

Integrity for Hire: An Analysis of a Widespread Customs Reform

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Abstract

Can governments improve bureaucratic performance by “hiring integrity” from the private sector? In the past two decades, a number of developing countries have hired private firms to conduct preshipment inspections of imports, generating independent data on the value and tariff classification of incoming shipments. I find that countries implementing such inspection programs subsequently experience large increases in import duty collections. By contrast, the growth rate of other tax revenues does not change appreciably. Additional evidence suggests that declines in falsification of import documentation are behind the import duty improvements: the programs also lead to declines in undervaluation and in misreporting of goods classifications. Historically, this hired integrity appears to have been cost-effective, with improvements in import duty collections in the first five years of a typical inspection program amounting to 2.6 times program costs.

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1 Introduction

Corruption and bureaucratic inefficiency are widely cited as important barriers to economic development.¹ 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 along these lines. An ineffective customs bureaucracy may be unable to prevent fraud by dishonest importers seeking to pay less than the legal tariffs due on their shipments. What’s more, corrupt customs officers may be complicit in the revenue fraud, turning a blind eye to falsified import documentation in return for bribes. A corrupt customs agency may also delay incoming shipments (often under the pretext of problems in import documentation) to extract bribes from importers, potentially discouraging import trade.² Revenue drains due to ineffective or corrupt customs agencies can have important consequences, as import duties are important for public finances in the developing world: in 1990, the midpoint year in the sample used in this paper’s analysis, import duties accounted for an average of 23% of central government revenue across developing countries.³

There is little systematic empirical evidence on the effectiveness of specific approaches to improving bureaucratic efficiency or combating corruption. Starting with Becker and Stigler (1974), theoretical work has proposed a number of remedies for bureaucratic corruption, such as increased monitoring and higher wages.⁴ But there are many reasons to be pessimistic about the efficacy of such reforms. Consider, for example, attempts to monitor corrupt or ineffective officials more closely. The individual monitors themselves might themselves be dishonest, and so not provide useful information to higher authorities. Even if lower-level monitors are honest, higher-level authorities might themselves be corrupt and so tolerate or participate in the corrupt dealings.⁵ Empirical work is necessary to determine the effectiveness of any given reform effort.

When there are concerns that lower-level monitors who are part of a reform effort may themselves be corrupt or ineffective, higher authorities may find it appealing to rely on private firms as monitors. For example, securities regulators typically require that the financial statements of publicly-traded firms be audited by certified accounting firms. Hiring private firms as moni-

¹For recent overviews of the relationship between corruption and development, see Bardhan (1997) and Rose-Ackerman (2004).

²However, 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.

³The sample is described in Section 3 below.

⁴Recent contributions in this vein include Besley and McLaren (1993), Mookherjee and Png (1992 and 1995), and Polinsky and Shavell (2001).

⁵This point is made by Cadot (1987), Chand and Moene (1999), and Fjeldstad and Tungodden (2003).

tors may make sense if competition among the private monitors generates strong incentives for integrity. Can "hiring integrity" from the private sector to collect information for bureaucratic reform efforts be effective?

This paper is the first empirical analysis of a bureaucratic reform involving hired integrity. In the past two decades, over 50 developing countries have tried a specific approach to customs reform with the 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 origin countries, and forward this information to the client government. In nearly all cases, however, the responsibility for collecting customs duties remains in the hands of the importing country's customs officials. A client government seeks to take advantage of an inspection firm's reputation for honesty, essentially "hiring integrity" from the firm to provide objective data on the contents of imported shipments.

There are various channels through which preshipment inspections can reduce customs fraud, and eventually lead to higher import duty collections. First, PSI, as an independent source of information, improves the monitoring ability of enforcers at various levels: it can help honest customs officers in identifying fraudulent import documentation, and can also help higher-level enforcers prosecute customs officers who may be allowing or encouraging such fraud. In the absence of PSI, uncovering customs fraud requires time-consuming investigative work, and is made particularly difficult by the large number of separate import transactions. PSI helps investigators identify import transactions where duties (as calculated from the PSI report) diverge substantially from duties actually collected, 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-a-vis a corrupt customs officer, potentially reducing customs clearance times.⁶

However, the success of preshipment inspection programs is far from guaranteed. Success requires client governments to actually use the PSI-generated information to seek out and prosecute

⁶Low (1995) and Jenkins (1992) cite survey evidence that PSI was accompanied by dramatic reductions in customs clearance times in Indonesia.

corrupt importers or customs agents. Governments may simply be hiring 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 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. So PSI may raise importers' costs, reduce import volumes, and ultimately reduce duty collections. Furthermore, importers whose costs are raised by PSI may seek out alternative methods of avoiding import duties. In a detailed analysis of a pre-shipment inspection program in the Philippines between 1989 and 1992, Yang (2006) finds that expansion of import monitoring caused substantial displacement of imports to unmonitored import categories, so that the hypothesis of zero change in import duty avoidance cannot be rejected.

It is therefore an open question whether, on average across many countries, PSI programs help raise import duty collections. The empirical analysis uses panel data on country-level outcomes to examine the relationship between the implementation of PSI programs and import duty collections for the years 1980 to 2000. I find that PSI programs are associated with increases in import duty collections, by 15-30 percentage points in the first five years after implementation. Additional evidence suggests that reductions in the falsification of import documentation are behind the revenue improvements: PSI programs are accompanied by declines in underinvoicing and in misreporting of goods classifications in customs. Pre-shipment inspection appears to be cost-effective: improvements in import duty collections in the first five years of a typical inspection program were 2.6 times program costs.⁷

The crucial empirical question is whether the association between PSI programs and improvements in import duties reflects the causal impact of PSI. For instance, if countries implement PSI programs at the same time as they make substantial public finance reforms, it may be that the observed import duty growth is not due to PSI, but rather to other actions the country takes coinciding with the introduction of PSI. I use several approaches to 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

⁷These findings are not inconsistent with the results in Yang (2004), as the current paper estimates PSI's average effect across many countries, of which the Philippines is only one. It appears that in the Philippines between 1989 and 1992, importers did find that PSI raised their costs, and sought out alternative duty-avoidance methods. Switching to alternative methods was possible because the Philippine PSI program was only a *partial* PSI program during those years: only a defined subset of import categories amounting to less than 50% of imports were subject to the inspections. By contrast, most PSI programs provide much less opportunity for displacement. The Philippine program was eventually expanded (in March 1992) to cover essentially all imports, reducing substantially the opportunities for displacement.

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 document 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, it might be that concurrent reforms specific to the customs agency (other than PSI) are the true causal factor behind the change import duties. While it is difficult to obtain data on organizational reforms within customs across countries and over time, data does exist 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 flexibly for the current average tariff rate.

Of course, there may be still be other unobserved policy changes taking place alongside PSI. An innovation of this paper 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,⁸ further bolstering the case for PSI's causal impact.

To the extent that the estimated effects of PSI on import duty collections in part are due to reductions in customs corruption, this paper is related to a nascent empirical literature on the impact of monitoring on bureaucratic corruption worldwide. Di Tella and Schargrodsky (2003) examine the impact of increased enforcement on corruption in hospital procurement in Argentina. Olken (2004) provides field experimental evidence on how different types of monitoring affect corruption in Indonesian road projects. In Uganda, Reinikka and Svensson (2004) find that capture of government funds intended for education is reduced when intended funding levels are publicized in newspapers. In a U.S. private-sector context, Nagin, Rebitzer, Sanders, and Taylor (2002) use a field experiment to document the impact of increased monitoring on opportunistic behavior by telephone call center employees.

This paper also relates to research on avoidance of taxes on international trade. Existing work documents the existence of import duty avoidance, but does not examine the impact of

⁸Specifically, fixed effects for each distinct policy regime are included in the regressions.

enforcement on these activities (with the exception of Yang (2006)).⁹ Pritchett and 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. Fisman and Wei (2004) find that the extent of import underinvoicing rises in the tariff rate among Chinese imports from Hong Kong. A number of authors examine tax-induced transfer pricing within multinational firms (Bernard and Weiner (1990), Hines and Rice (1994) and Clausing (1998), among others). In the related realm of income tax evasion, Klepper and Nagin (1989) examine cross-sectional correlates of income underreporting on specific line items of US tax returns, and Slemrod, Blumenthal, and 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 paper is organized as follows. Section 2 provides background on pre-shipment inspection programs worldwide. Section 3 presents the empirical evidence on the impact of pre-shipment inspection on import duty collection and on the channels that appear to mediate PSI's effects, and conducts several robustness checks. Section 4 concludes.

2 Background on pre-shipment inspection

A handful of multinational inspection firms—all headquartered in Europe—provide pre-shipment inspection 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 des-

⁹Slemrod and Yitzhaki (2002) appeal for research on the responses of tax evaders to greater enforcement.

tinuation country, the client government can use the PSI firm’s assessment to identify dishonest importers as well as 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 Rege 2001).

