Do Private Inspection Programs

Affect Trade Facilitation?[1§]

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Abstract

This paper explores the "facilitation" effect of private inspection programs on trade volumes using a panel gravity equation. We use a differences-in-differences (DID) estimator correcting for serial correlation using the method of Bertrand et al. (2004) and also instrument for inspection programs using political variables. We find that private inspection has had a positive and significant trade-facilitation effect, raising import volumes for countries using those programs by 5 to 10%. Given that the existing literature has failed to find much effect of inspection programs on collected tariff revenue, our finding suggests that the benefit of those programs may be associated with reforms and best practices brought in by private inspection firms in terms of clearance times and process reliability rather than in improved tax collection.

Keywords: International trade, facilitation, PSI, gravity

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1. Introduction

Private inspection of international trade shipments, whether at embarkation or at disembarkation, has been used over the last half-century for a variety of purposes. In the 1970s, when capital controls and exchange-rate regulations were widespread, governments used pre-shipment inspection (PSI) to prevent under-invoicing of exports and over-invoicing of imports, two common forms of capital flight. As exchange-rate misalignment and capital controls progressively receded in the late 1980s, governments turned to PSI to assist weak and corrupt customs to improve import-duty collection.[2] In both cases, the vast majority of client governments were in developing countries and PSI was sometimes pressed on less-than-enthusiastic governments by donors. Since the adoption in 2002 of the US Container Safety Initiative (CSI), inspection has started to be used for an altogether different purpose –to control the risk of smuggling of weapons of mass destruction by international terrorist networks. Client governments for this type of services are typically industrial countries rather than developing ones, and the purpose of the inspection is to check the exact nature of the shipment rather than its value. Both CSI and PSI are elements which impact the transparency of the trading environment of nations and ought to be looked at carefully (on this, see Helble et al., 2009).

In all cases, private inspection entails potential costs to traders and thus, ultimately, to final consumers. First, private inspection costs are typically billed to the importing country government and the fees can be substantial (up to 1% of shipment value). Second, inspection can slow down the logistics at embarkation and disembarkation points. The physical form of inspection varies and technology, soft and hard, has much improved the way it is done. Best practices today combine scanning (which in itself takes only a few minutes for each container, but often creates bottlenecks in harbor logistics) with random inspection based on risk-assessment techniques (e.g. "profiling" of traders and transit companies). Nevertheless, even if technology has made inspection less burdensome than it used to be, time to export is a significant determinant of comparative advantage for a country (cf. Wilson and Li, 2009) and speed very much defines competitive advantage in the logistics industry - (on this see e.g. Shawdon 2006), and thus concerns have been recently voiced about the nuisance potential of mandatory inspection programs.[3] Regions like Sub-Saharan Africa suffer from already high trading costs and the

[2] The first PSI program set up with the mission of improving import-tax collection was Indonesia's, in 1985. The mission was then to curb import under-invoicing instead of over-invoicing.

[3] These concerns have been voiced in professional publications like the *Journal of Commerce* or *Traffic World*, in particular in the context of the CSI. Of particular concern are the 24-hour rule and the Customs-Trade Partnership Against Terrorism program. See also Edmonson (2006). Shawdon (2006) also reports concerned voiced in a series of interviews conducted with 52 intercontinental shippers. Giermanski (2007) however discusses how new technology like "smart containers" can mitigate the slowing-down effect of inspection.

apprehension stemming from increased pre-shipment controls are understandable[4].

Because the CSI is relatively new, it is too early to assess its effect on aggregate trade flows. By contrast, evidence on pre-shipment inspection (PSI) and more recent Destination Inspection (DI) programs is readily available since it has been in use for many years, and the experience should be relevant since the inspection techniques are essentially the same irrespective of what the inspection is done for.[5] The idea of this paper is thus to draw from the experience of private inspection programs (PSI and DI) to assess whether inspection affects trade flows and, if yes, how.

Although private inspection and PSI have always been controversial, the empirical literature on its effects is relatively limited and its results are ambiguous. Low (1995) surveyed the evidence from an institutional point of view and noted that performance varied widely depending on the form of the contract and on the relationship between the surveillance company and the client government.[6] However one empirical regularity was that capacity building (the transfer of knowledge by private surveillance companies to customs) was typically the weak point of PSI. Yang (2005) studied the Philippines' PSI program and showed that each time a new origin country was covered, imports from that country were deflected to the Philippines' export-processing zones and then brought in fraudulently into the domestic market. Thus, the program was generating new forms of fraud rather than curbing it. Anson et al. (2006) studied four PSI programs and showed that they had two effects working at cross-purposes: on one hand, PSI generates valuable information that the client government can use to curb fraud; on the other hand, it de-motivates customs administrations, so that part of the information brought in by the private company simply substitutes for slackening customs effort. They showed with a simple model that the "perverse" effect (customs de-motivation) can sometimes more than offset the direct effect, resulting in less duty collection rather than more, and verified that this seemed indeed to be the case in Argentina and Indonesia. Yang (2008) was more positive, showing that PSI did have some effect in curbing customs corruption.

