The Costs and Benefits of Public Intervention

Micro and Macro Evidence

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Abstract

My thesis collects four essays about the causes and consequences of government intervention in the economy and one essay about the effects of immigration. The first chapter quantifies the private returns and the social costs of political connections. The second chapter studies the relationship between individual trust toward the others and preferences for government intervention, and it draws the implications of this relationship for re-interpreting previous evidence about the effects of regulation. The third chapter examines the substitutability between financial markets and public pensions as two alternative ways to provide for retirement. The fourth chapter estimates the effect of political fragmentation on the timing of structural reforms, focusing in particular on privatization. Finally, the last chapter empirically investigates the relationship between immigration and crime.

La meva tesis consisteix en quatre assaigs sobre les causes i consequències de la intervenció del govern en l'economia, i un assaig sobre els efectes de la immigració. El primer capítol quantifica els retorns econòmics de les connexions polítiques, i examina els canals a través dels quals aquestes afecten les empreses. El segon capítol estudia la relació entre la confiança entre els individus i les preferències d'aquests per la intervenció governamental, i utilitza aquesta relació per reinterpretar evidència existent sobre els efectes de les regulacions. El tercer capítol examina la substituibilitat entre els mercats financers i les pensions publiques, com a dos alternatives per proveir per la jubilació. El quart capítol estima els efectes de la fragmentació poltica sobre la velocitat de les reformes estructurals, centrant-se en el cas particular de les privatitzacions. Finalment, l'últim capítol investiga empíricament la relació entre immigració i crim.

Introduction

The role of government in market economies is a fascinating theme, embracing several fields in economics and spreading out into other disciplines such as political science, ethics and philosophy. During the last few decades, the economics literature has devoted particular attention to the inefficiencies of the public sector. In some cases, however, public intervention may also provide a (second best) solution to market failures existing in the first place. Most importantly, the welfare costs and benefits of government intervention may both respond to differences in formal and informal institutions (like the type of legal system or the moral values and social norms prevailing in each country). In particular, economies characterized by a higher propensity to rent-seeking behavior may exhibit both a greater severity of market failures and a higher demand for government in response to such failures, even though the incentives of bureaucrats and politicians might be worse too (from a social point of view) in those economies. Separately identifying the costs and benefits of public intervention is thus complicated by the fact that they can be positively correlated across countries.

The first two chapters of the thesis make extensive use of micro data to overcome this problem. In the first one, "Politicians at work", co-authored with Federico Cingano, we quantify the private benefits and the social costs of political connections taking advantage of a unique longitudinal dataset that merges the balance sheets of a representative sample of Italian firms, the social security records of all their employees and administrative information on all individuals appointed in a local government. The identification strategy is based on a simple theoretical framework relating the demand and supply effects of political connections to alternative hypotheses about their welfare consequences. Estimation of the model parameters exploits within-firm variation in political connections and outcomes, controlling for unobserved firm and individual heterogeneity, time-varying common shocks, firm-specific trends and for the selection of local politicians into firms. We find that the revenue-premium granted by political connections ranges between 0 and 25 percent, depending on the characteristics of the industrial sector and the geographic region in which the firm operates. In particular, market share reallocations induced by political connections favor exclusively the upstream producers for the public administration; they are stronger in areas characterized by high public expenditure and high corruption; they occur through changes in domestic sales (as opposed to exports) and they are not related to improvements in firm productivity. These findings suggest that the private benefits of political connections descend from the distortion of public demand in favor of connected firms.

Private benefits from political activity may thus entail significant social costs on the rest of the economy. Still, the demand for government intervention remains high in most countries. The second chapter of the thesis, "Trust and regulations: addressing a cultural bias", offers a view that can potentially reconcile the existence of positive (excess) demand for regulations by individuals at large with the available empirical evidence about the effects of government intervention across countries. This explanation hinges crucially on within- and between-country variation in two cultural traits, namely trust and trustworthiness. It first shows, both theoretically and empirically, that individual preferences for regulation depend negatively on trust toward the others. If trust predicts trustworthiness across countries and if trustworthiness is (negatively) related to the incidence of market failures, then omitted variation in trustworthiness will bias the estimated effects of regulation on market failures upward. The empirical evidence suggests that, indeed, a large part of the previously estimated negative effects of regulation can be attributed to omitted variation in cultural traits.

Two older articles about the determinants of government activity are included in Chapters 3 and 4 of the thesis, respectively. The first one, "Financial development and pay-as-you-go social security", empirically studies the relationship between financial markets and public pensions as two alternative ways to provide for retirement. Using legal origin as a proxy for financial frictions that may hold back financial development, the empirical analysis yields two main results: first, legal origin-driven differences in financial frictions are an important determinant of social security, common law countries exhibiting significantly smaller public pension programs; second, two-stage estimates suggest that legal origin impacts on social security through financial market development. The second article, "Delayed privatization", co-authored with Bernardo Bortolotti, studies the relationship between political fragmentation and the speed of public sector reform. Using data for the timing of privatization in 21 major developed economies in the 1977-2002 period, we show that the dismissal of state-owned enterprises was delayed longer in democracies characterized by a larger number of parties and operating under proportional electoral rules, as predicted by war of attrition models of economic reform.

Finally, the last chapter, co-authored with Milo Bianchi and Paolo Buonanno, departs from the main topic of the thesis to examine the empirical relationship between immigration and crime across Italian provinces during the period 1990-2003. Drawing on police administrative records, we first document that the size of the immigrant population is positively correlated with the incidence of property crimes and with the overall crime rate. Then, we use instrumental variables based on immigration toward other European countries to identify the causal impact of exogenous changes in Italy's immigrant population. According to these estimates, immigration increases only the incidence of robberies, while leaving unaffected all other types of crime. Since robberies represent a very minor fraction of all criminal offenses, the effect on the overall crime rate is not different from zero.

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Chapter 1

Politicians at work

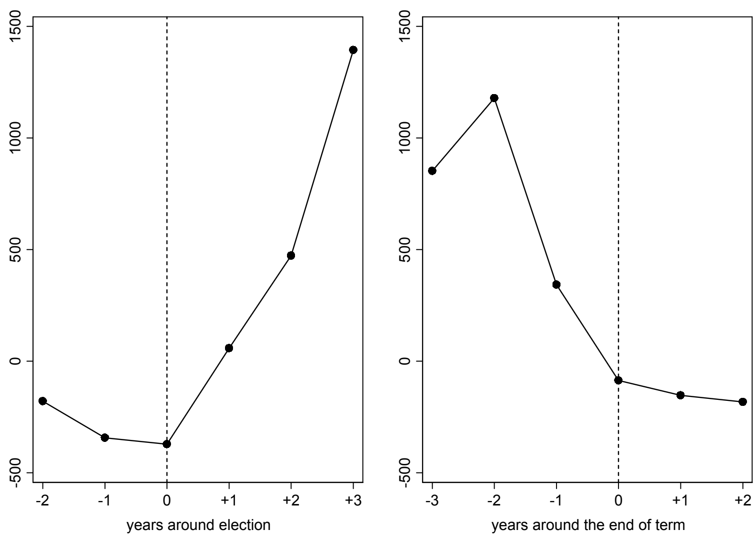
Political connections are highly valued by investors. As a matter of fact, shares of politically connected firms trade at substantially higher prices in financial markets (Faccio, 2006a). The mechanisms inducing the (expected) profits of connected firms to raise are largely unexplored, however, and can in principle bear very different implications in terms of social welfare. On the one hand, rent-seeking practices enacted by firms and politicians could impose large social costs on the rest of the economy. On the other hand, if the competitive advantage of connected firms stems from higher productivity, political connections might not necessarily imply negative effects on welfare. Addressing these issues requires moving beyond financial market evaluations of political connections.

In this paper we examine the effects of political connections in product and factor markets. We do so within a simple theoretical framework allowing us to quantify the private returns to political connections in terms of revenues, profits and wages, and the associated social costs in terms of misallocation of public expenditure. The model is then estimated exploiting a unique longitudinal dataset matching detailed information on a representative sample of Italian manufacturing firms and all of their employees with administrative archives on the universe of Italian local politicians over the period 1985-97.

Detailed firm- and individual-level data provide several advantages for the purpose of this work. First, they allow to identify connections on the basis of precise links between firms and politicians. In particular, we will define as connected those firms employing (at least) one individual appointed in a local government. This is a meaningful definition because, differently from the national members of parliament, most local politicians retain other occupations alongside their political career. Moreover, despite being much less monitored than their national-level colleagues, Italian local politicians directly manage over one third of total public expenditure (and retain much discretionary power over the allocation of the remaining part). Second, the longitudinal dimension of our data set allows to control for unobserved firm- and individual-level heterogeneity, time-varying shocks and for the selection of local politicians into firms, thus leading to a much cleaner estimate of the effects of political connections. Third, detailed firm-level data on productive inputs, output and prices permit to identify connection-induced shifts in firm demand and supply. Distinguishing between the two is crucial in our framework for assessing the welfare consequences of political connections. Finally, individual-level data on employment and wages provide us with a measure of private market returns to political careers.

Estimates based on within-firm variation in revenues and connections indicate that connected firms experience an average increase in revenues equal to approximately 5 percent, yielding to an almost equivalent increase in current profits (Figure 1.1). We find that output gains *only* accrue to firms establishing a connection through winning politicians (i.e. politicians appointed with the party or coalition of parties that won the elections): firms connected through minority (or out-of-office) politicians see no increase in market shares, just as non-connected firms. These findings are robust to controlling for local and industry yearly shocks and for firm-specific trends. They are also unaffected when we restrict ourselves to changes in connections that are *not* due to worker flows between firms, thus excluding the confounding effect of self-selection of politicians into expanding or contracting firms.

Figure 1.1: Political connections and profits



Note: These figures show the average changes induced by political connections on firm profits. In particular, the figure on the left plots the residuals of a regression of profits (Earnings Before Taxes, in ths. €) on firm, province-year and sector-year fixed effects, averaged over politically connected firms around the year in which they get access to at least one connection (year=0). The figure on the right plots the same variable for firms losing all their connections.

Finally, we exploit variation in individual-level wage data to estimate the degree to which the benefits granted by political connections are remunerated. Mincerian wage regressions show that being appointed in a local government shifts the wage schedule upward by about 2%. This result too is robust to controlling for the selection of politicians into firms through match-specific (individual-firm) fixed effects and for differences in wage profiles between politicians and non-politicians. According to these estimates, rent-splitting strongly favors firm owners relative to employee-politicians. One explanation for this finding lies in the possibility of non-wage compensations, which we can not observe in our data.

The competitive advantage enjoyed by politically connected firms can in principle be traced to alternative mechanisms, with relevant differences in terms of welfare implications. On the one hand, greater revenues could descend from increased productivity, for example because employees accessing political power help reduce the burden of administrative (e.g. red tape) costs, or grant privileged access to production factors (like public utilities). Whether such a channel entails a social cost is not clear, however. According to the *greasing wheel hypothesis* (Kaufmann and Wei, 1999), these practices would increase aggregate welfare by relieving economic activity from burdensome regulation (Leff, 1964; Lui, 1985; Shleifer and Vishny, 1994). On the other hand, local politicians could simply be driving public demand toward the firms they are employed in. For instance, they could favor connected firms in public procurement, as shown by Goldman et al. (2008). The misuse of public office for private gains is a distinctive feature of outright predatory corruption (Treisman, 2000) and entails large social costs in terms of inefficient provision of goods and services (Krueger, 1974; Mauro, 1998). This alternative explanation is labeled *grabbing hand hypothesis*, after Shleifer and Vishny (1998).

Our evidence is largely consistent with this second hypothesis. In particular, estimates from alternative production function specifications indicate that political connections do not have any impact on productivity. Rather, the average effect on revenues turns out to be driven by firms operating in markets in which public demand plays a major role. Specifically, it is entirely due to changes in domestic sales (as opposed to exports) and to firms operating in sectors that are intensive providers of inputs to the public administration (6.2 percent), and it is larger in regions characterized by high public expenditure (16.4 percent) and high corruption (9.7 percent).

This work is related to a recently expanding literature on the consequences of political connections. Most of these papers detect (abnormal) financial returns of connected firms around particular events like national elections (Faccio, 2006a; Jayachandran, 2006; Knight, 2007; Claessens et al., 2008; Ferguson and Voth, 2008), crises (Johnson and Mitton, 2003) and news about politicians' health (Fisman, 2001; Faccio and Parsley, 2006); political connectedness is defined on the basis of campaign contributions or personal relationships, the latter being mostly collected from newspapers. We rather focus on a direct measure of political connections and depart from the event study approach. In both respects, our work is closest to Khwaja and Mian (2005), who take advantage of a data set similar to ours. However, they focus exclusively on preferential access to credit, which is only one of the advantages possibly granted to connected companies. By contrast, we investigate a variety of outcomes and distinguish between alternative channels through which political connections may impact on firm performance.[1]

We also add to a burgeoning literature on individual returns to political careers. Dier-

[1] Faccio (2006b) and Li et al. (2008) also focus on different outcomes and channels, respectively. However, their identification strategy is based only on cross-sectional variation in a single year.

meier et al. (2005), Eggers and Hainmueller (2008) and Gagliarducci et al. (2008) estimate positive market returns to political careers in the United States Congress and in the British and Italian Parliament, respectively. Since national-level politicians hardly maintain outside occupations while in office, they measure economic returns by post-congressional salary, financial wealth and total income (inclusive of capital income and property rents), respectively. We complement this evidence by estimating the effect of political appointment on the labor earnings of local-level politicians that maintain stable outside occupations alongside their political career.

The rest of the paper is structured as follows. The next section outlines a simple theoretical framework that derives equilibrium distribution of revenues, profits and wages across firms as a function of connection-induced supply and demand shifts. We then discuss how to identify such shifts and their implications for the private benefits and the social costs of political connections. Section 1.2 describes the main sources and features of our data. In Sections 1.3 we present the empirical results. Finally, Section 1.4 concludes.

1.1 Theoretical framework

Consider an economy inhabited by households, firms and a local government. Households value consumption of both private and public goods. The former are produced by monopolistically competitive firms, while public goods are provided by the local government using the varieties of private goods as inputs. Public procurement of these varieties may respond to the existence of political connections between private firms and the local government. In this set up, we will characterize firm revenues as a function of productivity and/or demand shifts, which may both depend on political connections, and derive estimating equations from these equilibrium relationships.

1.1.1 Preferences and technology

Let C and G denote consumption of private and public goods, respectively. Specifically, households have CES preferences over different varieties of private goods, which implies that

$$C = \left[\int B_j^{\frac{1}{\sigma}} Q_j^{\frac{\sigma-1}{\sigma}} dj \right]^{\frac{\sigma}{\sigma-1}}, \tag{1.1}$$

where Q_j is consumption of variety j and $\sigma > 1$ is the elasticity of substitution between varieties. The latter are produced by a measure J of (monopolistically) competitive firms according to technology:

$$Y_j = A_j f(X_j) \tag{1.2}$$

where Y_j is the output of firm j, $f(.)$ is a constant returns to scale production function and X_j is the vector of production factors employed by the firm. The (positive) parameters A_j and B_j are productivity and preference shifters, respectively, which may depend, among other things, on the political connections of firm j.

Turning to public goods, they are produced combining different varieties of private goods according to technology

$$G = \left[\int_J \tilde{Q}_j^{\frac{\sigma-1}{\sigma}} dj \right]^{\frac{\sigma}{\sigma-1}}, \tag{1.3}$$

where $\tilde{Q}_j$ is the amount of each j-th input purchased by the local government. Political connections may however distract public spending from its efficient allocation, i.e. the one that maximizes G. We allow this possibility by specifying the following utility function for local politicians:

$$\tilde{U} = \left[\int_J \tilde{B}_j^{\frac{1}{\sigma}} \tilde{Q}_j^{\frac{\sigma-1}{\sigma}} dj \right]^{\frac{\sigma}{\sigma-1}} \tag{1.4}$$

where $\tilde{B}_j \geq 0$ is a demand shifter that may also depend (analogously to A_j and B_j) on the political connections of firm j.

1.1.2 Equilibrium

Households and the local government in each region take prices as given and maximize utility subject to the budget constraints $\int_J P_j Q_j dj \leq E$ and $\int_J P_j \tilde{Q}_j dj \leq \tilde{E}$, where E and $\tilde{E}$ are the aggregate expenditure by households and the local government, respectively, and P_j is the market price of variety j. The implied total demand for variety j is then

$$P_j \left(Q_j + \tilde{Q}_j \right) = P_j^{1-\sigma} \left[B_j \left(\frac{E}{P} \right) + \tilde{B}_j \left(\frac{\tilde{E}}{\tilde{P}} \right) \right] \tag{1.5}$$

where $P = \int_J B_j P_j^{1-\sigma} dj$ and $\tilde{P} = \int_J \tilde{B}_j P_j^{1-\sigma} dj$ are the price indexes for private and public consumption, respectively. Profit maximization leads firms to charge a constant mark-up over marginal cost,

$$P_j = \frac{\sigma}{\sigma - 1} \frac{\omega}{A_j}, \tag{1.6}$$

where ω is also constant across firms within the same market, depending only on the factor prices prevalent in that market. Substituting the last expression into equation (1.5) delivers the equilibrium revenues of each firm:

$$R_j = \Theta A_j^{\sigma-1} \left[B_j \left(\frac{E}{P} \right) + \tilde{B}_j \left(\frac{\tilde{E}}{\tilde{P}} \right) \right], \tag{1.7}$$

with $\Theta = \left(\frac{\sigma\omega}{\sigma-1} \right)^{1-\sigma}$.

How are total revenues distributed among the different firm stakeholders? If the market for factors is competitive, a constant fraction is paid to production factors,

$$\frac{\omega}{A_j} \left(Q_j + \tilde{Q}_j \right) = \left(\frac{\sigma - 1}{\sigma} \right) R_j,$$

while the remaining part goes to remunerate firm owners and, possibly, political connections. In particular, if political connections can move freely from one firm to the other, then they should be remunerated at the same price in each firm, so that

$$\Pi_j = R_j/\sigma - w^P \cdot POL_j, \tag{1.8}$$

where Π_j are the profits of firm j, POL_j are its political connections and w^P is their equilibrium price. If political connections are granted by firm employees, then w^P is also their wage premium relative to the other individuals employed by the firm.

1.1.3 Estimating equations and identification

According to equation (1.7), political connections can affect firm-specific revenues only through productivity and/or preference shifters. Distinguishing the relative importance of these alternative channels is crucial for assessing the welfare implications of political connections. Our identification strategy will rely mainly on within-firm variation over time in political connections and outcomes, controlling for transitory local and sectoral shocks.

Specifically, let A, B and $\tilde{B}$ depend on political connections in the following way:

$$\begin{aligned}
\ln A_{jt} &= a_j + a_{rt} + a_{st} + a \cdot POL_{jt} + \upsilon_{jt} \\
\ln B_{jt} &= b_j + b_{rt} + b_{st} + b \cdot POL_{jt} + \nu_{jt} \\
\ln \tilde{B}_{jt} &= \tilde{b}_j + \tilde{b}_{rt} + \tilde{b}_{st} + \tilde{b} \cdot POL_{jt} + \tilde{\nu}_{jt}.
\end{aligned}$$

where the subscripts $t = 1, 2, ..., T$ refer to the year periods over which we observe the firms in our sample. The first term on the right hand side of each equation summarizes firm-specific, time-invariant characteristics; the second and third terms capture, respectively, year t shocks specific to region r and industrial sector s in which the firm operates; and υ_{jt}, ν_{jt} and $\tilde{\nu}_{jt}$ are normally distributed error terms not correlated with political connections. Coefficients a, b and $\tilde{b}$ represent the (percentage) increase in firm-specific productivity and demand (private and public), respectively, granted by political connections.

Substituting the expressions for A_j, B_j and $\tilde{B}_j$ into the revenues equation (1.7) and log-linearizing it around $A = B = \tilde{B} = 1$ delivers the estimating equation

$$r_{jt} = \phi_j + \phi_{rt} + \phi_{st} + \beta \cdot POL_{jt} + \varepsilon_{jt}, \tag{1.9}$$

where r_{jt} is the log of revenues raised by firm j during year t; ϕ_j summarizes firm-specific, time-invariant terms; ϕ_{rt} and ϕ_{st} reflect region- and sector-specific shocks and ε_{jt} is an error term. The estimating coefficient β in (1.9) equals the average (percentage) change in market power associated with political connections and it is the (weighted) sum of both demand and supply effects,

$$\beta = (\sigma - 1)a + (1 - \tilde{e})\, b + \tilde{e}\tilde{b}. \tag{1.10}$$

with $\tilde{e} = (\tilde{E}/\tilde{P})/[(E/P) + (\tilde{E}/\tilde{P})]$ being the incidence of public demand over total sales in the market.

In order to separately identify the different components of β, we proceed in two steps. First, we exploit the fact that productivity changes affect output for any given level of production factors, while demand shifts are entirely accomodated by expanding the scale of production. Therefore, keeping factors constant in a production function framework allows to isolate productivity effects from demand shifts. Specifically, taking logs in (1.2) and substituting the expression for A_{jt}, we obtain

$$y_{jt} = a_j + a_{rt} + a_{st} + a \cdot POL_{jt} + \sum_k \mu^k x_{jt}^k + v_{jt} \tag{1.11}$$

where x_{jt}^k is the log of each k-th factor employed by firm j during year t and μ^k is its share in total production; notice that the coefficient of POL_{jt} in (1.11) depends only on the effect of political connections on firm productivity (as captured by a). Therefore, productivity effects of political connections should drive a positive and statistically significant estimated coefficient of POL_{jt} both in (1.9) and (1.11), while demand effects would show up in (1.9) but not in (1.11).

