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 2 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 misreporting of goods classifications. Historically, this hired integrity appears to have been cost-effective, with improvements in import duty collections in the first 5 years of a typical inspection program amounting to 2.6 times the program's costs.

1. Introduction

Corruption and bureaucratic inefficiency are widely cited as important barriers to economic development.[1] Within a developing country's 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

[1] For recent overviews of the relationship between corruption and development, see Bardhan (1997) and Rose-Ackerman (2005).

importers seeking to pay less than the legal tariffs due on their shipments. Even 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 article's analysis, import duties accounted for an average of 23 percent of central government revenue across developing countries.

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 (see Besley and McLaren 1993; Mookherjee and Png 1992, 1995; Polinsky and Shavell 2001). 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 might themselves be dishonest and so might 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 (see Cadot 1987; Chand and Moene 1999; Fjeldstad and Tungodden 2003). 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 level 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 monitors 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 article is the first empirical analysis of a bureaucratic reform involving hired integrity. In the past 2 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 (PSI) of imports. 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 hon-

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[2] However, it is also possible that importers may end up paying less than the legislated tariffs on their imports because of corruption, in which case corruption could encourage imports.

esty, essentially hiring integrity from the firm to provide objective data on the contents of imported shipments.

There are various channels through which PSI 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 to identify fraudulent import documentation and can also help higher level enforcers to 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. Preshipment inspection helps investigators identify import transactions for which duties owed (as calculated from the PSI report) diverge substantially from duties actually collected and thereby suggests 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 an importer's customs declaration and a shipment's actual contents. A PSI 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, and this power potentially reduces customs clearance times.[3]

However, the success of PSI programs is far from guaranteed. Success requires client governments to actually use the PSI-generated information to seek out and prosecute 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 reduce costs 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 PSI program in the Philippines between 1989 and 1992, I (Yang 2008) found that expansion of import monitoring caused substantial displacement of imports to unmonitored import categories, so 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 in this article uses panel data on country-level outcomes to examine the relationship

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[3] Low (1995) and Jenkins (1992) cite survey evidence that preshipment inspection (PSI) was accompanied by dramatic reductions in customs clearance times in Indonesia.

between the implementation of PSI programs and import duty collections for the years 1980–2000. I find that PSI programs are associated with increases in import duty collections, by 15–30 percent in the first 5 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 misreporting of goods classifications in customs. Preshipment inspection appears to be cost-effective: improvements in import duty collections in the first 5 years of a typical inspection program were 2.6 times the program's costs.[4]

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 that coincide 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 duties) could increase because of 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 in import duties. While it is difficult to obtain data on organizational reforms within customs across countries and over time, data do 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 article is to examine the impact of PSI in

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[4] These findings are not inconsistent with the results in Yang (2008), as the current article 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 percent of imports was 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, thereby reducing substantially the opportunities for displacement.

the midst of periods in which 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,[5] and this finding further bolsters the case for PSI's causal impact.

To the extent that the estimated effects of PSI on import duty collections are due in part to reductions in customs corruption, this article 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 (2005) provides field experimental evidence on how different types of monitoring affect corruption in Indonesian road projects. In Uganda, Reinikka and Svensson (2004a, 2004b) 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 et al. (2002) use a field experiment to document the impact of increased monitoring on opportunistic behavior by telephone call center employees.

This article also relates to research on the effect of avoidance of taxes on international trade. Existing work documents the existence of import duty avoidance but does not examine the impact of enforcement on these activities (with the exception of Yang 2008).[6] 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 finding as evidence of tax evasion or avoidance. Fisman and Wei (2004) find that the extent of import underinvoicing increases in the tariff rate among Chinese imports from Hong Kong. A number of authors examine tax-induced transfer pricing within multinational firms (for example, Bernard and Weiner 1990; Hines and Rice 1994; Clausing 2001). In the related realm of income tax evasion, Klepper and Nagin (1989) examine cross-sectional correlates of income underreporting on specific line items of U.S. 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 article is organized as follows. Section 2 provides background on PSI programs worldwide. Section 3 presents the empirical evidence on the impact of PSI on import duty collection and on the channels that appear to mediate PSI's effects and includes several robustness checks. Section 4 concludes.

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[5] Specifically, fixed effects for each distinct policy regime are included in the regressions.
[6] Slemrod and Yitzhaki (2002) appeal for research on the responses of tax evaders to greater enforcement.

2. Background of Preshipment Inspection

A handful of multinational inspection firms—all headquartered in Europe—provide PSI services. Implementing a PSI program involves hiring one or more of these firms to inspect incoming shipments and using their established worldwide network of inspection agents. Preshipment inspection 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 those shipments 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. Preshipment inspection 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, the 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 dishonest importers as well as customs officials who may be complicit in allowing misreporting of shipment contents and underpayment of import duties. Preshipment inspection 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 they have been reported, the data are usually in the 80–90 percent range (see Rege 2001).