Preshipment inspection originated as a product innovation by the Geneva-based firm Societe Generale de Surveillance (SGS), which proposed and implemented the first PSI program for the government of Indonesia in 1985. SGS’s role as innovator has allowed it to maintain its position as the largest firm in the industry, but three other firms are also major players, with numerous contracts worldwide: Bureau Veritas (based in Paris), Cotecna (Geneva), and Inchcape Testing Services (London). Adoption of PSI by other countries was slow at first, with Guinea and Bolivia implementing programs in 1986, followed by the Philippines in 1987. In 1990, PSI programs were active (for at least part of the the year) in 13 countries. Thereafter, adoption was more rapid, so that in 1995 PSI programs were active in 34 countries. After 1995, use of PSI among developing countries was roughly stable, with between 35 and 39 active programs in each year through 2000. A total of 50 countries had implemented PSI programs for some period of time by the end of 2000.

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 realm 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 in the order of US\$500 million annually during the same years.¹⁰

¹⁰For 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.

3 Empirical evidence on the impact of preshipment inspection

This section documents the impact of preshipment inspection programs on import duties collected by national governments. I first describe the data sources used in the empirical analysis, and discuss systematic differences between countries that did and did not implement preshipment inspection programs in the 1985-2000 period. I then present the main empirical results on the relationship between PSI programs and import duties, and conduct several robustness checks. The remainder of the empirical section provides evidence on the channels through which PSI's effects operate, and discusses the cost effectiveness of PSI.

3.1 Data sources and sample composition

The main outcome variable is the natural log of import duty collections, which is reported annually in World Development Indicators 2004 (WDI 2004).¹¹ There are several occasions when reported import duty collections are very different from other values of the same variable for the same country, and are highly likely to be reporting errors. So I replace a reported observation of log import duties with a missing value if it takes a value greater than 4 standard deviations away from the mean of other reported import duties for the same country.¹²

The independent variables of interest, related to the existence and age of countries' PSI programs, require data on the start and end dates of such programs. 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 2000.¹³

Other tax revenues (excluding import duties) and average tariffs are used as control variables in the main regression analyses. Data on other tax revenues are from WDI 2004, and tariff data are compiled from various sources by the World Bank's trade research group.¹⁴ The tariff data

¹¹Unless otherwise specified, all data in monetary units are in current US dollars.

¹²All told, this replacement affects just 10 observations that would otherwise have been included in the sample. Of these 10, only two are for PSI countries observed before and after the start of a PSI program (and so would affect the estimate of PSI's effect): Democratic Republic of Congo (the former Zaire) in 1998, reported to be \$1.18 million (reported import duties for other years range from \$80 million to \$396 million); and Belarus in 1992, reported to be \$18 million (reported import duties for other years range from \$123 million to \$344 million). Omitting these 10 outliers turns out to have little effect on the ultimate regression estimates, as will be shown in the table of robustness checks to follow in subsection 3.3.3.

¹³The handful of remaining PSI firms had contracts that entirely overlapped with those of the four largest firms, so that the four largest firms' contracts provide a complete accounting of past programs.

¹⁴The tariff data (including details on the sources used) are available at: <http://siteresources.worldbank.org/INTRANET/TRADE/Resources/tar2002.xls>

are simple average tariffs across all tariff lines. The tariff data contain a number of missing values; when missing values occur in between years of available data, I fill in missing values via linear interpolation between the two non-missing years that bracket the missing data.

Bilateral trade data used in the construction of measures of mis-reporting in customs are from the World Bank's Trade and Production dataset. Some subsidiary regressions use data on per capita GDP (from WDI 2004), a survey measure of bureaucratic corruption (from the International Country Risk Guide, ICRG), and import data from an alternative source (IMF Direction of Trade Statistics).

The first PSI contract started in 1985, so I limit the analyses to the years 1980 through 2000. PSI is used exclusively in developing countries, so I restrict the sample to countries in Africa, Asia, Europe, and Latin America/Caribbean that are not classified as 'high income' by the World Bank.¹⁵ I also drop countries from the analysis if they have complete data for less than three years between 1980 and 2000.¹⁶

The largest resulting sample contains 1,372 observations from 104 countries. 19 of these countries are observed in this sample before and after the start of their PSI programs (and so directly contribute to the estimated effect of PSI on import duty collections). 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). The bottom rows of each results table will indicate the number of countries included in the regression and the number of PSI-using countries observed before and after the start of their PSI programs. The panel is unbalanced, with the number of observations varying across countries depending on data availability.¹⁷

Table 2 presents summary statistics for the observations included in the sample. The unit of observation is a country-year. "PSI" is an indicator variable for whether a given country had an active PSI program for at least half of the given year; 9 percent of observations have an active PSI program. The median observation had \$228 million in import duties, and \$1 billion in other tax revenues. Import duties as a share of total tax revenues has a median of 0.19, and a mean of 0.23. Median imports are \$2.4 billion and the median simple average tariff rate is 19 percent.

¹⁵Constructing the sample this way eliminates Pacific island nations and dependencies, none of which have ever used PSI, and which are not likely to have served as useful controls.

¹⁶Including such countries does not contribute to the analysis, as the outcomes for countries with only one or two observations are entirely explained by the country fixed effect and the country-specific linear time trend.

¹⁷The regression results are robust to conducting the estimation on more balanced panels (limiting the sample to countries that are observed for most of the sample years), as will be discussed in subsection 3.3.3.

3.2 Which countries adopt PSI, and when?

Prior to proceeding to the main empirical analysis, it is useful to shed light on the kinds of countries that eventually adopt PSI programs. Table 3 presents results from cross-country regressions where the dependent variable is an indicator for a country implementing a PSI program sometime between 1985 (the year of the world’s first PSI program) and 2000. The right-hand-side variables are values in the first year of non-missing data between 1980 and 1984. (The countries in the sample are a subset of those in the sample used in the main results of this paper, because not all countries have complete data in the years 1980-1984.)

The first four columns of Table 3 are regressions of the indicator for PSI adoption on each independent variable separately. Two coefficient estimates are negative and highly statistically significant: countries with lower per capita GDP and with more bureaucratic corruption are more likely to use PSI. In column 5 all independent variables are included in the regression, and both per capita GDP and bureaucratic corruption remain statistically significant at conventional levels. In sum, countries that were poorer and that were judged to have higher bureaucratic corruption in the early 1980s were more likely to adopt PSI programs between 1985 and 2001.

The pre-existing differences documented in Table 3 between PSI and non-PSI countries suggest that it would be invalid to infer the impact of PSI by simply comparing PSI and non-PSI countries at some point in time. Instead, it is crucial that the impact of PSI be inferred from *changes* in import duty collections for PSI-using countries between pre- and post-PSI periods, in order to account for time-invariant differences between countries that do and do not implement the program.

It is also useful to understand why PSI-using countries adopt PSI in specific years. Table 4 presents hazard ratios on various time-varying characteristics of countries from a Cox proportional hazard model of years until implementation of PSI for the countries in the dataset. Years until implementation are measured from 1980. The unit of observation is the country-year, with years after the first year of PSI adoption eliminated and censoring occurring for countries that had not adopted PSI by 2000. In the first column, where 100 countries are included in the estimation, higher GDP growth in the previous year (from year $t - 2$ to $t - 1$) makes a country less likely to implement PSI in a given year t , while estimated impacts of changes in import duties or having a new International Monetary Fund program (current or lagged) are not statistically significantly different from zero.¹⁸ Higher growth in import duties (from $t - 1$ to t) also is associated with

¹⁸Data on the timing of IMF programs across countries were graciously provided by the IMF’s Independent Evaluation Office.

implementing a PSI program in year t , but this almost surely better interpreted as a causal effect of PSI on import duties (to be documented further in subsequent tables).

It is also of interest to examine the impact of changes in leadership on PSI adoption, so the second column includes indicator variables for current and lagged changes in leadership positions likely to influence PSI adoption: the head of state, the minister of finance, and the head of customs. (These data are described in more detail in Section 3.3.4.) Due to difficulties in obtaining leadership data, this regression is restricted to observations for countries that ever adopted PSI in the 1980-2000 period. Having a new minister of finance in the previous year ($t - 1$) makes a country more likely to implement PSI in year t .¹⁹ Other factors do not have statistically significant relationships with adoption in this subsample, such as economic growth, changes in import duties, having a new IMF program, or changes in the identity of the head of state or the head of customs.

3.3 The impact of PSI

I estimate here the relationship between the implementation of PSI programs and changes in total import duties collected, total imports, and the extent of import mis-reporting in customs. The main empirical approach is detrended differences-in-differences estimation. For outcome variable Y_{jt} (say, log import duties) for country j in year t , the basic regression equation is:

$$Y_{jt} = \alpha PSI_{jt} + \boldsymbol{\zeta}' \mathbf{X}_{jt} + \mu_j + \delta_t + \gamma_j TREND + \varepsilon_{jt} \quad (1)$$

PSI_{jt} is an indicator variable for whether country j had an active PSI program for at least half of year t . \mathbf{X}_{jt} is a vector of contemporaneous control variables (discussed below). Country fixed effects μ_j control for time-invariant differences across countries. Year fixed effects δ_t control for changes common to all countries in the same year. $TREND$ is a linear time trend. Country-specific time trends (γ_j , the country-specific coefficient on the linear time trend) help account for the effect of slow-moving changes over time that occur throughout the sample period, and that differ across countries. ε_{jt} is a mean-zero error term.

