PSI coincides with other policy or macroeconomic changes that also affect import duty collections. For example, overall tax revenues (including import duties) could rise due to concurrent general reforms of public finances or an increase in economic activity, and not because of the causal effect of PSI. As evidence against this concern, I show that there is no appreciable change in other tax revenues (exclusive of import duties) when PSI is introduced. In addition, the regression results are highly robust to controlling for the current level of other tax revenues (which may be considered a proxy for other policy and macroeconomic changes affecting tax collections).

Second, concurrent reforms specific to the customs agency (other than PSI) might be the true causal factor behind the change in import duties. To test this hypothesis, I make use of data on an important determinant of customs duty collections: tariff rates. I find no indication that the average tariff rate changes alongside PSI introduction, and the estimated impact of PSI on import duties is essentially unchanged when controlling for the current average tariff rate.

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<sup>10</sup>Formally, the conditional means are generated by running the following regression, where the outcome variable is log import duties:

$$\begin{aligned}
 Y_{jt} = & \theta_{-20}PSI_{-20jt} + \theta_{-19}PSI_{-19jt} \\
 & \dots + \theta_{-1}PSI_{-1jt} + \theta_0PSI_{0jt} + \theta_1PSI_{1jt} \\
 & \dots + \theta_{13}PSI_{13jt} + \theta_{14}PSI_{14jt} \\
 & + \gamma_j TREND + \mu_j + \delta_t + \varepsilon_{jt}
 \end{aligned}$$

The variables  $PSI_{-20jt}$ ,  $PSI_{-19jt}$ , ...,  $PSI_{14jt}$  are indicators for the observation occurring a certain number of years before or after the start year of a country's PSI program, for 20 years before up to 14 years after (the complete set of before and after years observed in the data). These indicators are all zero if the country never used PSI. The remaining variables are year fixed effects, country fixed effects, and country-specific linear time trends. The points comprising the solid line in Figure 1 are the coefficients  $\theta_{-20}$  through  $\theta_{14}$  on these indicator variables, and the dotted lines depict the 95% confidence intervals of each coefficient estimate.



Finally, there may be still be other unobserved policy changes taking place alongside PSI, and that are the true causal factors behind the increase in import duties. An innovation of this research is to examine the impact of PSI in the midst of periods where countries' economic policies are likely to be relatively stable, to better help establish that PSI was the causal factor behind the concurrent increases in import duties. I define distinct "policy regimes" for each country as periods when key leaders who might affect import duty collection (the national leader, the finance minister, and the head of the customs agency) were unchanged. The regression results are robust to estimating PSI's association with import duties only from variation within so-defined policy regimes, further bolstering the case for PSI's causal impact.

## **PSI's effects on import misreporting and on import volumes**

If PSI is accompanied by a growth in import duty collections, one would like to know how these improvements came about. Improvement in duty collections can occur in a number of ways: either theft of import duties by customs officers declines, or bribes paid by importers decline (which lowers market prices and raises import demand), or both.

If the customs agency turns over to the government a fraction of true import values that is lower than the official tariff rate, it must alter its records to hide evidence of such theft. Thus evidence that mis-reporting of import data has declined is indirect evidence of a decline in customs corruption. I focus on measures that are likely to capture two types of mis-reporting: 1) mis-reporting of import values ('undervaluation'), and 2) mis-reporting of goods classifications.

Import duties are typically assessed as a fraction of declared shipment values; so a main method of duty avoidance is simply to declare on a customs declaration that an imported shipment has a value lower than its true value ('undervaluation'). A natural measure of undervaluation is the fraction of the value of imports sent to a country (as reported by trade partners) that are actually recorded in a country's import statistics. Specifically, I construct what I call the 'import capture ratio': a country's total reported imports in a given year, divided by the total reported *exports* of trade-partner countries to the same country. All other things equal, countries with less undervaluation in customs should have higher import capture ratios.<sup>11</sup>

Undervaluation is not the only method of concealing the avoidance or theft of import duties, however. Another generic strategy is to mis-report the goods classification of a shipment, to make it appear that the shipment is in a category subject to lower tariffs. As a quantitative measure of the extent of mis-classifying of goods, I use the *coefficient of variation* of import capture ratios across goods within a country. The basic insight is that mis-reporting increases the *dispersion* of import capture ratios across goods, vis-à-vis a benchmark situation where there was no mis-reporting. Import capture ratios fall for goods with higher tariffs (as goods are misreported as being in other categories with lower tariffs), and import capture ratios rise for goods with lower tariffs. All other things held equal, then, an increase in the mis-classification of goods should lead an *increase* in the coefficient of variation of import capture ratios across goods within a country, while declines should lead

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<sup>11</sup>On the likely sources of error in import capture ratios and their implications for empirical analysis, see the discussion in the Colombian section (Section ) below.

to a corresponding *decrease*.

In addition to these two measures of import mis-reporting, I also examine the impact of PSI on the total volume of imports, to identify any trade-facilitating effect of the program which may flow from declines in importers' costs (due to declining bribe payments). To separate PSI's trade-facilitating effect from its effect on mis-reporting, it is useful to use an import measure that is less prone to undervaluation. Thus I use the total value of *exports* recorded by all other countries as destined for the country in question as the import measure (which I call 'partner-reported imports').

The empirical results detailed in Yang (2005a) indicate that PSI programs are indeed associated with improvements in import capture ratios and in reductions in the coefficient of variation of import capture ratios across goods in the first five years of PSI programs. Total imports also tend to improve, but these improvements come some years after PSI implementation, so that any causal link between PSI and import volumes (a trade facilitation effect) is more speculative.

## Microeconomic evidence from the Philippines

Although the international evidence outlined above documents that preshipment inspection programs can yield substantial benefits in terms of increases in import duties, and reductions in mis-reporting, these results are averages across countries and over two decades. But success with PSI is not guaranteed. It therefore makes sense to look in detail at specific country experiences with PSI, to get some insight into how a PSI program can fail. Here, I outline microeconomic empirical work on the impact of a PSI expansion in the Philippines, which is more fully elaborated in a separate paper, Yang (2005b). In the following section, I present new empirical analyses on the partial implementation of PSI in Colombia.

A frequent concern in crime studies is that increased enforcement could lead criminal activity to be displaced to alternative lawbreaking methods (Repetto 1976). A simple model predicts that, when alternative lawbreaking methods involve fixed costs of entry, crime displacement should respond positively to the size of illicit profits threatened by enforcement. But there is little empirical evidence on the relationship between crime displacement and basic economic factors. For the most part, empirical analyses of enforcement's impact address displacement as a mere sidenote, at most examining the existence or amount of displacement.<sup>12</sup> Evidence on the determinants of crime displacement could shed light on the importance of economic motives in the decisions of lawbreakers more generally. Moreover, existing studies typically conclude that displacement is a minor phenomenon, finding either no evidence of displacement or that it is small in magnitude. For example, John DiNardo and Thomas Lemieux (2001) find small amounts of displacement from alcohol to marijuana consumption in response to increases in state-level drinking ages. But in theory, increased enforcement can actually backfire,

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<sup>12</sup>See Chaiken, et al (1974), McPheters, et al (1984), Ayres and Levitt (1998), Levitt (1998), Braga, et al (1999), and Di Tella and Schargrodsky (2004), among others. See also Hesselting's (1994) overview.

leading crime rates to be unchanged or even to increase. This perverse outcome can occur when alternative methods have higher fixed costs but lower variable costs than previously-used methods.