Preshipment inspection originated as a product innovation by the Geneva-based firm Société Générale de Surveillance (SGS), which proposed and implemented the first PSI program for the government of Indonesia in 1985. The role of SGS 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 year) in 13 countries. Thereafter, adoption was more rapid; 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 percent 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 percent of the central government's tax revenues. The total fees paid worldwide to PSI firms were on the order of U.S.$500 million annually during the same years.[7]

3. Empirical Evidence on the Impact of Preshipment Inspection

This section documents the impact of PSI 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 PSI 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 duties, which is reported annually in *World Development Indicators* (*WDI*), and my data are from 2004.[8] There are several occasions when reported import duty collections are very different from other values of the same variable for the same country, and they are highly likely to be reporting errors. So I replace a reported observation of the log of import duties with a missing value if it has a value greater than 4 standard deviations (SDs) away from the mean of other reported import duties for the same country.[9]

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

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[7] For these fee calculations, I use data from International Monetary Fund (2004) and a historical database of PSI programs I collected. The estimate of PSI fees paid in year t by country j is $\text{Fees}_{jt} = (.01)(.8) \times M_{jt}\text{PSIfrac}_{jt}$, 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 required only for a fraction .8 of imports and that the PSI fee is a fraction .01 of the total imports inspected. The annual worldwide total of Fees_{jt} averages $547 million per year from 1990 to 2000.

[8] Unless otherwise specified, all data in monetary units are in current U.S. dollars.

[9] All told, this replacement affects just 10 observations that would otherwise have been included in the sample. Of these 10, only two are for countries observed before and after the start of a PSI program (and so would affect the estimate of PSI's effect): the 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 robustness checks in Section 3.3.3.

provided by the four largest multinational firms that offer PSI services, for all programs through the end of 2000.[10]

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 World Bank (2004), and tariff data are compiled from various sources by the World Bank's trade research group.[11] The tariff data are simple average tariffs across all tariff lines. The tariff data contain a number of missing values; when missing values occur between years of available data, I fill in missing values via linear interpolation between the 2 years with data that bracket the missing data.

Bilateral trade data used in the construction of measures of misreporting in customs are from the World Bank's Trade and Production data set. Some subsidiary regressions use data on per capita gross domestic products (GDPs; World Bank 2004), a survey measure of bureaucratic corruption (PRS Group 2004), and import data from an alternative source (International Monetary Fund [IMF] 2004).

The first PSI contract started in 1985, so I limit the analyses to the years 1980 through 2000. Preshipment inspection is used exclusively in developing countries, so I restrict the sample to countries in Africa, Asia, Europe, and Latin America/ the Caribbean that are not classified as high-income countries by the World Bank.[12] I also drop countries from the analysis if they have complete data for fewer than 3 years between 1980 and 2000.[13]

The largest resulting sample contains 1,372 observations from 104 countries. Nineteen 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 results tables indicate the number of countries included in the regressions 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.[14]

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

[11] For the tariff data (including details on the sources used), see World Bank, Table 1: Trends in Average Tariff Rates for Developing and Industrial Countries, 1981–2003 (Unweighted in %) (http: //siteresources.worldbank.org/INTRANETTRADE/Resources/tar2002.xls).

[12] Constructing the sample in 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.

[13] Including 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.

[14] The 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 discussed in Section 3.3.3.

Table 1
Active Dates for Preshipment Inspection Programs, 1985–2000

Country	Start Date	End Date
Indonesia	April 11, 1985	April 1, 1997
Bolivia	April 21, 1986	
Philippines	April 1, 1987	March 31, 2000
Cameroon	December 1, 1988	
Madagascar	January 1, 1989	
Pakistan	April 18, 1990	November 15, 1997
Sierra Leone	November 15, 1990	
Peru	January 15, 1992	
Burkina Faso	September 23, 1992	
Côte d'Ivoire	March 11, 1993	
Republic of Congo	June 9, 1993	
Uganda	January 15, 1994	
Kenya	January 31, 1994	
Colombia	June 9, 1995	July 9, 1999
Democratic Republic of Congo	June 15, 1995	
Paraguay	May 6, 1996	June 9, 1999
Belarus	January 6, 1997	March 31, 1999
Argentina	September 23, 1997	
Georgia	August 15, 1999	

Note. The start and end dates for countries' preshipment inspection (PSI) programs were obtained directly from the four major PSI firms. No specified end date means that the contract was still active as of the end of 2000. Three countries experienced interruptions in their PSI programs: Pakistan between November 30, 1991, and September 1, 1994; the Republic of Congo between May 31, 1998, and March 4, 1999; and Madagascar between July 31, 1992, and December 4, 1992. Only countries with data on import duties before and after the contract start date are listed.

Table 2 presents summary statistics for the observations included in the sample. The unit of observation is a country-year. The term 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 the observations occur in countries with an active PSI program.

3.2. Which Countries Adopt Preshipment Inspection 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 in which 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 nonmissing data between 1980 and 1984. (The countries in this sample are a subset of those in the sample used in the main results of this article because not all countries have complete data in the years 1980–84.)