The coefficient of interest is α , the effect of having a PSI program on the outcome variable. The specification imposes that the estimated effect of PSI is the same in all years when a PSI program is active. However, for various reasons the association between PSI and a given outcome variable may vary with the age of the PSI program, and so I also estimate a regression equation where

¹⁹This latter result provides justification for later analyses with policy-regime fixed effects in Section 3.3.4.

the effect is allowed to vary across earlier and later years of a PSI program:

$$Y_{jt} = \beta_1 PSI_NEW_{jt} + \beta_2 PSI_OLD_{jt} + \zeta' \mathbf{X}_{jt} + \mu_j + \delta_t + \gamma_j TREND + \varepsilon_{jt} \quad (2)$$

Here, PSI_NEW_{jt} is an indicator variable equal to 1 if a PSI program has recently been initiated (in practice, this will be PSI programs in their first 5 years of operation). PSI_OLD_{jt} is an indicator variable equal to 1 if a PSI program has been operating for some time (in practice, in its 6th year of operation or after).

Serial correlation in the outcome and PSI variables are likely to be problems in this panel dataset, biasing OLS standard error estimates downward (Bertrand, Duflo and Mullainathan (2004)), so standard errors allow for an arbitrary variance-covariance structure within countries (standard errors are clustered by country).

The primary identification worry is that, simultaneous with PSI, changes in policy or broad economic conditions may occur that *also* affect import duty collection. For example, the implementation of PSI could coincide with the installation of a more honest, technocratic government (or, more narrowly, a more effective minister of finance or head of the customs agency) that is better at collecting taxes overall. Or PSI could coincide with periods of higher economic growth, which raises tax collections simply via increases in taxable economic activity. If PSI programs are indeed accompanied by more technocratic government, or by higher economic growth, the estimated impact of PSI on import duty collections would be biased upward.

To account for such concurrent changes, I include the natural log of other tax revenues (total taxes minus import duties) in the vector of contemporaneous controls \mathbf{X}_{jt} when estimating the impact of PSI on import duties. Other tax revenues (which include revenue from consumption taxes, income taxes, and social security taxes) should be a useful proxy for the general factors affecting overall tax revenue collections (honesty/ability of high government officials, or economic growth) to the extent that these general factors have similar effects on import duties and on other tax revenues.²⁰ In addition, changes in tariff rates, non-tariff trade barriers, or organizational reforms in customs could change simultaneously with PSI and be the true causes of any observed change in import duties. By nature, it is substantially more difficult to find measures of these types of changes. However, information on tariff rates are available for a subset of countries and years. I will therefore test the sensitivity of the results to inclusion of a country's simple average

²⁰It is also of interest to consider other tax revenues as a comparison group for import duties, as I do in the graphical analysis of the next subsection.

tariff rate in the vector of controls \mathbf{X}_{jt} . To the extent that other types of changes in customs are correlated with changes in tariff rates, inclusion of this control may also capture the impact of those changes.

Of course, there may be other policy changes affecting import duties that are unobserved. I test the sensitivity of the results to controlling for unobserved policy changes that are associated with distinct "policy regimes" within countries (to be described in section 3.3.4 below).

3.3.1 Graphical analysis

Prior to discussing the empirical results, a graphical view of the relationship between import duties and PSI programs is informative. 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 immediately prior to the starting year of the program, year 0 is the starting year, etc.)

Formally, the conditional means are generated by running the following regression on the 1,372-observation sample described in subsection 3.1, where the outcome variable is log import duties:

$$\begin{aligned}
 Y_{jt} = & \theta_{-20}PSI_{-20jt} + \theta_{-19}PSI_{-19jt} \\
 & \dots + \theta_{-1}PSI_{-1jt} + \theta_0PSI_{0jt} + \theta_1PSI_{1jt} \\
 & \dots + \theta_{13}PSI_{13jt} + \theta_{14}PSI_{14jt} \\
 & + \mu_j + \delta_t + \gamma_j TREND + \varepsilon_{jt}
 \end{aligned} \tag{3}$$

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 were described in the discussion of the main regression equation 1 above (year fixed effects, country fixed effects, and country-specific linear time trends). The points comprising the solid line in Figure 1 are the coefficients θ_{-20} through θ_{14} on these indicator variables, and the dotted lines depict the 95% confidence intervals of each coefficient estimate.

The figure reveals that the conditional mean of log import duties for countries using PSI shows a marked positive change immediately after the PSI start year. By contrast, there is no obvious

trend prior to the PSI start year, apart from a very gradual long-term decline. In the immediate pre-PSI years (years -9 to -1), the graph is quite flat, suggesting that the later increase in import duties is unlikely to be driven by mean reversion. Each coefficient on indicators for years after PSI start 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.

To gain confidence that this increase in import duties is not being driven by unobserved changes occurring in countries concurrently with PSI programs, it is useful to conduct the same graphical analysis for an outcome that should be unaffected by preshipment inspection, but that is likely to respond to similar third factors influencing tax collections overall (a change in government, or economic growth). As mentioned above, other tax revenues (total taxes minus import duties) is such an outcome. The more similar is the graph for other tax revenues to the graph for import duties, the more concerned one might be that unobserved changes aside from PSI are explaining the post-PSI growth in import duties.

Figure 2 presents regression coefficients and standard errors from a regression identical to equation 3 above but where the outcome variable is the log of other tax revenues. (For comparison, the vertical axes are identical in Figures 1 and 2.) The graph is essentially flat for several years before and after the PSI start year. Starting from roughly year 9, there appears to be a slight increase in log other tax revenues, but the size of this increase is quite small compared to the corresponding change for log import duties.

To the extent that other tax revenues are a reasonable proxy for unobserved changes affecting tax revenues overall, Figure 2 provides little reason to be concerned that such unobserved changes are the primary drivers of the post-PSI changes in import duties, particularly in the years immediately following the PSI start year. Even so, the empirical analyses to follow will account for the impact of country-level policies affecting tax revenue collection overall by controlling for the log of other tax revenues.

3.3.2 Main regression results: impact of PSI on import duties

The graphical analysis indicates that PSI was accompanied by an increase in log import duties. This subsection shows that this conclusion holds in a more parsimonious specification, where the impact of PSI on import duties is not allowed to vary completely flexibly for every year before and after PSI start. I constrain the effect depicted in Figure 1 to be summarized in an overall PSI effect α (as in equation 1), and also separate effects for the earlier and later stages of a PSI

program (parameters β_1 and β_2 in equation 2).

Table 5 presents regression results for equations 1 and 2 that are more or less inclusive of the equations' right-hand-side control variables. All equations in the table include country fixed effects, year fixed effects, and country-specific linear time trends as right-hand side variables.

Columns 1 to 4 present results for the largest (1,372-observation) sample. In the first column, the regression includes only the dummy variable PSI_{jt} for a PSI program being active for a given country-year. The coefficient on PSI_{jt} is positive and statistically significant at the 5% level. In column 2, the log of other tax revenues is included in the regression. The coefficient on PSI_{jt} is essentially unchanged in magnitude, and becomes statistically significant at the 1% level.

In the next two columns, versions of equation 2 are estimated, allowing the effect of PSI to vary across relatively new versus older PSI programs. PSI_NEW_{jt} is an indicator variable equal to 1 if an active PSI program in its first 5 years of operation (years 0-4), while PSI_OLD_{jt} is equal to 1 if an active PSI program is in its 6th year of operation or after (years 5+). The coefficient estimates in columns 3 and 4 (without and with the control for log other tax revenues, respectively) are quite similar. Having a PSI program in its first 5 years of operation is associated with log import duties that are higher by 0.20, while the coefficient on the indicator for PSI programs in years 5+ is roughly twice that magnitude.²¹

To gauge whether changes in customs itself concurrent with PSI are a likely source of omitted variable bias, it is important to also control for the simple average tariff across tariff lines. So columns 5 to 10 limit the observations to those with complete tariff information. Columns 5-8 are identical specifications to columns 1-4 respectively, to confirm that the change in sample composition does not materially affect the estimates. The coefficients on PSI_{jt} and on PSI_NEW_{jt} and PSI_OLD_{jt} are very similar in magnitude to the results in the larger sample, and not greatly affected when the control for $\ln(\text{other tax revenues})$ is included.