The preshipment inspection program in the Philippines allows an empirical study of such unintended consequences of law enforcement. Prior to 1990, shipments valued under the minimum value threshold for inspection, US\$5,000, were exempt from preshipment inspection. Thus a common method of avoiding the inspection was to split shipments into pieces so each could be valued below that level. Over a six-month period in 1990, the government clamped down on this loophole, reducing the minimum value threshold for inspection first to \$2,500 and then to \$500.

Because only shipments from a subset of countries were subject to PSI in the first place, the reform constituted a quasi-experiment. The increased enforcement applied only to shipments from some countries, so that corresponding shipments from all other countries serve as a comparison group. Increased enforcement reduced the original method of duty avoidance (valuation under the old minimum value threshold), but led to substantial displacement to an alternative duty-avoidance method (shipping via duty-exempt export processing zones). The shift amounted to 2.7 percent of total imports from treatment countries. I cannot reject the hypothesis that the reform led to zero change in total duty avoidance. Displacement was greater for products with higher tariff rates and import volumes, consistent with the existence of fixed costs of switching to alternative duty-avoidance methods.

Figures 2 and 3 illustrate the fundamental aspects of the empirical results emerge in simple summary statistics and graphs for imports from treatment and comparison countries. Figure 2 displays the fraction of total imports entering the Philippines in shipments with declared values equal to or above \$2,500 but below \$5,000. The solid line is the fraction for treatment countries, while the dotted line is for control countries. The most striking aspect of this graph is the decline in the fraction of total imports in this value range for treatment countries after May 1990, just as the minimum value threshold for PSI was lowered. By contrast, the fraction of total imports from control countries declared to be in this value range displays no similar change during these months. The explanation for these differential patterns is quite certain: prior to May 1990, some fraction of imports from PSI countries were being intentionally declared as valued in this range to avoid the PSI requirement. When the minimum value threshold was lowered, this practice ceased, as it was presumably impractical to split shipments into shipments small enough to be valued below \$500.

Figure 3 displays the fraction of total imports from the two country groups that were destined for export processing zones. A differential increase in export processing zone shipments from treatment countries is apparent, suggesting that importers from these countries may have been encouraged to take advantage of the PSI exemption for export processing zone shipments as the minimum value threshold was lowered. Imports brought into the export processing zones could then have been smuggled out of the zones for sale in the domestic market.

Conservative estimates of tariff revenue gains and losses (net of PSI fees) suggest that the minimum value threshold reductions were a starkly uneconomic proposition, leading to significant losses in net revenue for the Philippine government. I estimate that the minimum value threshold reductions led to a net loss of \$36.8 million for the Philippine

government.<sup>13</sup>

## Microeconomic evidence from Colombia

Data from Colombia indicates that smuggling displacement increases with the size of profits threatened by enforcement, and declines with enforcement levels on alternative methods of duty evasion. My analysis exploits the fact that when the Colombian government implemented its PSI program, it only required preshipment inspections for a defined subset of products (which I will call “PSI products”). The analysis in this section asks how PSI affects duty avoidance on PSI products, using as a control group other products for which PSI was not required (“non-PSI products”).

Colombia's PSI program started in mid-1995, and the list of PSI products was finalized in March 1996.<sup>14</sup> The product-level measure of the extent of duty avoidance is the “import capture ratio”: Colombia's reported imports of a product, divided by other countries' reported exports of the same product to Colombia. Lower values of this ratio indicate that more of that good was diverted compared to other goods. The source for trade data is the UN Comtrade database.<sup>15</sup>

Essentially, the export reports of trade partner countries become the benchmark against which the corresponding import data are compared. But due to transport costs and export misreporting, cross-sectional differences between product-level import capture ratios cannot be completely ascribed to differences in undervaluation. Import data include the cost of freight and insurance (they are c.i.f., or “cost, insurance, and freight”), while export data collected by origin countries do not (they are f.o.b., or “free on board”). That said, fixed effects included in the estimation will account for level differences in the import capture ratio across products. So transport costs and misreporting of partner country exports will not be problematic if *changes* in these factors are not correlated with the imposition of PSI for specific products. Using a measure such as the import capture ratio also presumes that undervaluation does not occur in the customs declarations in the country of export. This assumption is most plausible if customs officers (not importers) are primarily the ones falsifying import data in customs, as Colombian customs officers should

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<sup>13</sup> While in retrospect the minimum value threshold reductions were clearly uneconomic from the standpoint of raising import duties net of fees, it is not obvious that the Philippine government could have known this in advance. At the time of the changes, Philippine customs was not computerized, the number of shipments in the under-\$5,000 value range might not have been known exactly, and so it might have been difficult to estimate the cost of the additional inspections. It was also unclear *ex ante* what fraction of shipments under \$5,000 were declared as being in that value range purely to avoid the PSI requirement. Finally, the large displacement to export processing zones was probably unanticipated.

<sup>14</sup> Implementing legislation is contained in Colombian government Decree 861 of May 26, 1995. Changes in the list of PSI products were made via Decrees 1574 (September 18, 1995) and 567 (March 21, 1996). The program was cancelled in July 1999 in the course of large-scale modernization and simplification of Colombian public administration (Decree 1122, June 26, 1999). Colombian government Decree 567 lists Harmonized System (version 1996) codes requiring PSI, from the 2- to 10-digit level. The trade data I use is in the SITC (Rev. 3) system, at the level of 4/5-digit products, so the measure of PSI coverage must also be at that level. I simply define a 4/5-digit SITC product as a “PSI product” if PSI is required for some HS (1996) tariff line within the 4/5-digit product. (For 94.2% of PSI products, PSI is required for all HS (1996) tariff lines within the product.)

<sup>15</sup> All trade data used in this section are in nominal US dollars.

have no ability to alter export data in the shipment's origin country. Even if importers play a role in making false statements on customs declarations, they have no direct reason to falsify their declarations to the exporting country. There is essentially no sharing of export and import statistics between origin and destination countries for the purposes of customs enforcement.

All else equal, a product's import capture ratio should be lower when importers conceal the value of shipments from customs authorities to reduce their import duty payments (typically assessed as a percentage of reported value). Three alternative methods of duty avoidance should lower a product's import capture ratio. Importers may:

- Falsely provide import values lower than true values (“undervaluation”),
- Falsely classify products into other product categories that are not subject to PSI (“misclassification”), or
- Avoid formal customs procedures entirely (“outright smuggling”).