The data in columns 1–4 of Table 3 are the result of 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 more bureaucratic corruption are more likely to use PSI. For

Table 2
Summary Statistics

	Mean	Median	SD	Minimum	Maximum	Observations
PSI	.09	.00	.28	.00	1.00	1,372
Import Duties	722	228	1,368	.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	.70	156,810	1,372
ln(Other Tax Revenues)	6.89	6.91	2.20	−.39	11.96	1,372
Import duties as a share of the total tax revenues	.23	.19	.17	.00	.80	1,372
Imports	7,772	2,353	16,268	.00	162,659	1,369
ln(Imports)	7.53	7.76	1.97	.00	12.00	1,369
Tariff Rate	22.03	19.00	15.20	.00	102.20	998

Note. The unit of observation is a country-year for 104 developing countries between 1980 and 2000. Developing countries are those not classified as high-income countries by the World Bank. The sample excludes countries with fewer than 3 years of data on import duties between 1980 and 2000, Pacific Island nations, and dependencies. The variable PSI is an indicator that a preshipment inspection (PSI) program is active for at least half of a given year (the program dates were collected directly from the four main firms). Other Tax Revenues is the total tax revenue minus the import duties. Import Duties and Other Tax Revenues are in millions of current U.S. dollars (data from World Bank 2004). Tariff Rate is the simple average tariff (in percentage points) across all tariff lines, with some years of missing data interpolated (World Bank, Table 1: Trends in Average Tariff Rates for Developing and Industrial Countries, 1981–2003 (Unweighted in %) [http://siteresources.worldbank.org/INTRANETTRADE/Resources/tar2002.xls]).

the data 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 more bureaucratic corruption in the early 1980s were more likely to adopt PSI programs between 1985 and 2001.

The preexisting differences between PSI-using and non-PSI-using countries, which are documented in Table 3, suggest that it would be invalid to infer the impact of PSI by simply comparing PSI-using and non-PSI-using 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 countries that 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. 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. As can be seen from the data in the first column, for which 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 IMF program (current or lagged) are not statistically significantly different from zero.[15]

[15] Data on the timing of International Monetary Fund (IMF) programs across countries were graciously provided by the IMF's Independent Evaluation Office.

Table 3
Predicting Preshipment Inspection Adoption

	(1)	(2)	(3)	(4)	(5)
ln(Per Capita GDP)	−.207**				−.193**
	(.044)				(.060)
ln(Import Duties)		−.001			−.052
		(.029)			(.055)
ln(Imports)			.000		.037
			(.028)		(.066)
Bureaucratic Corruption (absence of)				−.834**	−.470+
				(.273)	(.272)
Constant	1.863**	.433**	.426*	.803**	2.010**
	(.309)	(.153)	(.209)	(.130)	(.428)
Observations	76	77	77	55	55
R^2	.23	.00	.00	.15	.32

Note. The dependent variable in these ordinary least squares estimates is the indicator for a country adopting a preshipment inspection (PSI) program by the end of 2000. The mean of the dependent variable is .48. Standard errors (corrected for clustering by country) are in parentheses. Per Capita GDP is in constant 1995 U.S. dollars. Import Duties and Imports are in millions of current U.S. dollars. Bureaucratic Corruption is normalized to range from zero to one, with zero being worst and one being best (data from PRS Group 2004). Values for Per Capita GDP and Import Duties are from World Bank (2004). Values for Imports are from International Monetary Fund (2004), as reported by trade partner countries.
+ $P < .10$.
* $P < .05$.
** $P < .01$.

Higher growth in import duties (from $t − 1$ to t) also is associated with implementing a PSI program in year t, but this finding almost surely is 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 another regression, with the results shown in column 2, 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.) Because of difficulties in obtaining leadership data, this regression is restricted to observations for countries that 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.[16] Other factors, such as economic growth, changes in import duties, presence of a new IMF program, or changes in the identity of the head of state or the head of customs, do not have statistically significant relationships with adoption in this subsample.

3.3. The Impact of Preshipment Inspection

I estimate here the relationship between the implementation of PSI programs and changes in total import duties collected, total imports, and the extent of

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[16] This latter result provides justification for the later analyses with policy regime fixed effects in Section 3.3.4.

Table 4
Determinants of the Timing of Preshipment Inspection Implementation

	(1)		(2)	
New IMF Program:				
Year t	1.845	(.590)	3.248	(.980)
Year $t-1$	.884	(.090)	.821	(.050)
Change in ln(GDP):				
Year $t-1$ to t	.198	(.990)	.418	(.500)
Year $t-2$ to year $t-1$	.045*	(2.17)	.253	(.750)
Change in ln(Import Duties):				
Year $t-1$ to t	6.413[+]	(1.91)	1.325	(.410)
Year $t-2$ to year $t-1$	1.510	(.480)	.541	(.540)
New Head of State:				
Year t			1.950	(.940)
Year $t-1$			.940	(.040)
New Minister of Finance:				
Year t			.725	(.310)
Year $t-1$			4.257*	(2.24)
New Head of Customs:				
Year t			.208	(1.220)
Year $t-1$			1.676	(1.280)
Country-year observations	990		147	
Countries	100		18	

Note. Hazard ratios are presented for each independent variable, with t-statistics in parentheses; t-statistics account for clustering at the country level. $N = 14$ preshipment inspection implementations for both regressions. New IMF Program is an indicator variable equal to one if the country had a new International Monetary Fund program in the given year and is zero otherwise. New Head of State is an indicator variable equal to one if the country had a new head of state in the given year and is zero otherwise. New Minister of Finance and New Head of Customs are defined similarly. GDP = gross domestic product.
  [+] $P < .10$.
  * $P < .05$.

import misreporting in customs. The main empirical approach is detrended differences-in-differences estimation. For outcome variable Y_{jt} (say, the log of import duties) for country j in year t, the basic regression equation is

$$Y_{jt} = \alpha \text{PSI}_{jt} + \zeta' X_{jt} + \mu_j + \delta_t + \gamma_j \text{TREND} + \varepsilon_{jt}. \tag{1}$$