The second step consists in distinguishing between different types of demand effects, namely from private consumers and from the public administration, as captured by coefficients b and $\tilde{b}$, respectively. This is also a very important distinction because only the latter cause a distortion of allocative efficiency; the former just redistribute profits across the firms active in the market. The relative importance of these two effects can be assessed by comparing estimates of β across different markets. According to equation (1.10), in fact, if demand effects occur mainly through public procurement, politically connected firms should experience a greater increase in revenues if they operate in markets characterized by a greater incidence of public expenditure in total demand (i.e. the parameter $\tilde{e}$). The opposite would occur if demand effects are driven instead by the preferences of private consumers. Therefore, we will estimate equation (1.9) separately for firms operating in industrial sectors and/or geographic regions characterized by a different weight of public demand.

Finally, the empirical analysis of the factor market returns to political connections will be based on firm-level estimates of equation (1.8) as well as on individual-level Mincerian wage regressions. These estimates allow to assess the degree of rent-splitting between firm owners and the employees granting the political connection.

1.1.4 The misallocation of public expenditure

The effects of political connections on public sector efficiency depend crucially on the channels through which they impact on firm revenues. If political connections mainly help firms to overcome burdensome administrative barriers (e.g. red tape), they would improve public sector efficiency by raising the productivity of input providers to the public administration. On the other hand, the possible distortion of public demand in favor of politically connected firms would negatively impact on efficiency.

These two effects are intimately related with the different components of β in equation (1.10). This can be seen by computing the equilibrium provision of public goods. Substituting the public demand and the supply of inputs (equations 1.5 and 1.6, respectively) into (1.3), plugging the expressions for shifters A, B and $\tilde{B}$, and exploiting the properties of the log-normal distribution delivers

$$\ln G - \ln\left(G_0 \cdot \tilde{E}\right) = \underbrace{aE(POL) + \left(\frac{\sigma-1}{2}\right)a^2V(POL)}_{greasing\ wheel} - \underbrace{\frac{1}{2\sigma}\tilde{b}^2V(POL)}_{grabbing\ hand} + \Sigma, \tag{1.12}$$

with $G_0 = \left(\frac{\sigma-1}{\sigma}\right)\frac{J^{\frac{1}{\sigma-1}}}{\omega}$ and $\Sigma = \left(\frac{\sigma-1}{2}\right)V(\upsilon) - \frac{1}{2\sigma}V(\tilde{\nu})$.[2] Notice that G_0 would be the public good provision per unit of expenditure absent any supply and/or demand shock, i.e. $A = \tilde{B} = 1$. Taking this as a benchmark, the right hand side of equation (1.12) may be interpreted as the change in public good provision that is due to variation in productivity demand across firms.

Part of this variation depends directly on the first and second moments of the distribution of political connections across firms. In particular, "greasing wheel effects" increase public expenditure efficiency by raising the average productivity of input providers for the public administration (as captured by the first term on the right hand side). Since mark ups are fixed and demand is elastic, this effect would be magnified by the fact that greater shares of total public demand are re-directed toward high-productivity, low-price firms (the second term). "Grabbing hand effects", at the opposite, lower the efficiency of public procurement by distorting the relative demand for each input relative to its optimal level. The benefits (costs) of greater dispersion in productivity (public demand) are increasing (decreasing) in the elasticity of substitution σ. Intuitively, the higher the substitutability between different varieties, the greater the advantage of shifting production toward the most efficient firms, and the lower the costs of forcing a disproportionate share of public demand toward some firms.

The necessary conditions for greasing and grabbing effects to be different from zero are that a and $\tilde{b}$ are also different from 0, respectively. Empirically estimating such coefficients is exactly the purpose of the next sections.

1.2 Data

Our data set merges informations from three main sources: firm-level balance sheet data, individual-level social security archives and administrative registries on local politicians.

[2]The expression in equation (1.12) is computed assuming that the firm, sector-year and region-year components in A and $\tilde{B}$ are all equal to 0.

1.2.1 Employer-employee data

We started from the Bank of Italy survey on investments (INVIND), an open panel of about 1200 Italian manufacturing firms representative of those with at least 50 employees. The survey was integrated with balance-sheet data collected by the Company Accounts Data Service (CADS) to obtain firm-level information on revenues, exports, value added, real output, profits and production factors.[3]

These data were merged with Social Security archives to recover individual-level information on weekly wages, age, gender and (most importantly) the fiscal identifier of all individuals ever employed in an INVIND firm during the period 1985-97. More specifically, the Italian Social Security Institute (INPS) provided the complete work histories of any workers ever employed by an INVIND firm for the period 1981-1997, including spells of employment in non-surveyed firms.[4] The final matched employer-employee dataset includes nearly 1.4 millions of individuals employed in 1227 firms. Table 1.1 presents the characteristics of our sample.

1.2.2 Political connections

The Italian system of local governments comprises 8100 municipalities, 103 provinces (95 until 1995) and 20 regions. Each of them is formed by a legislative council and an executive cabinet. They are renewed through elections regularly held every five years; of course, earlier elections may be called if the executive resigns the mandate before its term expires.

Within our sample period, local elections were held in 1985, 1990 and 1995, appointing 307,783 local politicians in total; about 135,000 of them were in office, on average, during each year. Detailed information on these individuals is available from the Registry of Local Politicians (RLP), maintained by the Italian Ministry of Interior and made publicly available according to National Law 267/2000, art. 76. The RLP records include (among other things) the informations required to generate the fiscal identifier of each politician: name, birth date and birth place (at the municipality-level). This allowed us to merge the data on local politicians with the employer-employee dataset in order to identify firms' connections with the local government.

Our main measure of political connections is a binary variable indicating firms that have at least one employee appointed in a local government. Since the RLP also reports the party affiliation, we are able to further distinguish between politicians appointed with parties entering the executive cabinet, i.e. parties that won the elections, as opposed to minority parties. More specifically, we define $POLCON_{jt} = 1$ if firm j is employing at

[3]The Company Accounts Data Service (Centrale dei Bilanci) is a large data set collected by a consortium of banks to pool information on borrowers. It contains detailed balance-sheet information on sample of between 30,000 and 40,000 firms published yearly since 1982. The nature of the dataset (help banks' credit decisions) implies the data are carefully quality controlled. Firms in the sample account for approximately half of total manufacturing employment in Italy and for a larger share of sales.

[4]More than half of these workers are employed in INVIND firms in any year. The rest are employed in 100,000 other firms for which data report a fiscal identifier.

Table 1.1: The INVIND-INPS sample

	SUMMARY STATISTICS				DISTRIBUTION		
	firms	obs.	mean	std. dev.	10%	50%	90%
			firm-level variables				
Total revenues, ths. €	1226	12561	90172.7	486361.3	5776.6	23892.6	168391.8
Value added, ths.€	1226	12561	24582.1	110835.6	1928.1	7244.7	46815.6
Exports, ths.€	1226	12561	22278.2	161837.5	0	918.8	36102.9
Domestic sales, ths.€	1226	12561	67894.5	339858	4002.9	17908	129598.5
r	1226	12561	10.22	1.38	8.66	10.08	12.03
va	1226	12461	9.05	1.27	7.49	8.90	10.76
ln(1+Exports)	1226	12561	5.02	4.69	0	6.82	10.49
ln(Domestic)	1226	12526	9.90	1.45	8.31	9.80	11.77
Δy, %	717	4075	-0.2	25.1	-19.7	0.1	19.6
Δp, %	719	4077	3.2	6.8	-3.0	3.0	9.0
Workers	1226	12561	895.5	2473	113	355	1708
Capital, ths.€	978	10287	49662.5	299341.5	2167.7	11358.3	85979.6
Intermediate inputs, ths.€	1226	12561	65847.1	382496.8	3451.8	16232	120161.4
EBITDA, ths.€	1226	12561	8029.4	39767.1	215	2126.1	16999.8
ROA, %	1226	12561	9.4	10.7	0	8.3	21.4
EBT, ths.€	1226	12561	2626.7	31584.1	-1063	407.7	8323.9
Wages, ths.€	1226	12561	15679.2	46120.4	1518.9	5120.5	32269.7
$POLWIN$	1226	12561	0.552	0.497	0	1	1
$POLCON$	1226	12561	0.617	0.486	0	1	1
			individual-level variables				
Weekly wage, €		13756683	455.9	356.7	261.3	388.7	675.3
Age, years		13756683	38.15	10.29	24	39	51
$polwin$		13756683	0.004	0.060	0	0	0

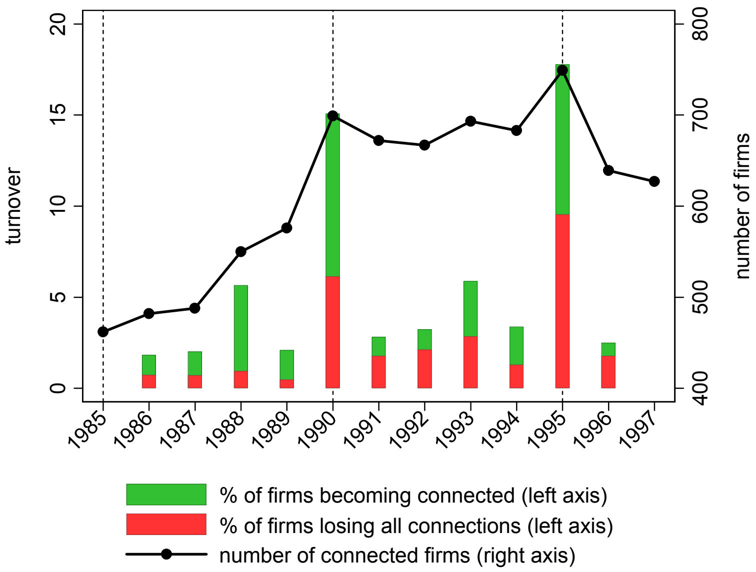
Note: This table reports the main characteristics of our sample. The symbol € denotes variables expressed in constant 1991 Italian liras and then converted into euros at official exchange rates; ths. € indicates that firm-level variables are expressed in thousands of euros.

least one individual appointed during year t, and $POLWIN_{jt} = 1$ if the firm is employing at least one individual elected in a party entering the executive. This distinction is useful to explore the differential effect of access to greater administrative power.

Since our identification strategy is mostly based on within-firm changes in connection status, it is important that our indicators display enough variation along the time dimension. This seems indeed to be the case in our sample. About 40% of firms switch connection status at least once, the average yearly turnover rate in each year being close to 6%. Figure 1.2 shows its evolution over time, distinguishing between entry into and exit from connection status. Obviously, the turnover rate peaks during the electoral years (1990 and 1995). The number of connected firms too is higher in those years, due to the facts that we counted as connected both firms entering and exiting the connection status.

Turning to employees, we define $polwin_{it} = 1$ if individual i is appointed in a local government with a party forming the executive during year t. This variable captures the shift in the intercept of the individual wage profile associated with access to administrative power. We will also estimate piecewise-linear Mincerian wage equations, interacting

Figure 1.2: Political connections (firms)



Note: This graph shows the turnover of connection status (decomposed by entry and exit flows) and the total number of connections for the firms in our sample. The dotted vertical lines indicate the electoral years.

polwin with functions of individual age and tenure in the firm, to examine the effect of political careers on the age gradient of the individual wage schedule.

Figure 1.3 shows that the incidence of local politicians over the employees in our sample is substantially higher than their incidence over the Italian population aged over 18; on the other hand, the two lines parallel each other quite closely.

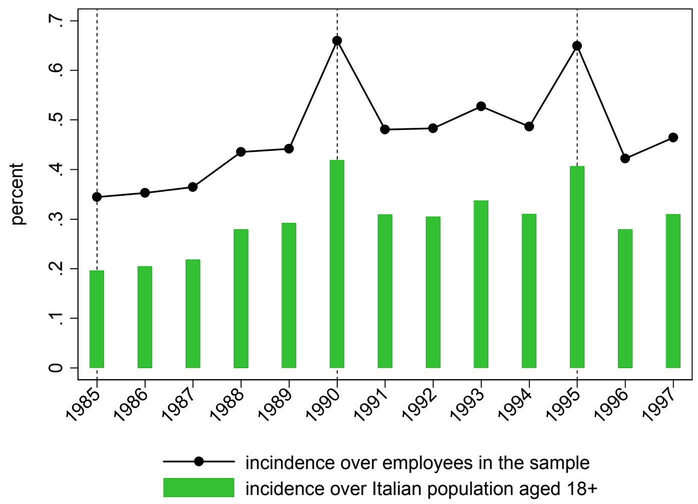
1.3 Empirical results

Our empirical results are organized as follows. We first estimate equation (1.9) to detect whether within-firm changes in connection status induce variation in (the log of) revenues. Focusing on the role of connections in the production function framework (1.11) allows to determine to what extent changes in market power can be attributed to the effect of connections on firm-productivity (*greasing wheel hypothesis*). To assess the relative importance of public demand (*grabbing hand hypothesis*), we exploit firms' proximity to public procurement along both sectoral and geographical dimensions. Finally, we will investigate the degree of rent-splitting between different firm stakeholders by estimating profit and Mincerian wage equations.

1.3.1 Baseline estimates

Table 1.2 presents the results of baseline estimates on equation (1.9). The dependent variable is firm revenues deflated using 2-digit industry indexes from the National Accounts. In cols. 1 to 3 we start by investigating the correlation between political connections and

Figure 1.3: Political connections (employees)



Note: This graph shows the incidence of individuals appointed in a local government among employees in our sample and among the Italian population as a whole. The dotted vertical lines indicate the electoral years.

market power across firms. We cross-sectionalized data by taking within-firm averages of both dependent and explanatory variables. Hence, in these specifications $POLCON$ ($POLWIN$) equal the fraction of sample period in which a firm was connected to a politician (to a politician elected with a party that won the elections). To reduce the scope for omitted variable bias, we control non-parametrically (i.e. by including category-specific fixed effects) for differences in industrial sectors, provinces and firm size (as measured by total employment).

According to these estimates, connected firms are characterized by significantly higher (average) revenues relative to non-connected ones. However, there are striking differences between different types of political connections. In particular, the difference in revenues, equal to approximately 40%, is completely attributable to connections with politicians that won the elections. These estimated coefficients are very large and might reflect, to a great extent, within-category spurious correlation between the likelihood of employing a politician and other (possibly unobserved) firm characteristics.

For this reason, all other specifications add firm, province-year and sector-year fixed effects. Identification of the effect of political connection thus exploits within-firm changes in revenues and connection status conditional on aggregate (demand or productivity) province- and sector-specific transitory shocks. Once we do that, the coefficient of $POLWIN$ drops by an order of magnitude to 4.6%, while that of $POLCON$ remains not significantly different from zero (col. 6). According to these estimates the only connections that matter are those established directly with the local executive power. Therefore, we will focus on $POLWIN$ as our main measure of political connections. We also tried other measures of political connections that account for (i) differences in the number of appointed individu-

Table 1.2: Baseline

	(1)	(2)	(3)	(4)	(5)	(6)
	CROSS	SECTION	ESTIMATES	FIXED	EFFECTS	ESTIMATES
$POLCON$	.414***		.080	.032*		-.008
	(.081)		(.176)	(.017)		(.023)
$POLWIN$		.457***	.387**		.046***	.052**
		(.082)	(.177)		(.017)	(.023)
obs.	1226	1226	1226	12561	12561	12561
firms	1226	1226	1226	1226	1226	1226
firm FE	NO	NO	NO	YES	YES	YES
R^2	.589	.591	.591	.892	.892	.892
$adj\ R^2$	.552	.554	.554	.866	.866	.866

Note: The dependent variable is revenues at the firm level deflated with industry-level indexes from the Italian National Accounts. The sample is a panel of manufacturing firms observed during the period 1985-97. Cols. (1) to (3) present cross sectional estimates on within-firm average variables, while cols. (4) to (6) present (fixed effects) panel estimates on yearly observations. $POLCON$ is an indicator variable for at least one employee of firm j being appointed in a local government during year t. $POLWIN$ is an indicator variable for at least one employee of firm j being appointed in a local government with the winning coalition during year t. Regressions in cols. (1) to (3) include group size, province and sector fixed effects, while regressions in cols. (4) to (6) include firm, province-year and industry-year fixed effects. Robust standard errors in parenthesis. *, ** and *** denote coefficients significantly different from zero at the 90% confidence, 95% confidence and 99% confidence, respectively.

als employed in the firm, and (ii) differenced in the size of the local administration each firm is connected to. Results are qualitatively unaffected. Therefore, we stick to binary indexes of political connectedness for the sake of comparability with previous literature.

1.3.2 Robustness

Of course, the fixed effects estimator may still be biased for several reasons. Therefore, in table 2.3 we investigate the robustness of these findings with respect to alternative sources of bias. One first concern is that there are other (possibly unobserved) factors affecting both the probability of being connected and changes in output. In particular, expanding (contracting) firms could be hiring (firing) workers more intensively than other firms, thus raising (lowering) the chances of employing a local politician. For this reason, in col. 1 we allow for firm-specific trends (in addition to firm-specific fixed effects), which do not affect the results. Rather than following a linear trend, however, production levels could respond to transitory firm-specific shocks. A more severe test consists then in restricting the attention to those connections established through tenured employees in year t, i.e. those who were employed in the same firm also in previous (at least since $t-1$) and subsequent (at least until $t+1$) years. In this way, we excluded those cases in which the variable $POLWIN_{jt}$ changes *only* as a consequence of firm j hiring (firing) decisions at time t. This alternative definition too does not affect the results (col. 2). Pushing this argument further, we exploit only variation in political connections due to employees that were already in the firm in the first year this entered the sample. In other words, we exclude political connections granted by (possibly endogenous) worker flows across firms.

Even in this case, results are not affected (col. 3).

Table 1.3: Robustness

	(1) *trend*	(2) *tenured*	(3) *stayers*	(4) *ability*	(5) *exports*	(6) *domestic*
$POLWIN$	.052*** (.016)			.065*** (.022)	-.008 (.107)	.044** (.019)
$POLWIN$ (*ten*)		.045** (.020)				
$POLWIN$ (*stay*)			.054** (.022)			
$POLPRE$				.028 (.023)		
$POLPOST$				.030 (.019)		
obs.	12561	10747	10747	10747	12561	12526
firms	1226	1219	1219	1219	1226	1226
R^2	.939	.898	.898	.898	.623	.864
adj R^2	.924	.871	.871	.871	.533	.832

Note: The dependent variable is revenues at the firm level deflated with industry-level indexes from the Italian National Accounts. Cols. (5) and (6) distinguish between exports and domestic sales, respectively. The sample is a panel of manufacturing firms observed during the period 1985-97, with the exception of cols. (2) and (4), in which we restrict to the 1986-96 period. $POLWIN$ is an indicator variable for at least one employee of firm j being appointed in a local government with the winning coalition during year t. $POLWIN$ (ten) is an indicator variable for at least one appointed employee being tenured, i.e. he/she being hired and/or fired in year $s \neq t$. $POLWIN$ ($stay$) is an indicator variable for at least one appointed employee being in the firm since year 1985. $POLPRE$ is an indicator variable for at least one employee of firm j being subsequently appointed in a local government with the winning coalition, i.e. being appointed in year $s > t$. $POLPOST$ is an indicator variable for at least one employee of firm j being previously appointed in a local government with the winning coalition, i.e. being appointed in year $s < t$. All regressions include firm, province-year and industry-year fixed effects, except in column (1) where we included firm-specific trends and aggregate trend. Robust standard errors in parenthesis. *, ** and *** denote coefficients significantly different from zero at the 90% confidence, 95% confidence and 99% confidence, respectively.

A different concern is that the correlation between output and political connections picks up the effect of politicians' ability rather than their access to executive power. This would be the case whenever productive human capital and political skills are correlated, which is indeed a recurrent assumption in the literature (see, for instance, Mattozzi and Merlo, 2008). For example, outstanding sales managers permanently raise gross output, independently of other choices. But they might also be more likely to be elected than the average individual. In this case the coefficient of $POLWIN$ would be capturing the output consequences of having a brilliant sales manager, irrespective of the connection. We net out these effects adding dummies for the presence in the firm of employees who at some point establish the connection. This implies that β is estimated exploiting the within-firm correlation between output and the connection status net of the fixed-effect traceable to specific politician-employees. We allowed for separate dummies, $POLPRE$ and $POLPOST$, equal to 1 before and after appointment, respectively, because the individual effect might be different before and after the appointment as a local politician, for instance because of experience accumulated or networks established while in office, see

Diermeier et al. (2005) and Kramarz and Thesmar (2006), respectively. If anything, the estimated effect of political connections increases to 6.5% (col. 4).

Finally, in the last two columns of the table we start distinguishing among alternative channels through which political connections may affect firm revenues. In order to do that, we estimate the baseline specification separately for (the log of) exports and domestic sales. It turns out that the increase in revenues is exclusively due to changes in the latter component, while the effect of political connections on exports is not significantly different from zero.[5] This last finding is consistent with the grabbing hand hypothesis, because domestic sales may possibly depend on purchases from the public administration while exports do not. Moreover, the absence of any effect on exports downplays productivity-based explanations of the effect of political connections, which according to the heterogeneous-firms-and-trade literature should result in higher sales in foreign markets (e.g. Melitz, 2003; Bernard et al., 2007). Of course, domestic sales and exports are very rough measures of public demand and productivity, respectively. We next turn to examine more systematically these issues.