When PSI is required for a certain product, it becomes more difficult to reduce one's import duties via the first method, undervaluation. However, the remaining two methods are still available: importers may still misclassify shipments into false product categories, or engage in outright smuggling. Even if PSI reduces underinvoicing, helping raise import capture ratios, any displacement to either misclassification or outright smuggling should lower import capture ratios of PSI products; so the net effect on PSI products' import capture ratios is ambiguous.

The empirical analysis examines whether--as predicted by theory--PSI raises the import capture ratios of PSI products *less* when the illicit profits threatened by enforcement are larger, and raises import capture ratios of PSI products *more* when enforcement is higher on alternative methods of duty evasion.

The size of profits threatened by enforcement is simply the tariff rate on the PSI product. To avoid confounding empirical estimates with any endogenous changes in the tariff rate, I use a product's mean tariff rate prior to the start of the program (in 1993-94), which is highly correlated with tariff rates during the program. Tariff rates are the simple average tariff across tariff lines within the product category. In 1993 and 1998, tariff data are unavailable, and for these years the tariff rates used in the analysis are the simple average of tariff rates in the two years immediately before and after. Tariff data are from the UNCTAD Trains database.

The measure of enforcement levels on alternative duty evasion methods is the mean PSI coverage in the PSI product's aggregate product group (where the product group is the 3-digit SITC Rev. 3 level). This measure is sensible, as it should be easier for importers to successfully misclassify products as other products in the same product group. For example, an importer of a PSI product such as “new pneumatic car tires” (SITC Rev. 3 code 6251) should find it easier to misclassify the shipment as a non-PSI product in product group 625 (such as “used pneumatic tires”, code 62593) than as an entirely unrelated product. Fisman and Wei (2004) provide evidence that misclassification tends to be towards similar products, documenting that import capture ratios for Chinese imports from Hong Kong are higher for products where other products in the same aggregate product group have higher tariffs (making misclassification less desirable).

## **Empirical analysis of Colombian import capture**

## ratios

The average impact of PSI requirements on PSI products was estimated via the following difference-in-difference regression equation for the log of the import capture ratio, for product  $i$  in year  $t$  :

$$\ln \gamma_{it} = \beta_0 + \beta_1 (PSI_i * AFTER_t) + \beta_2 (SIM_i * AFTER_t) + \eta_i + v_t + \varepsilon_{it} \quad (1)$$

$\gamma_{it}$  is the import capture ratio. Because there is wide variety in the import capture ratio across products, it is more sensible to examine the log of the import capture ratio as the outcome variable.<sup>16</sup>  $PSI_i$  is the indicator for a PSI product, and  $AFTER_t$  is an indicator for the years of full PSI implementation (1997-98). A positive coefficient on  $\beta_1$  would suggest that PSI was effective in reducing duty evasion.

If importers respond to PSI by misclassifying PSI products as non-PSI products, then import capture ratios of these “recipient” non-PSI products should *rise*. For this reason, the non-PSI products that are the recipients of such misclassification are not likely to be the best control group. As discussed above, it should be easier for importers to misclassify PSI products as non-PSI products that are in some sense “similar”. Therefore, I estimate a separate effect of the PSI program on non-PSI products that are in the same aggregate product group as some PSI product. I include in the regression an indicator variable ( $SIM_i$ ) for a non-PSI product being in the same 3-digit SITC Rev. 3 group as some PSI product, interacted with the  $AFTER_t$  indicator.<sup>17</sup> The control group then becomes the omitted category: non-PSI products that *do not* have some PSI product in the same aggregate product group (and thus are less likely to be “recipients” of misclassification). If there is still some misclassification of PSI products into this omitted category, the estimated impact on PSI products' import capture ratios will be biased in a negative direction. However, such a negative bias is not problematic if the goal of the analysis is simply to determine whether *any* displacement has occurred. This negative bias will only occur if some amount of misclassification is going on. So finding any *negative* effect of PSI on PSI products' import capture ratios in this setting should be taken as evidence of displacement.

$\eta_i$  is a product fixed effect, and captures time-invariant differences across products in log import capture ratios.  $v_t$  is a year fixed effect, and captures changes in log import capture ratios common across all products within a year. (Main effects for  $PSI_i$ ,  $SIM_i$ , and  $AFTER_t$  are absorbed by these product and year fixed effects.)  $\varepsilon_{it}$  is a mean-zero error term. It is possible that error terms may be serially correlated among observations for the same product (Bertrand, Duflo and Mullainathan (2001)), so I calculate standard errors clustered by product. So that the estimates can more accurately reflect the impact of PSI on Colombia's overall imports, observations are weighted by the

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<sup>16</sup>Fisman and Wei (2004) also examine a similar outcome variable in log form.

<sup>17</sup> In other words, the indicator is zero for all PSI products and for all non-PSI products with no similar PSI products.

product's mean annual dollar imports in 1993-94.

Products subject to PSI were in fact not chosen randomly, and certain types of products were more likely to require PSI than others. For example, those with higher tariff rates prior to the PSI program were more likely to be included under the PSI program. In addition, those with higher pre-PSI import capture ratios were less likely to be included.<sup>18</sup> The proportion of PSI products among “manufactured goods” in the sample is 0.30, while for “machines and transport equipment” products it is 0.13. Because PSI products differ in their initial characteristics from non-PSI products, it is crucial that identification of the impact of PSI focuses on *changes* in import capture ratios accompanying the introduction of PSI requirements on certain products, not on cross-sectional *level* differences. The identification assumption is that, in the absence of the PSI program, changes in import capture ratios would have been similar for PSI products and for non-PSI products that are not in the same (3-digit SITC Rev. 3) aggregate product group.

To examine heterogeneity in the impact of PSI, I also estimate regressions where the PSI variable ( $PSI_i$ ) and the indicator for being similar to a PSI product ( $SIM_i$ ) are interacted with the pre-PSI (1993-94) tariff rate ( $\tau_i^{pre}$ ) and the mean PSI coverage in the 3-digit product group ( $PSI_i^{agg}$ ):

$$\begin{aligned} \ln \gamma_{it} = & \alpha_0 + \alpha_1 (PSI_i * AFTER_t) \\ & + \alpha_2 [(PSI_i * AFTER_t) * PSI_i^{agg}] + \alpha_3 [(PSI_i * AFTER_t) * \tau_i^{pre}] \\ & + \alpha_4 (SIM_i * AFTER_t) \\ & + \alpha_5 [(SIM_i * AFTER_t) * PSI_i^{agg}] + \alpha_6 [(SIM_i * AFTER_t) * \tau_i^{pre}] \\ & + \alpha_7 [AFTER_t * \tau_i^{pre}] \\ & + \eta_i + \nu_t + \varepsilon_{it} \end{aligned}$$

(2)

The interaction term  $AFTER_t * \tau_i^{pre}$  is included to capture any changes over time in import capture ratios related to a product's initial tariff rate.<sup>19</sup>

The economic model of crime displacement predicts that, in response to increased enforcement, displacement to alternative methods will be lower when enforcement is higher on alternative lawbreaking methods. Products with higher  $PSI_i^{agg}$  face higher

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<sup>18</sup>A regression of the PSI indicator on a product's initial (1993-94) mean tariff rate yields a coefficient on the tariff rate of .030 (standard error .002). A regression of the PSI indicator on a product's initial (1993-94) mean import capture ratio yields a coefficient on the import capture ratio of -.136 (standard error .080). (Regressions are OLS and weighted by product's initial (1993-94) mean dollar imports.)