This literature leaves unanswered an important question. Improving tax collection for budget purposes is one thing; encouraging trade is another. A vast literature suggests that trade openness is correlated with economic growth (for a recent empirical investigation of this link, see Wacziarg and Welsh 2008). Another strand of literature highlights the importance of trade facilitation in encouraging trade, relative to the reduction in traditional tariff and non-tariff barriers (see e.g. Wilson, Mann and Otsuki 2003, Portugal-Perez and Wilson 2010). Does private inspection encourage trade? Private

[4] Portugal-Perez and Wilson (2009) provide an overview of the barriers to trade in Africa

[5] The technology, however, has changed as scanning has become sufficiently cheap to be used in almost all ports and is used by DI.

[6] Ramirez (1992) and Byrne (1995) also provide brief discussions of the arguments pro and against PSI.

surveillance companies typically bring with them best practices in terms of customs and transit procedures, potentially resulting in shorter and less variable clearance times. This could encourage trade, partly or totally offsetting the slowing-down effect discussed above.

We set out in this paper to explore the effect of private inspection on trade volumes, using the most common vehicle to test the effect of policy changes on trade volumes: the gravity equation. We estimate a standard version of the gravity equation with importer and exporter fixed effects on a large panel of countries tracked over twenty-six years.

Estimating the effect of private inspection on trade flows is a standard case of "treatment effects". We use a difference-in-differences (DID) estimator which consists, intuitively, of comparing the difference in an outcome variable (here the volume of trade) between before and after the treatment for a "treatment group" (here the countries that had private inspection programs at some point) and for a "control group" (here the countries that never had private inspection programs). The average effect of the treatment on the treated is then the estimate of the coefficient on a dummy variable equal to one during the treatment period (private inspection program) for the treatment group (private inspection-using countries).

DID estimation raises two specific issues. First, as discussed in Bertrand, Duflo and Mullainathan (2004), possible serial correlation in the outcome variable (here trade) is exacerbated by very strong autocorrelation in the treatment variable (a binary variable that changes value only once or twice in the sample period). The result, they show, is a high probability of type-I error (rejecting the null hypothesis of no effect when it is true, i.e. when there is actually no effect).[7] We control for this using a two-step method suggested in their paper. In a first stage, we run a standard panel gravity equation for all countries and years and retrieve the residuals. In a second stage, we keep those residuals only for the treatment group (countries that had a private inspection program at some point), take their without- and with-treatment averages, and run a panel regression on the resulting two-period panel with a dummy for the treatment period.

Second, when the treatment is a policy –as it is here– its assignment is at least partly the voluntary decision of the treated; therefore, it depends on observed and unobserved characteristics of the country. If it depended only on observed characteristics ("selection on observables") those could be controlled for in the treatment-effect regression. But if it depends also on unobservable characteristics that are correlated with trade volumes, there will be correlation between one regressor (the dummy variable marking private inspection treatment) and the error term. OLS estimates will then be biased. We address

[7] Performing repeated estimations on "manufactured data" with non-existent (placebo) treatments, they showed that the null was rejected in up to half the cases when it should be no more than 5% if standard errors had been correctly estimated.

this endogeneity bias in several ways. First, following the tradition in the policy-evaluation literature, we use fixed effects to control for unobserved time-invariant country characteristics that might affect both their trade performance and their willingness to adopt a private inspection program. However, as discussed in Besley and Case (2000), this fix may not be enough if omitted characteristics are time-variant. They suggest an instrumental-variable approach in which domestic political factors are used as instruments. We follow this approach and instrument PSI by governance indicators and their square.

For robustness, we compare our IV estimates with those obtained from Blundell and Bond's system-GMM.[8] We also check for "fortuitous" policy effects by running our DID regression with fictitious starting points for private inspection programs, five years before and after the program was actually put in place, and comparing the measured effects with those when the true starting date was used. If effects are apparent several years *before* private inspection programs are put in place, it is likely that those measured effects are fortuitous.

The results so far suggest a positive effect of private inspection on trade volumes, with plausible magnitudes –roughly between 5% and 10% of trade volumes. The effect survives our various robustness checks and seems, as far as we can tell from aggregate data, to vindicate the anecdotal evidence on improvements in transit environments brought about by private firms as part of inspection contracts.

The paper is organized as follows. Section 2 presents the data and estimation issues, section 3 discusses the results, and section 4 concludes.

2. Data & estimation

2.1 Data

The sample is a panel including 179 importing countries and 170 exporting countries over 1980-2005, or a notional total of 791'180 one-way trade observations, of which 321'348 have positive trade (41%).