1.3.3 Productivity analysis

To what extent is the observed increase in market power attributable to productivity changes? This important issue has received so far little attention in the literature. Still, it is crucial to distinguish between efficient and inefficient forms of corruption (and the welfare implications that follow).

We identify productivity-effects by estimating the coefficient of $POLWIN$ in a production function framework, i.e. holding the factors of production constant. Results are reported in Table 1.4. In the first two columns we augment (1.9) with measures of production factors. In particular, in col.1 we include on the right hand side the (log of) employment, physical capital and intermediate inputs (along with firm, industry-year and province-year fixed effects). Employment is measured by the total amount of weeks worked by employees during the year, and the capital stock is constructed applying the perpetual inventory method to the investment series. Both revenues and capital series are deflated using 2-digit industry indexes from National Accounts. Our result point to no significant effects of connections on firm productivity. The coefficient of interest is not statistically significant even in col. 2, where we adopted a (log) value added specification of the production function.

Yet, industry-deflated value measures of firm output would reveal productivity only under very stringent conditions. In particular, whenever within-industry price differences

[5]Since exports are censored at zero in about 45% of the observations, the dependent variable in col. 5 is, more precisely, the log of (1+exports), which is of course still censored. Nevertheless, we estimated the export equation by OLS in order to sweep out fixed effects, which may instead bias non-linear maximum likelihood models (see Greene, 2004). The Logit fixed effect model does also escape the incidental parameters bias through a within-firm transformation, but this comes at the cost of an information loss due to the binary re-coding of the export variable. In any case, OLS, Tobit and Logit estimates convey the same result, namely that political connections do not affect exports (the results for Tobit and Logit are not reported but are available upon request).

Table 1.4: production function estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	RESTRICTED COEFFICIENTS				SECTOR-SPECIFIC COEFFICIENTS				NO FACTORS	
	r	va	Δy	Δp	r	va	Δy	Δp	Δy	Δp
$POLWIN$	-.001	.016			.0004	.014				
	(.005)	(.013)			(.005)	(.013)				
$\Delta POLWIN$			.399	.085			.602	.070	2.964**	.236
			(.711)	(.420)			(.734)	(.418)	(1.297)	(.419)
					control variables					
$\ln L$	YES	YES	NO	NO	YES	YES	NO	NO	NO	NO
$\ln K$	YES	YES	NO	NO	YES	YES	NO	NO	NO	NO
$\ln X$	YES	NO	NO	NO	YES	NO	NO	NO	NO	NO
$\Delta \ln L$	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO
$\Delta \ln K$	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO
$\Delta \ln X$	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO
obs.	10178	10110	4075	4076	10178	10110	4075	4076	4075	4076
firms	978	978	717	718	978	978	717	718	717	718
R^2	.99	.953	.689	.339	.991	.953	.7	.343	.208	.323
adj R^2	.988	.94	.617	.184	.989	.941	.629	.186	.024	.166

Note: The dependent variable is reported on top of each column. r and va are (the log of) yearly revenues and value added at the firm level, respectively, deflated with industry-level indexes from the Italian National Accounts. Δy and Δp are the log-difference, between year $t-1$ and t, of real output and prices at the firm level. The sample is a panel of manufacturing firms observed during the period 1985-97. $POLWIN$ is an indicator variable for at least one employee of firm j being appointed in a local government with the winning coalition during year t. $\Delta POLWIN$ denotes the log-difference of the same variable between year $t-1$ and t. The table reports also the control variables included in each column: $\ln L$ is the log of labor employed by the firm, expressed in terms of worker-weeks; $\ln K$ is the log of capital, reconstructed using the perpetual inventory method; $\ln X$ is the log of value of intermediate inputs; finally, $\Delta \ln L$, $\Delta \ln K$ and $\Delta \ln X$ are the log-difference of the same variables between year $t-1$ and t. The coefficients of all control variables are restricted to be equal in cols (1) to (4); instead, they are sector-specific in cols. (5) to (8). All regressions include firm, province-year and industry-year fixed effects. Robust standard errors in parenthesis. *, ** and *** denote coefficients significantly different from zero at the 90% confidence, 95% confidence and 99% confidence, respectively.

are relevant (as it is the case in imperfectly competitive markets) industry-deflated output might be a poor measure of productivity because idiosyncratic shocks induce simultaneous changes in firm-specific prices (not captured by aggregate deflators); see, for instance, Klette and Griliches (1996) and Foster et al. (2008). Firm-level price data provide a convenient way out of this problem. Information on prices is available for a subsample of our firms. Starting in 1988, the INVIND questionnaire asked firms to report the average sales price change over the previous year, Δp_{jt}. The response rate is 41.3%, restricting the sample to 717 firms. Column 3 reports estimates of equation (1.11), after taking first differences and measuring the log-change of real output as $\Delta y_{jt} = \Delta r_{jt} - \Delta p_{jt}$ (where Δ denotes year-to-year differences). Results are in line with those obtained using value measures of output, pointing at no effect of political connections on productivity. Firm-specific prices are also unaffected by political connections (col. 4).

Columns (5) to (8) replicate the production function estimates letting production factor coefficients free to vary across sectors; however, the coefficient of $POLWIN$ is still not significantly different from zero. Column (9) replicates the results in table 1 on the subsample of firms for which price-data are available, showing that the difference between the coefficient of political connections in the two tables is not driven by differences in the sample or to errors in the measurement of firm price changes. Finally, column (10) excludes that such difference is due to confounding price effects.

1.3.4 The role of public demand

Combining our previous results suggests that firms experiencing connection-induced increases in revenues respond to demand shifts rather than to productivity pushes. To distinguish between public and private demand shifts, we will exploit differences across firms in the weight of sales to the public administration over total sales. Ideally, we would want to look at this measure at the firm-level. Unfortunately, neither the INVIND questionnaires nor the firm balance sheets report this information. We circumvent this problem by examining the heterogeneity in the effect of political connections across industrial sectors and geographical areas characterized by a different incidence of public expenditure over total demand. These exercises are reported in Tables 1.5 and 1.6.

Table 1.5: The role of public demand

	(1)	(2)	(3)	(4)	(5)	(6)
	SECTORAL DEP.		REGIONAL DEP.		CORRUPTION	
	high	*low*	*high*	*low*	*high*	*low*
$POLWIN$	.062***	.008	.164**	.030*	.097***	.032
	(.021)	(.033)	(.065)	(.017)	(.034)	(.021)
obs.	6922	5631	1770	10782	3922	8498
R^2	.93	.88	.93	.88	.92	.88
adj R^2	.90	.83	.89	.86	.89	.85

Note: The dependent variable is revenues at the firm level deflated with industry-level indexes from the Italian National Accounts. The sample is a panel of manufacturing firms observed during the period 1985-97. Columns (1) and (2) consider only the subsample of firms operating in manufacturing sectors above and below the median in terms of sales to the public administration over total sales, respectively. Columns (3) and (4) consider only the subsample of firms operating in regions above and below the median in terms of public expenditure over total value added in the manufacturing sector. Columns (5) and (6) consider only the subsample of firms operating in provinces above and below the median in terms of corruption, respectively. $POLWIN$ is an indicator variable for at least one employee of firm j being appointed in a local government with the winning coalition during year t. All regressions include firm, province-year and industry-year fixed effects. Robust standard errors in parenthesis. *, ** and *** denote coefficients significantly different from zero at the 90% confidence, 95% confidence and 99% confidence, respectively.

The extent of firms reliance on demand by the public sector largely depends on their specific line-of-work. To attribute each firm in the sample a degree of "proximity" to public demand we exploited the Italian input-output matrix and ranked manufacturing industries based on the ratio of sales to the public sector over total sales. We then estimated the effect of political connections separately for firms operating in industries above and below the median of such ranking. The average ratio of sales to the public administration over total sales in the two groups of sectors is 4.5% and 0.3%, respectively; the average over all sectors is 2.45%.[6] The estimates presented in cols. 1 and 2 of Table 1.5 show that the effect is significant (at the 1 percent level) *only* for firms operating in industries that rely more heavily on demand by the public administration. At the opposite, the revenues of firms that sell their products almost exclusively to private consumers are not affected by political connections.

This finding, taken together with the productivity analysis above, suggests that political connections impact on firm revenues only through demand by the public administration (as opposed to firm productivity and/or private demand). In terms of equation (1.10), $\beta = 0$ whenever $\tilde{e} = 0$, which in turn implies that $a = b = 0$, or

$$\beta = \tilde{e}\tilde{b}. \tag{1.13}$$

This result is confirmed also when we exploit variation in the relevance of public demand across geographical areas (as opposed to industrial sectors). Based on recently issued Italian Treasury data on expenditure by local administrations (*Conti Pubblici Territoriali*) we distinguished firms operating in regions characterized by above- and below-median values of public expenditure over value added in manufacturing. The average of this ratio for the two groups of regions is 31% and 8%, respectively.[7] While the effect of political connections is greater than zero in both groups of regions (cols. 3 and 4, respectively), its magnitude is more than five times larger in high-expenditure than in low-expenditure regions. Moreover, the effect in low-expenditure regions is very weakly statistically significant.[8]

These results are consistent with the grabbing hand hypothesis, according to which the private benefits from political connections descend from distorting the allocation of public expenditure. A first approximation of such distortion is provided by the grabbing hand

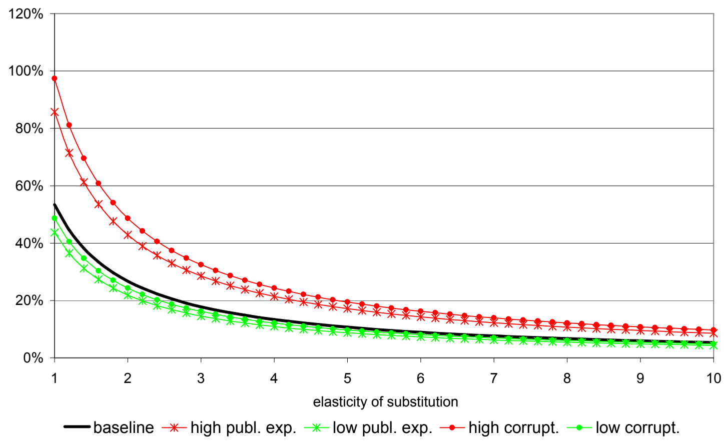
[6]The measure of industry dependence on public demand was computed from the 2-digit IO matrix issued by the Italian National Statistical Institute (Istat) in 1992. Specifically, manufacturing industries were ranked based on the fraction of demand of their products ("use") from the PA, Education, Health and Waste sectors. According to this classification, industries with high shares of sales to the public sector include for example basic pharmaceutical products and pharmaceutical preparations, fabricated metal products, coke and refined petroleum products, chemicals and chemical products, non metallic mineral products, machineries and equipment, paper and paper products. Among low-dependence industries are transport equipment, watches and optical products, domestic appliances, agricultural and forestry machinery, computers and precious metal.

[7]Specifically, we computed the average current and capital expenditure in infrastructures (as defined by the Italian Treasury, see http://www.dps.mef.gov.it/cpt/cpt.asp) by Italian local administrations in 1996 and 1997, the first two years for which data are available. The corresponding figured for industry value added were taken from the Regional Economic Accounts (Conti Territoriali, see http://www.istat.it/conti/territoriali). According to these calculations high-expenditure regions include Valle d'Aosta, Trentino Alto Adige and Liguria (North), Lazio and Molise (Centre), and Campania, Basilicata, Calabria, Sicilia and Sardegna (South).

[8]Because it includes items other than direct purchases from manufacturing industries, the (geographical) measure of dependence does not capture the incidence of sales to the public administration over total sales as precisely as the (sectoral) measure based on input-output coefficients; in particular, the first measure over estimates the incidence of public demand over total sales. It does adequately capture relative differences in the reliance on public demand across geographical areas, though, under the assumption that the fraction of public resources directed to manufactures is constant across regions (e.g. it depends only on the "technological", sectoral coefficients).

component in (1.12). We may thus estimate its empirical counterpart by computing $\tilde{b}$ in equation (1.13) as the ratio of the estimated β (equal to 5.2% in our baseline estimates) over the average ratio $\tilde{e}$ of sales to the public administration over total sales (equal to 2.5% according to the input-output matrix). After plugging the sample variance of $POLWIN$ (0.247), the baseline estimate of the misallocation of public expenditure implied by political connections is plotted in figure 1.4. This estimate depends on the elasticity of substitution across firm products, ranging between 0 (for the case of perfect substitutability) to slightly more than 50% (as σ tends to 1). While performing an analogous exercise, Hsieh and Klenow (2009) assume an elasticity of substitution equal to 3 (based on estimates by Broda and Weinstein, 2006) which in our case implies a decrease in the provision of public good equal to 18% (relative to the case without political connections and for any given level of public expenditure).

Figure 1.4: The misallocation of public expenditure

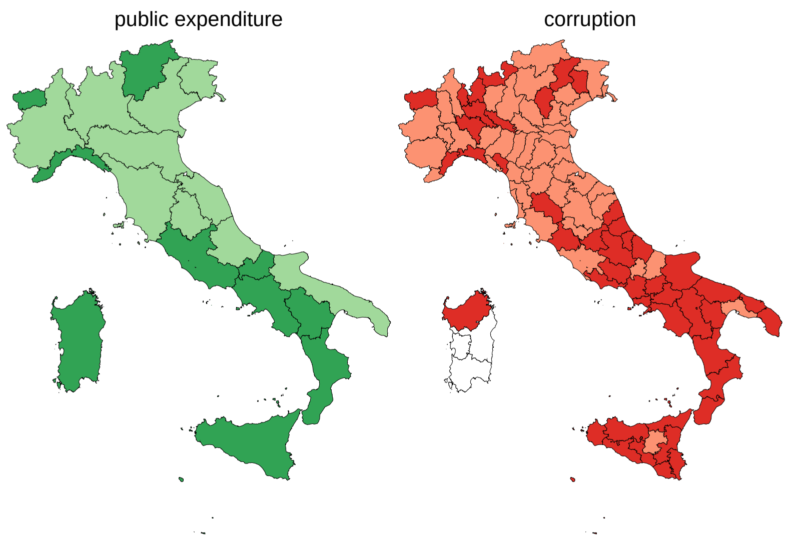


Note: This graph shows the estimated degree of misallocation of public expenditure that is due to political connections, distinguishing between different geographical areas. High and low public expenditure areas include regions above and below the median in terms of public expenditure over total value added in manufacturing, respectively. High and low corruption areas include provinces above and below median value of the corruption index constructed by Golden and Picci (2005).

The same graph also plots the (estimated) degrees of misallocation in regions characterized by high and low public expenditure, which turn out to be greater and lower than in the baseline case, respectively. This is because the revenue premium differs more than the incidence of public expenditure in manufacturing between high and low expenditure regions, which according to equation (1.13) implies a greater $\tilde{b}$ (and thus a greater distortion) in the former group of regions. This finding may be interpreted as a higher degree of rent-seeking (as captured by $\tilde{b}$) arising in regions where the payoffs from such activities are greater (i.e. public expenditure is higher).

In order to explicitly isolate the role of differences in attitudes toward rent-seeking, in the last two columns of table 1.5 we separately estimate the effect of political connections in provinces that lie above and below the median in terms of corruption, respectively. Our measure of corruption comes from Golden and Picci (2005), who construct it as the difference between the cumulative amount of public resources devoted to public works in each province and the physical quantities of realized infrastructures (after controlling for other determinants of the costs of construction). The rationale of this approach is that, keeping constant the technological determinants of production costs, the residual of public expenditure per unit of infrastructure can be attributed to bribes and other forms of corruption. As can be seen from the map in Figure 1.5, this approach produces a significant overlap with variation in public expenditure, both measures broadly yielding the north-south divide with some relevant exceptions. Results in cols. 5 and 6 show that the benefits from connections are significant only for firms located in high corruption areas.[9] The implied degree of misallocation in high and low corruption regions is analogous to that estimated in regions characterized by high and low public expenditure, respectively (see Figure 1.4).

Figure 1.5: Regional characteristics



Note: These figures show the distribution of public expenditure across Italian regions and of corruption across provinces. Darker colors correspond to higher quantiles (white denotes missing values).

Combining the sectoral and the geographical dimensions confirms that the average estimated effect of connections on market shares is mainly driven by firms combining technological proximity to public demand and localization in high-expenditure, high-propensity to official misconduct areas. This can be seen in Table 1.6, where we reported the results obtained running our revenues regression on separate subsamples corresponding to the intersection of the sectoral and (each of the two) geographical breakdowns. The estimated

[9] These findings are unaffected when using a different index of corruption, namely the incidence of *parliamentary malfeasance* by deputies elected in a given electoral district, as computed by Golden (2007).

coefficient is never statistically significant for firms operating in sectors with limited (technological) interaction with the public administration (second row). On the other hand, it is always significant for highly dependent firms (first row), being higher in magnitude (up to five times larger than the average effect) for firms located in high expenditure and high corruption areas.

Table 1.6: The role of public demand (sectors $\times$ regions)

SECTORAL DEP.	REGIONAL DEP.		CORRUPTION	
	high	*low*	*high*	*low*
high	.243***	.043**	.104***	.060**
	(.075)	(.022)	(.040)	(.024)
low	.028	-.003	.071	-.024
	(.068)	(.033)	(.067)	(.037)

Note: The dependent variable is yearly revenues at the firm level, deflated with industry-level indexes from the Italian National Accounts. The sample is a panel of manufacturing firms observed during the period 1985-97. This table reports the coefficients and standard errors of $POLWIN$, an indicator variable for at least one employee of firm j being appointed in a local government with the winning coalition during year t, estimated on different subsamples. The upper and lower row restrict the sample to firms operating in manufacturing sectors above and below the median in terms of sales to the public administration over total sales, respectively. Columns (1) and (2) restrict the sample to firms operating in regions above and below the median in terms of public expenditure over total value added in the manufacturing sector. Columns (3) and (4) restrict the sample to firms operating in provinces above and below the median in terms of corruption, respectively. All regressions include firm, province-year and industry-year fixed effects. Robust standard errors in parenthesis. *, ** and *** denote coefficients significantly different from zero at the 90% confidence, 95% confidence and 99% confidence, respectively.

1.3.5 Factor market returns

A final interesting question we can address with our matched employer-employee data is how the revenue premium from political connections is distributed. We start looking at firm earnings by replicating our baseline revenues regression (Table 1.2, col. 5) replacing the dependent variable with alternative measures of profits (Table 1.7). In column 1 the measure of earnings is Earnings Before Interests Taxes Depreciation and Amortization (EBITDA), which takes non-negative values in almost all observations and can therefore be taken in log, thus favoring comparability with the results for revenues. Estimates indicate that firms see a 5% increase in EBITDA in correspondance of the connection period, nearly the same increase experienced by revenues. To check whether this result is affected by the different impact of interest payment and depreciation figures, in column 2 we used firms' profits (Earnings Before Taxes, EBT). Since this figure is negative in more than one fourth of cases, it is taken in levels rather than in logs. Results indicate that establishing a connection increases EBT on average by 800 thousands euros with respect to the baseline scenario. For comparison, the distance between profits of firms at the 50th and firms at the 75th percentile in our sample is slightly less than 1600 thousands euros.[10]. In col. 3 we look at what these results imply for firms profitability, as measured by the Return on Asset (ROA). According to our estimates, the latter increases by almost 0.6 percentage point in connected firms (the 50th and the 75th percentile difference amounting to about 4 percentage points). Regressions of income and total tax rates paid out by the firm, reported in cols. (4) and (5), confirm that higher profitability descends directly from changes in revenues rather than from lower taxes, the effect on taxes being not significantly different from zero. This is consistent with the fact that taxes in Italy are, for the most part, beyond the control of local politicians.

Table 1.7: Factor market returns to political connections, firm-level estimates

	(1) $\ln \Pi$	(2) *EBT*	(3) *ROA*	(4) *tax (income)*	(5) *tax (total)*	(6) *W (INPS)*	(7) *W (CADS)*
$POLWIN$	.051**	893.532**	.653***	-.037	-.056	.0001	.008
	(.023)	(354.039)	(.227)	(.036)	(.039)	(.001)	(.007)
$\ln L$ $(weeks)$						.980***	
						(.005)	
$\ln L$ $(workers)$							.751***
							(.044)
obs.	11694	12561	12561	12539	12539	12389	12450
firms	1217	1226	1226	1225	1225	1213	1226
R^2	.863	.341	.612	.227	.228	.999	.985
adj R^2	.828	.183	.519	.041	.043	.999	.981

Note: The dependent variables is reported on top of each column. $\ln \Pi$ is the log of Earnings Before Interests Taxes Depreciation and Amortization (EBITDA); EBT is Earnings Before Taxes; ROA is Return on Assets; *tax (income)* is the rate of income taxes over EBT; *tax (total)* is the rate of total taxes (income and property) over EBT; *W (INPS)* is total wages paid by the firm according to social security archives; *W (CADS)* is total wages paid by the firm according to balance sheets. The sample is a panel of manufacturing firms observed during the period 1985-97. $POLWIN$ is an indicator variable for at least one employee of firm j being appointed in a local government with the winning coalition during year t. $\ln L$ $(weeks)$ is the log of labor employed by the firm, expressed in terms of worker-weeks (from social security archives); $\ln L$ $(workers)$ is the log of the number of workers employed by the firm (from balance sheet data). All regressions include firm, province-year and industry-year fixed effects. Robust standard errors in parenthesis. *, ** and *** denote coefficients significantly different from zero at the 90% confidence, 95% confidence and 99% confidence, respectively.