<sup>19</sup>Main effects for  $PSI_i$  and  $SIM_i$  (and their interactions with  $\tau_i^{pre}$  and  $PSI_i^{agg}$ ) do not need to be included as they are absorbed by product fixed effects. Also, it would be redundant to include an interaction term for  $AFTER_t * PSI_i^{agg}$ . Because  $PSI_i^{agg} = 0$  for all non-PSI products that are not in the same product group as some PSI product,  $AFTER_t * PSI_i^{agg}$  is a linear combination of the year effects,  $(PSI_i * AFTER_t) * PSI_i^{agg}$ , and  $(SIM_i * AFTER_t) * PSI_i^{agg}$ .

enforcement against an alternative duty-avoidance method, misclassification. So we should expect that  $\alpha_2 > 0$  (PSI coverage on a product should raise its import capture ratio more when PSI coverage is higher in its aggregate product group).

A further theoretical prediction is that displacement to alternative methods will be higher when the illicit profits threatened by enforcement are higher. Profits from duty evasion rise with tariffs, so we should expect that  $\alpha_3 < 0$  (PSI coverage on a product should raise its import capture ratio less when it has a higher tariff rate).

Constructing import capture ratios at the highly disaggregated product level invariably generates extreme import capture ratios for some products (that may be generated by inconsistencies in data reporting between Colombia and trade partners). Including such products in the analysis is likely to generate substantial noise that could obscure evidence of PSI's impact. So I exclude from the dataset all products whose mean import capture ratios prior to the PSI program (in 1993-94) were extremely high or low.<sup>20</sup> As the products to be excluded are determined on the basis of characteristics prior to the start of the PSI program (and so are not affected by PSI itself), their exclusion should not harm the internal validity of the estimates. The analysis must also exclude products with missing data on Colombia-reported imports and trade-partner-reported exports.

As the PSI product list was in flux during 1995 and 1996, I do not use data from these years. The empirical analysis simply compares import capture ratios in two pre-PSI years (1993 and 1994) with those in years when the rules were fully in place (1997 and 1998).

The empirical analysis includes 2,427 products, of which 19.4% are PSI products. Summary statistics for the regression sample are presented in Table 2. The median import capture ratio is 1.10. Ratios above unity should not be surprising, because import data (in the numerator) include freight and insurance costs while export data (in the denominator) do not. The mean import capture ratio is 1.76, reflecting the existence of some quite large import capture ratios. Very large import capture ratios will result from misclassification of imports into product categories whose true import volumes are small.<sup>21</sup> The mean tariff rate is 12.13 percent. 29% of products were “similar” to (in the same 3-digit SITC Rev. 3 group as) some PSI product.

Coefficient estimates are presented in Table 3. Column 1 displays the coefficient on the  $PSI_i * AFTER_t$  variable in equation (1). On average across PSI products, there is no evidence that the introduction of product-level PSI requirements is associated with changes in import capture ratios: the coefficient on  $PSI_i * AFTER_t$  is essentially zero and is not statistically significant.

As it turns out, though, Column 1's estimate conceals heterogeneity within the set of PSI products. Column 2 displays regression coefficients from estimation of equation (2). The coefficient on the interaction term with 3-digit group PSI coverage is positive and highly statistically significant. This result is consistent with displacement of duty avoidance for PSI products from underinvoicing to misclassification, if importers find it

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<sup>20</sup>Below the 5th percentile and above the 95th percentile of the 1993-94 mean import capture ratio distribution by product, where each product is weighted by its 1993-94 mean dollar imports.

<sup>21</sup> Of course, inconsistencies in data recording across countries can also lead to extreme import capture ratios. As long as such inconsistencies are for the most part unrelated to intentional misclassification for the purpose of duty evasion, their main effect on the analysis should be to decrease the precision of coefficient estimates.



easiest to misclassify their imports as similar (but non-PSI) products. If similar products have higher levels of PSI coverage, importers fear that misclassification will be more easily detected, leading PSI to have a greater positive effect on the import capture ratio. The coefficient in Column 2 on the interaction term with the initial tariff rate is negative and statistically significant. When the profits threatened by enforcement (import tariffs) are higher, PSI may make importers more likely to seek alternative means of avoiding import duties, leading to greater declines in import capture ratios.

These coefficient estimates imply that when PSI products faced relatively high tariffs (higher potential profit from displacement) and had relatively low PSI coverage in the aggregate product group (low enforcement against displacement via misclassification), the imposition of PSI requirements actually led to *declines* in import capture ratios. Column 2's estimates imply that for a PSI product at the 75th percentile of the initial tariff rate distribution (35.4 percent) and the 25th percentile of the 3-digit product group PSI coverage distribution (0.72), the differential decline in its import capture ratio was -0.483 (standard error 0.227).<sup>22</sup>

At this point, it is important to address a potential omitted-variable concern: other product-level trade policies may have changed for PSI products, and may affect import capture ratios as well. For example, the government could have raised tariffs and other trade restrictions differentially on PSI products. If such trade restrictions themselves encouraged displacement to alternative methods of duty avoidance, the estimated impact of PSI on import capture ratios would be biased downwards. Although time-series data on all forms of trade restrictions are unavailable, time-series tariff rates are available by product, and tariffs are likely to be the most salient form of product-level trade policy in the minds of importers. So the remaining columns of the table include controls for a product's current tariff rate, as well as the interaction between the current tariff rate and the  $PSI_i * AFTER_t$  and  $SIM_i * AFTER_t$  variables. The inclusion of these additional controls leaves essentially unchanged the coefficient on the  $(PSI_i * AFTER_t) * PSI_i^{agg}$  term. The coefficient on  $(PSI_i * AFTER_t) * \tau_i^{pre}$  (in column 4) is larger in magnitude and its sign is still negative, but its standard error has risen so that it is no longer statistically significantly different from zero. This latter change is not particularly worrying, as pre-PSI tariff rates and current tariff rates are highly correlated, so that insufficient variation remains in the regression for precise estimation of the pre-PSI tariff rate coefficient. Adding current tariff rate controls makes little difference for the conclusions from Table 3; differential changes in trade restrictions for PSI products do not seem to be driving the results.