The dependent variable is the aggregate one-way trade value reported in the IMF's Direction of Trade Statistics (DOTS). It is customary in trade-volume studies to "mirror" export statistics, i.e. to disregard direct export statistics from the exporting country and instead to use import data from its partners. The reason is that customs typically monitor imports (on which duties are

[8] System-GMM runs two equations, one in levels and one in first differences, in which endogenous RHS variables are instrumented in the level equation by first differences and in the differenced equation by their past levels.

based) better than exports (rarely taxed). However our study purports to measure only the trade-facilitation effect of PSI, not its effect on the capacity of customs to record imports correctly. Mirrored import data would confound these two effects and would thus potentially bias results upward, generating a statistical illusion of increased volumes. In order to avoid this source of bias, we use direct export statistics, at the cost of having noisier data than if we had used mirroring.

Standard gravity regressors include GDPs in constant 1995 dollars, taken from the World Bank's World Development Indicators; "great-circle" distances between the main industrial agglomerations of countries in the sample, taken from CEPII,[9] and dummy variables for common land borders, common official languages, and formal colonial ties.

The "treatment variable" is equal to one when an inspection program is in force in the importing country *j* of a pair (*i,j*) at time *t*. It covers programs run by the largest four firms in the industry, Société Générale de Surveillance (SGS) and Cotecna Inspection SA, both based in Geneva; BIVAC International (a subsidiary of Paris-based Bureau Veritas), and Intertek, based in London. The list of programs is given in Table 1.

Table 1
Programs covered

Table 2 shows descriptive statistics. For dummy variables, the mean is the proportion of the sample's observations for which the variable is equal to one, i.e. the incidence of the variable.

Table 2
Descriptive statistics

Figure 1 shows the percentage of importing countries with inspection programs in force over the sample period and the percentage of trade flows covered.

Figure 1
Percent of importing countries and trade flows covered by inspection programs, 1980-2006

It can be seen that, at about 5%, the proportion of trade covered by inspection is much smaller than the proportion of countries with inspection programs in force (close to a quarter); this is because countries with inspection programs tend to be poor ones. Also, both proportions have been rising roughly up till

[9] So-called "great-circle" distances are simply the shortest routes along the earth's surface, irrespective of actual terrain.

the mid-1990s, after which they peaked and show a timid downward blip in the last year or so.

2.2 Estimation

Our basic equation is a Difference-in-Differences (DID) augmented-gravity equation in which the "treatment group" is the set of countries having used inspection programs and the "control group" is the set of countries never having used such programs. That is, let M_{ijt} be the log of *j*'s aggregate imports from *i* in year *t*, y_{it} and y_{jt} the log of the exporter's and importer's GDPs in year *t*, δ_{ij} the log of their distance, $\mathbf{X}_{ij}$ a vector of time-invariant controls (common language, common border, past colonial ties), c_i and c_j exporter and importer fixed effects, and τ_t a time effect for year *t*. The equation is

$$\begin{aligned} M_{ijt} = {} & \alpha_1 y_{it} + \alpha_2 y_{jt} \\ & + \alpha_3 \delta_{ij} + \alpha_4 p_{jt} + \mathbf{X}_{ij}\boldsymbol{\beta} \\ & + \alpha_i + \alpha_j + \alpha_t \end{aligned} \qquad (1)$$

where

$$p_{jt} = \begin{cases} 1 & \text{if } j \text{ has an inspection program in force in year } t \\ 0 & \text{otherwise.} \end{cases} \qquad (2)$$

The estimate of α_4 is the basic estimate of the average treatment effect for the treated. However, as discussed in the introduction, OLS estimates of (1) suffer from several possible sources of bias. First, as discussed in Bertrand, Duflo and Mullainathan (2004), possible serial correlation in the outcome variable (here trade) is exacerbated by very strong autocorrelation in the treatment variable (a binary variable that changes value only once or twice in the sample period). The result, they show, is a high probability of type-I error (rejecting the null hypothesis of no effect when it is true, i.e. when there is actually no effect).[10] We control for this using a two-step method suggested in their paper. In a first stage, we run a standard panel gravity equation for all countries and years and retrieve the residuals. In a second stage, we keep those residuals only for the treatment group (countries that had an inspection program at some point), take their without- and with-treatment averages, and run a panel regression on the resulting two-period panel with a dummy for the treatment period.

[10] Performing repeated estimations on "manufactured data" with non-existent (placebo) treatments, they showed that the null was rejected in up to half the cases when it should be no more than 5% if standard errors had been correctly estimated.