In columns 9 and 10, a linear control for the tariff rate is included in both regressions, and the respective PSI coefficients are essentially unchanged in magnitude and statistical significance levels. There is no indication that the estimate of PSI's effect is confounded by concurrent changes in the average tariff rate within countries (or by other factors specific to import duty collection that tend to change in the same direction as tariffs).²²

²¹These patterns are not highly sensitive to alternative definitions of PSI_NEW_{jt} and PSI_OLD_{jt} , such as placing the cutoff between "new" and "old" programs at 3, 4, or 6 years.

²²The reason why the coefficient estimates of PSI's effect do not change substantially when controls are added to the regression for other tax revenues and the tariff rate is that neither of these variables change materially with the introduction of PSI. Appendix Table 1 presents regression results from estimation of equations 1 and 2 where the outcome variable is $\ln(\text{other tax revenues})$ in columns 1 and 2, and the simple average tariff rate in columns

One reason why the coefficient on PSI_OLD_{jt} tends to be larger than the coefficient on PSI_NEW_{jt} could be that PSI programs become more effective over time. It is possible that client governments may need time to set up the information systems and install the skilled and honest enforcers that are necessary for effective use of program-generated information. Higher authorities also presumably learn over time the best ways to use the new information to identify and prosecute corrupt customs officers. Learning could also take place on the part of the private firms, who may need some time to acquire expertise in pricing a particular country's basket of imports. On the other hand, there is the possibility that increases in log import duties in later years of PSI programs may be due to other policy changes that occur some time after PSI programs are implemented (on this issue, see the discussion in section 3.5 below).

All told, the association between PSI programs and log import duties appears larger in later years of a program's existence. So subsequent analyses in this paper allow effects of newer and older programs to differ (as in equation 2).

3.3.3 Robustness checks

It is important to test the robustness of the main empirical results to alternative sets of assumptions. Table 6 presents regression results from a range of additional specifications of the main regression equation 2. Each column is a different specification. Columns 1 to 5 of the table should be compared with column 4 of Table 5, the specification controlling for log other tax revenues.

The samples used in the regressions of Table 5 are unbalanced: the countries included in the sample vary substantially in the number of observations, ranging from 3 to 21 observations over the 1980-2000 period of analysis. One might be concerned that patterns of entry into and exit from the sample may be driving the empirical results. So columns 1 and 2 of Table 6 present regression results when the sample is restricted to countries that are observed for all or nearly all years. In column 1, countries are included in the sample if they are observed for 15 or more years during the period of analysis; in column 2, the threshold is 18 or more years. The results provide no indication that the use of an unbalanced panel in the main regressions affects the fundamental conclusions. The coefficients on the PSI variables are positive and highly statistically significantly different from zero in both subsamples, and are very similar in magnitude to those in column 4 of Table 5.

The main estimation sample includes a number of very small countries whose trends in import

3 and 4. The coefficients on all PSI variables are all small in magnitude and none are statistically significantly different from zero.

duties may not serve as useful counterfactuals (in particular, small island nations such as St. Vincent and the Grenadines, the Seychelles, and the Maldives). So the regression results in column 3 of Table 6 are for a sample that excludes observations for countries with populations under 1 million (on average from 1980-2000). Exclusion of small countries from the regression has very little effect on the results: the coefficients on the PSI variables are essentially identical to those in column 4 of Table 5.

There are a few observations in the dataset for countries that used PSI in the past, but that no longer do so. One might argue that these observations should not be considered controls, since any impact of PSI could persist beyond the end of a PSI program. In column 4, I estimate a separate ‘Past-PSI’ effect by including a variable in the regression analysis ($PastPSI_{jt}$), which is an indicator for a country *not* using PSI for at least half a year, in a year after the *end* of a previous PSI program. With the inclusion of the $PastPSI_{jt}$ variable, the regression estimates of the impact of PSI in effect only derive from changes in outcomes associated with the adoption (not the elimination) of PSI programs. As it turns out, in this specification the coefficients on the PSI variables are essentially unchanged, and the coefficient on $PastPSI_{jt}$ is itself small and statistically insignificant.

Column 5 presents regression results when a small number of outliers of log import duties (previously excluded) are included in the regression. The inclusion of these outlier observations reduces the coefficient on PSI_NEW_{jt} only slightly (from 0.199 to 0.169), and the coefficient remains statistically significantly different from zero. The coefficient on PSI_OLD_{jt} is essentially unchanged.

Finally, one might be concerned that linear controls for log other tax revenues and the simple average tariff rate are not sufficiently flexibly specified to properly account for other changes concurrent with PSI that may also affect import duty collections. So in column 6, I control for these two variables specified very flexibly, as 10-piece linear splines. (These regression results should be compared with those in column 10 of Table 5). Controlling more flexibly for these two variables has very little effect on the estimated coefficients, providing little indication that one should be concerned about improper specification of log other tax revenues and the tariff rate.

3.3.4 Controlling for policy regime

In interpreting the coefficient estimates on PSI_NEW_{jt} and PSI_OLD_{jt} in Tables 5 and 6, the central issue is whether the association between PSI and changes in import duties reflects the

causal impact of PSI, rather than the influence of other concurrent policy changes. The stability of the coefficient estimates on the PSI indicators when including controls for the tariff rate and for log other tax revenues provides the first evidence for PSI having a causal effect. However, a lingering concern is that there may be other unobserved policy changes taking place alongside PSI, and that are the true causal factors behind the increase in import duties.

It should therefore be useful to estimate the impact of PSI in the midst of a period of relative stability in countries' economic policies. One way to do so would be to include fixed effects in the regression for each distinct "policy regime" for each country, a period in which a country's policies could be taken to be generally stable. For example, if country X was characterized by two distinct policy regimes (1 and 2) during the sample period, one could replace the single country X fixed effect with two separate fixed effects for regime "X1" and regime "X2". The impact of PSI would then only be identified from variation in PSI implementation within (and not across) policy regimes.

The key question is how one defines a policy regime using data that are reasonably consistent and obtainable for a large number of countries. The approach taken in this section starts with the following assumption: economic policies are likely to be relatively stable in periods when countries' political and bureaucratic leadership is constant, compared to periods when leadership is changing. This requires that the variation in policies enacted *across* leaders is greater than variation in policies enacted *within* a given leader's term of office.²³

I therefore define policy regimes in practice as periods in which a country's leadership structure is unchanged. In particular, I focus on three leadership positions that are likely to matter for customs reforms and import duty collections. First, the *national leader* is clearly important for setting high-level goals on anti-corruption efforts and revenue collection. Second, the *finance minister* sets priorities between collection of customs duties versus other types of government revenue, and may direct changes in collection and enforcement methods. Third, the *head of the customs agency* implements a variety of policies affecting revenue collection and enforcement against corruption and fraud in customs.

I construct indicator variables for separate country-specific policy regimes, defined as periods in which the individuals occupying these three leadership positions leaders were all unchanged. I collected data on heads of customs from a variety of in-country sources including government agencies, academic institutions, media outlets, and NGOs.²⁴ Data on national leaders and finance

²³Jones and Olken (2005) find that exogenous changes in the identity of the national leader lead to shifts in country growth rates and to changes in economic policy.

²⁴In small spans of years for four countries, information on the head of customs was not available, and I assumed

ministers come from four published sources: the Statesman’s Yearbook, the International Yearbook and Statesmen’s Who’s Who, the Europa World Yearbook, and the Worldwide Government Directory. In cases where there was more than one of a given leader type in a given year, the leader was taken to be the one who had served the longest in that position in that year. The leadership data were collected for the 19 countries whose import duties were observed before and after the start of their PSI programs (listed in Table 1), and so policy regime fixed effects were only defined for those countries.²⁵ An example of the data collected is given in Appendix Table 2, for Indonesia. The periods of constant leadership across the three positions separate Indonesia into 9 policy regimes, indicated by the horizontal lines in the table.

The empirical exercise involves gauging the stability of the coefficient estimates in equations 1 and 2 to inclusion of the policy regime fixed effects. If coefficient estimates decline substantially in magnitude, this would suggest that the association between PSI and increases in import duties is mainly due to other concurrent policy changes that accompany leadership changes. On the other hand, if coefficient estimates remain stable, this would bolster the case for PSI’s causal impact on import duties.

Results are presented in Table 7. The baseline specifications (from Table 5) are provided for comparison in the odd-numbered columns of the table, while the specifications controlling for policy regime fixed effects are in the even-numbered columns. In columns 1-4, the sample consists of all observations with import duty data (as in columns 1-4 of Table 5).