If PSI products are being misclassified as non-PSI products in the same SITC 3-digit product group, import capture ratios should rise for these non-PSI products, so that the coefficient on  $SIM_i * AFTER_t$  should be positive. We might also expect that import capture ratios would rise *more* for such non-PSI products when the share of PSI products in the 3-digit product group is higher, as more PSI products would be misclassified into the remaining non-PSI products in the group (the coefficient on  $(SIM_i * AFTER_t) * PSI_i^{agg}$

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<sup>22</sup>Distributions of initial tariff rate and 3-digit product group PSI coverage weighted by initial (1993-94) mean dollar imports.

should be positive). Finally, misclassification into a certain non-PSI product should be less prevalent when the non-PSI product in question itself has a higher tariff rate (the coefficient on  $(SIM_i * AFTER_t) * \tau_i^{pre}$  should be negative).

The coefficient estimates in Table 3 for  $SIM_i * AFTER_t$  and its interaction terms indeed have the predicted signs. However, standard errors are quite large, so that none of the coefficients is statistically significantly different from zero. Due to the imprecision of these estimates, these results should be taken as inconclusive. At the same time, these results provide no reason to doubt the interpretation of the positive and statistically significant coefficient on the  $(PSI_i * AFTER_t) * PSI_i^{agg}$  term as due to increased effectiveness of PSI (at raising import capture ratios) when enforcement against misclassification is greater.

In sum, the evidence presented in this section documents that displacement of duty avoidance in Colombia rises with the size of illicit profits threatened by enforcement, and declines with enforcement levels on alternative methods of duty evasion. The distinctive feature of the Colombian preshipment inspection program is that it required inspections for only a subset of products, leaving large categories of products uncovered by the program. Thus, importers could continue to evade import duties by misclassifying imports into non-PSI product categories. I find that the higher the illicit profits threatened by PSI (proxied by the product's tariff rate), the greater is misclassification to alternative product categories. In addition, when enforcement levels are higher on alternative methods of duty evasion (when PSI is also required on other similar product categories), there is less misclassification to other product categories.

## Conclusion: implications for anti-corruption efforts more broadly

This chapter has surveyed new research on a widespread approach to combating corruption in customs: the use of preshipment inspection (PSI) services. PSI improves the information available to higher level enforcers on the contents of incoming shipments, and so has the potential to help reduce corruption in customs, raise import volumes, and ultimately raise import duty revenue. In a study of PSI-implementing countries over two decades, I find that implementation of PSI programs leads to increases in import duties, and is accompanied by declines in underinvoicing and in misclassification of goods classifications in customs. The programs appear to be highly cost-effective on average. However, such programs are not guaranteed to succeed. In micro-level studies of the workings of PSI programs in the Philippines and Colombia, I identify conditions under which such programs may fail.

Aside from shedding light on the effectiveness of a widely-implemented anti-corruption reform in customs, these findings also suggest lessons for anti-corruption efforts more broadly. In PSI programs, foreign inspectors simply provide additional information to higher levels of government while keeping duty collection and enforcement in the hands of government employees. As such, PSI is a specific case of a potentially large category of interventions that improve the information-gathering capability of

anti-corruption entities. The evidence outlined in this chapter points to the conclusion that the PSI-generated information is used, in that it changes the incentives of customs agents and importers in all the situations studied. On average across countries, PSI programs lead to increases in import duties collected and reductions in indicators of corruption and fraud in customs. However, in some circumstances (Colombia and the Philippines), the reactions of importers or customs agents can offset the information improvements. But even in the Philippine and Colombian cases, the fact that importers or customs agents are reacting means that the information is being put to some use by enforcers. Overall, the evidence indicates that information is a key constraint facing anti-corruption enforcers, and policies that find innovative ways to alleviate information constraints can have large returns in terms of reducing corruption.

The PSI experience in the Philippines and Colombia suggests that to be successful, anti-corruption reforms should be “broad” in the sense of encompassing a wide range of possible alternative methods of committing the illegal activity of interest. Otherwise, displacement to alternative methods can negate the original goals of the reform.

Finally, the experience of PSI in customs demonstrates that private firms can successfully be used to generate information for anti-corruption efforts. This finding suggests a new direction for anti-corruption initiatives. I am aware of no other anti-corruption effort that relies on private firms to generate information for improved enforcement, but concerns about the corruptibility of enforcers or monitors from within the government extend far beyond the customs context. Although private firms certainly have their own problems with corruption, competition among private firms providing monitoring services may provide them with strong incentives to root out corruption among their employees. There does not appear to be any strong reason why anti-corruption efforts should not experiment more broadly with using private firms as monitors, in areas such as government procurement, provision of licenses, public works, or other forms of taxation.

## Appendix: Robustness checks for Colombia analysis

To address potential concerns about the robustness of the regression results for the Colombian analysis, I experimented with alternative definitions of the sample. Appendix Table 1 shows that similar coefficient estimates and levels of statistical significance result for alternative sample definitions. For comparison, the first column of Appendix Table 1 repeats the coefficient estimates of Table 3, column 2; all remaining regressions include independent variables identical to those in Table 3, column 2. The focus here is on the extent to which the coefficients on the key triple interaction terms

$(PSI_i * AFTER_t) * PSI_i^{agg}$  and  $(PSI_i * AFTER_t) * \tau_i^{pre}$  (the second and third rows of the table) differ substantially from the original specification in the first column.

The second column of the table reports coefficient estimates when the sample is expanded to include observations from the two additional pre-period years for which data are available, 1991 and 1992. The inclusion of these additional years potentially allows a better estimate of pre-period import capture ratios, and could in principle change results if 1993-94 were unusual years in some way. As it turns out, the coefficients on the key triple

interaction terms are close in magnitude to the original specification and maintain their levels of statistical significance.

The third column of the table reports coefficient estimates when the sample is expanded to include observations for which import capture ratios were previously missing, because of missing data on either Colombian-reported imports or partner-reported exports to Colombia. Products where data are missing on Colombian-reported imports but with data on partner-reported exports to Colombia can be thought of as having “very low” import capture ratios, so I let their import capture ratios be the 1st percentile of the distribution of non-missing import capture ratios (weighted by 1994-94 mean dollar imports), which is 0.07. Products missing data on partner-reported exports to Colombia but with data on Colombian-reported imports in principle can be thought of as having “very high” import capture ratios, so I let their import capture ratios be the 99th percentile of the distribution of non-missing import capture ratios (weighted by 1994-94 mean dollar imports), which is 4.39. Although the coefficients on the key triple interaction terms are somewhat smaller in magnitude to those in the original specification, they are still of the same sign and remain statistically significant at conventional levels.