In addition to autocorrelation issues, the coefficient on the treatment variable would provide an unbiased estimate of the effect of the treatment only if that treatment was randomly assigned, so that individuals who did not get it (the control group) were statistically identical to those who did (the treatment group). This is obviously not the case here since the treatment (an inspection program) is chosen by the individuals (the countries). Beyond the observable characteristics that may influence the decision to adopt an inspection program, which can be controlled for in the regression, there may be unobservable characteristics, picked up by the error term, that also influence the decision to adopt inspection programs. In that case the inspection variable will be correlated with the error term and all estimates will be inconsistent. Several fixes can be used, none of which is perfect (see Besley and Case 2001 for a discussion).

First, country fixed effects control for time-invariant country characteristics that may simultaneously depress the country's foreign trade and raise its probability of adopting inspection programs. However, fixed country effects do not control for time-variant omitted variables. In order to control for those, one may instrument the treatment variable with variables correlated to it but not to the error term. The estimation procedure is implemented by Stata's treatreg command and is akin to a Heckman two-stage procedure. The first stage is a probit regression of the treatment on the instruments and all exogenous variables, and the second stage is an OLS regression of the outcome variable on the treatment, all exogenous variables, and the estimated hazard rate retrieved from the first stage. The two stages can also (and more efficiently) be estimated simultaneously by maximum-likelihood.[11]

The key issue is of course to find good instruments. We follow the approach suggested by Besley and Case (2001) and use political instruments taken from the World Bank's worldwide governance database. Specifically, we use Kaufman's governance index (the WB- Worldwide Governance Indicators (WGI) project[12]) and its square, based on the idea that very corrupt and very honest governments would be reluctant to use inspection programs, the first because it would reduce corruption opportunities and the second because inspection would not be needed. This suggests that the probability of using an inspection program (the dependent variable in the first-stage probit) is a concave function of governance, something that can be captured by a second-degree polynomial.

When no good instruments are available, the alternative route consists of using the Generalized Method of Moments (GMM), and in particular Blundell and Bond's system-GMM (Blundell and Bond 1998). The system-GMM estimator runs two simultaneous equations, one in levels and one in first differences, in which endogenous RHS variables in levels are instrumented by

[11] Both options are available in Stata's treatreg command, and results reported in this paper are based on the second method.
[12] http://info.worldbank.org/governance/wgi/index.asp

their lagged first differences in the level equation, and by their lagged levels in the differenced equation. Suitable instruments can of course be added in the levels equation if available. The number of lags used as instruments is the experimenter's choice, and it has been argued that this latitude sometimes makes results unstable. There is however a rule of thumb according to which the number of lags should not be larger than the number of individuals in the panel. In our case, this rule of thumb is of course not binding because the number of lags is anyway severely limited by the panel's short duration. We report GMM results and compare them with IV results for robustness.

3. Results

3.1 Baseline estimates

Table 3 presents estimation results using OLS and instrumental-variable estimation.

Table 3

Estimation results : OLS and IV

Regression (a) gives OLS estimates (with importer, exporter and time effects) for comparison with instrumental-variable regressions. Regressions (b) and (c) are two-stage treatment-effect regressions where PSI is instrumented in two different ways. In (b), the first-stage regression (a probit) is on corruption and its square. As expected, the probability of observing a PSI program is a concave function of corruption control, but the turning point is around an index value of 10, so over the relevant range it is rising in corruption control. The two instruments together pass Sargan's overidentification test. In (c), the first-stage regression is on a variety of instruments, which again together pass Sargan's test. Results are very similar both qualitatively and quantitatively.

The coefficient on private inspection is stable across specifications and there is no significant bias in OLS estimation. Given the semi-log specification, the coefficient on private inspection is the percent increase (decrease) in trade associated with a change in the private inspection variable from zero to one. The estimates in Table 3 suggest an increase lying between 8% and 11.5%. While in a plausible range, these estimates are quite high, suggesting, if true, that with an average tariff rate of, say 10% private inspection would roughly just pay for itself *even if it did nothing to tariff avoidance*.

3.2 Robustness

Table 4 reports several robustness exercises. First, as discussed earlier, we follow the methodology suggested by Bertrand, Duflo and Mulainathan (2004, henceforth BDM) in order to verify that our diff-in-diff estimates are not driven by serial correlation. In that procedure, the first stage is a standard gravity equation *without* the treatment variable (private inspection), and the second stage regresses, for treated countries only, the average residuals from the first stage, averaged over pre- and post-treatment periods, on the treatment variable. The sample for the second stage is thus a two-period panel including private inspection countries only (9'884 observations). Results are reported in the first two columns of Table 4, labeled (a). It can be seen that private inspection remains significant at the 1% level with an estimated coefficient (0.104) that that is just in the middle of the interval of OLS and IV estimates (0.0804-0.115). This suggests that our positive and significant results from diff-in-diff estimation are not attributable to the over-rejection of the null hypothesis generated by serial correlation in the error term.