The coefficients on the PSI indicator in column 2 (PSI_{jt}) and on the new PSI indicator in column 4 (PSI_NEW_{jt}) are actually larger in magnitude than the corresponding baseline coefficients, and each remains statistically significant at conventional levels. For example, the coefficient on PSI_NEW_{jt} is 42% larger in the specification with policy regime fixed effects than in the baseline specification. The coefficient on PSI_OLD_{jt} in column 4 remains positive and statistically significantly different from zero, but its magnitude has declined by roughly one-fourth. The patterns are similar in the sample with controls for the tariff rate (columns 5-8): in the specifications with policy regime fixed effects, coefficients on PSI_{jt} and on PSI_NEW_{jt} are larger in magnitude than in the baseline specification (and remain statistically significant

that the head of customs was unchanged during these years (Paraguay 1980, Peru 1980-1984, Pakistan 1980-1985 and Kenya 1980-1986). None of these countries implemented PSI during these periods, so this assumption should not affect the results. Links to the leadership data as well as further details on data collection methods and sources can be found at the following website: www.umich.edu/~deanyang/papers/papers.html

²⁵Creating policy regime fixed effects only for these 19 PSI countries in Table 1 is sufficient for the purpose of this analysis, as the focus is on how inclusion of these fixed effects affects the estimated impact of PSI. Implicitly, all other countries are assumed to each have a single policy regime, captured by the country fixed effects.

at the 10% level), and the coefficient on PSI_OLD_{jt} declines slightly but remains statistically significant.

The results in Table 7 lend support to PSI's having a causal impact on import duties, particularly in the the early years of a PSI program (years 0-4). When controlling for unobserved policy changes that tend to coincide with changes in national and bureaucratic leadership, the estimated impact of PSI programs in the first 5 years (the coefficient on PSI_NEW_{jt}) actually rises in magnitude. PSI programs appear to have larger effects (in their initial years) when their impact is estimated solely from program variation within periods characterized by unchanged governmental leadership.²⁶ This may reflect the fact that changes in leadership tend to cause disruption that hampers import duty collection.

Of course, these findings need to be taken with the usual caveats. A main concern is the potential endogeneity of policy regimes. For example, policy regimes that are generally more successful (at import duty collection, among other things) may have longer tenure, while policy regimes with lackluster performance may end quickly (via replacement of, say, the head of customs or the finance minister). Inclusion of policy regime fixed effects would then increase the estimated effect of PSI, because identification would tend to derive from variation in PSI within relatively successful policy regimes. In sum, these results most convincingly support the causal impact of PSI if one believes that governmental leadership is relatively exogenous with respect to the success of import duty collection efforts, and if policy changes within policy regimes (so defined) indeed tend to be fewer and less extensive than policy changes across regimes.

3.4 Channels of PSI's effect on import duties

If PSI is accompanied by improvements in import duty collections, the question remains as to how these improvements come about. Whenever import duties paid are lower than the amounts legally due on a shipment, official import documentation must be altered to hide evidence of such theft. PSI could help raise import duty collections by simply making it more difficult for importers or customs officials to falsify import documentation. In addition, if customs corruption leads to higher prices paid for imports by domestic consumers, PSI could lead to reduced import prices, higher import demand, and increased import duty collections.

In this section, I first examine the impact of PSI on measures that are likely to capture

²⁶However, this statement about relative coefficient sizes should be taken as merely suggestive. Standard errors are too large to reject the hypothesis that the coefficients have remained the same between the specifications with and without the policy regime fixed effects.

two types of mis-reporting: 1) under-reporting of import values, 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 to simply 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.²⁷ All other things equal, countries with less undervaluation in customs should have higher import capture ratios.

Essentially, the export reports of trade partner countries become the benchmark against which the corresponding import data are to be compared. But due to transport costs and export misreporting, cross-sectional differences between countries' import capture ratios cannot be completely ascribed to differences in undervaluation.²⁸ That said, fixed effects and country-specific time trends included in the estimation will account for level and trend differences in the import capture ratio across countries. 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 in destination countries. Using a measure such as the import capture ratio also presumes that undervaluation does not also 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 the destination country's customs officers should 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, as there is essentially no sharing of export and import statistics between exporting and importing countries for the purposes of customs enforcement.

To construct import capture ratios, I use the World Bank's Trade and Production dataset.²⁹

²⁷The basic strategy of inferring underinvoicing from discrepancies between a country's import data and its trade partners' export data has a long history. See, for example, Morgenstern (1950), Bhagwati (1964), Naya and Morgan (1969), and De Wulf (1981). Most recently, Fisman and Wei (2004) use a similar measure at the disaggregated product level to demonstrate the relationship between tariffs and underinvoicing in China-Hong Kong trade.

²⁸Import data reported by destination countries typically 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').

²⁹The crucial feature of this dataset is its inclusion of a country's import data as well as the corresponding export data from partner countries. (In addition, the trade data is also disaggregated by product, which will be useful for the next measure of mis-reporting, discussed below.) The number of observations in the sample falls due to the more limited inclusion of countries in this dataset. The resulting sample includes 581 observations from 39 countries, 9 of which are observed before and after the start of their PSI programs.

Discrepancies in some trade data items creates import capture ratios that are in some cases quite extreme (very small and very large). To reduce the influence of such extreme values, I focus on the log of the import capture ratio. The sample mean of the log import capture ratio is -0.25, with a standard deviation of 0.60.

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 and thus lower import duty payments. As a quantitative measure of the extent of mis-reporting of goods classifications, I use the *coefficient of variation* of log 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-a-vis the initial 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 in turn for goods with lower tariffs. All other things held equal, then, an increase in the mis-reporting of goods classifications should lead an *increase* in the coefficient of variation of import capture ratios across goods within a country, while declines in mis-reporting of goods classifications should lead to a corresponding *decrease*. To construct the coefficient of variation of log import capture ratios across goods within a country, I use bilateral import and corresponding export data for 82 ISIC 4-digit goods classifications in the World Bank Trade and Production dataset.³⁰ The mean of this measure in the sample is 0.54, with a standard deviation of 0.39.

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 reflect 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. So I use the total value of *exports* recorded by all other countries as destined for the country in question as the import measure ('partner-reported imports'). The source for these data is also the World Bank Trade and Production dataset.³¹

³⁰Import capture ratios are likely to contain substantial noise due simply to reporting errors in both the importing and exporting country data that have nothing to do with intentional fraud or corruption. For example, errors in the goods classification in the export data will lead to fluctuation in the denominator of the import capture ratio. For goods categories imported in large volumes, noise from this source may be averaged out, but noise from data errors is likely to be quite large for small trade flows. So before taking the coefficient of variation of product-level import capture ratios, I exclude trade flows (at the product level within countries) amounting to less than \$100,000 (partner-reported). This exclusion eliminates a very small amount of trade by value (substantially less than one percent of total trade in the dataset).

³¹This dataset includes export data from 67 countries, so partner-reported import data will be from this set of countries.

To assess the impact of PSI on mis-reporting and on import volumes, I estimate equation 2 where the outcome variable is either a country's overall import capture ratio, the coefficient of variation of the log import capture ratio across product groups, or the log of partner-reported imports. The results are presented in Table 8. All regressions include country and year fixed effects, country-specific time trends, and the log of other tax revenues.

The sample size has changed from regressions in previous tables, so to confirm that the basic results still hold, the first column presents coefficient estimates on the PSI variables when the outcome variable is log import duties. As before, the coefficients on the new and old PSI indicators (0.23 and 0.53, respectively) are positive and statistically significantly different from zero, and they are very similar in magnitude to estimates from previous tables.

In column 2, the dependent variable is the log import capture ratio. The coefficient on the PSI_NEW_{jt} indicator is positive and statistically significantly different from zero. Its magnitude indicates that PSI programs are associated with an improvement in import capture ratios of roughly 7 percentage points. However, these improvements do not appear to persist: the coefficient on PSI_OLD_{jt} is small in magnitude (and actually negatively signed) and is not statistically significantly different from zero.

In column 3, the dependent variable is the coefficient of variation of log import capture ratios across 82 product groups. The coefficient on PSI_NEW_{jt} is negative and statistically significantly different from zero. As discussed above, reductions in mis-reporting of goods classifications should lead to decreased dispersion of import capture ratios (*lower* coefficient of variation) within a country-year cell. So this result suggests that PSI leads to a decline in mis-reporting of goods classifications. The coefficient estimate, -0.064, is not small, amounting to roughly one-sixth of a standard deviation of the dependent variable. The coefficient on the PSI_OLD_{jt} indicator is negative and similar in magnitude, but its standard error is quite large so that it is only of marginal statistical significance (the p-value is 0.15). This may be taken as merely suggestive evidence that PSI's impact on the dispersion of product-level import capture ratios persists into the later years of a PSI program.

In column 4, the dependent variable is the log of partner-reported imports. There is little indication of an initial boost to imports from PSI: the coefficient on the PSI_NEW_{jt} indicator is small in magnitude (and is actually negative in sign), and is not statistically significantly different from zero. But in later years of a PSI program, there is an increase in log imports: the coefficient on PSI_OLD_{jt} is positive and statistically significant.

In sum, then, the improved growth in import duties is likely to be driven by reductions in

undervaluation and in mis-reporting of goods classifications in the years immediately following PSI implementation. Increases in imports tend not to occur immediately, but instead show up some years after the start of PSI programs, and so may be related to the continued increases in import duty collections in later PSI years.