The term PSI_{jt} is an indicator variable for whether country j had an active PSI program for at least half of year t. The term 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. The term 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. The term 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 in which the effect is allowed to vary across earlier and later years of a PSI program:

$$Y_{jt} = \beta_1 \text{PSI\_NEW}_{jt} + \beta_2 \text{PSI\_OLD}_{jt} + \zeta' \mathbf{X}_{jt}$$
$$+ \mu_j + \delta_t + \gamma_j \text{TREND} + \varepsilon_{jt}. \tag{2}$$

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

Serial correlation in the outcome and PSI variables are likely to be problems in this panel data set, biasing ordinary least squares standard error (SE) estimates downward (Bertrand, Duflo, and Mullainathan 2004), so standard errors allow for an arbitrary variance-covariance structure within countries (SEs 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 the implementation of PSI could coincide with periods of greater economic growth, which raises tax collections simply via increases in taxable economic activity. If PSI programs are indeed accompanied by the installation of a more technocratic government or by greater 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 or 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.[17] In addition, changes in tariff rates, nontariff trade barriers, or organizational reforms in customs could change simultaneously with the implementation of 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 is available for a subset of countries and years. I therefore test the sensitivity of

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

the results to inclusion of a country's simple average tariff rate in the vector of controls, 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 (described in Section 3.3.4).

3.3.1. Graphical Analysis

Prior to discussing the empirical results, it is informative to see a graphical view of the relationship between import duties and PSI programs. In Figure 1, the solid line plots the conditional mean of the log of 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, and so on.)

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

$$
\begin{aligned}
Y_{jt} = \; & \theta_{-20}\text{PSI}\_20_{jt} + \theta_{-19}\text{PSI}\_19_{jt} \\
& \ldots \; + \theta_{-1}\text{PSI}\_1_{jt} + \theta_0\text{PSI0}_{jt} + \theta_1\text{PSI1}_{jt} \\
& \ldots \; + \theta_{13}\text{PSI13}_{jt} + \theta_{14}\text{PSI14}_{jt} \\
& + \; \mu_j + \delta_t + \gamma_j\text{TREND} + \varepsilon_{jt}.
\end{aligned}
\tag{3}
$$

The variables PSI_20_{jt}, PSI_19_{jt}, ..., PSI14_{jt} are indicators for the observation occurring for 20 years before and up to 14 years after the start year of a country's PSI program (the complete set of before and after years observed in the data). These indicators are all zero if the country never used PSI. 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 percent confidence intervals of each coefficient estimate. Other tax revenues are the total tax revenues minus the import duties.

The figure reveals that the conditional mean of the log of 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, which suggests that the later increase in import duties is unlikely to be driven by mean reversion. Each coefficient on indicators for years after the PSI start year is statistically significantly different from zero
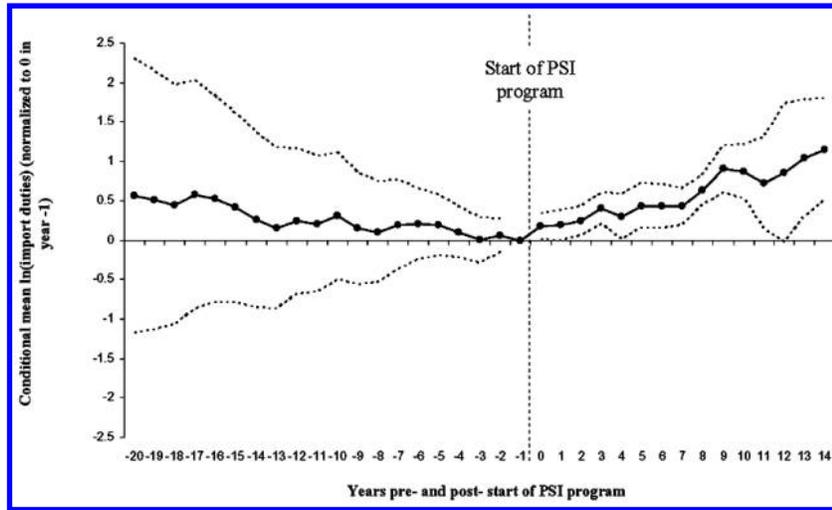
Figure 1. Preshipment inspection and import duties

at the 95 percent confidence level, while none of the coefficients for years prior to the PSI start year are statistically significant.