Table 4
Robustness checks 1

Second, we re-run our treatment-effect regression but we "trick" the estimator by feeding in wrong dates for the start and end of private inspection programs. If we were to find significant estimates with the wrong dates, it would be likely that those estimates (and, by the same argument, those obtained with the right dates) were spurious. In regressions (b) and (c), we move the start and end dates of private inspection programs by 5 years forward and 5 years backward. It can be seen that the estimates become insignificant, suggesting that the estimates in Table 3 are *not* spurious.

Third, we re-estimate (1) by GMM. Estimates are all cut significantly, both on standard gravity variables (the coefficient on distance is cut in half from 1.6 to 0.8). Thus, one should be cautious about the magnitudes, although the qualitative results seem robust.

Fourth, we re-estimate (1) taking into account zero-trade values.[13] The first column of

[13] Zero trade values were also replaced for missing trade values in two different cases – first, when trade values appeared missing for all 26 years from 1980 to 2005 and second when trade values were missing for at least 6 consecutive years after 1992.

Table 5 shows results using a Tobit estimator. The private inspection variable remains significant and its marginal effect is in the same ballpark as under OLS.

Table 5
Robustness checks 2

Finally, and perhaps more importantly, it may be argued that private inspection programs have been introduced as part of broader reform packages that sometimes included trade liberalization. If that was the case, the effect of private inspection would be confounded with that of trade liberalization and we would attribute to private inspection effects that do not belong to it. The last columns of

Table 5 show estimation results with a direct measure of trade liberalization included as an additional regressor. We used the binary trade-liberalization index constructed by Wacziarg and Welsh (2008), which takes into account a broad range of trade-policy measures to identify precisely the timing of trade-liberalization episodes. Wacziarg and Welsh's index bears some similarity with the celebrated Sachs-Warner index but excludes non-trade related components like the black-market premium on foreign exchange, and covers a broad range of years and countries. Including the Wacziarg-Welsh dummy to control for trade liberalization does not affect our central result.

All in all, at this stage it seems safe to say that private inspection seem to be robustly associated with an increase in bilateral trade volumes in the range of 5%-10%. Because our exercise is based on aggregate data, we can't tell whether this increase is uniformly spread over product categories, so no inference should be drawn on the effect that private inspection can have on collected duties. Indeed, as discussed in the introduction, results so far are inconclusive on that front.

4. Concluding remarks

Our results seem to suggest that the facilitation effect of private inspection is statistically traceable in trade data. Using bilateral export volumes as our variable of interest (in order to avoid picking up the effect of improved book-keeping on the importing side, which would have no "real" counterpart) we find that private inspection seems to have been associated with an increase in trade between 5% and 10%. Thus, as conjectured at the start of this paper, the burden of inspection and paperwork associated with inspection seems to have been more than offset by improved facilitation at destination. Indeed, anecdotal evidence seems to suggest that at least some surveillance companies have put in place programs to foster the adoption of best practices at destination ports and customs, including container tracking systems, electronic payment of duties, and so on. Technology is clearly part of the story. For instance, recent destination inspection programs have spread the use of scanners in lieu of physical inspection (physical inspection being reserved for suspicious cases). The spread of control systems in international trade may thus be similarly associated with improved logistics and enhanced used of information systems which may, in the end, facilitate transit rather than hampering it.

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Tables and Figures

Tables

Table 1
Programs covered

Country	Period	Company	Type of contract	Contract value a/
	1979-2001	SGS	Exclusive	15
Angola	Jan 2002-Jun 2005	Cotecna		
	2002-2006	BIVAC		
Argentina	1998-2001	SGS	Importer's Choice	25
Bangladesh	1994-1997	SGS	Geographical	10
	Nov 1994-Jan 1999/Sept 2005 - 2006	Cotecna	3 other companies involved	
Belarus	1997-1999	BIVAC		
Benin	Jan 1991 - Jan 1996	Cotecna	Exclusive	
	2001-2006	BIVAC		
Bolivia	1986-1987/1990-2003	SGS	Importer's Choice	10
Burkina Faso	1992-2004	SGS	Exclusive	5
	July 2004-2005	Cotecna	Exclusive	
Burundi	1978-2008	SGS	Exclusive	1
Cambodia	1995-1999/2000-2004	SGS	Exclusive	3
	2005-2006	BIVAC		
Cameroon	1988-2006	SGS	Exclusive	15
Central Afr. Rep.	1990-2003	SGS	Exclusive	1
	2004-2006	BIVAC		
Chad	Sept 1994-Jan 1996	Cotecna	Exclusive	
	2004-2006	BIVAC		
Colombia	95- Feb 1998-Jun 1999	Cotecna	1 other company involved	
Comoros	1995-2005	Cotecna	Exclusive	
Congo (Brazzaville)	1993-1998	SGS	Exclusive	2
	2001-2006	BIVAC/Cotecna		
Congo (Dem. Rep.)	1965-2006	SGS	Exclusive	3
	2006	BIVAC		
	1975-2000	SGS	Exclusive	15
Cote D'ivoire	Jul 2000-Mar 2005	Cotecna	1 other company involved	
	2006	BIVAC		
Djibouti	Jun 1996-Nov 1997	Cotecna	Exclusive	
Dominican Republic	Apr 2003-Apr 2005	Cotecna	Exclusive	
Ecuador	1985-1988/1994-2006	SGS	Importer's Choice	12
	1994-2005	Cotecna	3 other companies involved	
Equat. Guinea	1982-1989	SGS	Exclusive	0.1
Ethiopia	1993-1995/2000-2004	SGS	Exclusive	10
Ghana	1971-1997	SGS	Geographical Split	6
	Aug1994-2005	Cotecna	2 other companies involved	
Gambia	2001-2002	BIVAC		
Georgia	1999-2001	Intertek		
Guatemala	1986-1987	SGS	Exclusive	3
Guinea (Conakry)	1996-2004	SGS	Exclusive	6