Construction of import capture ratios generates some extremely large and small values. If these extreme values are due to inconsistencies in data reporting between Colombia and its trade partners, they generate noise that can reduce precision and obscure the true impact of PSI. The main sample for analysis therefore excludes products that have very large or small import capture ratios, as evidenced by their being below the 5th percentile and above the 95th percentile of the 1993-94 mean import capture ratio distribution (weighted by 1993-94 mean dollar imports). To show the importance of this sample restriction, the fourth column of Appendix Table 1 presents coefficient estimates where products with extreme 1993-94 import capture ratios are included. The coefficient on the  $(PSI_i * AFTER_t) * PSI_i^{agg}$  term is somewhat smaller in magnitude than in the original specification (0.322 vs. 0.456), while the coefficient on the  $(PSI_i * AFTER_t) * \tau_i^{pre}$  term is essentially the same as in the original specification. Standard errors have risen substantially, however, so that neither coefficient estimate is statistically significant at conventional levels. This result is likely simply to be due to the substantial increase in noise generated by including products with very poorly-measured import capture ratios.

The fifth column of Appendix Table 1 illustrates the impact on the coefficient estimates of further restrictions on the range of products included in the sample on the basis of pre-period import capture ratios. This sample drops products whose pre-period import capture ratios were outside the 10th-90th percentile of that distribution. Compared to the corresponding coefficients in the original specification, the coefficient on the  $(PSI_i * AFTER_t) * PSI_i^{agg}$  term is approximately the same in magnitude, the coefficient on the  $(PSI_i * AFTER_t) * \tau_i^{pre}$  term is essentially identical, and both coefficients have similar levels of statistical significance.

Finally, the last column of the table answers a different question. Is there evidence that similar changes in import capture ratios were occurring in a period *prior* to the introduction of the PSI program? The coefficient estimates in this column are for observations from the pre-period (1991-1994), where 1991-92 is taken to be the “before” period, and 1993-94 is taken to be the “after” period. PSI coverage and tariff rate variables

are as defined before. As such, this “false experiment” is a partial test of the identification assumption that these patterns would not have been observed in the absence of the PSI program. The coefficient estimates on the key triple interaction terms are substantially smaller in magnitude than in the original specification, and are not statistically significant from zero. The lack of statistical significance does not stem from a decline in precision, as coefficient standard errors on the triple interactions of interest are similar to those in the original specification. There is therefore no indication that similar differential changes in import capture ratios were occurring prior to the PSI program.

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**Table 1: Active dates for PSI programs**  
**(as of end of year 2000)**

<u>Country</u>	<u>Start date</u>	<u>End date</u>
Indonesia	11-Apr-85	01-Apr-97
Bolivia	21-Apr-86	
Philippines	01-Apr-87	31-Mar-00
Cameroon	01-Dec-88	
Madagascar	01-Jan-89	
Pakistan	18-Apr-90	15-Nov-97
Sierra Leone	15-Nov-90	
Peru	15-Jan-92	
Burkina Faso	23-Sep-92	
Cote d'Ivoire	11-Mar-93	
Congo, Rep.	09-Jun-93	
Uganda	15-Jan-94	
Kenya	31-Jan-94	
Colombia	09-Jun-95	09-Jul-99
Congo, Dem. Rep.	15-Jun-95	
Paraguay	06-May-96	09-Jun-99
Belarus	06-Jan-97	31-Mar-99
Argentina	23-Sep-97	
Georgia	15-Aug-99	

NOTES-- Start and end dates for countries' PSI programs obtained by author directly from the four major PSI firms. Unspecified end date means contract was still active as of the end of year 2000. Three countries experienced interruptions in their PSI programs: Pakistan between 11/30/91 and 9/1/94; Rep. of Congo between 5/31/98 and 3/4/99; Madagascar between 7/31/92 and 12/4/92. Only countries with data on import duties before and after contract start date are listed.

**Table 2: Summary statistics for Colombian import data**

Years: 1993, 1994, 1997, and 1998

	<u>Mean</u>	<u>Median</u>	<u>Std. Dev.</u>	<u>Minimum</u>	<u>Maximum</u>
PSI product (indicator)	0.19	0	0.40	0	1
Similar to PSI product (indicator)	0.29	0	0.46	0	1
Import capture ratio	1.76	1.10	14.99	0.0000	1334.21
Ln (import capture ratio)	-0.09	0.09	1.13	-11.17	7.20
Tariff rate	12.13	12.50	5.87	0.00	35.39
Trade-partner-reported exports to Colombia	4,335,819	956,127	20,913,226	533	892,013,124
Colombia-reported imports	4,676,492	936,865	19,703,477	10	684,459,547
Number of product-year observations:	9,314				

NOTES -- An observation is a 4/5-digit SITC Rev. 3 product in a particular year. "PSI product" is indicator for PSI being required for some HS (1996) tariff line within SITC 4/5-digit product. "Similar to PSI product" equal to 1 if product is in same 3-digit product group as some PSI product, and 0 otherwise. "Import capture ratio" is Colombian own-reported imports divided by trade-partner-reported exports to Colombia. "Tariff rate" is unweighted mean of tariff rate across tariff lines within 4/5-digit product. Tariff data are unavailable for 1993 and 1998, tariff rate in these years are replaced with simple average of tariff rate in adjacent years (1993 data are mean of 1992 and 1994; 1998 data are mean of 1997 and 1999.) Trade data are in nominal US dollars. Summary statistics are for exact observations used in empirical analysis. Products are excluded from analysis if their initial (1993-94) average import capture ratio was below 0.08 or above 3.67: respectively, 5th and 95th percentiles of 1994 distribution of import capture ratio (distribution weighted by initial dollar imports).

Data sources: Trade statistics are from UN Comtrade database. Tariff data are from UNCTAD Trains database. PSI coverage data are from Colombian government Decree 567 (March 1996).

**Table 3: Impact of PSI coverage on product-level import capture ratio, Colombia**

(Weighted fixed-effects estimates)

Years: 1993, 1994, 1997, and 1998

Dependent variable: Ln(import capture ratio)

	(1)	(2)	(3)	(4)
(PSI product) * (After)	-0.001 (0.060)	-0.024 (0.124)	0.003 (0.056)	-0.019 (0.124)
(PSI product) * (After) * (PSI coverage in 3-digit group)		0.456 (0.172)***		0.447 (0.173)***
(PSI product) * (After) * (Pre-PSI tariff rate)		-0.022 (0.010)**		-0.033 (0.022)
(PSI product) * (After) * (Current tariff rate)				0.012 (0.021)
(Similar to PSI product) * (After)	0.051 (0.072)	0.092 (0.186)	0.051 (0.072)	0.091 (0.186)
(Similar to PSI product) * (After) * (PSI coverage in 3-digit group)		0.267 (0.761)		0.266 (0.761)
(Similar to PSI product) * (After) * (Pre-PSI tariff rate)		-0.008 (0.018)		-0.016 (0.032)
(Similar to PSI product) * (After) * (Current tariff rate)				0.009 (0.027)
(After) * (Pre-PSI tariff rate)		0.012 (0.008)		0.008 (0.023)
(After) * (Current tariff rate)				0.003 (0.023)
Current tariff rate			0.005 (0.012)	-0.007 (0.019)
Observations	9,314	9,314	9,314	9,314
R-squared	0.69	0.69	0.69	0.69