In the last two columns we use yearly wage and employment data to show that (perhaps unsurprisingly) there are no benefits from connections in terms of average wages paid to employees. This is true irrespective of the wage and employment data source used: both social security (INPS) data on firm-level average yearly wages and weeks worked (col. 6) and balance sheet (CADS) data on labour compensation and number of workers (col. 7) strongly indicate the absence of any effect of connections on wages.

While this is totally plausible in the aggregate, competition for political connections in the labor market should, on the other hand, command higher wages for appointed employees. However, the wage premium could be too small to be detected at the aggregate level. Hence, addressing this issue amounts to asking whether the wage pattern of individuals appointed in a local government systematically differs from that of other employees,

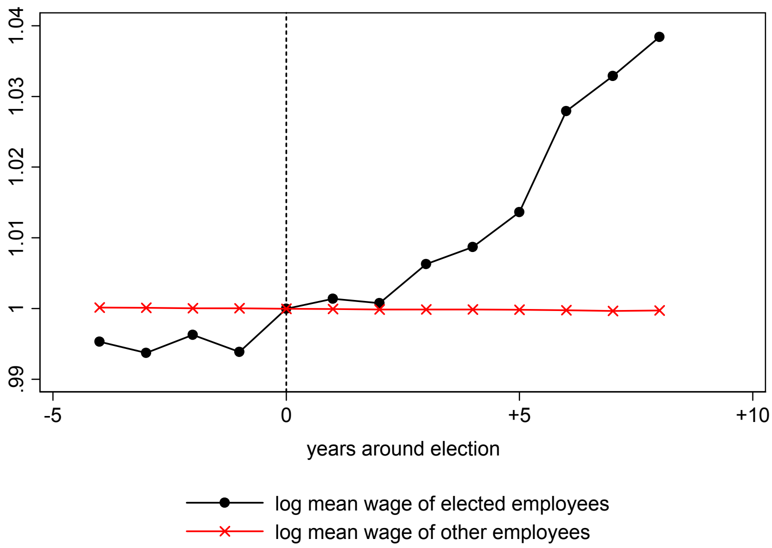
[10]Similar results are obtained using operating profits (Earnings Before Interests and Taxes, EBIT), not reported here.

once standard wage determinants are taken into account. We investigate this possibility exploiting individual-level social security data to run a wage regression of (log) individual wages on age and tenure profiles, year effects and individual fixed effects:

$$\ln w_{it} = h(age_i, ten_i) + \mu_t + \mu_i + \epsilon_{it}$$

The latter are meant to absorb all time invariant individual differences (as gender, education and ability) that might influence wage profiles as well as the probability of election. Figure 1.6 plots the average residuals from such regression for the group of local politicians around the election year (indicated by the 0 on the horizontal axis) against those of other employees. The graph provides several important insights. First, while the pre-election dynamics of the wages of would-be administrators does not seem to differ significantly from that of other workers, the two diverge sharply after appointment. Second, the wage differential persists over time and does not revert to the baseline even after a substantial number of years. Finally, the wage premium gained by elected employees appears to be limited to around 2 percentage points on average over the period reported.

Figure 1.6: Political connections and individual wages



Note: This figure shows the average change induced by appointment in a local government on individual (log) wages. In particular, the darker line graphs the residuals of a regression of the log wage on individual age, age squared and year fixed effects, averaged over all individuals ever appointed in a local government around the year of appointment (year=0). The lighter line graphs the same average for individuals never appointed in a local government.

In Table 1.8 we more precisely quantify these visual insights through a set of wage regressions. We first look for the effect of political appointment on the level of wages. Column 1 reports the results obtained augmenting the wage specification above with the same dummy used for firms (i.e. $polwin_{it}$). The estimated coefficient is small and negative, consistent with the visual evidence above, which suggests that wage increases are deferred with respect to the election date and progressively increasing even after the av-

Table 1.8: Factor market returns to political connections, individual-level estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	INTERCEPT EFFECTS			SLOPE EFFECTS		
$Age/10$	0.4123***	0.4124***	0.4127***	0.4123***	0.4123***	0.4062***
	(0.0047)	(0.0047)	(0.0047)	(0.0047)	(0.0047)	(0.0047)
$(Age/10)^2$	-0.0352***	-0.0352***	-0.0352***	-0.0352***	-0.0352***	-0.0353***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
polwin	-0.0058***		-0.0398***			
	(0.0013)		(0.0018)			
polpost		0.0253***	0.0570***			
		(0.0016)	(0.0022)			
$agepost/10$				0.0736***	0.0284***	0.0281***
				(0.0031)	(0.0055)	(0.0055)
$agepol/10$					0.0119	0.0105
					(0.0132)	(0.0132)
$(agepol/10)^2$					0.0029*	0.0030*
					(0.0017)	(0.0017)
individual FE	YES	YES	YES	YES	YES	NO
individual-firm FE	NO	NO	NO	NO	NO	YES
obs.	13756683	13756683	13756683	13725367	13725367	13725367
R^2	0.178	0.178	0.178	0.178	0.178	0.178
adj R^2	0.794	0.794	0.794	0.794	0.793	0.792

Note: The dependent variable is the log of real weekly wages of all individuals employed in the firms in our sample during the period 1985-97. *polwin* is an indicator variable for individual j being appointed in a local government with the winning coalition during year t. *polpost* is an indicator variable for individual j having been appointed in a local government with the winning coalition at some year $t_0 <= t$. $Agepost_{it}$ is an individual-specific trend starting the year of election, i.e. $agepost_{it} = polpost * (age_{it} - age_{0,it})$ where age_0 is age of the elected the year before election. $agepol = age * pol_i$ is the interaction between age and a dummy identifying local administrators (i.e. $pol_i = 1$ if $polwin_{it} = 1$ at some t). *, ** and *** denote coefficients significantly different from zero at the 90% confidence, 95% confidence and 99% confidence, respectively.

erage appointment spell (amounting to nearly 4 years in our sample). In column 2 we captured this feature using the variable $polpost_{it}$, which takes value 1 in all years *following* the appointment of the individual in a local government. The estimated coefficient indicates an average post-election increase in wages by nearly 2.5 percentage points. Combining the two previous variables allows to confirm the graphical impression that, after the election year, wages follow a stepwise pattern, raising by less than 2 points during appointment and nearly 6 points afterwards. The last figure would amount to just 1000 euros per year if evaluated at the average wage in our sample. An alternative way of capturing the diverging pattern of politicians' wages consists in allowing for changes in slope of the age-earnings profile following their election. This is obtained augmenting the baseline wage regression with the term $agepost_{it} = polpost_{it} * (age_{it} - age_{i0})$ where age_{i0} is the age of individual i in the year of election. The result in column 4 indicates that the slope of the age-earnings profile increases by around 10% for elected employees with respect to the baseline profile. Interestingly, such estimate implies that, at the average duration of appointments, wages of elected employees increase by 2.8 percentage points, just above the average wage premium estimated with the specification in column 1.

All the above results could just reflect the fact that winning politicians are a selected sample of employees characterized by a steeper wage profile relative to the average worker. We address this issue allowing their age-earnings relationship to differ from the profile of other workers in the specification of column 4, where age variables are interacted with a dummy for the individual being elected in a winning coalition at some point within our time-window. Hence, the coefficient on the term $agepost_{it}$ now captures differences in the profile of elected workers with respect to would-be (or former) local politicians, rather than with respect to the average individual. Results reported in column 5 still indicate that wages grow at faster rate after being elected, even if by a lower amount with respect to the previous results. Interestingly, the coefficients of age-interactions suggest that the profile of appointed politicians is not significantly different from that of the average worker. This finding survives a final check where we looked for potential confounding factors arising from mobility of workers across firms. By focusing on changes in individual age profiles, in fact, our results might be driven by the fact that local politicians systematically move towards high wage firms. We account for this possibility estimating our wage equation accounting for individual-firm fixed effects. Results reported in column 6 show that our main findings are unaffected by this extension.

1.4 Conclusions

Connections between firms and the public administration are widespread throughout most countries in the World. The advantages granted by such linkages, in terms of market power and profits, are often criticized on both ethical and efficiency grounds. Our analysis deals with the second dimension, asking whether the existence of political connections actually conditions the activity of the public sector, distorting in particular the allocation of public expenditure.

Our results confirm that this is indeed the case. We find that greater market power experienced by politically connected firms is not driven by greater productivity; rather, it is favored by a greater weight the public administration in total demand. Also, a higher degree of corruption seems to advantage politically connected firms relative to other firms. These findings suggest that political connection may entail economic losses. At the same time, the also suggest that the severity of these losses depends strongly on the set of external conditions present in each economy.

Finally, we showed some evidence that political connections are remunerated in equilibrium. In particular, employees appointed in a local government experience a steeper wage profile after election. Overall, this premium appears however to be rather limited, suggesting either that firm owners are able to appropriate most of the rents or that connected employees benefit of non-wage compensations.

Chapter 2

Trust and Regulations: Addressing a Cultural Bias

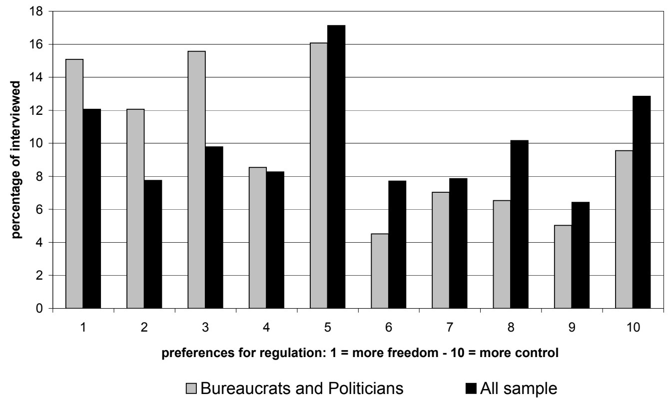
Government intervention is often blamed for entailing large economic inefficiencies. In particular, burdensome regulation of economic activity may distort the efficient allocation of resources by driving individuals and firms out of the official markets and into the informal sector (Johnson et al., 1998; Friedman et al., 2000; Schneider and Enste, 2000; Djankov et al., 2002). Most importantly, regulations seem also ineffective in preventing or correcting market failures. Available empirical evidence suggests that, on average, greater regulation is not associated with lower negative externalities across countries (Djankov et al., 2002).

These findings are consistent with public choice theories that consider regulation as a purely rent-seeking device that benefits a restricted group of insiders (bureaucrats, politicians and industry incumbents) at the expenses of the other agents in the economy (Tullock, 1967; Stigler, 1971; Peltzman, 1976). One problem with this interpretation is that, while in every country the insiders represent only a minor fraction of the population, a much larger share would support greater government intervention. Recent survey evidence shows that, in a large sample of countries, about half of the people believe that the government should actually *increase* control over firms; see figure 2.1. Most importantly, this fraction is much lower among bureaucrats, politicians and industry incumbents, which is strongly at odds with rent-seeking models of regulation.[1]

This paper offers an alternative explanation that can reconcile the existence of positive excess demand for government intervention by individuals at large with the empirical relationship between regulations and market failures that is observed across countries. This explanation hinges crucially on within- and between-country variation in two cultural traits, namely trust and trustworthiness. In particular, the first contribution of this paper is to show that, within each economy, the individual demand for regulation depends negatively on trust toward the others. This relationship has far-reaching implications for

[1] These results refer to a sample of 37,222 individuals from 32 countries interviewed during the period 1999-2002. The questionnaire and the data are described in great detail in the next sections.

Figure 2.1: Opinions about regulation



The histogram shows the distribution of answers to question E042 of World Values Survey in 32 European countries, distinguishing between two categories of individuals: bureaucrats and politicians vs. other individuals. Answers take on discrete values between 1 and 10, where 1 means *'State should give more freedom to firms'* and 10 means *'State should control firms more effectively'*.

the empirical pattern of regulation and market failures across countries characterized by a different degree of (average) trustworthiness. In particular, if average trustworthiness is negatively related to the incidence of market failures and if average trust predicts average trustworthiness across countries, then omitted variation in trustworthiness will bias the estimated effect of regulation on market failures upward. The second contribution of this paper is thus to address the importance of this bias by explicitly incorporating heterogeneity in trust and trustworthiness into the analysis.

Preliminary evidence presented in the next section suggests that the bias induced by omitted variation in culture may be relevant. After controlling for differences in trust, regulation is *not* related anymore with the size of the unofficial sector and it is *negatively and significantly* related to the level of negative externalities (as proxied by water pollution). These findings are not in contrast with public choice theories of government intervention. The supply of regulations could indeed respond to the predatory motives emphasized by previous literature. On the demand side, however, pressures for regulations are driven (among other things) by market failures occurring in the first place. This view is in line with the public interest theory of regulation initiated by Pigou (1938), according to which government intervention provides a (second best) solution to market inefficiencies (see also Banerjee, 1997; Acemoglu and Verdier, 2000). As a matter of fact, all countries impose pervasive regulations in those sectors characterized to a greater extent by negative externalities, natural monopolies, incomplete information and moral hazard (e.g. public utilities, professional services and the health care sector).

Culture enters in this framework by influencing both the risk of market failures in the economy and the individual beliefs about such risks. With respect to the first issue, some cultural traits may prevent individuals from taking advantage of market imperfections at the expenses of other agents, even when it would be optimal to do so from an economic point of view. While this is a departure from the standard *homo economicus* assumption, it is a widely accepted one. In particular, both experimental and non-experimental evidence show that elements of the cultural sphere such as moral values, religious beliefs and social norms may induce individuals to privilege social outcomes over their own private economic interests. Cultural tendencies toward cooperation have been alternatively referred to in the literature as civicness, generalized (as opposed to limited) morality, social capital or, especially in experimental economics, *trustworthiness* (for a recent survey, see Tabellini, 2008).

It is even more uncontroversial in the literature that individual trust reflects, to some extent, expectations about others' trustworthiness. In particular, several papers characterize trust as a mixture of "rational" beliefs about trustworthiness and other behavioral components such as risk preferences and betrayal aversion (Barr, 2003; Bohnet and Zeckhauser, 2004; Bohnet et al., 2008; Fehr, 2008).[2] Therefore, individuals characterized by lower trust would predict on average an higher propensity of the other individuals in the economy to take advantage of market failures; if the demand for government regulation is motivated (at least in part) by concerns for market failures, these same individuals should then prefer more government intervention.

Section 2.2 frames this idea into a simple model with heterogeneity in individual values and beliefs. In particular, some agents are trustworthy and never take advantage of market imperfections, while the others do so whenever it is in their economic interest. In particular, untrustworthy entrepreneurs exert negative externalities on the rest of the economy by adopting cheaper (but polluting) technologies. Trust is simply the subjective belief about the fraction of trustworthy agents in the population.

Entry regulations impose an upfront (wasteful) cost on all entrants in the market. This cost depends on entrepreneurial ability. Since less efficient producers have also the greatest incentives (whenever they are untrustworthy) to adopt cheaper technologies, regulation acts as a screening device in a way analogous to Banerjee (1997). Indeed, regulation effectively screens entrants in the market, but it does so inefficiently for two reasons. First, the untrustworthy producers averted from the official market may still operate unofficially; if they choose to do so, however, they must limit their size, which in turn reduces also the equilibrium level of negative externalities. Second, regulation imposes a burden on all producers, including the trustworthy ones.

The expected costs and benefits of regulations differ across agents according to their beliefs about the incidence of untrustworthy producers. In particular, trustful agents predict less market failures absent regulations so that, other things equal, they demand less government regulations. In 2.3 4 I test this empirical implication of the model on individual data from the World Values Survey. Individual-level estimates allow to control

[2]For a different view see Glaeser et al. (2000), according to whom individual trust reflects one's own (rather than others') trustworthiness.

for country-specific factors by simply including country fixed effects, which considerably reduces the scope for omitted variable and endogeneity bias. Also, difference-in-difference estimates relate the trust-driven component of the individual demand for regulation to economic and institutional characteristics at the country level. Overall, I find that trust has a robust, negative effect on individual preferences for regulation, this effect being greater in countries where market failures are more widespread and the bureaucracy is less corrupt and more efficient.

Finally, in 2.4 5 I examine the implications of these findings for the cross country pattern of regulations and market failures. The main model implication in this respect is that a lower incidence of trustworthiness drives higher levels of unofficial activity, negative externalities and government regulations (through trust), thus inducing a positive (spurious) correlation between all these variables. To take this into account, I explicitly control for average trust (as a predictor for average trustworthiness) when estimating the effect of regulations on unofficial activity and negative externalities. The empirical evidence confirms that, indeed, keeping constant (across countries) the level of average trust considerably weakens the negative effects of regulations relative to previous estimates.

This paper contributes to a large body of research, initiated by Putnam (1993) and Fukuyama (1995), investigating the effect of trust on economic activity.[3] In particular, a series of papers Guiso, Sapienza and Zingales explore the role of trust in markets characterized by the possibility of market failures, and find that trust fosters financial development, stock market participation and international exchange (Guiso et al., 2004, 2009b,a).

More closely related to this work, La Porta et al. (1997), Slemrod (2002) and Lassen (2007) consider the effect of trust on the size of the informal sector, pointing at the existence of a negative relationship between the two. Aghion et al. (2008) and Algan and Cahuc (2009) emphasize instead the interplay between culture and formal institutions, focusing in particular on labor market institutions. I contribute to this strand of literature by investigating the relationship between trust and entry regulations. In this respect, the paper that comes closest to mine is Aghion et al. (2009), who also estimate the negative relationship between trust and entry regulations. However, they do not examine the implications of this finding for the empirical evidence about the effects of regulation. My contribution in this respect is to show that omitted variation in trust and trustworthiness may bias previous estimates of such effects.

2.1 Data and preliminary evidence

This section introduces some of the measures of regulations and culture that will be used throughout the paper and reviews some previous findings about the relationship between regulations and market failures.

[3]An important antecedent to this literature is Arrow (1972), who remarked that 'much of the economic backwardness in the world can be explained by the lack of mutual confidence'. For a recent review see Tabellini (2008)

2.1.1 Regulation and market failures

Unbundling and measuring institutions is never an easy task. This is especially true for regulations, due to the extreme variability of formal and informal regulatory practices around the World. In an extremely influential paper, Djankov et al. (2002) proposed to measure entry regulations by the number of procedures required to start a new business. Such procedures include "*obtaining all necessary licenses and permits and completing any required notifications, verifications or inscriptions with relevant authorities*" and range, for instance, from opening a bank account to scheduling sanitary inspections to the production plants. Since its introduction, this indicator has been updated yearly on behalf of the World Bank's Doing Business project.[4]

While any measure of regulations has its own shortcomings, the Doing Business indicator has the advantage of being available and roughly comparable for almost all countries in the World. Since its introduction, it has thus been extensively used to empirically evaluate the effects of regulations. In the same paper, in fact, Djankov et al. (2002) examine the correlation between regulations and some measures of market failures. Two such measures are the (estimated) size of the informal sector and the level of water pollution, as measured by the emissions of kilograms of organic pollutant per day per worker. The results are shown in the top graphs of figure 2.2. Heavier regulations seem associated with worse outcomes in both respects, the relationship being particularly strong and statistically significant for the case of unofficial activity. Djankov et al. (2002) interpret these findings as evidence in favor of public choice theories of regulation.

2.1.2 Trust toward the others

Cultural traits such as moral values, beliefs and social norms vary widely both within and across countries. The World Values Survey (WVS hereafter) represents a formidable attempt to provide an adequate account of such heterogeneity by the means of international questionnaires collecting detailed individual data along several dimensions (economic, social, cultural, etc.) for more than 200,000 people in 83 countries.[5] The results of this survey deliver, among other things, a measure of individual trust toward the others. Question A165 of the survey asks: *"Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?"*. I define a binary variable *trust* equal to 1 if the answer was *'Most people can be trusted'* and 0 otherwise. This is by far the most widely adopted measure of trust existing in the literature; examples include Knack and Keefer (1997), Guiso et al. (2006), Tabellini (2005) and Aghion et al. (2009).

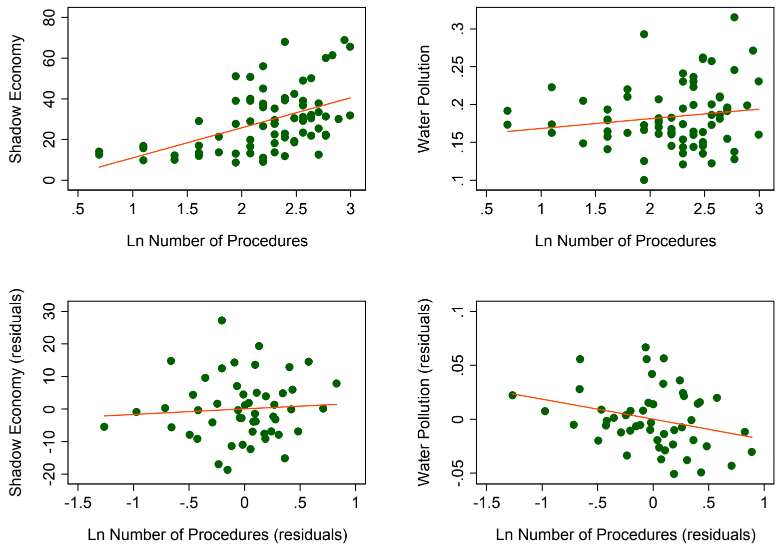
Since all other variables refer to the 1990s, I combined data from the second and

---

[4] Arrunada (2007) presents a critique of the Doing Business indicators, while Woodruff (2006) provides a more general discussion of the issues involved in the measurement of institutions.