To gain confidence that this increase in import duties is not being driven by unobserved changes occurring in countries concurrently with the implementation of PSI programs, it is useful to conduct the same graphical analysis for an outcome that should be unaffected by PSI but that is likely to respond to similar third factors influencing tax collections overall (a change in government or economic growth). As was mentioned above, other tax revenues (the total taxes minus the 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 SEs from a regression identical to equation (3) but in which 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 the log of other tax revenues, but the size of this increase is quite small compared with the corresponding change for the log of 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
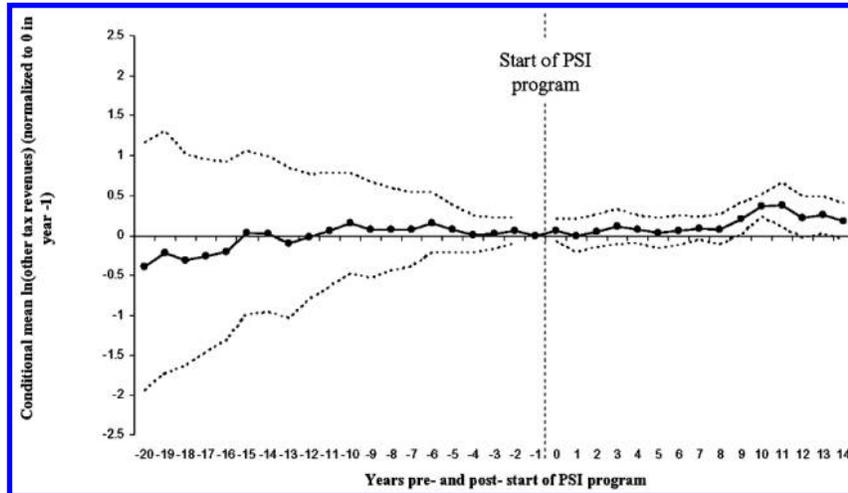
Figure 2. Preshipment inspection and other tax revenues

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 Preshipment Inspection on Import Duties

The graphical analysis indicates that PSI was accompanied by an increase in the log of import duties. This section shows that this conclusion holds in a more parsimonious specification, in which the impact of PSI on import duties is not allowed to vary completely flexibly for every year before and after the PSI start year. I constrain the effect depicted in Figure 1 to be summarized in an overall PSI effect, α (as in equation [1]), and 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. Columns 1–4 present results for the largest sample. For the data 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 percent level. For the data 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 percent level.

For the data in columns 3 and 4, versions of equation (2) are estimated, allowing the effect of PSI to vary across relatively new versus older PSI programs. The coefficient estimates in columns 3 and 4 (without and with the control for the log of other tax revenues, respectively) are quite similar. Having a PSI program

Table 5
Impact of Preshipment Inspection on Import Duty Collection

Sample	All Observations (N = 1,372)				Observations with Tariff Data (N = 998)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PSI	.233*	.229**			.231*	.197*			.192*	
	(.090)	(.067)			(.102)	(.078)			(.079)	
PSI, years 0–4 (PSI_NEW$_{jt}$)			.2*	.199**			.194*	.160*		.154*
			(.079)	(.056)			(.086)	(.062)		(.065)
PSI, years 5+ (PSI_OLD$_{jt}$)			.458**	.43**			.458**	.428**		.431**
			(.153)	(.132)			(.165)	(.123)		(.126)
ln(Other Tax Revenues)		.548**		.545**		.672**		.673**	.678**	.68**
		(.064)		(.063)		(.096)		(.093)	(.098)	(.095)
Simple Average Tariff Rate									.005	.005⁺
									(.003)	(.003)
R²	.97	.97	.97	.97	.96	.97	.96	.97	.97	.97

Note. For these fixed-effects estimates, the dependent variable is the log of Import Duties. Standard errors (corrected for clustering by country) are in parentheses. All regressions include country fixed effects, year fixed effects, and country-specific linear time trends. For the regressions using all observations, $N = 104$ total countries and 19 countries observed before and after PSI program implementation. For the regressions using observations with tariff data, $N = 85$ total countries and 18 countries observed before and after PSI program implementation. The variable PSI is an indicator that a preshipment inspection (PSI) program is active for at least half of a given year. PSI, years 0–4 (PSI_NEW$_{jt}$) is a variable equal to one if an active PSI program is in its first to fifth year of operation. PSI, years 5+ (PSI_OLD$_{jt}$) is a variable equal to one if an active PSI program is in its sixth or later year of operation. Other Tax Revenues is the total tax revenue minus the import duties. One country with a PSI program (Belarus) was dropped from the calculations used for the data in columns 4–7 because of an absence of sufficient tariff data.

⁺ $P < .10$.
* $P < .05$.
** $P < .01$.

in its first 5 years of operation is associated with the log of import duties that are higher by .20, while the coefficient on the indicator for PSI programs in years 5+ is roughly twice that magnitude.[18]

To gauge whether changes in customs 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. For the data in columns 5–10, the observations are limited to those with complete tariff information. The data in columns 5–8 are derived from specifications identical to those used for the data in columns 1–4 to confirm that the change in sample composition does not materially affect the estimates. The coefficients on PSI_{jt}, PSI_NEW_{jt}, and PSI_OLD_{jt} are very similar in magnitude to the results in the larger sample and are not greatly affected when the control for the log of other tax revenues is included.

For the data in columns 9 and 10, a linear control for the tariff rate is included in both regressions, and the 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).[19]

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. However, there is the possibility that increases in the log of import duties in later years of PSI programs may be due to other policy changes that occur some time after PSI programs are implemented (see the discussion in Section 3.5).

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

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[18] These 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.

[19] The 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 changes materially with the introduction of PSI. Table A1 presents regression results from estimations of equations (1) and (2) in which the outcome variable is the log of other tax revenues for the data in columns 1 and 2 and the simple average tariff rate for the data in columns 3 and 4. The coefficients on all PSI variables are all small in magnitude, and none is statistically significantly different from zero.