NOTES -- Unit of observation is a 4/5-digit SITC Rev. 3 product in a certain year. Standard errors (clustered by product) in parentheses. Each observation weighted by initial (1993-94) mean annual dollar imports. Years 1993-94 are prior to the imposition of PSI requirements. PSI program started in August 1995. Years 1995 and 1996 excluded from analysis because list of products requiring PSI changed over this period (list finalized in March 1996). PSI program operational for all of 1997 and 1998, and ended in July 1999. "After" is indicator for 1997 or 1998. "PSI coverage in 3-digit group" is fraction of 4/5-digit products within 3-digit SITC Rev. 3 group with any PSI requirements. All regressions include fixed effects for year and product. (After)\*(PSI coverage in 3-digit product group) not included because redundant. See previous table for variable definitions, data sources, and other notes.

**Appendix Table 1: Impact of PSI coverage on item-level import capture ratio, Colombia (Additional specifications)**

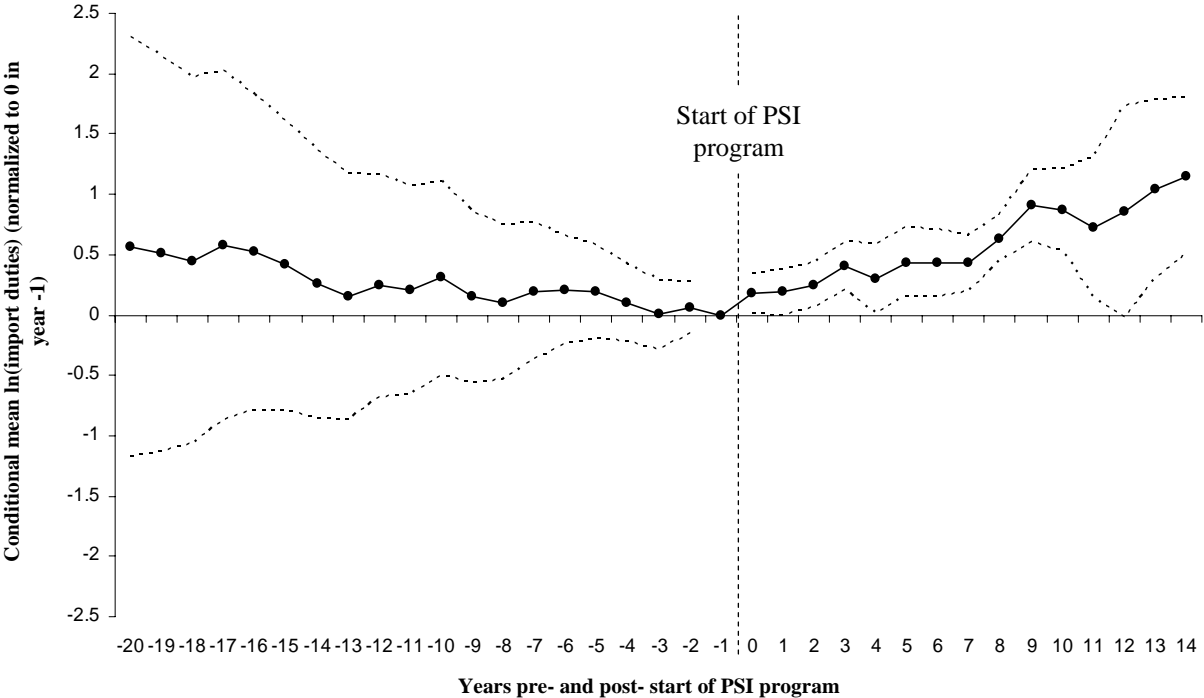
(Weighted fixed-effects estimates)

Dependent variable: Ln(import capture ratio)

<i>Specification:</i>	Original (from Table 2, col. 2)	Alternative definitions of sample for analysis				Pre-test
		Including observations for 1991 and 1992	Replace missing import capture ratio	Not dropping products outside 5th-95th pctl. of pre-period import capture ratio	Dropping products outside 10th-90th pctl. of pre-period import capture ratio	1991-92 is "before" period and 1993-94 is "after" period
(PSI product) * (After)	-0.024 (0.124)	-0.025 (0.128)	0.003 (0.139)	0.124 (0.282)	-0.03 (0.122)	-0.011 (0.194)
(PSI product) * (After) * (PSI coverage in 3-digit group)	0.456 (0.172)***	0.514 (0.157)***	0.300 (0.132)**	0.322 (0.251)	0.496 (0.185)***	0.179 (0.195)
(PSI product) * (After) * (Pre-PSI tariff rate)	-0.022 (0.010)**	-0.023 (0.011)**	-0.015 (0.009)*	-0.023 (0.021)	-0.022 (0.009)**	-0.001 (0.010)
(Similar to PSI product) * (After)	0.092 (0.186)	0.159 (0.180)	0.115 (0.200)	0.085 (0.282)	-0.059 (0.108)	0.098 (0.176)
(Similar to PSI product) * (After) * (PSI coverage in 3-digit group)	0.267 (0.761)	0.377 (0.742)	-0.139 (0.646)	0.364 (0.696)	-0.039 (0.657)	-0.382 (0.398)
(Similar to PSI product) * (After) * (Pre-PSI tariff rate)	-0.008 (0.018)	-0.014 (0.017)	0.000 (0.018)	-0.01 (0.026)	0.005 (0.011)	0.000 (0.015)
(After) * (Pre-PSI tariff rate)	0.012 (0.008)	0.012 (0.009)	0.01 (0.008)	0.014 (0.020)	0.009 (0.006)	0.000 (0.008)
Observations	9,314	13,684	9,603	10,918	7,599	9,138
R-squared	0.69	0.58	0.61	0.76	0.64	0.66