[5] Throughout this paper I will refer to version v.20060423 of the database (FOUR-WAVE INTEGRATED DATA FILE, 1981-2004), which is available at http://www.worldvaluessurvey.org/. See the documentation available therein for an exhaustive description of the survey.

Figure 2.2: Regulations and market failures

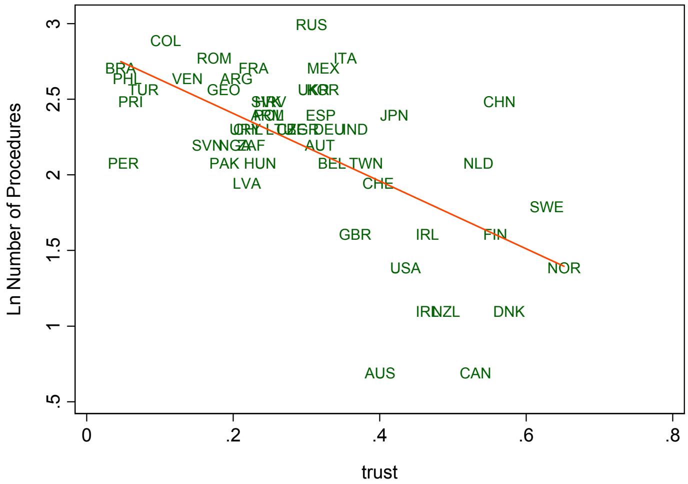


These graphs show the cross country correlation between entry regulations and some types of market failure. The measure of entry regulations is the (log) number of procedures required to open a business; Shadow Economy is the (estimated) size of the informal sector; water pollution are emissions of organic water pollutant (kilograms per day per worker). All three measures come from Djankov et al. (2002). The two graphs on the bottom show the relationship between the residuals of a regression of each of variables on average trust. Average country trust is fraction of people interviewed by the World Values Survey in each country that declared that 'Most people can be trusted'.

third wave of the WVS (which span the 1990-1993 and 1994-1999 periods, respectively) to obtain a cross country measure of average trust. Trust is negatively associated with entry regulations across countries; see figure 2.3.

If trust does also reflect, negatively, the average propensity of individuals in each economy to take advantage of market imperfections, then omitted variation in trust and trustworthiness could drive part of the (positive) correlation between regulations and market failures. Unfortunately, reliable measures of trustworthiness, allowing to verify its relationship with average trust, are not easily comparable across countries. For instance, data about blood donations and voting turnout, which capture the propensity of individuals to privilege social over private interests, could depend on the development of the non-profit sector and on the electoral system, respectively, both of which may be also correlated with regulations. Still, these same measures are more easily comparable across regions within the same country. In particular, Guiso et al. (2004) have used blood donations and voting turnout at referenda to measure variation in social capital across Italian regions. Both measures are indeed positively and significantly correlated with average trust across regions (the correlation coefficient is 0.38 and 0.48 for blood donations and voting turnout, respectively) other than strongly correlated with each other (correlation equal to 0.63). While it is not possible to conclude that such correlations are actually due to the fact that trust predicts trustworthiness, this evidence suggests nevertheless that trust

Figure 2.3: Trust and regulations



This graph shows the cross country correlation between trust and entry regulations. Average country trust is the fraction of people interviewed by the World Values Survey in each country that declared that 'Most people can be trusted'. The measure of entry regulations is the (log) number of procedures required to open a business, from Djankov et al. (2002).

may be a proxy for (unobserved) average trustworthiness. Therefore, partialing out the effect of trust on both regulations and market failures should control, at least in part, for differences in trustworthiness across countries. After doing that, heavier regulations are *not* associated anymore with worse outcomes; see the bottom graphs in figure 2.2. My interpretation of this finding is thus that culture is both an important determinant of regulations and a confounding factor in previous estimates of their effects. The simple model in the next section formalizes this hypothesis.

2.2 The model

Consider a continuum of (infinitesimal) agents with total mass 1. All agents can work as employees, in which case they earn a wage standardized to 0, or they can set up a firm and become entrepreneurs.

Production costs depend on both the technology adopted by the firm and on entrepreneurial ability. Technologies differ in terms of emissions of negative externalities (think of pollution). Dirty technologies do not require any investment by the entrepreneur but emit a high level of externalities, equal to 1. Clean technologies, instead, prevent externalities but impose an upfront cost equal to ω on the entrepreneur. This cost varies across agents, reflecting (negatively) exogenous differences in entrepreneurial ability. Assume for simplicity that ω is uniformly distributed between 0 and 1 over the population of agents.

Entrepreneurs must choose whether they want to operate in the official or unofficial sector. In the former case, they enjoy greater (gross) profits, equal to $\pi < 1$, but must comply with entry regulations. The cost of complying with entry regulations also depends, negatively, on entrepreneurial ability; it equals ωR, where R is the amount of red tape required to set up a firm. Therefore, both the cost of preventing externalities and the cost imposed by the red tape are proportional to ω; the main difference between the two is that the former remains unobserved, while compliance with the red tape is verifiable.

Alternatively, entrepreneurs may escape entry regulations by hiding in the shadow economy. If they do, they must however restrict firm size in order not to attract regulators' attention; in particular, operating profits and negative externalities generated by each unofficial firm are, respectively, $\theta\pi$ and θ, with $\theta < 1$.[6] The parameter θ may be interpreted as a proxy for the quality of institutions: the lower is θ, the higher the degree of law enforcement.

The private utility of each agent is equal to

$$U = y - X, \tag{2.1}$$

where y is income and X is the total level of negative externalities present in the economy. Later on I will slightly modify the utility function in (2.1) to introduce the key ingredient of the model, namely heterogeneity in values and beliefs. Before doing that, let me clarify the role of entry regulations in the simple economy described so far, first under a (benevolent) social planner and then in the decentralized equilibrium.

2.2.1 First and second best optimal regulatory policy

The inequalities $\pi < 1$, $\theta < 1$ and $\omega < 1$ imply that the social optimum W^* is attained when (i) all entrepreneurs prevent externalities, (ii) all firms operate in the official economy, and (iii) all and only the agents for which $\omega < \bar{\omega}^* = \pi/(1+R) < 1$ set up a firm (notice that $\bar{\omega}^*$ equals also the measure of entrepreneurs active in the market). The resulting social welfare function is monotonically *decreasing* in R,

$$W^*(R) = \bar{\omega}^*\pi - (1+R)\int_0^{\bar{\omega}^*} \omega d\omega = \frac{\pi^2}{2(1+R)},$$

which implies that the first best regulatory policy is $R^* = 0$.

Absent a benevolent social planner, agents do not internalize negative externalities, because they take aggregate output and total externalities as given. Therefore, entrepreneurial activity always entails (net) social costs in the decentralized economy. Such costs are greater when firms operate in the official sector, because $\pi - 1 < \theta(\pi - 1)$. Therefore, the optimal policy is to set regulatory barriers so high to drive all entrepreneurs into the unofficial sector.

More formally, agents enter the official sector if and only if $\pi - \omega R > \theta\pi$, or $\omega < \bar{\omega} =$

[6] The "size dualism" is a well established theoretical and empirical result in the literature on unofficial activity (see, for instance, Rauch, 1991; Fortin et al., 1997; Amaral and Quintin, 2006; Antunes and Cavalcanti, 2007).

$\pi(1-\theta)/R$, otherwise they hide in the shadow economy. Aggregate welfare equals

$$\widetilde{W}(R) = -[\bar{\omega} + (1-\bar{\omega})\theta](1-\pi) - R\int_0^{\bar{\omega}} \omega d\omega < 0,$$

which is an *increasing* function of R; therefore, the second-best regulatory policy is $\tilde{R} \to +\infty$ (i.e $\lim_{R\to+\infty} \widetilde{W} = 0$).

Thus, the optimal regulatory policy changes dramatically when moving from the social planner solution to the decentralized economy equilibrium. In the former, entry regulations represent simply a deadweight cost; in the latter, they provide a second best solution to market failures. Notice that this second best regulatory policy would also be the one preferred by all agents under the veil of ignorance, i.e. it would be the majority voting equilibrium as far as agents vote before learning their ability.

In the remaining of this section I allow for a richer characterization of individual values, assuming that some agents voluntarily abstain from exerting negative externalities on the rest of the economy. Under this new assumption, the second best regulatory policy will be an interior of the set of feasible policies and will depend (negatively) on the incidence of the new type of agents. I will also investigate the interplay between values, trust, regulatory policies and the equilibrium level of externalities and unofficial activity, both within and across different economies.

2.2.2 Trust, trustworthiness and regulations

In this section I introduce agent heterogeneity in individual values and beliefs. Starting with the former, a fraction τ of agents are *trustworthy*, while the remaining part are not. The utility of the latter is the same as in (2.1), while that of trustworthy agents suffers psychological penalties for emitting negative externalities or hiding in the shadow economy.[7] In particular, assume that these penalties are so high that trustworthy entrepreneurs always operate in the official sector and invest to prevent externalities.

Turning to beliefs, agents do not know the exact proportion of trustworthy individuals in the economy, but hold expectations about this proportion, denoted by $\hat{\tau}$. Such expectations reflect both the true (unobserved) τ and idiosyncratic prediction errors, so that $\hat{\tau}$ is independently distributed across agents according to the cumulative density function $\Phi(\hat{\tau};\tau) = Prob(t \leq \hat{\tau};\tau)$.

It seems natural to assume (and even impose) that idiosyncratic errors are not *systematically* biased, so that

$$\tau' > \tau \Rightarrow \Phi(\hat{\tau};\tau') < \Phi(\hat{\tau};\tau),\ \forall\hat{\tau}. \tag{2.2}$$

By the law of large number, the expected fraction of trustworthy agents in the economy is also the subjective probability that each other individual is trustworthy. Therefore, $\hat{\tau}$ may be interpreted as *trust toward the others*.

[7]This way of modeling trustworthiness is adopted, for instance, by Frank (1987) and Kandel and Lazear (1992).

Individual preferences for regulation

According to the assumptions above, trustworthy agents only enter the official sector and, whenever they do, they invest to prevent negative externalities. Therefore, the only choice they face is whether to become entrepreneurs or not, which they do if and only if $\omega \leq \bar{\omega}_t = \pi/(1+R)$.

Untrustworthy agents, instead, never invest in the clean technology and must decide whether to enter the official sector or hide in the shadow economy; they will choose the former if and only if $\omega \leq \bar{\omega}_u = \pi(1-\theta)/R$.[8]

The social welfare function under the new assumptions is a weighted average of W^* and $\widetilde{W}$, with weights equal to τ and $(1-\tau)$, respectively. This is not surprising given that trustworthy agents behave in line with social welfare maximization (the social planner case), while the untrustworthy behave non-cooperatively. The optimal regulatory policy is $R_\tau = \arg\max(W_\tau)$, or

$$r_\tau = \left(\frac{1-\tau}{\tau}\right)\phi,$$

with $r_\tau = \left(\frac{R_\tau}{1+R_\tau}\right)^2$ and $\phi = \left(\frac{2-\pi}{2\pi}\right)(1-\theta)^2$. Notice that R_τ is an interior solution between R^* and $\tilde{R}$, and it approaches one extreme or the other depending on τ: the higher the fraction of trustworthy agents, the closer the market equilibrium is to the first best outcome and the lower the need for public intervention, i.e. R_τ approaches $R^* = 0$; at the opposite, the lower the fraction of trustworthy agents, the farther away the market equilibrium is from the first best outcome and the higher the need for public intervention, i.e. R_τ approaches $\tilde{R} \to +\infty$. Therefore, the higher the extent of market failures in the first place, the higher the optimal level of entry regulations. Individual demand for regulation depends in a similar way on expected trustworthiness. Therefore, it differs from R_τ (and from one agent to the other one) because of heterogeneity in $\hat{\tau}$.[9]

The expected utility of trustworthy agents, conditional on trust, is thus

$$U_t(\hat{\tau}) = \bar{\omega}_t\pi - (1+R)\int_0^{\bar{\omega}_t} \omega d\omega - (1-\hat{\tau})[\bar{\omega}_u + (1-\bar{\omega}_u)\theta],$$

which is maximized for

$$r_t(\hat{\tau}) = 2(1-\theta)^2/\pi(1-\hat{\tau}). \tag{2.3}$$

According to this condition, the preferred level of entry barriers depends, negatively, on trust toward the others. Moreover, the effect of trust is stronger the greater is the cost of negative externalities relative to the benefits of economic activity, $1/\pi$, and the better is the quality of institutions, i.e. the lower is θ.

[8] In principle, the untrustworthy could also decide not to enter at all but this choice is always dominated by that of hiding in the shadow economy since $\theta\pi > 0$

[9] It would also differ because of heterogeneity in entrepreneurial ability. However, in order to focus on the role of cultural traits, let me introduce a veil of ignorance about ω, so that each agent knows only its distribution over the population (but not her own ability).

Turning to the untrustworthy agents, their expected utility is

$$U_u(\hat{\tau}) = [\bar{\omega}_u + (1-\bar{\omega}_u)\theta][\pi - (1-\hat{\tau})] - R\int_0^{\bar{\omega}_u} \omega d\omega,$$

which is increasing (decreasing) in R if and only if $\hat{\tau} < 1-\pi/2$ ($\hat{\tau} > 1-\pi/2$), so that the preferred level of regulations is one of the two corners of the set of feasible policies:

$$r_u = \begin{cases} 0 \Leftrightarrow \hat{\tau} > 1-\pi/2 \\ \infty \Leftrightarrow \hat{\tau} < 1-\pi/2 \end{cases} \tag{2.4}$$

Therefore, the level of red tape preferred by each untrustworthy agent is also inversely related to individual trust.

The first implication of the model is thus that, at the individual level, preferences for regulation depend negatively on trust toward the others. Next, I turn to examine the implications of this result for the cross country pattern of trust, regulations and market failures.

Trust, regulations and market failures across countries

There are several economies, $j = 1, 2, ..., J$, like the one just described. Within each economy, regulatory policy is chosen by majority voting. Individuals vote behind the veil of ignorance about their own ability, even though they know the distribution of ω over the whole population.[10] Voting entails some effort so that only trustworthy individuals vote; by contrast, the untrustworthy take electoral results as given. While somewhat ad hoc, this assumption is consistent with other work relating electoral participation to civicness (see for instance Guiso et al., 2004). Under this last set of assumptions, the regulatory policy voted in country j is

$$r^j = 2(1-\theta^j)^2/\pi^j(1-\hat{\tau}_m^j), \tag{2.5}$$

where $\hat{\tau}_m^j$ denotes the median level of trust.

Equations (2.2) and (2.5) taken together deliver two additional implications of the model. First, the negative relationship between regulations and trust carries over across countries. Second, regulatory policy ultimately depends, negatively, on trustworthiness. By the implicit function theorem, in fact, the stochastic dominance assumption in (2.2) defines $\hat{\tau}_m$ as a positive function of τ,

$$\Phi(\hat{\tau}_m; \tau) = 1/2 \ \Rightarrow d\hat{\tau}_m/\tau > 0. \tag{2.6}$$

Finally, the equilibrium level of unofficial activity (as measured by total profits of firms operating in the informal sector) and externalities in each country depends on regulations

[10] You can think of this as a two-period economy: in the first period individuals vote over regulatory policies; in the second period they learn they ability and decide whether they want to set up a firm or not.

and also, negatively, on trustworthiness

$$S^j = (1-\tau^j)(1-\bar{\omega}_u^j)\theta^j\pi^j = (1-\tau^j)[1-\pi^j(1-\theta^j)/R^j]\theta^j\pi^j, \tag{2.7}$$

$$X^j = (1-\tau^j)[\bar{\omega}_u^j + (1-\bar{\omega}_u^j)\theta^j] = (1-\tau^j)[\theta^j + \pi^j(1-\theta^j)^2/R^j]. \tag{2.8}$$

Therefore, the common dependence on trustworthiness affects the observed relationship with regulation. For the case of unofficial activity, it steepens the positive univariate regression line; for the case of externalities, it flattens the negative relationship and it may also turn it into a positive one. Section 2.4 will provide some evidence to quantify this bias; before doing that, I empirically examine the predictions of the model for the individual-level demand for regulation.

2.3 Individual-level evidence

The model in the previous section implies that, at the individual level, preferences for regulation depend negatively on trust.

2.3.1 Estimating equation

The WVS provides a measure of preferences for regulation. Question E042 in section 'Politics and Society' of WVS asks each individual whether *'The state should give more freedom to firms'* or rather *'The state should control firms more effectively'.* It thus measures the individual *excess* demand for regulations (relative to the level that is actually observed in the country). The answer to this question takes on discrete values between 1 and 10, with higher values corresponding to higher demand for government intervention, and it was included in the survey sent to 32 European countries participating into the fourth wave of the survey (1999-2004), listed in table 2.1. This measure will serve as the dependent variable of the estimating equation; let denote it by y.

In the model above, the individual demand for regulation is always inversely related to trust; however, the exact form of this relationship depends on the agent's trustworthiness, according to conditions (2.3) and (2.4). For simplicity, my estimating equation will be based on the sole preferences of the trustworthy, which depend continuously on trust. In particular, the excess demand by each individual in country j can be obtained by subtracting equation (2.5) from (2.3),

$$\underbrace{r - r^j}_{y^*} = \underbrace{2\hat{\tau}_m^j(1-\theta^j)^2/\pi^j}_{\alpha^j} - \underbrace{2(1-\theta^j)^2/\pi^j}_{\beta^j}\,\hat{\tau}. \tag{2.9}$$

One complication arises from the fact that y^* is a continuous variable, while its empirical counterpart y is a discrete index between 1 and 10. Following a standard approach, let y^* be the (unobserved) latent preferences driving the choice of each i-th individual among the k's possible values of y. In particular,

$$y_i = k \;\Leftrightarrow\; \gamma_{k-1} \leq y_i^* + \epsilon_i \leq \gamma_k \;\; k = 1, 2, ..., 10$$

where ϵ is an error term and the γ's are unknown thresholds to be estimated. The odds ratio of individual i preferring a higher level of regulation, i.e. $y_i > k$, is

$$\Delta_i(y_i > k) = \frac{Prob(y_i > k)}{Prob(y_i \leq k)} = \frac{1 - \Lambda(\gamma_k - y_i^*)}{\Lambda(\gamma_k - y_i^*)} \;\forall k,$$

where Λ is the c.d.f. of ϵ. Assuming that Λ is logistic and plugging $trust_i$ for $\hat{\tau}$ into equation (2.9) delivers

$$\ln \Delta_i(y_i > k) = -\gamma_k + \alpha^j - \beta^j trust_i. \tag{2.10}$$

The linear log-odds ratio in (2.10) characterizes the ordered logit model, which can be estimated by Maximum Likelihood.

The main advantage of the logit model (relative, for instance, to the ordered probit) is that it provides an easy interpretation of the coefficients. In particular, the exponentiated coefficient equals the ratio of the odds of $y > k$ over the same odds when the explanatory variable is lower by one unit. This is a particularly useful property given that *trust* is a binary variable, so that

$$e^{-\beta} = \frac{\Delta_i(y_i > k | trust_i = 1)}{\Delta_i(y_i > k | trust_i = 0)}$$

i.e. the exponentiated coefficient of trust is simply the ratio between the odds of preferring more regulation of a trustful individuals over the same odds of a non-trustful individual.

The term α^j in (2.10) will be absorbed by country-specific fixed effects, thus reducing the scope for omitted variable bias and reverse causality due to differences in country-specific factors, like the severity of market failures and the quality of regulation. One complication with this approach is that fixed effects are unattractive in non-linear models because of the so-called incidental parameters' problem. Basically, the estimator of each fixed effect uses only information from the corresponding group so that, when the size of each group is limited and small, the variance of the estimator (both of the intercepts and the slope) does not asymptotically converge to 0 (see, for instance, Greene, 2004). This is usually the case for panels of N cross sectional units observed over T periods. However, in this case I have thousands of (individual) observations available to estimate each (country) fixed effect, so that the relevant asymptotics allow for consistent estimation.

The right hand side of equation (2.10) will also control extensively for individual characteristics like age, gender, income, education, occupation, etc., which are included in the WVS data. Table 2.1 reports the sample size for each country included in the survey, along with the total population, which makes apparent the unbalanced coverage of WVS across countries (coverage is relatively lower for larger countries). I thus weighted observations according to the product of national sampling weights (provided by the WVS) and country populations. However, all results presented below are unaffected when using unweighted observations.