Table 1 (continued)
Programs covered

Country	Period	Company	Type of contract	Contract value a/
Haiti	1983-1994/2003-2006	SGS	Exclusive	1/3
India	2001-2006	SGS	Exporter/Importer Choice	n.a.
Indonesia	1985-1997/2003-2006	SGS	Exclusive	2
Irak	Feb 1998-Oct 2004	Cotecna	Exclusive	
Iran	1996-2006	SGS	Importer's Choice	3
Jamaica	1985-1988	SGS	Exclusive	4
Kazakhstan	1995-1997	SGS	Exclusive	11
Kenya	1972-1990/1994-1999	SGS	Geographical Split	3/10
	Jan 1988 –June 2005	Cotecna	2/1 other companies involved	
Laos	2001-2002	BIVAC		
Liberia	1985-1997	SGS	Exclusive	4
	1997-2006	BIVAC		
Madagascar	1983-1991/2003-2007	SGS	Exclusive	12
	1993-2002	BIVAC		
Malawi	1989-1997/2001-2003	SGS	Exclusive	5
	2004-2006	Intertek		
Mali	1989-2003	SGS	Exclusive	7
	Oct 2003-2005	Cotecna	Exclusive	
	2007	Intertek		
Mauritania	1994-1995/1999-2001/2004-2005	SGS	Exclusive	2
Mexico	1985-2006	SGS	Importer's Choice	1
	Mar 2006-	Cotecna	2 other companies involved	
Moldova	2001-2003	SGS	Exclusive	4
Mozambique	1991-1996	SGS	Exclusive	5
	2001-2006	Intertek		
Niger	1996-2005	Cotecna	Exclusive	
Nigeria	1979-1984/1990-1997/1999-2006	SGS	Geographical Split	30
	1984-1997/1999-2006	Cotecna	3 other companies involved	
Pakistan	1995-1997	SGS	Geographical Split	45
	Jul 1990-Nov 1991/Jan 1995-Mar.1997	Cotecna	1 other company involved	
Paraguay	1983-1988/1996-1999	SGS	Importer's Choice	20
Peru	1987-1989/1992-2004	SGS	Importer's Choice	25
	Mar 1992-May 2004	Cotecna	2 other companies involved	
Philippines	1986-2000	SGS	Exclusive	112
Rwanda	1977-2003	SGS	Exclusive	2
Senegal	1991-2001	SGS	Importer's Choice	7
	Oct 2001-2005	Cotecna	Exclusive	
Sierra Leone	1990-2003	BIVAC		
	2004-2006	Intertek		
Somalia	Sept 1990 -1991	Cotecna	Exclusive	
Suriname	1982-1990	SGS	Exclusive	0.5
Tanzania	1972-1998	SGS	Exclusive	9
	Sep 1992-April 1995/1999-2005	Cotecna	with SGS and then Exclusive	
Togo	1988-1989	SGS	Exclusive	4
	1995-2006	Cotecna	Exclusive	
Uganda	1982-1998	SGS	Exclusive	4
	2001	Intertek		
Uzbekistan	2001-2007	SGS	Importer's Choice	2
Venezuela	1986-1989/2003-2005	SGS	Importer's Choice	40
	Sept 2003-Aug 2005	Cotecna	3 other companies involved	
Zambia	1977-1998	SGS	Exclusive	5
Zanzibar	1982-2004	SGS	Exclusive	0.1