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 main regression equation (2). Each column contains data for a different specification. Columns 1–5 should be compared with column 4 of Table 5, the specification controlling for the log of 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 three 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 for when the sample is restricted to countries that are observed for all or nearly all 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 duties may not serve as useful counterfactual cases (in particular, small island nations such as St. Vincent and the Grenadines, the Seychelles, and the Maldives). 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 to 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 data set for countries that used PSI in the past but 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. For the data in column 4, I estimate a separate past-PSI effect by including a variable in the regression analysis (Past PSI) that 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 Past PSI, the regression estimates of the impact of PSI in effect derive only 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 Past PSI is itself small and statistically insignificant.

Column 5 presents regression results for when a small number of outliers of the log of 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 .199 to .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 the log of other tax

Table 6
Robustness Checks for the Impact of Preshipment Inspection on Import Duties

Specification	Countries with ≥15 Observations (1)	Countries with ≥18 Observations (2)	Countries with Population ≥1 Million (3)	Past PSI (4)	With Outliers of Import Duties (5)	With Controls for Other Tax Revenues and Tariffs (6)
PSI, years 0–4 (PSI_NEW$_{jt}$)	.194**	.185**	.194**	.202**	.169**	.143*
	(.055)	(.060)	(.057)	(.071)	(.063)	(.068)
PSI, years 5+ (PSI_OLD$_{jt}$)	.445**	.539**	.425**	.436*	.4**	.376**
	(.109)	(.104)	(.132)	(.174)	(.135)	(.109)
Past PSI				.013		
				(.161)		
Observations	937	745	1,188	1,372	1,382	998
R^2	.98	.98	.96	.97	.97	.97
Countries	49	37	88	104	105	37
Countries observed pre- and post-PSI	14	11	19	19	19	11

Note. For these fixed-effects estimates, the dependent variable is the log of Import Duties. Standard errors (corrected for clustering by country) are in parentheses. All regressions include country fixed effects, year fixed effects, and country-specific linear time trends. Past PSI is a variable equal to one if a preshipment inspection (PSI) program is absent for at least half a year but the country had previously had a PSI program and is zero otherwise. The regressions in columns 1–5 include a control for the log of Other Tax Revenues. The regression in column 6 includes controls for a 10-piece linear spline in the log of Other Tax Revenues and Simple Average Tariff Rate.
* $P < .05$.
** $P < .01$.

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 for the data 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, and this finding provides little indication that one should be concerned about improper specification of the log of 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 the log of 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 they 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, one for regime X1 and one for regime X2. The impact of PSI would then be identified from the variation in PSI implementation only within (and not across) policy regimes.

The key question is how one defines a policy regime through 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 more stable in periods when countries' political and bureaucratic leadership are constant than when leadership is changing. This assumption requires that the variation in policies enacted across leaders is greater than the variation in policies enacted within a given leader's term of office.[20]

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 anticorruption efforts and revenue collection. Second, the finance minister sets priorities between collection of customs duties and other types of government

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

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 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 nongovernmental organizations.[21] Data on national leaders and finance ministers come from four published sources: the *Statesman's Yearbook* (1980–2000), the *International Yearbook and Statesmen's Who's Who* (1980–2000), the *Europa World Yearbook* (1980–2000), and the *Worldwide Government Directory* (1980–2000). In cases in which 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), so policy regime fixed effects were defined only for those countries.[22] An example of the data collected is shown for Indonesia in Table A2. The periods of constant leadership across the three positions separate Indonesia into nine policy regimes.

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 the coefficient estimates decline substantially in magnitude, that would suggest that the association between PSI and increases in import duties is mainly due to other concurrent policy changes that accompany leadership changes. However, if the coefficient estimates remain stable, that would bolster the case for PSI's causal impact on import duties.

The results are presented in Table 7. The results of the baseline specifications (from Table 5) are provided in the odd-numbered columns of the table for comparison, while the results of the specifications controlling for policy regime fixed effects are in the even-numbered columns. For the data 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 percent

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[21] In small spans of years for four countries, information on the head of customs was not available, and I assumed that the head of customs was unchanged during these years (Paraguay 1980, Peru 1980–84, Pakistan 1980–85, and Kenya 1980–86). None of these countries implemented PSI programs during these periods, so this assumption should not affect the results. For the leadership data and further details on the data collection methods and sources, see Yang (2005).

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

Table 7
Impact of Preshipment Inspection on Import Duty Collection, Controlling for Policy Regime

Sample	All Observations ($N = 1{,}372$)				Observations with Tariff Data ($N = 998$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PSI	.229**	.291*			.192*	.262[+]		
	(.067)	(.114)			(.079)	(.138)		
PSI, years 0–4 (PSI_NEW$_{jt}$)			.199**	.282*			.154*	.248[+]
			(.056)	(.126)			(.065)	(.148)
PSI, years 5+ (PSI_OLD$_{jt}$)			.43**	.319**			.431**	.306**
			(.132)	(.089)			(.126)	(.113)
ln(Other Tax Revenues)	.548**	.481**	.545**	.481**	.678**	.598**	.68**	.598**
	(.064)	(.088)	(.063)	(.088)	(.098)	(.131)	(.095)	(.131)
Simple Average Tariff Rate					.005	.007*	.005[+]	.007*
					(.003)	(.003)	(.003)	(.003)
Policy regime fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
R^2	.97	.98	.97	.98	.97	.98	.97	.98

Note. For these fixed-effects estimates, the dependent variable is the log of Import Duties. Standard errors (corrected for clustering by country) are in parentheses. All regressions include country fixed effects, year fixed effects, and country-specific linear time trends. Policy regime fixed effects (defined only for the 19 preshipment inspection [PSI]-using countries in the sample) are separate indicator variables for each period in which national leader, finance minister, and customs head were all constant.