Table 2.1: Sample

country	code	sample	obs.	population
Austria	AUT	1523	1366	8,011,560
Belgium	BEL	1914	1769	10,252,000
Bulgaria	BGR	1002	875	8,060,000
Belarus	BLR	1002	832	10,005,000
Czech Republic	CZE	1911	1823	10,273,300
Germany	DEU	2045	1838	82,210,000
Denmark	DNK	1025	905	5,337,344
Spain	ESP	2417	1002	40,263,199
Estonia	EST	1007	893	1,369,512
Finland	FIN	1040	942	5,176,196
France	FRA	1617	1519	58,895,516
Great Britain	GBR	2005	1717	59,742,980
Greece	GRC	1142	948	10,917,500
Croatia	HRV	1004	940	4,502,500
Hungary	HUN	1003	910	10,210,971
Ireland	IRL	1014	923	3,805,399
Iceland	ISL	970	900	281,000
Italy	ITA	2002	1845	56,948,602
Lithuania	LTU	1020	884	3,499,527
Luxemburg	LUX	1211	1038	438,000
Latvia	LVA	1015	942	2,372,000
Malta	MLT	1004	980	390,000
Netherlands	NLD	1005	978	15,925,431
Polonia	POL	1098	1007	38,453,801
Portugal	PRT	1001	883	10,225,803
Romania	ROM	1148	1032	22,443,000
Russia	RUS	2504	2265	146,303,000
Slovak Republic	SVK	1334	1218	5,388,740
Slovenia	SVN	1008	926	1,989,000
Sweden	SWE	1018	948	8,869,000
Turkey	TUR	4609	1107	67,420,000
Ukraine	UKR	1196	1067	49,175,848
Total		45814	37222	759,155,750

Notes: This table lists all countries for which individual-level data were available. For each country, it reports the number of individuals interviewed by the the fourth wave of WVS, the number of non-missing observations (i.e. the number of individuals that answered both questions about trust and regulation) and the total population

2.3.2 Results

Tables 2.2 to 2.5 present the results of individual-level estimates. Both the simple and exponentiated coefficients (i.e. the odds ratios) are reported. The first column in table 2.2 presents the results of the univariate regression pooling all individuals. The coefficient of *trust* is negative and very high in absolute value. Removing cross-country heterogeneity (column 2) halves the coefficient, which however remains strongly statistically significant. According to this estimate, the odds of $y > k$ for a trustful individual are about 15% lower than the same odds for a non-trustful individual. Due to the inclusion of country specific intercepts, this difference may be interpreted as the "excess demand" for regulations by trustful individuals relative non-trustful ones, keeping constant the level and quality of actual regulation in the country (as well as all other country characteristics).

Table 2.2: Individual-level estimates (baseline)

	(1) *pooled*	(2) *country FE*	(3) *demo*	(4) *income*	(5) *school*
trust	-.349***	-.165***	-.154***	-.157***	-.130***
	[0.705]	[0.848]	[0.857]	[0.855]	[0.878]
	(.028)	(.046)	(.046)	(.047)	(.045)
$age/100$			.876	1.831**	1.911**
			[2.400]	[6.240]	[6.759]
			(.608)	(.811)	(.826)
$(age/100)^2$			-.024	-1.141	-1.338
			[0.977]	[0.319]	[0.262]
			(.678)	(.993)	(.982)
$female$			.188***	.187***	.189***
			[1.206]	[1.205]	[1.208]
			(.031)	(.034)	(.035)
high income				-.346***	-.297***
				[0.708]	[0.743]
				(.035)	(.034)
low income				.088	.059
				[1.092]	[1.061]
				(.060)	(.057)
high schooling					-.206***
					[0.814]
					(.050)
low schooling					.124***
					[1.132]
					(.045)
obs.	37222	37222	37078	31663	31489
country FE	NO	YES	YES	YES	YES
countries		32	32	32	32
log-L	-83648	-82317	-81830	-69661	-69216
log-L_0	-83802	-83802	-83487	-71310	-70913
pseudo R^2	.002	.018	.02	.023	.024

Notes: This table presents estimates of the effect of trust on preferences for regulation at the individual level. The dependent variable is the answer to question E042 in the WVS. It takes on discrete values from 1 to 10, where 1 means *State should give more freedom to firms* and 10 means *State should control firms more effectively*. The explanatory variable *trust* is the answer to question A165 in the WVS.: "Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?". It takes value 1 if the answer was *Most people can be trusted* and 0 otherwise. All other variables are described in the Appendix. The estimation method is the Maximum Likelihood ordered logit model. The log-likelihood at the last and first iteration are shown at the bottom of each column: the pseudo R^2 equals 1 minus the ratio between the two. Odds ratios are presented in square brackets. Robust standard errors clustered by country are presented in parenthesis. Observations are weighted by the product of national sampling weights and country populations. *, ** and *** denote coefficients significantly different from zero at the 90% confidence, 95% confidence and 99% confidence, respectively.

The remaining columns of the table control for some individual characteristics: age, gender, income and schooling. These variables will be also included in all subsequent tables. The main result is that those groups that are traditionally disadvantaged in economic markets (by gender, income and education) prefer higher levels of regulations. In one of the next tables I will also allow the slope coefficient to differ across these groups.

Before doing that, Table 2.3 investigates the robustness of the results to an additional set of control variables that are likely correlated with both trust and preferences for regulation. Column (1) starts with the labor market condition. It turns out that those unemployed are on average more in favor of regulations. The next column distinguishes between (potential) insiders and non-insiders. In line with the descriptive evidence in figure 2.1, bureaucrats and politicians do *not* seem to be more attached to regulations relative to other agents, while entrepreneurs and other self employed individuals are bitterly against. In column (3) I include in the specification measures of trust toward those groups in charge of dictating and enforcing regulations, namely politicians and civil servants. I also include trust toward the judicial system, which may potentially substitute regulation in addressing some types of market failures (reconducible for instance to moral hazard and asymmetric information). None of these variables, however, subtracts explanatory power to *trust*; indeed, they are not even significant in the regression, suggesting that the coefficient of main interest is not capturing the effect of some other cultural traits possibly correlated with trust. This is also true when controlling for political ideology, even though in this case the coefficient of *partisan* is strongly statistically significant and it has the expected sign (individuals leaning toward the right preferring less government intervention). The next column distinguishes individuals according to whether they profess a hierarchical religion or not. After controlling for individual *trust*, hierarchical religion does not seem to affect directly the demand for regulation. In the next section I will rely on this finding to exploit religion as a source of exogenous variation in individual trust across countries. Overall, the coefficient of *trust* remains strongly statistically significant and extremely stable throughout all columns of table 2.3; this conclusion holds true also in the last column, which includes all control variables in the same specification.

Table 2.4 shows how the slope (other than the intercept) of the regression changes with individual and country characteristics. The first two columns distinguish between non-insiders and insiders. It turns out that trust affects only the demand for regulation by the former group. The preferences of the insiders, indeed, could respond more to the private interests emphasized by the public choice literature. The next two columns distinguish individuals according to their educational attainment, the demand by less educated individuals being more responsive to trust. Actually, this segment of the population may be more vulnerable to some types of market failures (like for instance asymmetric information), so that the mechanism proposed in this paper may be more relevant for this category. Finally, the effect of trust does not significantly differ by individual income, age and gender.[11]

[11]These results are not reported but are available upon request

Table 2.3: Individual-level estimates (robustness)

	(1) *unempl*	(2) *insiders*	(3) *trust*	(4) *partisan*	(5) *religion*	(6) *all*
trust	-.126***	-.141***	-.132***	-.124**	-.130***	-.139**
	[0.882]	[0.869]	[0.876]	[0.883]	[0.878]	[0.870]
	(.046)	(.046)	(.047)	(.051)	(.045)	(.057)
unemployed	.174*					.116
	[1.190]					[1.123]
	(.094)					(.305)
self employed		-.479***				-.453***
		[0.620]				[0.636]
		(.088)				(.124)
manager		-.295*				-.250
		[0.745]				[0.779]
		(.176)				(.181)
burpol		.278				.262
		[1.320]				[1.300]
		(.283)				(.266)
trust parliament			-.007			-.074
			[0.993]			[0.929]
			(.057)			(.067)
trust civil servants			-.002			.057**
			[0.998]			[1.059]
			(.033)			(.028)
trust justice			-.035			-.020
			[0.965]			[0.981]
			(.032)			(.038)
partisan				-.141***		-.172***
				[0.868]		[0.842]
				(.025)		(.018)
hierarchical					.004	.047
					[1.004]	[1.048]
					(.057)	(.042)
obs.	31357	22875	29107	25862	31489	17748
countries	32	30	32	32	32	30
log-L	-68924	-50282	-64057	-56648	-69216	-38811
log-L_0	-70616	-51534	-65602	-58409	-70913	-40101
pseudo R^2	.024	.024	.024	.03	.024	.032

Notes: This table presents estimates of the effect of trust on preferences for regulation at the individual level. The dependent variable is the answer to question E042 in the WVS. It takes on discrete values from 1 to 10, where 1 means *State should give more freedom to firms* and 10 means *State should control firms more effectively.* The explanatory variable *trust* is the answer to question A165 in the WVS.: "Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?". It takes value 1 if the answer was *Most people can be trusted* and 0 otherwise. All other variables are described in the Appendix. All regressions include also *age*, age^2, *female*, *high income*, *low income*, *high schooling*, *low schooling* and country fixed effects. The estimation method is the Maximum Likelihood ordered logit model. The log-likelihood at the last and first iteration are shown at the bottom of each column: the pseudo R^2 equals 1 minus the ratio between the two. Odds ratios are presented in square brackets. Robust standard errors clustered by country are presented in parenthesis. Observations are weighted by the product of national sampling weights and country populations. *, ** and *** denote coefficients significantly different from zero at the 90% confidence, 95% confidence and 99% confidence, respectively.

Table 2.4: Individual-level estimates (sample splits)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	INSIDERS		SCHOOLING		COUNTRIES		GERMANY	
	no	*yes*	*low*	*high*	*Eastern*	*Western*	*Eastern*	*Western*
trust	-.132***	-.063	-.228***	.009	-.231**	-.085***	-.450***	-.098
	[0.877]	[0.939]	[0.796]	[1.009]	[0.794]	[0.919]	[0.637]	[0.907]
	(.043)	(.100)	(.077)	(.065)	(.112)	(.027)	(.159)	(.145)
obs.	28370	3013	11122	6185	12046	19443	695	715
countries	32	32	32	32	11	21		
log-L	-62398	-6433	-24189	-13681	-26041	-42801	-1569	-1549
log-L_0	-63907	-6658	-24938	-13899	-26578	-43944	-1582	-1561
pseudo R^2	.024	.034	.03	.016	.02	.026	.008	.007

Notes: This table presents estimates of the effect of trust on preferences for regulation at the individual level. The dependent variable is the answer to question E042 in the WVS. It takes on discrete values from 1 to 10, where 1 means *State should give more freedom to firms* and 10 means *State should control firms more effectively.* The explanatory variable *trust* is the answer to question A165 in the WVS.: "Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?". It takes value 1 if the answer was *Most people can be trusted* and 0 otherwise. Different columns refer to different subsamples, indicated on top of each column. The category INSIDERS includes entrepreneurs, managers, bureaucrats and politicians; the categories for schooling refer to the binary variable classifications. All other variables are described in the Appendix. All regressions include also *age*, age^2, $female$, *high income*, *low income*, *high schooling*, *low schooling* and country fixed effects. The estimation method is the Maximum Likelihood ordered logit model. The log-likelihood at the last and first iteration are shown at the bottom of each column: the pseudo R^2 equals 1 minus the ratio between the two. Odds ratios are presented in square brackets. Robust standard errors clustered by country are presented in parenthesis. Observations are weighted by the product of national sampling weights and country populations. *, ** and *** denote coefficients significantly different from zero at the 90% confidence, 95% confidence and 99% confidence, respectively.

The last columns of the table start distinguishing the effect of trust according to country (rather than individual) characteristics. Intuitively, the effect of trust should be stronger in countries in which the costs of market externalities are higher relative to the benefits of economic activity. This is captured in the model by the term π^{-1} in equation (2.3), which increases (in absolute value) the slope of *trust*. Even though an exact empirical counterpart for this parameter is not easily available, Djankov et al. (2002) suggest that "*Market failures are likely to be both more pervasive and severe in poor countries than in rich ones*". This may be due for instance, to the fact than less developed countries have backward and more polluting technologies. Therefore, in columns (5) and (6) I compare the effect of trust separately in Eastern and Western European countries, respectively. In line with the discussion above, the effect is stronger in the former group of countries; in particular, the ratio between the odds of demanding more regulation for an untrustful individual over those of a trustful one is twice as much in Eastern than in Western countries.

On the other hand, the quality of government intervention may be worse too in less developed countries (for instance because of more widespread corruption), which in turn would discourage (untrustful) individuals from demanding more regulations. This last effect is captured in the model by the term θ in equation (2.3), a higher θ (indicating a lower quality of institutions) decreasing the coefficient of trust. To disentangle the effect of economic development from that of institutional quality, the last two columns of the table present separate regressions for Eastern and Western Germany, respectively. Still one decade after the reunification, in fact, the two areas were characterized by very different levels of economic development; on the other hand, they shared the same formal institutions, which in turn allows to isolate the effect of economic and institutional factors. As expected, keeping institutional quality constant further increases the differential effect of trust in less developed regions.

Table 2.5: Individual-country interactions

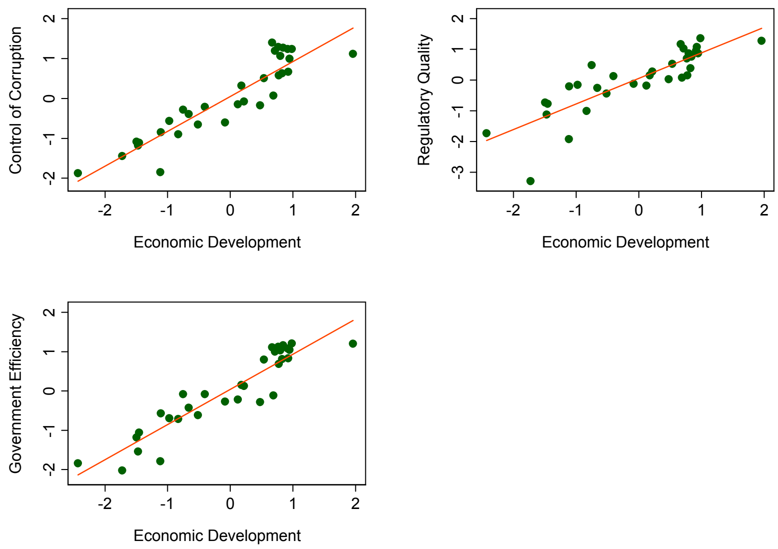
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
trust	-.111**	-.115***	-.114***	-.114***	-.120***	-.135***	-.125***
	[0.895]	[0.892]	[0.892]	[0.892]	[0.887]	[0.874]	[0.883]
	(.046)	(.044)	(.038)	(.043)	(.030)	(.031)	(.028)
trust × *ECONDEV*	.115*				.290***	.284***	.322***
	[1.122]				[1.337]	[1.329]	[1.379]
	(.060)				(.102)	(.094)	(.098)
trust × *CORRCTR*		.067			-.170**		
		[1.069]			[0.843]		
		(.059)			(.073)		
trust × *REGQUAL*			.052			-.173***	
			[1.054]			[0.842]	
			(.054)			(.064)	
trust × *GOVEFF*				.065			-.203***
				[1.068]			[0.816]
				(.058)			(.072)
obs.	31489	31489	31489	31489	31489	31489	31489
countries	32	32	32	32	32	32	32
log-L	-69201	-69209	-69212	-69210	-69194	-69191	-69192
log-L_0	-70913	-70913	-70913	-70913	-70913	-70913	-70913
pseudo R^2	.024	.024	.024	.024	.024	.024	.024

Notes: This table presents estimates of the effect of trust on preferences for regulation at the individual level. The dependent variable is the answer to question E042 in the WVS. It takes on discrete values from 1 to 10, where 1 means *State should give more freedom to firms* and 10 means *State should control firms more effectively*. The explanatory variable *trust* is the answer to question A165 in the WVS.: "Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?". It takes value 1 if the answer was *Most people can be trusted* and 0 otherwise. The other variables are interactions of *trust* with country characteristics: *DEVEL* is the level of development, as measured by the (log of) GDP per capita at constant 2005 international dollars; *CORRCTR*, *REGQUAL* and *GOVEFF* are the World Bank Governance Indicators for the control of corruption, the regulatory quality and the government effectiveness. All country variables are standardized to have mean 0 and standard deviation equal to 1. All regressions include also *age*, age^2, *female*, *high income*, *low income*, *high schooling*, *low schooling* and country fixed effects. The estimation method is the Maximum Likelihood ordered logit model. The log-likelihood at the last and first iteration are shown at the bottom of each column: the pseudo R^2 equals 1 minus the ratio between the two. Odds ratios are presented in square brackets. Robust standard errors clustered by country are presented in parenthesis. Observations are weighted by the product of national sampling weights and country populations. *, ** and *** denote coefficients significantly different from zero at the 90% confidence, 95% confidence and 99% confidence, respectively.

The difference-in-difference specification in table 2.5 takes a more systematic approach by interacting individual trust with country characteristics possibly correlated with the risk and the severity of market failures and with the effectiveness and efficiency of regulatory responses. The first of these measures is the (log of) real GDP per capita in year 2000 (at constant 2005 PPP International Dollars), which proxies for the level of economic development. Institutional characteristics are measured by indexes of corruption, regulatory quality and government effectiveness provided by the last release of the World Bank Governance Indicators (Kaufmann et al., 2008). All indexes are increasing in institutional quality and they have been rescaled to have zero mean and standard deviation equal to 1 (the same is true also for the index of economic development), so that the odds ratio of the interaction term may be read as the ratio between the odds of preferring more regulations for a trustful individual that lives in a country that has a (one standard deviation) higher level of economic and/or institutional development, and the same odds for a trustful individual living in the average country.

The interaction of individual trust with economic development (column 1) is only weakly statistically significant, and the interactions with each of the institutional variables are not significant. This is due to the fact that the three institutional indexes are positively and strongly correlated with the level of economic development, as show in figure 2.3; at the same time, economic development and institutional quality affect the slope coefficient of trust in opposite directions.

Figure 2.4: Economic development and the quality of regulations



This graph shows the cross country correlation between several measures of institutional quality. The index of economic development is the standardized logarithm of real GDP per capita in year 2000 (at constant 2005 PPP International Dollars). The indexes of Control of Corruption, Regulatory Quality and Government Efficiency are from Kaufmann et al. (2008).

In fact, interacting both economic and institutional factors with trust into the same specification raises both the magnitude and the statistical significance of their effects (columns 5 to 8). In particular, the effect of trust seems stronger in less developed countries, and in countries with relatively less corrupt public officials, better regulatory quality and more efficient governments. Consistently with the previous comparison between Eastern and Western Europe, the effect of economic development dominates that of institutional quality, the coefficient of the former being twice as much that of the latter. This means that the greater risks and greater severity of market failures in less developed countries more than compensate for the lower quality of government responses in driving a higher demand of regulation by untrustful individuals.

2.4 Cross country evidence

The results presented above suggest that, within each country, trust is a significant determinant of individual preferences for regulation. In this section I investigate whether this relationship carries over across countries and its implications for the cross country pattern of regulations, externalities and the size of unofficial activity.

2.4.1 Culture and regulation

At the aggregate, cross country level, the first prediction of the model is that regulations depend negatively on (median) trust. The empirical counterpart of condition (2.5) is

$$R = a - b\hat{\tau}_m + \upsilon, \tag{2.11}$$

where b is the coefficient to be estimated and υ is an error term.

The cross country measures of regulations and trust were already introduced in Section 2.1; they are the log number of entry procedures from Djankov et al. (2002) and the average level of trust (as measured by the WVS), respectively. The results of OLS estimates on equation (2.11) are presented in table 2.6. The first column shows the univariate regression of regulations on trust. A one percentage point increase in trust is associated on average with a 2 percent reduction in the number of procedures, this effect being very precisely estimated. Controlling for the level of economic developments, as proxied by the log of GDP per capita in year 1999, weakens only slightly the effect of trust (column 2).[12]. In column 3 I add the log of total population in the same year and a dummy variable equal to 1 for countries of British legal origin. The inclusion of these variables in the specification is motivated by the fact that the creation of new institutions entails significant fixed costs and is therefore limited by the size of the market and the level of transaction costs (Demsetz, 1967). Mulligan and Shleifer (2005) provide evidence consistent with this theory for the specific case of regulatory institutions using population to measure the size of the market and British legal origin as a proxy for (higher) transaction costs.[13] In my sample too, regulations increase with population and are less pervasive in countries with British legal origin. Most importantly, both variables provide sources of exogenous variation in

[12] The data for all control variables also come from Djankov et al. (2002)

[13] With respect to the second issue, their argument is that, historically, the cost of incremental regulations was lower in France than in England thanks to the pervasive administrative state introduced after the Revolution. As legal and regulatory frameworks have spread through conquest and colonization, so did the cost structures of incremental regulations.