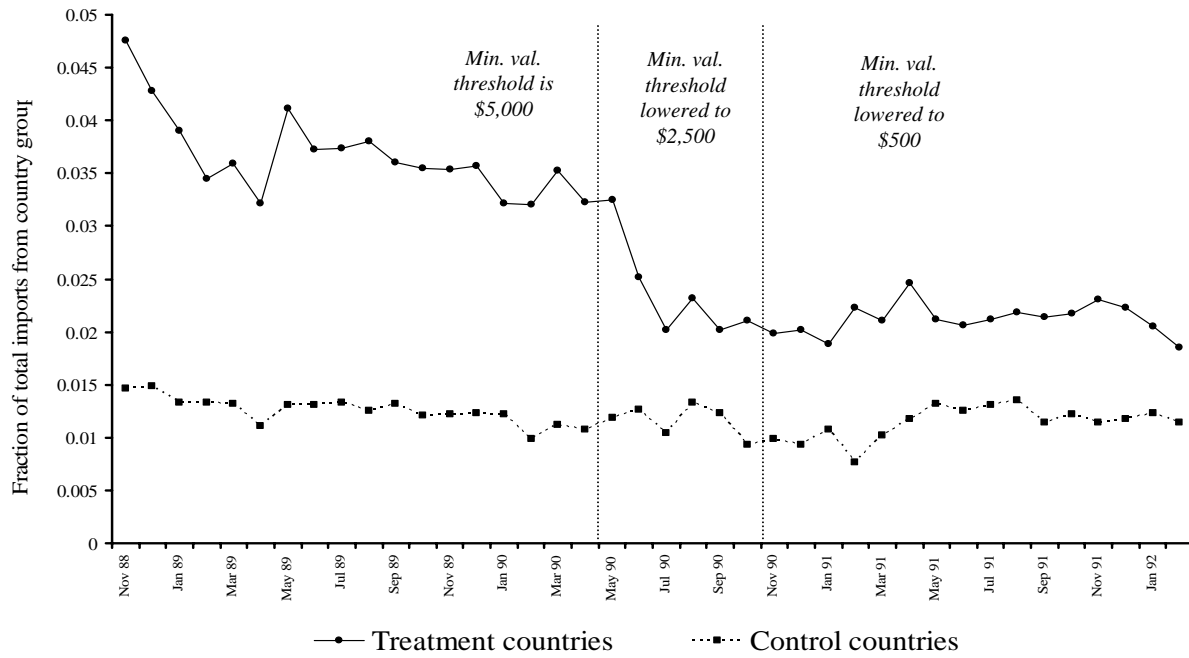
NOTES -- Unit of observation is a 4/5-digit SITC Rev. 3 product in a certain year. Standard errors (clustered by product) in parentheses. Each observation weighted by initial (1993-94) mean annual dollar imports. Years up to 1994 are prior to the imposition of PSI requirements. PSI program started in August 1995. Years 1995 and 1996 excluded from analysis because list of products requiring PSI changed over this period (list finalized in March 1996). PSI program operational for all of 1997 and 1998, and ended in July 1999. "After" is indicator for 1997 or 1998 (except in last column, when it indicates 1993-94). "PSI coverage in 3-digit group" is fraction of 4/5-digit products within 3-digit SITC Rev. 3 group with any PSI requirements. All regressions include fixed effects for year and product. (After)\*(PSI coverage in 3-digit product group) not included because redundant. See Table 1 for variable definitions, data sources, and other notes.

**Figure 1: PSI and Import Duties**



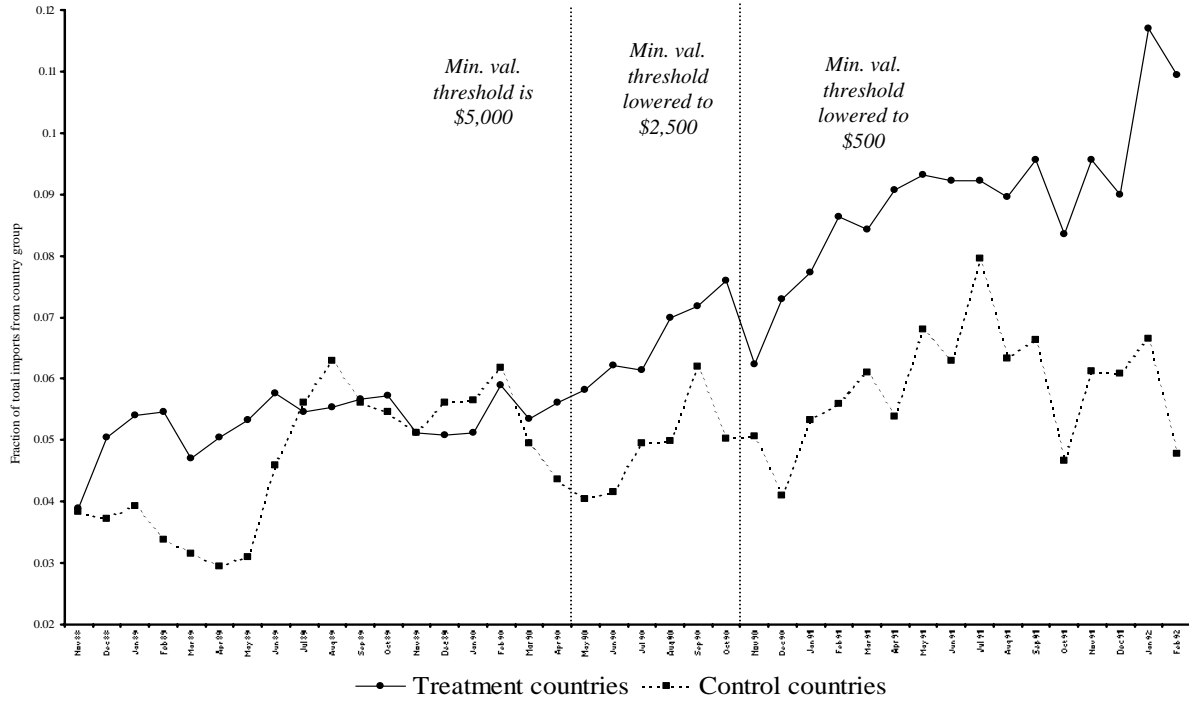
NOTES— Plotted points are coefficients on indicator variables for each year before and after the start of a PSI program, in regression with ln(import duties) as dependent variable. Year 0 is first year that a PSI program has been active for more than half a year. Omitted year indicator is “year -1” (year immediately prior to PSI start year). Dotted lines depict 95% confidence intervals. Other right-hand-side variables are: year fixed effects, country fixed effects, and country-specific linear time trends. Unit of observation is a country-year; see text for sample composition.

**Figure 2: Fraction of total imports entering in shipments valued between \$2,500 and \$5,000 (November 1988 – February 1992)**



NOTES-- Chart plots fraction of total imports by value entering in shipments valued between \$2,500 and \$5,000 in the given month, from treatment (PSI) countries and from control (non-PSI) countries. Treatment countries during the period depicted are Hong Kong, Japan, Taiwan, Brunei, Indonesia, Malaysia, Singapore, South Korea, and Thailand. All other countries are control countries. Shipments in overlapping shipment types (e.g., shipment is both "under \$500" and "destined for export processing zone") are allocated to the low-value types (either "between \$5000 and \$500" or "under \$500"). Data source: shipment database of the National Statistics Office of the Philippines.

**Figure 3: Fraction of total imports destined for export processing zones (November 1988 – February 1992)**



NOTES-- Chart plots fraction of total imports destined for export processing zones in the given month, by country group. Data are smoothed to reduce noise (each data point is a three-month centered moving average). For all other notes, see Figure 2.