3.5 Overall discussion

A key pattern emerges from the empirical results: PSI programs are associated with increases in import duty collections, and the relationship is larger in magnitude for programs that have been in place longer. For a few reasons, however, it may be sensible to be conservative and only consider the causal impact of *newer* PSI programs as relatively well-established, and to take the estimated effect in later years as more speculative. First, consider the graphical evidence of Figures 1 and 2. In Figure 1 the post-PSI graph of log import duties appears to reach a temporary plateau in years 3 to 7, before jumping up in year 8 to a higher level and then roughly stabilizing again. This upward jump roughly coincides with the increase in log other tax revenues in Figure 2. This pattern is consistent with the implementation of other policy changes affecting tax revenue collections several years after the start of PSI programs, rather than a gradual increase in the effectiveness of PSI programs.

Furthermore, controlling for policy regime fixed effects (in Table 7) somewhat reduces the estimated impact of older PSI programs (the coefficient on PSI_OLD_{jt}), suggesting that the large increases in import duties associated with older PSI programs may in part be driven by unobserved policy changes. Finally, the channels analysis (Table 8) only finds statistically significant declines in underinvoicing and goods misclassification in the initial period of PSI programs.³²

Do PSI-generated improvements in import duties exceed program costs? Here I present a rough estimate of the cost-effectiveness of PSI, focusing solely on the ratio of improvements in import duty collections to the fees paid to PSI firms for their services. As such, this is not a welfare calculation: I am excluding, for example, any changes in consumer welfare due to changes in import prices, and the losses experienced by customs officials from any declines in their corrupt profits.

For the reasons just discussed, in this cost-effectiveness calculation I focus on estimates of the impact of newer PSI programs. The coefficient estimates on PSI_NEW_{jt} range from 0.14 to 0.28 across Tables 5-7. To be conservative, I take the lowest of these estimates, 0.14, as the

³²This logic also suggests that it may be wise to consider as merely speculative the causal impact of PSI programs on the volume of imports in years 5 and after.

impact of PSI on log import duties in the first 5 years of a PSI program. I assume that 90% of a country's imports are inspected, and that PSI fees are 1% of the value of inspected goods. I further assume no impact of new PSI programs on growth of imports (as found in Table 8). Finally, I let import duties start at 15.25% of imports prior to the introduction of PSI (the mean value in the 5 years prior to the PSI starting year among the 19 countries observed before and after the PSI start date in the 1,372-observation sample). Normalizing the level of imports to 100 prior to the program, this means that import duties are 15.25 prior to PSI.

Table 9 presents a comparison of estimated annual costs and benefits in the first five years (years 0-4) of a typical PSI program. The first section of the table calculates annual fees paid to PSI firms (0.90). The second section of the table calculates the improvement in annual import duties from before to after PSI implementation (2.29). PSI appears to be a highly cost-effective program, with a ratio of import duty improvements to PSI fees paid of 2.55.

4 Conclusion

When governments fear that bureaucratic reforms will fail due to the corruptibility or ineffectiveness of monitors, it is often proposed to "hire integrity" from private firms. In contrast to existing empirical work on bureaucratic reform, this paper is the first to examine the effectiveness of information generated by private firms in reform efforts. In addition, it provides evidence that increased monitoring by higher authorities can be effective in improving the effectiveness of a government bureaucracy.

I examine the impact of programs in a number of developing countries where governments hire private firms to conduct preshipment inspections of imports (PSI), generating data that higher authorities can use to prevent importers from evading import duty payments or to identify corrupt customs officers. Preshipment inspection programs lead to large increases in import duties, by 15-30 percentage points during the five years after program implementation. This improvement does not appear to be due to concurrent macroeconomic or policy changes: the growth rate of other tax revenues does not increase accordingly, and results are robust to estimating the impact of the program during periods when national and bureaucratic leadership are unchanged. Reductions in falsification of import documentation are likely to be behind the improvements in import duties: the programs also lead to declines in underinvoicing and in mis-reporting of goods classifications in customs. Hired integrity in this case is quite cost-effective, with improvements in import duty collections during the first five years of a typical inspection program amounting to 2.6 times

program costs.

While this paper sheds light indirectly on the channels through which PSI programs affect import duty collections, any study using country-level data is necessarily limited in how much it can reveal about microeconomic channels at work. Valuable future research could explore the micro-level impact of preshipment inspection programs in particular countries. For example, product-level data on the volume and prices of imported goods within a country could be used to ask whether PSI raises import demand by reducing the domestic-market prices of inspected goods. Surveys of importers could shed light on whether PSI reduces clearance times and bribes paid. Disaggregated trade data could be used to further document changes in mis-reporting of goods classifications. In addition, even though PSI appears to be effective on average across countries, micro-studies could identify the conditions under which expanding monitoring may not be effective,³³ and ways in which the programs can be modified to improve their effectiveness.

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³³For example, Yang (2006) documents the failure of increased monitoring within a PSI program.

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

<u>Countries, sorted by start year</u>	<u>Start date</u>	<u>End date</u>
1985		
Indonesia	11-Apr-85	01-Apr-97
1986		
Bolivia	21-Apr-86	
1987		
Philippines	01-Apr-87	31-Mar-00
1988		
Cameroon	01-Dec-88	
1989		
Madagascar	01-Jan-89	
1990		
Pakistan	18-Apr-90	15-Nov-97
Sierra Leone	15-Nov-90	
1992		
Peru	15-Jan-92	
Burkina Faso	23-Sep-92	
1993		
Cote d'Ivoire	11-Mar-93	
Congo, Rep.	09-Jun-93	
1994		
Uganda	15-Jan-94	
Kenya	31-Jan-94	
1995		
Colombia	09-Jun-95	09-Jul-99
Congo, Dem. Rep.	15-Jun-95	
1996		
Paraguay	06-May-96	09-Jun-99
1997		
Belarus	06-Jan-97	31-Mar-99
Argentina	23-Sep-97	
1999		
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

	<u>Mean</u>	<u>Median</u>	<u>Std. Dev.</u>	<u>Minimum</u>	<u>Maximum</u>	<u>Num. of obs.</u>
PSI	0.09	0.00	0.28	0.00	1.00	1,372
Import duties	722	228	1,368	0.007	12,010	1,372
Ln (import duties)	5.34	5.43	1.83	-4.95	9.39	1,372
Other tax revenues	5,976	1,006	12,926	0.7	156,810	1,372
Ln (other tax revenues)	6.89	6.91	2.20	-0.39	11.96	1,372
Import duties as share of total tax revenues	0.23	0.19	0.17	0.00	0.80	1,372
Imports	7,772	2,353	16,268	0.0	162,659	1,369
Ln(imports)	7.53	7.76	1.97	0.00	12.00	1,369
Tariff rate	22.03	19.00	15.20	0.00	102.20	998

NOTES-- Unit of observation is a country-year, for 104 developing countries between 1980 and 2000. Developing countries are those not classified as "high income" by World Bank's country groupings. Sample excludes countries with less than three years of data on import duties between 1980 and 2000, and Pacific island nations and dependencies. "PSI" is an indicator that a PSI program is active for at least half of a given year (program dates collected by author directly from the four main PSI firms). Other tax revenues is total tax revenues minus import duties. Import duties and other tax revenues are in millions of current US\$ (source: World Development Indicators 2004). "Tariff rate" is simple average tariff (in percentage points) across all tariff lines, with some years of missing data interpolated (source: World Bank).

Table 3: Predicting PSI adoption
(OLS estimates)

Dependent variable: Indicator for country adopting PSI by end of year 2000

	(1)	(2)	(3)	(4)	(5)
Ln (per capita GDP)	-0.207 (0.044)***				-0.193 (0.060)***
Ln (import duties)		-0.001 (0.029)			-0.052 (0.055)
Ln (imports)			0.000 (0.028)		0.037 (0.066)
Bureaucratic corruption (absence of)				-0.834 (0.273)***	-0.470 (0.272)*
Constant	1.863 (0.309)***	0.433 (0.153)***	0.426 (0.209)**	0.803 (0.130)***	2.010 (0.428)***
Num. of obs.	76	77	77	55	55
R-squared	0.23	0.00	0.00	0.15	0.32

* significant at 10%; ** significant at 5%; *** significant at 1%

NOTES-- Mean of dependent variable is 0.48. Standard errors in parentheses. Right-hand-side variables are values in the first year of non-missing data from 1980-1984. No PSI program was implemented prior to 1985. Per capita GDP is in constant 1995 dollars. Import duties and imports are in millions of current US dollars. "Bureaucratic corruption" normalized to range from 0 to 1, with 0 being "worst" and 1 being "best" (source: ICRG). Per capita GDP and import duties are from World Development Indicators 2004. Imports are from IMF Direction of Trade Statistics, as reported by trade partner countries.