Notes
a/ In million US$ (current)
Source: industry data

Table 2
Descriptive statistics

		Values				Log			
	Obs	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Exports (US$ th.)	538'362	175539.50	2271267.00	0.00	303000000	7.84	3.64	-4.61	19.53
Distance	670'566	7438.38	4382.42	39.43	19904.45	8.67	0.80	3.67	9.90
Importer's GDP	618'121	1.67E+11	7.29E+11	2.06E+07	1.24E+13	2.32E+01	2.38E+00	1.68E+01	3.02E+01
Exporter's GDP	615'315	1.75E+11	7.45E+11	4.08E+07	1.24E+13	2.33E+01	2.34E+00	1.75E+01	3.02E+01
Importer's GDP/ cap	587'844	7272.45	7989.35	336.20	60228.41	8.31	1.13	5.82	11.01
Exporter's GDP/ cap	587'268	7407.10	8108.80	336.20	60228.41	8.31	1.13	5.82	11.01
Private inspection	670'566	0.16	0.36	0.00	1.00	0.16	0.36	0.00	1.00
Common language	670'566	0.16	0.36	0.00	1.00	0.16	0.36	0.00	1.00
Colony	670'566	0.01	0.12	0.00	1.00	0.01	0.12	0.00	1.00
Common border	670'566	0.02	0.14	0.00	1.00	0.02	0.14	0.00	1.00
Control of Corruption	250'399	50.95	29.04	0.00	100.00	50.95	29.04	0.00	100.00
Voice & Accountability	254'308	50.06	28.78	1.00	100.00	50.06	28.78	1.00	100.00
Political Stability	251'790	48.15	28.62	0.00	100.00	48.15	28.62	0.00	100.00
Governance Effectiveness	253'758	51.91	28.84	0.00	100.00	51.91	28.84	0.00	100.00
Regulatory Quality	253'775	51.72	28.31	0.00	100.00	51.72	28.31	0.00	100.00
Rule of Law	252'563	50.03	28.84	0.00	100.00	50.03	28.84	0.00	100.00
IMF SAF	537'000	0.02	0.15	0.00	1.00	0.02	0.15	0.00	1.00
IMF PRGF	537'000	0.12	0.32	0.00	1.00	0.12	0.32	0.00	1.00
IMF EFF	537'000	0.05	0.22	0.00	1.00	0.05	0.22	0.00	1.00
IMF SBA	537'000	0.15	0.35	0.00	1.00	0.15	0.35	0.00	1.00
Aid/cap (US$)	565'500	45.87	71.80	-203.59	1294.42	3.09	1.53	-6.12	7.17

Notes : For dummy variables, the mean is the proportion of observations for which the variable is equal to one, i.e. the variable's sample incidence.

Table 3
Baseline estimation results

	OLS	Treatreg			
	(a)	*(b)*		*(c)*	
		1st stage	2nd stage	1st stage	2nd stage
Constant	-10.83***		0.444		4.906***
	(0.54)		(1.14)		(1.61)
Gravity variables					
Distance	-1.567***		-1.648***		-1.693***
	(0.0058)		(0.0082)		(0.010)
Importer's GDP	0.565***		0.618***		0.669***
	(0.014)		(0.035)		(0.040)
Exporter's' GDP	0.655***		0.281***		0.108**
	(0.016)		(0.036)		(0.046)
Colony	1.190***		1.130***		0.934***
	(0.021)		(0.031)		(0.039)
Common language	0.682***		0.777***		0.861***
	(0.013)		(0.018)		(0.021)
Common border	0.500***		0.623***		0.812***
	(0.025)		(0.035)		(0.039)
Treatment					
Private inspection	0.0811***		0.0894*		0.119**
	(0.017)		(0.049)		(0.058)
Instruments 1					
Control of Corruption		0.0123***		0.0320***	
		(0.00079)		(0.0012)	
Control of Corruption (square)		-0.000576***		-0.000543***	
		(0.000010)		(0.000015)	
Voice & Accountability				0.0751***	
				(0.0012)	
Voice & Accountability (square)				-0.000809***	
				(0.000015)	
Political Stability				0.00675***	
				(0.00092)	
Political Stability (square)				-0.000272***	
				(0.000013)	
Governance Effectiveness				-0.0650***	
				(0.0015)	
Governance Effectiveness (square)				0.000496***	
				(0.000019)	
Regulatory Quality				-0.0210***	
				(0.0014)	
Regulatory Quality (square)				0.000329***	
				(0.000016)	
Instruments 2					
Aid a\				-0.240***	
				(0.0043)	
IMF PRGF c\				1.024***	
				(0.013)	
IMF EFF d\				-0.249***	
				(0.021)	
IMF SBA e\				-0.295***	
				(0.017)	
Observations	*306'223*	*156'856*	*156'856*	*106'264*	*106'264*
R-squared	*0.71*				