[+] $P < .10$.
* $P < .05$.
** $P < .01$.

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, the coefficients on PSI_{jt} and on PSI_NEW_{jt} are larger in magnitude than those in the baseline specification (and remain statistically significant at the 10 percent 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 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 increases in magnitude. Preshipment inspection 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.[23] This result 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 the Effect of Preshipment Inspection 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, the official import documentation must be altered to hide evidence of such theft. Preshipment inspection could help raise import duty collections by simply making it more difficult for importers or customs officials to falsify the import documentation. In addition, if customs corruption leads to higher prices paid for imports by

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

domestic consumers, PSI could lead to reduced import prices, greater import demand, and increased import duty collections.

In this section, I first examine the impact of PSI on measures that are likely to capture two types of misreporting: (1) underreporting of import values and (2) misreporting of goods classifications. Import duties are typically assessed as a fraction of the 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.[24] All other things being 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 because of transport costs and export misreporting, cross-sectional differences between countries' import capture ratios cannot be completely ascribed to differences in undervaluation.[25] 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 Pro-

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[24] The 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.

[25] Import data reported by destination countries typically include the cost of freight and insurance, while export data collected by origin countries do not (exports are free on board).

duction data set (Nicita 2000).[26] Discrepancies in some trade data items create import capture ratios that are in some cases quite extreme (very small or 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 of the import capture ratio is −.25, with an SD of .60.

Undervaluation is not the only method of concealing the avoidance or theft of import duties, however. Another generic strategy is to misreport 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 misreporting of goods classifications, I use the coefficient of variation of the log of the import capture ratios across goods within a country. The basic insight is that misreporting increases the dispersion of import capture ratios across goods vis-à-vis the initial situation in which there was no misreporting: import capture ratios decrease for goods with higher tariffs (as goods are misreported as being in other categories with lower tariffs), and import capture ratios increase in turn for goods with lower tariffs. All other things held equal, then, an increase in the misreporting of goods classifications should lead to an increase in the coefficient of variation of import capture ratios across goods within a country, while declines in misreporting of goods classifications should lead to a corresponding decrease. To construct the coefficient of variation of the log of the import capture ratios across goods within a country, I use bilateral import and corresponding export data for 82 International Standard Industrial Classification four-digit goods classifications in the World Bank's Trade and Production data set.[27] The mean of this measure in the sample is .54, with an SD of .39.

In addition to these two measures of import misreporting, I examine the impact of PSI on the total volume of imports to identify any trade-facilitating effect of the program that may reflect declines in importers' costs (due to declining bribe payments). To separate PSI's trade-facilitating effect from its effect on misreporting, 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

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[26] The crucial feature of this data set is its inclusion of a country's import data as well as the corresponding export data from trade partner countries. (In addition, the trade data are also disaggregated by product, which is useful for the next measure of misreporting, discussed below.) The number of observations in the sample decreases owing to the more limited inclusion of countries in this data set. The resulting sample includes 581 observations from 39 countries, nine of which are observed before and after the start of their PSI programs.

[27] Import capture ratios are likely to contain substantial noise due simply to reporting errors in both the importing and the 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 fluctuations 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 calculating 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 (reported by the trade partner). This exclusion eliminates a very small amount of trade by value (substantially less than 1 percent of the total trade in the data set).

Table 8
Impact of Preshipment Inspection on Determinants of Import Duty Collection

Dependent Variable	ln(Import Duties) (1)	ln(Import Capture Ratio) (2)	Product-Level ln(Import Capture Ratio)[a] (3)	ln(Imports)[b] (4)
PSI, years 0–4 (PSI_NEW$_{jt}$)	.23**	.067*	−.064*	−.014
	(.082)	(.028)	(.026)	(.052)
PSI, years 5+ (PSI_OLD$_{jt}$)	.529**	−.012	−.057	.12*
	(.164)	(.032)	(.039)	(.046)
ln(Other Tax Revenues)	.667**	.08	−.033	.426**
	(.132)	(.049)	(.057)	(.071)
R^2	.97	.72	.54	.99

Note. Values presented are fixed effects estimates. Standard errors (corrected for clustering by country) are in parentheses. All regressions include country fixed effects, year fixed effects, country-specific linear time trends, and the log of Other Tax Revenues. The import capture ratio is the ratio of a country's self-reported total imports to the corresponding reported exports of other countries to said country. $N = 581$ observations, 39 total countries, and 9 countries observed before and after PSI program implementation.
  [a] Coefficient of variation of the log of the import capture ratios across 82 product groups within the country-year cell.
  [b] Exports reported by all other countries as destined for the country in question.
  * $P < .05$.
  ** $P < .01$.

as destined for the country in question as the import measure (partner-reported imports). The source for these data is also the World Bank's Trade and Production data set.[28]

To assess the impact of PSI on misreporting and on import volumes, I estimate equation (2), in which the outcome variable is a country's overall import capture ratio, the coefficient of variation of the log of the import capture ratios across product groups, or the log of partner-reported imports. The results are presented in Table 8.