Table 4: Determinants of timing of PSI adoption
(Cox proportional hazard estimates)

	(1)	(2)
New IMF program (year t)	1.845 (0.590)	3.248 (0.980)
New IMF program (year t-1)	0.884 (0.090)	0.821 (0.050)
Change in ln(GDP) (year t-1 to t)	0.198 (0.990)	0.418 (0.500)
Change in ln(GDP) (year t-2 to year t-1)	0.045 (2.17)**	0.253 (0.750)
Change in ln(import duties) (year t-1 to t)	6.413 (1.91)*	1.325 (0.410)
Change in ln(import duties) (year t-2 to year t-1)	1.510 (0.480)	0.541 (0.540)
New head of state (year t)		1.950 (0.940)
New head of state (year t-1)		0.940 (0.040)
New minister of finance (year t)		0.725 (0.310)
New minister of finance (year t-1)		4.257 (2.24)**
New head of customs (year t)		0.208 (1.220)
New head of customs (year t-1)		1.676 (1.280)
Num. of country-year observations	990	147
Number of countries	100	18
Number of PSI implementations	14	14

* significant at 10%; ** significant at 5%; *** significant at 1%

NOTES-- Table shows estimates from Cox proportional hazard model of years until implementation of PSI. Years until implementation measured from 1980. The unit of observation is the country-year, with years after the first year of PSI adoption eliminated and censoring occurring for countries that had not implemented by 2000. Hazard ratios are presented for each independent variable, with t-statistics in parentheses. T-statistics account for clustering at the country level. "New IMF program" is an indicator variable equal to 1 if the country had a new IMF program in the given year, 0 otherwise. "New head of state" is an indicator variable equal to 1 if the country had a new head of state in the given year, 0 otherwise. "New minister of finance" and "New head of customs" defined similarly.

Table 5: Impact of preshipment inspection on import duty collection

(Fixed effects estimates)

Dependent variable: Ln (import duties)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sample:	All observations				Observations with tariff data					
PSI	0.233 (0.090)**	0.229 (0.067)***			0.231 (0.102)**	0.197 (0.078)**			0.192 (0.079)**	
PSI, years 0-4 (<i>PSI_NEW_{jt}</i>)			0.2 (0.079)**	0.199 (0.056)***			0.194 (0.086)**	0.160 (0.062)**		0.154 (0.065)**
PSI, years 5+ (<i>PSI_OLD_{jt}</i>)			0.458 (0.153)***	0.43 (0.132)***			0.458 (0.165)***	0.428 (0.123)***		0.431 (0.126)***
Ln(other tax revenues)		0.548 (0.064)***		0.545 (0.063)***		0.672 (0.096)***		0.673 (0.093)***	0.678 (0.098)***	0.68 (0.095)***
Simple average tariff rate									0.005 (0.003)	0.005 (0.003)*
Observations	1,372	1,372	1,372	1,372	998	998	998	998	998	998
R-squared	0.97	0.97	0.97	0.97	0.96	0.97	0.96	0.97	0.97	0.97
<u>Number of countries:</u>										
Total	104	104	104	104	85	85	85	85	85	85
Observed pre- and post- PSI	19	19	19	19	18	18	18	18	18	18

* significant at 10%; ** significant at 5%; *** significant at 1%

NOTES -- Standard errors (corrected for clustering by country) in parentheses. All regressions include country fixed effects, year fixed effects, and country-specific linear time trends. "PSI" equal to 1 if country had an active PSI program for at least half of given year, 0 otherwise. "PSI, years 0-4" equal to 1 if active PSI program is in its 1st to 5th year of operation. "PSI, years 5+" equal to 1 if active PSI program is in its 6th year of operation or after. "Other tax revenues" is total tax revenues minus import duties. See Table 2 for notes on sample composition and other variable definitions. One PSI country (Belarus) dropped out in columns 4-7 due to absence of sufficient tariff data.

Table 6: Robustness checks for impact of PSI on import duties

(Fixed effects estimates)

	<u>Dependent variable:</u> Ln (import duties)					
<u>Specification:</u>	(1)	(2)	(3)	(4)	(5)	(6)
	Sample of countries with 15 or more obs. from 1980-2000	Sample of countries with 18 or more obs. from 1980-2000	Exclude countries with population < 1 million	Separate effect for "Past PSI"	Include outliers of import duties	Flexible controls for other tax revenues, tariffs
PSI, years 0-4 (<i>PSI_NEW_{jt}</i>)	0.194 (0.055)***	0.185 (0.060)***	0.194 (0.057)***	0.202 (0.071)***	0.169 (0.063)***	0.143 (0.068)**
PSI, years 5+ (<i>PSI_OLD_{jt}</i>)	0.445 (0.109)***	0.539 (0.104)***	0.425 (0.132)***	0.436 (0.174)**	0.4 (0.135)***	0.376 (0.109)***
Past PSI				0.013 (0.161)		
Observations	937	745	1,188	1,372	1,382	998
R-squared	0.98	0.98	0.96	0.97	0.97	0.97
<u>Number of countries:</u>						
Total	49	37	88	104	105	37
Observed pre- and post- PSI	14	11	19	19	19	11

* significant at 10%; ** significant at 5%; *** significant at 1%

NOTES -- Standard errors (corrected for clustering by country) in parentheses. All regressions include country fixed effects, year fixed effects, and country-specific linear time trends. "Past PSI" equal to 1 if PSI program is absent for at least half a year but country had previously had a PSI program, and 0 otherwise. Regressions 1, 2, 3, 4, and 5 include control for ln(other tax revenues). Regression 6 includes controls for 10-piece linear spline in ln(other tax revenues) and in simple average tariff rate. See Tables 2 and 5 for notes on sample composition and other variable definitions.

Table 7: Impact of preshipment inspection on import duty collection, controlling for policy regime

(Fixed effects estimates)

Dependent variable: Ln (import duties)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<u>Sample:</u>	<u>All observations</u>				<u>Observations with tariff data</u>			
PSI	0.229 (0.067)***	0.291 (0.114)**			0.192 (0.079)**	0.262 (0.138)*		
PSI, years 0-4 (<i>PSI_NEW_{jt}</i>)			0.199 (0.056)***	0.282 (0.126)**			0.154 (0.065)**	0.248 (0.148)*
PSI, years 5+ (<i>PSI_OLD_{jt}</i>)			0.43 (0.132)***	0.319 (0.089)***			0.431 (0.126)***	0.306 (0.113)***
Ln(other tax revenues)	0.548 (0.064)***	0.481 (0.088)***	0.545 (0.063)***	0.481 (0.088)***	0.678 (0.098)***	0.598 (0.131)***	0.68 (0.095)***	0.598 (0.131)***
Simple average tariff rate					0.005 (0.003)	0.007 (0.003)**	0.005 (0.003)*	0.007 (0.003)**
<i>Policy regime fixed effects?</i>	-	Y	-	Y	-	Y	-	Y
Observations	1,372	1,372	1,372	1,372	998	998	998	998
R-squared	0.97	0.98	0.97	0.98	0.97	0.98	0.97	0.98

* significant at 10%; ** significant at 5%; *** significant at 1%

NOTES -- Standard errors (corrected for clustering by country) in parentheses. All regressions include country fixed effects, year fixed effects, and country-specific linear time trends. Policy regime fixed effects (defined only for 19 PSI-using countries listed in Table 1) are separate indicator variables for each period in which national leader, finance minister, and customs head were all constant. See Tables 2 and 5 for notes on sample composition and other variable definitions.

Table 8: Impact of preshipment inspection on determinants of import duty collection

(Fixed effects estimates)

	(1)	(2)	(3)	(4)
<u>Dependent variable:</u>	Ln (import duties)	Ln (import capture ratio)	CV of product-level Ln(import capture ratio)	Ln(imports)
PSI, years 0-4 (<i>PSI_NEW_{jt}</i>)	0.23 (0.082)***	0.067 (0.028)**	-0.064 (0.026)**	-0.014 (0.052)
PSI, years 5+ (<i>PSI_OLD_{jt}</i>)	0.529 (0.164)***	-0.012 (0.032)	-0.057 (0.039)	0.12 (0.046)**
Ln(other tax revenues)	0.667 (0.132)***	0.08 (0.049)	-0.033 (0.057)	0.426 (0.071)***
Observations	581	581	581	581
R-squared	0.97	0.72	0.54	0.99
<u>Number of countries:</u>				
Total	39	39	39	39
Observed pre- and post- PSI	9	9	9	9

* significant at 10%; ** significant at 5%; *** significant at 1%

NOTES -- Standard errors (corrected for clustering by country) in parentheses. All regressions include country fixed effects, year fixed effects, and country-specific linear time trends. "Import capture ratio" is ratio of a country's self-reported total imports to corresponding reported exports of other countries to said country. Dependent variable in column 3 is coefficient of variation of ln(import capture ratio) across 82 product groups within country-year cell. Imports measure (in column 4) is exports reported by all other countries as destined for the country in question. See Tables 2 and 5 for notes on sample composition and other variable definitions.