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4
Robustness checks 1

	BDM (a)		Treatreg: 5 years after (b)		Treatreg: 5 years before (c)		GMM (d)
	1st stage	2nd stage	1st stage	2nd stage	1st stage	2nd stage	
Constant	-10.70***			0.193		0.185	-17.95***
	(0.54)			(1.14)		(1.14)	(0.31)
Gravity variables							
Lagged exports							0.414***
							(0.0040)
Distance	-1.567***			-1.648***		-1.648***	-0.775***
	(0.0058)			(0.0082)		(0.0082)	(0.0071)
Importer's GDP	0.557***			0.633***		0.633***	0.422***
	(0.014)			(0.035)		(0.035)	(0.0090)
Exporter's' GDP	0.655***			0.280***		0.280***	0.778***
	(0.016)			(0.036)		(0.036)	(0.0087)
Colony	1.190***			1.130***		1.130***	0.482***
	(0.021)			(0.031)		(0.031)	(0.024)
Common language	0.682***			0.778***		0.778***	0.478***
	(0.013)			(0.018)		(0.018)	(0.013)
Common border	0.500***			0.623***		0.623***	0.547***
	(0.025)			(0.035)		(0.035)	(0.019)
Treatment							
Private inspection		0.113***					0.0482**
		(0.025)					(0.024)
PSI +5years				0.00685			
				(0.049)			
PSI -5years						-0.0533	
						(0.052)	
Instruments							
Control of Corruption			0.0129***		0.00666***		0.00158***
			(0.00081)		(0.00083)		(0.00053)
Control of Corruption (square)			-0.000572***		-0.000410***		0.0000414***
			(0.000010)		(0.000010)		(0.0000060)
Observations	*306'223*	*12'079*	*156'856*	*156'856*	*156'856*	*156'856*	*129'863*
R-squared	*0.71*						

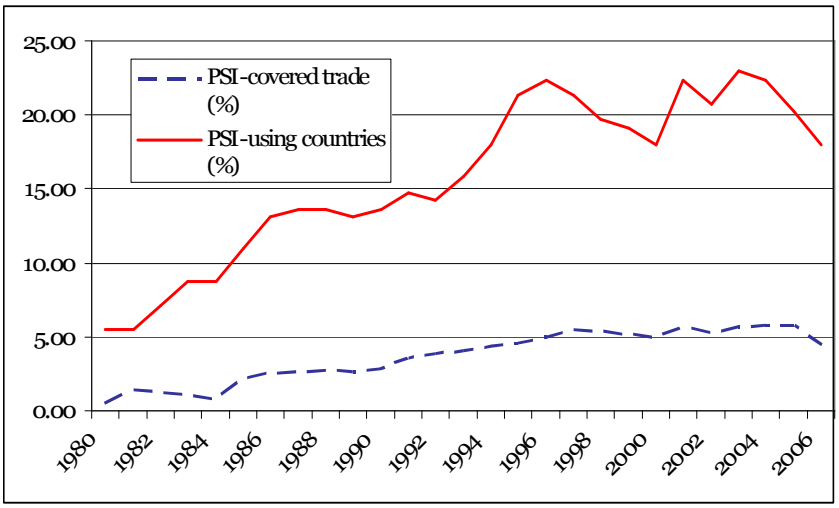
Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5
Robustness checks 2

	Tobit	OLS	Treatreg		BDM	
	(a)	*(b)*	©		*(d)*	
			1st stage	2nd stage	1st stage	2nd stage
Constant	-2.152***	-4.666***		-1.206	-4.633***	
	(0.63)	(0.93)		(1.29)	(0.93)	
Gravity variables						
Distance		-1.534***		-1.572***	-1.534***	
	-1.560***	(0.0076)		(0.0091)	(0.0076)	
Importer's GDP	(0.0073)	0.559***		0.613***	0.562***	
	0.880***	(0.026)		(0.039)	(0.026)	
Exporter's' GDP	(0.026)	0.473***		0.289***	0.473***	
	0.337***	(0.027)		(0.039)	(0.027)	
Colony	(0.026)	1.171***		1.159***	1.171***	
	1.058***	(0.027)		(0.033)	(0.027)	
Common language	(0.033)	0.767***		0.776***	0.767***	
	0.744***	(0.016)		(0.020)	(0.016)	
Common border	(0.016)	0.696***		0.710***	0.697***	
	0.574***	(0.031)		(0.038)	(0.031)	
a/ marginal effects	(0.029)	1.466***		1.842***	1.472***	
Trade openness (Wacziarg-Welsh)		(0.16)		(0.28)	(0.16)	
Treatment						
Private inspection	0.0547**	0.110***		0.0855*		0.129***
	(0.026)	(0.023)		(0.046)		(0.027)
Instruments						
Control of Corruption			0.000624			
			(0.00092)			
Control of Corruption (square)			-0.000325***			
			(0.000012)			
Voice & Accountability			0.0269***			
			(0.00090)			
Voice & Accountability (square)			-0.000416***			
			(0.000011)			
Observations	*223'163*	*188'555*	*131'739*	*131'739*	*188'555*	*101'46*
R-squared		*0.72*			*0.72*	

Figures

Figure 2
Percent of importing countries and trade flows covered by private inspection, 1980-2006



Source: Authors' calculations, DOTS, Industry.