Table 2.6: Trust and regulations

	OLS (1)	OLS (2)	OLS (3)	TSLS (4)	TSLS (5)
$TRUST$	-2.305***	-1.896***	-1.324***	-1.387***	-1.382***
	(.423)	(.454)	(.309)	(.438)	(.436)
$\ln GDP$		-.087*	-.080***	-.077***	-.077***
		(.050)	(.023)	(.030)	(.030)
$\ln POP$			.131***	.133***	.133***
			(.020)	(.019)	(.019)
$UK\ LEGAL$			-.825***	-.818***	-.818***
			(.103)	(.091)	(.091)
obs.	52	52	51	50	50
R^2	.4	.433	.816	.815	.815
F	29.715	18.376	36.203	37.016	36.986
FIRST STAGE REGRESSION FOR TRUST					
$CATHOL$				-.217***	
				(.042)	
$MUSLIM$				-.209***	
				(.083)	
$HIER$					-.216***
					(.040)
F (excl. instr.)				14.14	28.91
J				.463	
J (p-value)				.496	

Notes: This table presents OLS and TSLS estimates of the effect of trust on entry regulation across countries. The top and bottom panel report first and second stage results, respectively. The dependent variable is the (log of) number of procedures required to start a new business, from Djankov et al. (2002). The explanatory variable $TRUST$ is the country average of the measure of trust in the WVS. $\ln GDP$ and $\ln POP$ are the (log of) country GDP per capita and population in year 1999, and $UK\ LEGAL$ is a dummy equal to one for British legal origin; all three variables are also from Djankov et al. (2002). The first stage instruments $CATHOL$ and $MUSLIM$ equal the fraction of people professing Catholic and Muslim religion, respectively, in year 2000 (McCleary and Barro, 2006); $HIER$ is the sum of the two variables. Robust standard errors are presented (in parenthesis). The first stage F statistic for the excluded instruments and the Hansen J statistic are reported on bottom of each column. *, ** and *** denote coefficients significantly different from zero at the 90% confidence, 95% confidence and 99% confidence, respectively.

regulations, which will prove extremely useful to investigate the effects of regulations across countries.[14]

2.4.2 Instrumental variable estimates

In principle, one can not rule out reverse causality. For instance, burdensome regulation could affect average honesty by increasing the incentives for predatory practices and corruption, which would in turn impact (negatively) on average trust because of equation

[14]Including on the right hand side measures of education, democracy and ethno-linguistic fractionalization does not significantly affect the results, in line with the findings of Aghion et al. (2009).

(2.6). Culture could in fact respond endogenously to incentives because of evolutionary forces (Hirshleifer, 1984; Frank, 1987), rational choice (Benabou and Tirole, 2006) and intergenerational transmission of moral values (Bisin et al., 2004; Tabellini, 2009). For this reason, in the last two columns $TRUST$ is instrumented by the share of population professing either Catholic or Muslim religion, as reported by McCleary and Barro (2006). Seminal work by Putnam (1993) and La Porta et al. (1997) suggests in fact that hierarchical religions, namely Catholic and Muslim, are associated with lower trust toward the others relative, for instance, to Protestantism; the individual-level evidence presented in Guiso et al. (2003) is broadly consistent with this picture. Since (i) country religions are inherited from the ancient past and (ii) individual level estimates presented in the previous section exclude that religion affects directly the demand for regulation (after controlling for trust), I can use religion as a source of exogenous variation in trust, analogously to what is done by La Porta et al. (1997).

The results of first stage estimates confirm that, in my sample too, a higher fraction of the population professing a hierarchical religion brings a lower level of trust toward the others (column 4). The average effect is identical across different hierarchical religions (namely Catholic and Muslim), so that I group them into a single variable in column (5).

Second stage estimates show that the coefficient of trust remains negative and statistically significant. Actually, the TSLS coefficient is greater in absolute value than the OLS one; this would mean that, if there is a feedback effect from regulations to trust, this should be positive. According to these estimates, a standard deviation increase in the percentage of trustful individuals within the population (equal to 15.3 percent) cuts the red tape by about 20 percent.

2.4.3 The effects of regulations

Turning to examine the effects of regulations, the theoretical model predicts that heavier regulations and lower trustworthiness increase the size of the shadow economy across countries. The empirical counterpart of equation (2.7) is

$$S = \alpha - \delta\tau + \mu R + \nu, \tag{2.12}$$

where δ and μ are positive coefficients to be estimated and ν is an error term independently distributed from υ. Excluding τ from the specification introduces an (asymptotic) bias in the OLS estimated coefficient $\hat{\mu}$ equal to $plim(\hat{\mu} - \mu) = -\delta Cov(R, \tau)/Var(R)$. After plugging equation (2.11) for R, the resulting bias is proportional to the covariance between average trust and trustworthiness

$$BIAS = \delta \cdot \frac{b \cdot Cov(\hat{\tau}_m, \tau)}{Var(R)}. \tag{2.13}$$

Unfortunately, it is not possible to explicitly control for τ into equation (2.13) because, as discussed in Section 2.1, reliable measures of trustworthiness are not easily available across countries. However, notice that the bias is different from 0 only insofar as the covariance between trust and trustworthiness is greater than 0; but whenever the latter is true, average trust provides a proxy for average trustworthiness. In particular, the greater this covariance, the greater the bias and the better the extent to which trust approximates trustworthiness. As discussed before, there are several theoretical arguments and extensive empirical evidence suggesting that trust and trustworthiness are intimately related.

Table 2.7: Regulations and unofficial activity

	OLS (1)	OLS (2)	OLS (3)	OLS (4)	TSLS (5)	TSLS (6)
ENTRY	14.522***	1.797	5.328**	-.114	2.257	1.075
	(2.454)	(2.591)	(2.356)	(2.188)	(3.836)	(2.742)
TRUST		-44.208***		-17.177	-37.425*	-35.321**
		(12.546)		(12.142)	(20.357)	(16.973)
$\ln GDP$			-6.344***	-5.859***		-4.312***
			(.999)	(1.147)		(1.439)
obs.	74	51	74	51	49	49
R^2	.254	.385	.563	.663	.409	.633
F	35.02	22.293	45.391	31.59	4.976	28.332
			FIRST STAGE REGRESSION FOR ENTRY			
$\ln POP$					.195***	.161***
					(.036)	(.028)
UK LEGAL					-.967***	-.947***
					(.125)	(.095)
HIER					.278*	.241**
					(.145)	(.111)
F (excl. instr.)					27.64	41.03
			FIRST STAGE REGRESSION FOR TRUST			
$\ln POP$					-.027**	-.014
					(.013)	(.009)
UK LEGAL					.091**	.083***
					(.044)	(.031)
HIER					-.202***	-.188***
					(.051)	(.036)
F (excl. instr.)					8.76	13.18
J					.36	1.553
J (p-value)					.85	.213

Notes: This table presents OLS and TSLS estimates of the effect of entry regulations on unofficial activity across countries. The top and bottom panel report first and second stage results, respectively. The dependent variable is the size of the shadow economy as a percentage of GDP, from Djankov et al. (2002). The explanatory variable *ENTRY* is the (log of) number of procedures required to start a new business; $\ln GDP$ and $\ln POP$ are the (log of) country GDP per capita and population in year 1999; *UK LEGAL* is a dummy equal to one for British legal origin; all four variables are also from Djankov et al. (2002). *TRUST* is the country average of the measure of trust in the WVS. *HIER* is the fraction of people professing a hierarchical religion (Catholic and Muslim) in year 2000, from McCleary and Barro (2006). Robust standard errors are presented (in parenthesis). The first stage F statistic for the excluded instruments and the Hansen J statistic for the over-identifying conditions are reported. *, ** and *** denote coefficients significantly different from zero at the 90% confidence, 95% confidence and 99% confidence, respectively.

Table 2.7 presents the estimates of equation (2.12). The first column replicates the regression in table IV of Djankov et al. (2002). The dependent variable is the size of the unofficial sector (as a percentage of GDP) and the explanatory variable is the measure of entry regulations. According to this univariate regression, one standard deviation increase in entry regulations increases the size of the shadow economy by half standard deviation, the estimated coefficient being very statistically significant. However, after controlling for $TRUST$, the effect of regulations is not significantly different from 0 (column 2). These estimates suggest that the effect of regulations was also capturing omitted variation in cultural traits, as proxied by average trust. The relative magnitude of the effect of regulations in column 1 and of trust in column 2 are also identical: one standard deviation increase in the explanatory variable implies about an half standard deviation decrease in the size of the shadow economy. One may wonder whether the difference between the results in column (1) and (2) lies in the sample, due to the fact that data on trust are missing for almost one third of the countries. However, this is not the case; re-estimating the univariate regression in column (1) on the reduced sample available in column (2) leads a point estimate of 9.76, statistically significant at the 99% level (standard error 1.60).

Djankov et al. (2002) also present one further specification in which they include on the right hand side the level of country development, as measured by the log of GDP per capita, arguing that this variable controls for the risk and severity of market failures. In column (3) I replicate this specification, thus dropping average trust. Once I do that, the coefficient of regulation is again strongly statistically significant (even though smaller in magnitude relative to the univariate regression in column 1). When I plug back average trust into the equation (column 4), its coefficient is not statistically significant at the conventional 10% confidence level. This is probably due to the fact that trust and GDP are strongly correlated with each other (see table 2.9); in particular, both variables proxy for the propensity of individuals and firms to hide into the unofficial sector. Still, keeping average trust constant across countries is important for correctly evaluating the effects of regulations; in fact, the coefficient of $ENTRY$ drops from 5.3 to zero when moving from column (3) to column (4).

Finally, in the last two columns of the table I examine the causal impact of both regulations and culture taking a TSLS approach. In particular, I use population and legal origin as instruments for regulation. According to the results in table 2.6 and to those presented in Mulligan and Shleifer (2005), in fact, both factors are significant determinants of the sphere of government activity. In addition, after controlling for regulation, the size of the country and the type of legal origin are most likely exogenous (and certainly predetermined) to the level of informal activities observed in the 1990s. As for trust, I adopt the same approach as in the previous table, using the cross country diffusion of hierarchical religions as a source of exogenous variation in average trust. First stage estimates, reported in the bottom panels of table 2.7, confirm that the three instruments fit well the actual variation in both regulation and trust across countries. Moreover, the Hansen J over identifying restrictions test can not reject that they affect the size of unofficial activity only through variation in regulation and average trust. Turning to second stage estimates, the TSLS coefficient of $ENTRY$ is much lower than the OLS one and it is not statistically significant, regardless of whether GDP is included or not in the regression. By contrast, average trust has a negative causal effect on unofficial activity and, even though the two-stage approach makes the estimate somewhat more noisy, its coefficient remains statistically significant at conventional confidence level.

Table 2.8 estimates the effect of regulations on another outcome considered by Djankov et al. (2002), namely the level water pollution, which is a proxy for the negative externalities in the economy. In this case too, Djankov et al. (2002) estimate a positive coefficient on entry regulations, and they interpret this as evidence against public interest theories of regulations. Once again, however, this result disappears after controlling for the effect of culture; actually, the coefficient becomes negative and strongly statistically significant, this result being confirmed also in TSLS regressions.

2.5 Conclusions

Regulation is often blamed for being both ineffective and inefficient; however, people seem reluctant to abandon it. This paper offers a view that may potentially reconcile these two facts. The main insight is that, far from being exogenously determined, the actual level of regulation is an equilibrium outcome. In particular, stringent regulations may be enacted (at least in part) in response to market failure originating in (lack of) attitudes toward cooperation. Then, these attitudes may drive part of the correlation existing between the level of regulation and several economic outcomes, confounding inference about causality.

I addressed these issues by explicitly controlling for such omitted factors. Actually, controlling for trust leads to reconsider the effect of regulation on the size of the unofficial economy and the level of negative externalities. Of course, these results do not exclude the possibility that regulations may be a very inefficient solution to market failures. They suggest, however, that in order to make liberalization and deregulation politically appealing it might be necessary to foster and improve alternative institutions like, for instance, private litigation and collective class action.

Table 2.8: Regulations and negative externalities

	OLS (1)	OLS (2)	OLS (3)	OLS (4)	TSLS (5)	TSLS (6)
$ENTRY$	.013	-.018***	-.004	-.021***	-.019**	-.022**
	(.008)	(.007)	(.007)	(.007)	(.009)	(.009)
$TRUST$		-.066**		-.047	-.070	-.083*
		(.026)		(.031)	(.049)	(.047)
$\ln GDP$			-.013***	-.006**		-.004
			(.003)	(.003)		(.004)
obs.	77	50	77	50	49	49
R^2	.028	.095	.234	.156	.115	.151
F	2.667	4.408	12.409	4.588	1.987	2.705
FIRST STAGE REGRESSION FOR ENTRY						
$\ln POP$					.183***	.139***
					(.033)	(.029)
$UK\ LEGAL$					-.922***	-.901***
					(.122)	(.100)
$HIER$					.351**	.309***
					(.141)	(.116)
F (excl. instr.)					28.37	34.58
FIRST STAGE REGRESSION FOR TRUST						
$\ln POP$					-.019	-.003
					(.012)	(.010)
$UK\ LEGAL$					.069	.061*
					(.044)	(.035)
$HIER$					-.235***	-.219***
					(.051)	(.041)
F (excl. instr.)					8.92	11.46
J					.744	1.473
J (p-value)					.389	.225

Notes: This table presents OLS and TSLS estimates of the effect of entry regulations on negative externalities across countries. The top and bottom panel report first and second stage results, respectively. The dependent variable are emissions of organic water pollutant (kilograms per day per worker) for 1998, from Djankov et al. (2002). The explanatory variable $ENTRY$ is the (log of) number of procedures required to start a new business; $\ln GDP$ and $\ln POP$ are the (log of) country GDP per capita and population in year 1999; $UK\ LEGAL$ is a dummy equal to one for British legal origin; all four variables are also from Djankov et al. (2002). $TRUST$ is the country average of the measure of trust in the WVS. $HIER$ is the fraction of people professing a hierarchical religion (Catholic and Muslim) in year 2000, from McCleary and Barro (2006). Robust standard errors are presented (in parenthesis). The first stage F statistic for the excluded instruments and the Hansen J statistic are reported on bottom of each column. *, ** and *** denote coefficients significantly different from zero at the 90% confidence, 95% confidence and 99% confidence, respectively.

Appendix

Individual-level variables:

dependent variable: answer to question "How would you place your views on this scale? 1 means you agree completely with the statement *The state should give more freedom to firms*; 10 means you agree completely with the statement *The state should control*; and if your views fall somewhere in between, you can choose any number in between." Source: WVS, variable E042.

trust: answer to question "Generally speaking, would you say that most people can be trusted?". The variable takes value 1 if the individual answered *Most people can be trusted*, and 0 if the individual answered *Can't be too careful in dealing with people*. Source: WVS, variable A165.

high/low income: binary variables indicating the top and bottom category of a three-value index recoding household income on a country basis. Source: WVS, variable X047R.

high/low schooling: binary variables indicating the top and bottom category of a three-value index recoding individual education on a country basis. Source: WVS, variable X025R.

unemployment: binary variable indicating whether the individual is unemployed. Source: WVS, variable X028.

self employment: binary variable indicating whether the individual is self employed. Source: WVS, variable X028.

manager: binary variable indicating whether the individual is a manager. Source: WVS, variable X035 (2-digit profession classification).

burpol: binary variable indicating whether the individual is a politician and/or a high-ranking public official. Source: WVS, variable X035 (2-digit profession classification).

trust parliament, civil servants, justice: binary variables indicating whether the individual trusts each institution. Source: WVS, variables E075, E076, E085.

partisan: 10-category index of individual political ideology, where 1 is extreme left and 10 is extreme right. Source: WVS, variable E033.

hierarchical: binary variable equal to 1 if the individual belongs to either Catholic or Muslim religion. Source WVS, variable F025.

Cross-country variables:

ENTRY: The number of different procedures that a start-up has to comply with in order to obtain a legal status. Source: Djankov et al. (2002)

TRUST: percentage of respondents who answer that *Most people can be trusted* to the question "Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?". Source: WVS (II and III waves), variable A165.

Unofficial activity: Size of the informal economy as a percentage of GDP. Source: Djankov et al. (2002).

Water pollution: Emissions of organic water pollutants (kilograms per day per worker) in year 1998. Source: Djankov et al. (2002).

$\ln GDP$: log of gross domestic product per capita in current US dollars in 1999. Source: Djankov et al. (2002)

$\ln POP$: log of total country population in 1999. Source: Djankov et al. (2002)

UK LEGAL: binary variable indicating British legal origin. Source: Djankov et al. (2002)

HIER: Percentage of people in the country professing either Catholic or Muslim religion. Source: McCleary and Barro (2006).

Table 2.9: Correlation matrix and summary statistics

				Correlation matrix				
	ENTRY	*TRUST*	*Unoff. Act.*	*Water. Poll.*	$\ln GDP$	$\ln POP$	*LEGOR UK*	*HIER*
TRUST	-0.671							
Unoff. Act.	0.463	-0.648						
Water. Poll.	-0.097	-0.142	0.343					
$\ln GDP$	-0.481	0.629	-0.779	-0.273				
$\ln POP$	0.347	-0.233	0.166	-0.130	-0.257			
LEGOR UK	-0.614	0.250	-0.184	0.069	-0.028	0.234		
HIER	0.293	-0.538	0.327	0.084	-0.100	0.001	-0.144	
				Summary statistics				
obs.	86	52	74	77	86	198	86	190
mean	2.229	0.307	28.727	0.183	7.971	15.336	0.291	0.524
std. dev.	0.517	0.153	15.267	0.041	1.641	2.088	0.457	0.342
min.	0.693	0.046	8.600	0.100	5.247	10.652	0	0.002
max.	3.045	0.652	68.800	0.315	10.555	20.949	1	0.993

Notes: This table presents the correlation matrix of variables (top panel) and their summary statistics (bottom panel) across countries.

Chapter 3

Financial Development and Pay-As-You-Go Social Security

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Both pay-as-you-go social security and financial markets allow individuals to smooth consumption over their life cycle. Pay-as-you-go social security (PAYG henceforth) does it through a system of compulsory inter-generational transfers. The financial system, instead, channels private savings to productive investments that deliver a return once the individual has retired. It is therefore to be expected that there should be a relationship between social security and financial development.

In fact, previous work has shown that social security taxes lower the demand for financial assets by displacing private savings (see, among others, Feldstein, 1974, 1980; Kotlikoff, 1979; Modigliani and Sterling, 1981; Attanasio and Brugiavini, 2003). On the other hand, social security can hardly be considered an exogenous determinant of savings. More likely, the two are jointly determined since political support for public pensions should reflect alternative consumption smoothing options, which in turn depend, negatively, on the extent of financial frictions present in the economy.

In this paper I empirically explore this possibility by using legal origin as a proxy for frictions that hinder financial development. The finance literature suggests in fact that Common Law legal codes foster financial markets' efficiency relative to Civil Law systems. In particular, the British legal tradition seems to enhance effective property rights' protection (Mahoney, 2001; Claessens and Laeven, 2003), efficiency and flexibility at the procedural level (Djankov et al., 2003; Acemoglu and Johnson, 2005) and transparency of accounting standards and disclosure requirements (Rajan and Zingales, 1998; La Porta et al., 2006).[1]

The results of Ordinary Least Squares regressions yield a statistically significant, negative effect of legal-origin-driven differences in financial frictions on social security. This

[1]For a useful taxonomy, see Beck et al. (2003).

result is robust to controlling for other determinants of social security discussed in the literature. Then, I adopt a Two-Stage Least Squares approach to examine the channel through which legal origin impacts on social security. In particular, I will use legal origin as an instrument for total finance (both debt and equity) to estimate the causal effect of the latter on social security contributions.[2] The results of this exercise suggest that, indeed, financial frictions affect the social security budget through their effect on financial market development.

This paper is related to the empirical literature on the determinants of cross-country differences in the size of social security programs. Mulligan and Sala-i-Martin (1999) stress the role of the political demand for inter-generational redistribution by the older generations, while Persson and Tabellini (2003) focus instead on the importance of political institutions for the supply of public expenditure; they argue, in particular, that majoritarian and presidential political systems reduce the incentives for politicians to provide general (as opposed to local) transfer programs like social security. Mulligan et al. (2004) compare instead democracies and non-democracies, finding however only minor differences between their social security programs.[3] I contribute to this literature by examining the role of financial frictions and financial market development. The paper that comes closest to my work is Perotti and Schwienbacher (2007), who relate the choice between funded versus unfunded pension systems to differences in political preferences over the role of financial markets. They argue that such preferences were shaped, in turn, by shocks to the wealth distribution of voters (as proxied by the magnitude of inflationary shocks experienced during the 20th century). The present paper points instead at differences in the functioning of financial markets stemming from differences in legal origin.

My results suggest that the elasticity of social security to total finance (both expressed in terms of GDP) is between 1 and 2. During the period I considered, the ratio of total finance over GDP averaged 151 percent in Civil Law countries (France, Germany and Sweden) against 264 percent in UK and the US; social security taxes over GDP were 15.3 and 6.3 percent, respectively. Therefore, differences in financial development may potentially explain most of the difference between the two groups of countries in terms of social security arrangements.

The remainder of the paper is structured as follows. The next section discusses the importance of the relationship between financial markets and public pensions. Section 3.2 presents the estimating equations, the variables and the data sources. Section 3.3 comments on the empirical results. Section 3.4 concludes.

[2]The origin of legal codes is seen to be historically predetermined and therefore unaffected by recent economic developments (La Porta et al., 1998; Acemoglu and Johnson, 2005). It is for these reasons that legal origin has been used in a wide array of studies as an instrument for estimating the effect of financial development on income and growth (e.g Beck et al., 2000; Levine et al., 2000; Alfaro et al., 2004; Claessens and Laeven, 2005)

[3]Further evidence about the determinants of the social security budget is presented in Lindert (1996), Perotti (1996), Breyer and Craig (1997); for a review, see Galasso and Profeta (2002).