Table 9: Cost-benefit calculation

Fees paid to PSI firms

Annual imports	100.00
% of imports inspected	90%
PSI fees as % of value of imports inspected	1%
Annual fees paid to PSI firms	0.90

Improvements in import duties

Import duties as % of imports	15.25%
Pre-program level of annual import duties	15.25
Effect of PSI on log import duties	0.14
Post-PSI level of import duties (years 0-4)	17.54
Improvement in annual import duties	2.29

Ratio of import duty improvement to PSI fees paid **2.55**

NOTES-- Assumed impact of PSI on import duties (in years 0-4) is from column 6 of Table 6. Year 0 is first year PSI program has been active for at least half a year. Initial import duties as a share of imports calculated from 5 years immediately prior to PSI programs for the 19 countries observed before and after the PSI start date in column 4 of Table 5. Pre-program imports normalized to 100.

Appendix Table 1: Impact of preshipment inspection on other tax revenues and tariff rate

(Fixed effects estimates)

	(1)	(2)	(3)	(4)
<u>Dependent variable:</u>	Ln (other tax revenues)	Ln (other tax revenues)	Simple average tariff rate	Simple average tariff rate
PSI	0.007 (0.092)		0.908 (1.957)	
PSI, years 0-4		0.001 (0.097)		1.156 (2.087)
PSI, years 5+		0.051 (0.117)		-0.663 (2.533)
Observations	1,372	1,372	998	998
R-squared	0.99	0.99	0.93	0.93
<u>Number of countries:</u>				
Total	104	104	85	85
Observed pre- and post- PSI	19	19	18	18

* significant at 10%; ** significant at 5%; *** significant at 1%

NOTES -- Standard errors (corrected for clustering by country) in parentheses. All regressions include country fixed effects, year fixed effects, and country-specific linear time trends. See Tables 2 and 5 for notes on sample composition and other variable definitions.

Appendix Table 2: Indonesian national leader, finance minister, and head of customs, 1980-2000

<u>Year</u>	<u>National Leader</u>	<u>Finance Minister</u>	<u>Head of Customs</u>
1980	Suharto	Ali Wardhana	Sutadi Sukarya
1981	Suharto	Ali Wardhana	Salamun At
1982	Suharto	Ali Wardhana	Salamun At
1983	Suharto	Ali Wardhana	Salamun At
1984	Suharto	Radius Prawiro	Salamun At
1985	Suharto	Radius Prawiro	Salamun At
1986	Suharto	Radius Prawiro	Salamun At
1987	Suharto	Radius Prawiro	Salamun At
1988	Suharto	Radius Prawiro	Salamun At
1989	Suharto	J. B. Sumarlin	Mar'ie Muhammad
1990	Suharto	J. B. Sumarlin	Mar'ie Muhammad
1991	Suharto	J. B. Sumarlin	Mar'ie Muhammad
1992	Suharto	J. B. Sumarlin	Mar'ie Muhammad
1993	Suharto	Marje Muhammed	Mar'ie Muhammad
1994	Suharto	Marje Muhammed	Fuad Bawazier
1995	Suharto	Marje Muhammed	Fuad Bawazier
1996	Suharto	Marje Muhammed	Fuad Bawazier
1997	Suharto	Marje Muhammed	Fuad Bawazier
1998	Suharto	Faud Bawazir	Anshari Rutonga
1999	Bacharuddin Jusuf Habibie	Bambang Subianto	Anshari Rutonga
2000	Abdurrahman Wahid	Bambang Sudibyo	Anshari Rutonga

NOTES -- Horizontal lines separate periods with unchanged leaders of all three types. Data sources for national leader and finance minister are Stateman's Yearbook, International Yearbook and Statesmen's Who's Who, Europa World Yearbook, and Worldwide Government Directory. Source for head of customs is U.S. Commercial Service, Jakarta.

Figure 1: PSI and Import Duties

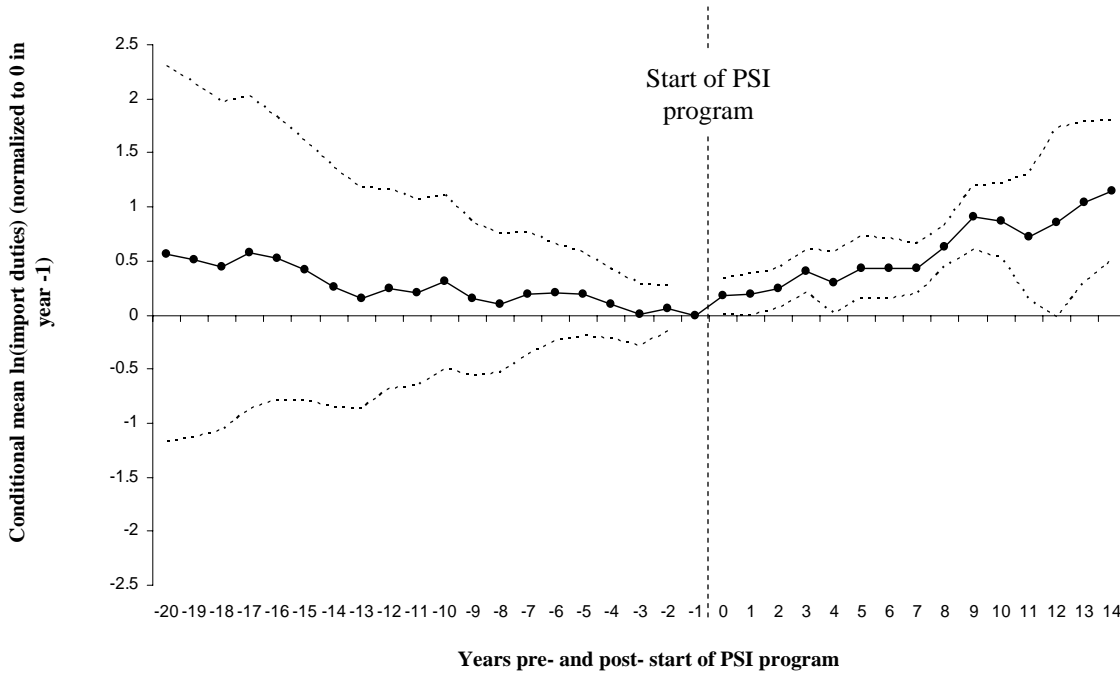
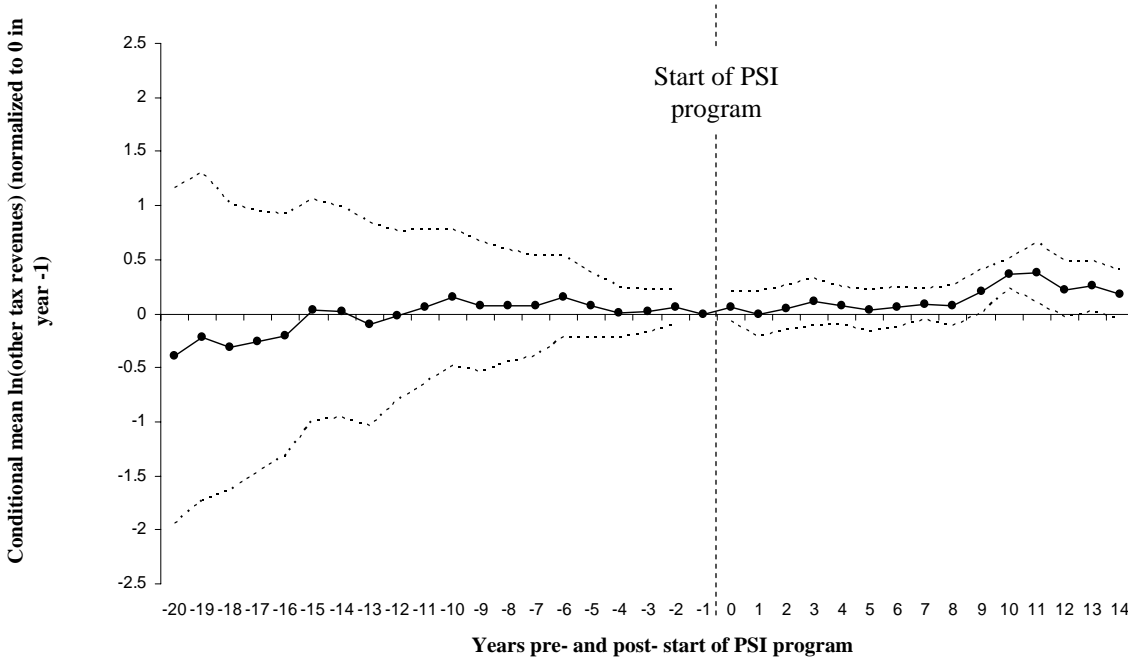


Figure 2: PSI and Other Tax Revenues



NOTES for Figures 1 and 2– Plotted points are coefficients on indicator variables for each year before and after the start of a PSI program, in regression of ln(tax collection), separately for each type of tax revenue. Figures 1 and 2 present coefficients from regressions for ln(import duties) and ln(other tax revenues), respectively. 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. “Other tax revenues” are total tax revenues minus import duties.