The sample size has changed from that used for the regressions reported 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 the log of import duties. As before, the coefficients on the new and old PSI indicators are positive and statistically significantly different from zero, and they are very similar in magnitude to the estimates from previous tables.

In column 2, 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 percent. 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 coefficient on PSI_NEW$_{jt}$ is negative and statistically significantly different from zero. As discussed above, reductions in misreporting of

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[28] This data set includes export data from 67 countries, so partner-reported import data will be from this set of countries.

goods classifications should lead to decreased dispersion of import capture ratios (a lower coefficient of variation) within a country-year cell. So this result suggests that PSI leads to a decline in misreporting of goods classifications. The coefficient estimate is not small, amounting to roughly one-sixth of an SD of the dependent variable. The coefficient on the PSI_OLD_{jt} indicator is negative and similar in magnitude, but its SE is quite large, so it is only of marginal statistical significance (the *P*-value is .15). This finding 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, 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 the log of partner-reported 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 misreporting 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 consider only the causal impact of newer PSI programs to be relatively well established and to take the estimated effect in later years to be more speculative. First, consider the graphical evidence of Figures 1 and 2. In Figure 1, the post-PSI graph of the log of import duties appears to reach a temporary plateau in years 3–7 before jumping in year 8 to a higher level and then roughly stabilizing again. This upward jump roughly coincides with the increase in the log of other tax revenues illustrated 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 (as was shown in Table 7) somewhat reduces the estimated impact of older PSI programs (the coefficient on PSI_OLD_{jt}), which suggests that the large increases in import duties associated with older PSI programs may be driven in part by unobserved policy changes. Finally, the channels analysis (shown in Table 8) finds statistically significant declines in underinvoicing and goods misclassification only in the initial period of PSI programs.[29]

[29] This 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.

Table 9
Cost-Benefit Calculation

Costs and Benefits	Value
Fees paid to PSI firms:	
Annual imports	100.00
Percentage of imports inspected	90
PSI fees as a percentage of the value of imports inspected	1
Annual fees paid to PSI firms	.90
Improvements in import duties:	
Import duties as a percentage of imports	15.25
Preprogram level of annual import duties	15.25
Effect of PSI on ln(Import Duties)	.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

Note. The assumed impact of preshipment inspection (PSI) on import duties (in years 0–4) is from a regression that includes controls for a 10-piece linear spline in the log of other tax revenues and the simple average tariff rate. Year 0 is the first year that a PSI program has been active for at least half a year. Initial import duties as a share of imports was calculated from 5 years immediately prior to the adoption of PSI programs for the 19 countries in the sample that were observed before and after the PSI start date. Preprogram imports were normalized to 100.

Do PSI-generated improvements in import duties exceed the program's 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 .14 to .28 across Tables 5–7. To be conservative, I take the lowest of these estimates to be the impact of PSI on the log of import duties in the first 5 years of a PSI program. I assume that 90 percent of a country's imports are inspected and that PSI fees are 1 percent of the value of inspected goods. I further assume that there is no impact of new PSI programs on the growth of imports (as shown in Table 8). Finally, I let import duties start at 15.25 percent 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). After normalizing the level of imports to 100 prior to the program, this means that import duties are 15.25 prior to the implementation of PSI.

Table 9 presents a comparison of estimated annual costs and benefits in the first 5 years (years 0–4) of a typical PSI program. Preshipment inspection 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 owing 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 article 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 in which governments hire private firms to conduct PSIs of imports and to generate 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 percent during the 5 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 the 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 misreporting of goods classifications in customs. Hired integrity in this case is quite cost-effective, with improvements in import duty collections during the first 5 years of a typical inspection program amounting to 2.6 times the program's costs.

While this article 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 PSI 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 misreporting 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[30] and ways in which the programs can be modified to improve their effectiveness.

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[30] For example, I document the failure of increased monitoring within a PSI program (Yang 2008).

Appendix

Table A1

Impact of Preshipment Inspection (PSI) on Other Tax Revenues and Tariff Rates

Dependent Variable	ln(Other Tax Revenues)		Simple Average Tariff Rate	
	Equation (1)	Equation (2)	Equation (1)	Equation (2)
PSI	.007 (.092)		.908 (1.957)	
PSI, years 0–4 (PSI_NEW$_{jt}$)		.001 (.097)		1.156 (2.087)
PSI, years 5+ (PSI_OLD$_{jt}$)		.051 (.117)		−.663 (2.533)

Note. The data are fixed-effects estimates, and standard errors (corrected for clustering by country) are in parentheses. All regressions include country fixed effects, year fixed effects, and country-specific linear time trends. For the regressions using the log of Other Tax Revenues, $R^2 = .99$, and $N = 1,372$ observations, 104 total countries, and 19 countries observed before and after PSI program implementation. For the regressions using Simple Average Tariff Rate, $R^2 = .93$, and $N = 998$ observations, 85 total countries, and 18 countries observed before and after PSI program implementation.

Table A2

Indonesian National Leaders, Finance Ministers, and Heads of Customs, 1980–2000

Year	National Leader	Finance Minister	Head of Customs
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

Sources. The data sources for national leader and finance minister are *Statesman's Yearbook* (1980–2000), *International Yearbook and Statesmen's Who's Who* (1980–2000), *Europa World Yearbook* (1980–2000), and *Worldwide Government Directory* (1980–2000). The data source for head of customs is the U.S. Commercial Service, Jakarta.

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