3.1 Historical perspectives and current debates

Social security was introduced in the US within the New Deal launched to mitigate the Great Depression. The original 1935 scheme was a two pillar system. On the one hand, the Old Age Insurance (OAI) aimed at creating a system of contributory, fully funded old age insurance. On the other hand, the Old Age Assistance (OAA) consisted of cash payments to destitute aged and it was specifically meant to provide income for a whole generation that lost most of its savings in the stock market crash of the 1929.[4] This historical episode, while extreme, is nevertheless suggestive that part of the demand for PAYG social security programs may be a consequence of financial underdevelopment.

Interest for the substitutability between social security and financial markets revived during the 1990s, as stock markets boomed right at the same time as most of the pension systems in developed economies were at the verge of collapse. Then, many advocates of social security reform argued that the growth in size and sophistication of financial markets around the world could provide an important opportunity for channeling increasing flows of savings toward long term investments aimed at the creation of personal accounts; see, for instance, the contributions collected in Feldstein (2000) and Campbell and Feldstein (2001). Actually, reforms of the US social security along these lines were on top of the political agenda during both mandates of Bush administration.[5]

In addition to relieving distressed public finances, social security reform is often considered a unique opportunity for boosting financial markets participation. This view is maintained especially by international financial institutions, which strongly support social security reform as a means toward financial development (The World Bank, 1994; Demirguc-Kunt and Schwartz, 1999; Walker and Lefort, 2002; Holzmann and Hinz, 2005).[6]

Notwithstanding these trends, people (and voters) seem so far reluctant to opt out of social security in favor of financial investment of publicly administered (possibly mandatory) private savings. Of course this may reflect, to some extent, status quo bias, procrastination and selfishness (Boeri et al., 2001, 2002). However, there appears to be something more.

Some recent opinion polls conducted by Bowman (2005) among US citizens show that most of them are aware of the unsustainability of the current system. Actually, more than 50% of those not retired do not expect to receive any benefit at all (which considerably weakens the importance of procrastination and selfishness, at least for a large fraction of the population). Yet, when the discussion comes to the possibility of transferring part of the social security taxes into personal savings accounts and to invest them in stocks, the evidence is mixed. About 60% of the interviewed seems to favour the idea, at least in principle. On the other hand, the percentage decreases dramatically, turning the supporters of

[4]Actually, it was clear from the very beginning that the OAI would have not been able to accumulate an adequate trust fund, so that in practice even this first pillar ran as an unbalanced transfer scheme. See Miron and Weil (1997) for a throughout description of the creation and evolution of the US social security program.

[5]See, in particular, chapter 6 of the Economic Report of the President 2004.

[6]For a countervailing view, see Orszag and Stiglitz (2001).

reform into minority, when the possibility of fluctuations in stock returns is mentioned in the question. Barabas (2006) provides more systematic time-series evidence, for the US, about the existence of a robust relationship between stock market performance and public support to personal savings accounts. When stocks rise, so does public support for social security reform; the opposite happens during periods of financial turmoil. Analogous data are not available for other OECD countries, but Casey (2003) documents important effects of financial scandals on opinions about PAYG pensions in several countries.

Yet, apart from these suggestive evidence, no systematic empirical analysis of the effect of cross-country differences in financial development on social security is available in the literature; this is precisely the purpose of the next sections of this paper.

3.2 Empirical framework and data

The hypothesis that I am interested in testing is that lower financial development due to frictions translates into larger social security programs. This requires a measure of the frictions that slow down financial development. Following the law and finance literature, I will use legal origin as a proxy of financial frictions. Therefore, the system of estimating equations is

$$FINANCE = \theta LEGAL + \mu SOCSEC + \delta Z^F + \upsilon, \quad (3.1)$$

$$SOCSEC = -\beta FINANCE + \gamma Z^S + \varepsilon, \quad (3.2)$$

where Z^F and Z^S are vectors of observable variables and υ and ε are additional unobservable factors affecting financial development and social security, respectively.

I will first estimate, through Ordinary Least Squares, the reduced form relationship between social security and legal origin obtained by combining equations (3.1) and (3.2). Then, I will move to consider whether the channel through which legal origin affects social security is actually the one suggested in this paper. In order to do so, I will estimate the system of equations (3.1)-(3.2) by Two Stage Least Squares, regressing financial market development on legal origin in the first stage (equation (3.1)) and using the predicted $FINANCE$ as a measure of the legal-origin-driven component of financial development in the second stage (equation (3.2)).

Turning to data, the dependent variable is (the log of) social security taxes in percentage of GDP, as reported by the IMF in its Government Finance Statistics. Availability of these data determines the sample, which reaches a maximum of 54 countries in 1997. Working with cross country averages for the period 1990-2000 expands the sample to 65 data points.[7]

[7] Tabellini (2000) argues that a measure of social security based on benefits captures the extent of PAYG transfer schemes better than one based on contributions. I will then check the robustness of results to using social security expenditures ($SOCSECEXP$) as the dependent variable. The drawback of this alternative measure is that it does not distinguish social security from other welfare expenditures, including "assistance delivered to clients or groups with special needs" (IMF - Government Finance Statistics Yearbook).

As for the main explanatory variable, $FINANCE$ is (the log of) total finance in percentage of GDP. Total finance consists of domestic credit to private firms plus stock market capitalization; the source is the World Development Indicators of the World Bank. Finally, binary indicators will distinguish between $COMMON$ law and $CIVIL$ law legal origins or, alternatively, between $COMMON$ law and different types of $CIVIL$ law (namely $FRENCH$, $GERMAN$ and $SCANDINAVIAN$ law). In either case, formerly Soviet Union countries entering my sample will provide the control group captured by the intercept.[8]

In order to reduce the scope for omitted variable bias, regressions will control for the effect of several variables possibly correlated with both social security and the instrument for financial development. The first candidate is income, as measured by the log of GDP per capita at purchasing power parity international dollars (GDP). Second, I will include total government revenues ($TOTREV$) in order to capture differences in the structure of public budgets descending from different preferences over alternative social insurance arrangements (which in principle could also be correlated with legal origin). For the same reason, I will control also for the type of electoral institutions and the political regime, which according to an extensive literature are correlated with both public expenditure and financial development (see Persson and Tabellini, 2003; Pagano and Volpin, 2005, respectively). In particular, I will include in the specification two binary variables, MAJ and $PRES$, which identify majoritarian electoral systems and presidential political regimes, respectively. Other controls include the proportion of people aged over 65 ($ELDERLY$), which captures pressures for inter-generational redistribution (Mulligan and Sala-i-Martin, 1999; Tabellini, 2000) and the quality of democratic institutions (Mulligan et al., 2004), as defined by the $POLITY$ Project. Finally, one may want to allow for a quantitatively weaker effect of (domestic) financial development in countries that are more open to international capital flows, since investors in these economies can resort to international financial markets.[9] Unfortunately, distinguishing countries according to financial openness is difficult because data are scarce; for this reason, a measure of real $OPENNESS$ (trade over GDP) is used instead.

Detailed definitions and sources for each variable are presented in the Appendix. Tables 3.1 and 3.2 report summary statistics and correlations among all variables, respectively. Indeed, social security is on average lower in $COMMON$ law countries, as opposed to $CIVIL$ and $SOCIALIST$ law countries. Also, it is negatively correlated with financial development. In the next section I will investigate whether such correlations reflects a causal, negative effect of financial development on social security.

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[8] As a robustness check, I will also present estimates excluding former socialist countries, thus comparing only common and civil law countries.

[9] In the limit, domestic financial development should be irrelevant for the demand for social security in a small open economy.

Table 3.1: Summary statistics

variable	*n*	*mean*	*std. dev.*	*max*	*min*
Social Security over GDP (percentage)	64	6.4	5.109	17.649	0.015
Total Finance over GDP (percentage)	64	99.7	76.626	317.178	8.869
SOCSEC	64	1.270	1.458	2.871	-4.219
FINANCE	64	4.290	0.851	5.759	2.183
COMMON	64	0.203	0.406	1	0
CIVIL	64	0.563	0.500	1	0
GDP	64	9.134	0.782	10.441	7.302
TOTREV	64	3.281	0.430	3.824	0.910
MAJ	49	0.245	0.434	1	0
PRES	49	0.327	0.474	1	0
ELDERLY	64	0.097	0.048	0.175	0.017
POLITY	63	6.090	3.856	10	0

Notes: This table presents summary statistics of all variables.

Table 3.2: Correlation matrix

	SOCSEC	*FINANCE*	*COMMON*	*CIVIL*	*GDP*	*TOTREV*	*MAJ*	*PRES*	*ELDERLY*
FINANCE	-0.067								
COMMON	-0.461	0.409							
CIVIL	0.052	0.215	-0.572						
GDP	0.327	0.635	0.169	0.105					
TOTREV	0.576	0.161	-0.214	-0.001	0.234				
MAJ	-0.421	0.257	0.49	-0.274	-0.072	-0.203			
PRES	-0.282	-0.183	-0.062	0.251	-0.391	-0.697	-0.093		
ELDERLY	0.746	0.173	-0.232	-0.021	0.617	0.561	-0.168	-0.538	
POLITY	0.384	0.383	0.102	0.032	0.644	0.37	0.094	-0.391	0.631

Notes: This table presents the correlation matrix among variables.

3.3 Estimation results

Tables 3.3 to 3.6 report the results of the econometric analysis. Table 3.3 presents OLS estimates of the reduced form relationship between financial frictions, as proxied by legal origin, and social security. According to the univariate regression in the first column, $COMMON$ law legal institutions are associated with lower social security budgets. In particular, social security taxes are, on average, about 1.4 percentage points lower in $COMMON$ law than in $CIVIL$ law countries, and 2.3 points lower than in the whole sample (which includes also former socialist countries). These differences are very statistically significant.

The other specifications presented in the table control for the effect of additional determinants of social security. Overall, the estimated effect of legal institutions remains stable and very statistically significant in all specifications. As for the other variables, income per capita is an important determinant of social security and is also likely correlated with legal origin. For this reason, I will include it in column [2] and in all subsequent specifications. The percentage of elderly in the population is also important, as predicted

Table 3.3: Reduced-form estimates

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
COMMON	-2.304***	-2.935***	-2.406***	-2.649***	-1.537**	-2.892***	-1.858***	-1.525**
	(0.554)	(0.459)	(0.485)	(0.393)	(0.62)	(0.461)	(0.585)	(0.671)
CIVIL	-.919***	-1.369***	-1.088***	-1.527***	-.534*	-1.332***	-1.088***	-.546*
	(0.216)	(0.183)	(0.212)	(0.262)	(0.312)	(0.212)	(0.346)	(0.285)
GDP		0.958***	0.745***	1.407***	0.065	0.639*	0.681	0.128
		(0.197)	(0.227)	(0.256)	(0.387)	(0.36)	(0.495)	(0.391)
TOTREV			1.148***				-.109	0.651*
			(0.315)				(0.83)	(0.36)
MAJ				-.390			-.482	
				(0.295)			(0.339)	
PRES				0.174			0.4	
				(0.239)			(0.444)	
ELDERLY					18.930***		13.258***	15.036**
					(5.666)		(5.033)	(6.505)
POLITY						0.099	0.029	
						(0.073)	(0.036)	
constant	2.254***	-6.118***	-8.203***	-10.136***	-.557	-3.842	-5.172*	-2.883
	(0.093)	(1.729)	(1.709)	(2.276)	(2.813)	(2.829)	(3.125)	(3.186)
Obs.	64	64	64	49	64	63	48	64
R^2	0.279	0.52	0.614	0.693	0.662	0.559	0.738	0.687
F statistic	16.52	29.245	29.981	14.153	21.275	16.656	31.845	26.715

Notes: This table presents reduced-form OLS estimates of the effect of legal institutions on social security. The dependent variable is the log of social security taxes in percentage of GDP. *COMMON* is a binary variable equal to 1 if the country's legal system is of common law type. *CIVIL* is a binary variable equal to 1 if the country's legal system is of civil law type. *GDP* is log of GDP per capita at constant 2000 purchasing power parity international dollars. All other variables are described in the Appendix. Robust standard errors are presented (in parenthesis). *, ** and *** denote rejection of the null hypothesis of the coefficient being equal to 0 at 10%, 5% and 1% significance level, respectively.

by political economy theories of social security.

While this evidence is suggestive about the importance of legal origin, it is silent about the channel through which legal origin impacts on social security. For this reason, Tables 3.4 to 2.3 regress $SOCSEC$ on $FINANCE$ in order to see whether the effect goes through financial market development.

The OLS univariate regression is presented in column [1] of Table 3.4. The estimated coefficient is negative but not significantly different from 0. However, this estimate is difficult to interpret as both social security contributions and finance are endogenous variables. In addition, total finance relative to GDP could be a noisy measure of financial development, which would entail attenuation bias. When I turn to the Two Stage Least Squares approach, the first stage estimates (bottom panel) confirm that $COMMON$ law countries are characterized by higher financial development relative to $CIVIL$ law countries, with former socialist economies lagging further behind. Both coefficients are very precisely estimated, which results in a first stage F-statistic greater than 25. This value is more than twice the threshold of 10 suggested, as a rule of thumb, by the literature on weak instruments (Bound et al., 1995a; Stock et al., 2002). Moreover, according to the first stage R^2, the sole variation in legal origin accounts for almost half of the variation in financial market development.

Table 3.4: OLS and TSLS estimates, baseline

	[1]	[2]	[3]	[4]
		second stage		
FINANCE	-0.114	-1.210***	-0.786***	-2.021***
	(0.171)	(0.342)	(0.252)	(0.405)
GDP			1.152***	2.005***
			(0.381)	(0.379)
constant	1.759**	6.461***	-5.879**	-8.371***
	(0.728)	(1.371)	(2.564)	(2.526)
obs.	64	64	64	64
R^2	0.004		0.232	
F	0.445	12.12	5.049	14.788
method:	OLS	TSLS	OLS	TSLS
		first stage		
COMMON		1.660***		1.321***
		(0.239)		(0.198)
CIVIL		1.136***		0.894***
		(0.194)		(0.159)
		statistics for the excluded instruments		
R^2 (excl. instr)		0.448		0.467
F (excl. instr.)		26.71		24.38
J		1.498		1.690
J (p-value)		0.221		0.194

Notes: This table presents OLS and TSLS estimates of the effect of financial development on social security. The dependent variable is the log of social security taxes in percentage of GDP. Columns [1] and [3] present OLS estimated coefficients, robust standard errors (in parenthesis), R^2 and F-statistic. Columns [2] and [4] presents the results of TSLS estimates in which *COMMON* and *CIVIL* (together with all second stage variables) are used as instruments for *FINANCE*. The top panel reports the second stage estimated coefficients, robust standard errors (in parenthesis), and F-statistic. The bottom panel presents first stage estimated coefficients of excluded instruments, standard errors (in parenthesis), first-stage R^2, the F-statistic for the excluded instruments and Hansen J-statistic. *, ** and *** denote rejection of the null hypothesis of the coefficient being equal to 0 at 10%, 5% and 1% significance level, respectively.

In the second stage (top panel), the estimated coefficient of $FINANCE$ is strongly statistically significant and increases its absolute value above unity. This result suggests that financial development has a causal, negative effect on the demand for social security. Columns [3] and [4] repeat the OLS and TSLS estimates controlling for the level of income per capita. The OLS coefficient is now statistically significant, confirming that the positive correlation of both social security and financial development with income per capita significantly weakens the univariate (negative) coefficient. Yet, the magnitude of the TSLS estimated coefficient is still more than twice that of the OLS one, suggesting that the difference between the two estimates may be due to omitted variation in other variables correlated with both social security and financial development.

For this reason, regressions reported in Table 3.5 control for the effect of income and other variables possibly correlated with both social security and the exogenous component of financial development. Financial development remains strongly significant in all specifications, its estimated coefficient ranging between approximately -1 and -2, and the standard errors being always below 0.5.

Table 3.5: TSLS estimates, controls

	[1]	[2]	[3]	[4]	[5]
FINANCE	-1.438***	-2.009***	-.879**	-2.003***	-1.619**
	(0.327)	(0.411)	(0.407)	(0.402)	(0.68)
GDP	1.366***	2.664***	0.413	1.696***	2.108*
	(0.406)	(0.575)	(0.608)	(0.582)	(1.206)
TOTREV	1.828***				0.246
	(0.38)				(0.881)
MAJ		-.039			-.054
		(0.436)			(0.474)
PRES		-.030			0.25
		(0.347)			(0.497)
ELDERLY			21.263***		7.473
			(6.134)		(9.431)
POLITY				0.093	-.027
				(0.113)	(0.06)
constant	-11.042***	-14.481***	-.807	-6.218	-12.569
	(2.457)	(4.039)	(3.491)	(4.286)	(7.896)
Obs.	64	49	64	63	48
F	16.879	8.001	15.589	8.953	10.495
statistics for the excluded instruments					
R^2 (excl. instr)	0.518	0.353	0.374	0.447	0.195
F (excl. instr.)	31.68	11.74	17.64	23.48	4.72
J	2.452	0.124	2.52	1.723	0.111
J (p-value)	0.117	0.725	0.112	0.189	0.74

Notes: This table presents TSLS estimates of the effect of financial development on social security. The dependent variable is the log of social security taxes in percentage of GDP. *COMMON* and *CIVIL* (together with all second stage variables) are used as instruments for *FINANCE*. The bottom panel presents the first-stage R^2, the F-statistic for the excluded instruments and the Hansen J-statistic. Robust standard errors are presented (in parenthesis). *, ** and *** denote rejection of the null hypothesis of the coefficient being equal to 0 at 10%, 5% and 1% significance level, respectively.

Table 3.6 checks the robustness of the preferred specification with respect unobserved heterogeneity and sample selection. In particular, column [1] adds continent dummies to the specification, while columns [2] and [3] exclude from the sample the Asian and former socialist economies, respectively. These two groups of countries were in fact characterized by a tumultuous pattern of financial development during the 1990s, as a consequence of economic and political turmoil, respectively. In addition, it is not clear whether social-

Table 3.6: TSLS estimates, robustness

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
FINANCE	-1.076***	-1.768***	-3.628**	-1.933***	-1.003***	-1.866***	-2.108***	-1.354***
	(0.37)	(0.395)	(1.653)	(0.387)	(0.201)	(0.45)	(0.595)	(0.22)
GDP	1.002**	2.105***	2.733***	1.944***	1.481***	2.002***	1.652**	1.635***
	(0.493)	(0.474)	(0.916)	(0.378)	(0.285)	(0.476)	(0.652)	(0.264)
constant	-2.689	-10.211***	-7.681**	-8.194***	-11.821***	-9.016***	-4.521	-7.685***
	(3.323)	(3.010)	(3.668)	(2.522)	(2.077)	(2.842)	(5.336)	(1.781)
Obs.	64	54	49	64	46	32	32	53
F	12.05	9.935	4.804	14.312	13.604	8.449	6.09	20.131
	statistics for the excluded instruments							
R^2 (excl. instr)	0.349	0.465	0.137	0.469	0.408	0.571	0.377	0.474
F (excl. instr.)	15.27	21.75	7.28	12.83	14.46	18.68	8.50	22.10
J	3.235	1.768		4.19	0.016	1.266	0.925	0.365
J (p-value)	0.072	0.184		0.242	0.899	0.261	0.336	0.546

Notes: This table presents TSLS estimates of the effect of financial development on social security. The dependent variable is the log of social security taxes in percentage of GDP. *COMMON* and *CIVIL* (together with all second stage variables) are used as instruments for *FINANCE.* Column [1] include geographic (continent) dummies. Column [2] and [3] exclude Asian and former socialist countries, respectively. Column [4] distinguishes between different types of *CIVIL* law in the first stage (*FRENCH*, *GERMAN* and *SCANDINAVIAN.*) In Column [5] the dependent variable is log of social security expenditure (as opposed to taxes) in percentage of GDP. Columns [6] and [7] consider only the least and most open countries, respectively. Column [8] uses data for the single year in which there were more observations available (that it 1997) rather than ten-year averages. The bottom panel presents the first-stage R^2, the F-statistic for the excluded instruments and the Hansen J-statistic. Robust standard errors are presented (in parenthesis). *, ** and *** denote rejection of the null hypothesis of the coefficient being equal to 0 at 10%, 5% and 1% significance level, respectively.

ist law represents a well defined legal tradition. The coefficient of *FINANCE* remains however very statistically significant. Column [4] distinguishes, in the first stage, among different types of civil law, namely French, German and Scandinavian. Even though the differences among the three are quite precisely estimated, they are quantitatively small; as a consequence, second stage results are broadly unaffected. In column [5] the whole of benefits, rather than contributions, is used to measure the social security budget. The coefficient of interest remains negative and strongly statistically significant, even though lower in magnitude. Columns [6] and [7] distinguish between more and less open countries, respectively. However, the estimated effect of domestic financial development remains remarkably similar in the two groups of countries (even though slightly weaker in more open countries). Finally, the last column repeats the estimation by considering yearly data points in 1997 (the single year for which the maximum number of countries is available) rather than 1990-2000 country averages.

Overall, the results of the econometric analysis uncover a robust, negative effect of the legal-origin-driven component of financial development on social security that is both statistically and quantitatively significant. In particular, according to these estimates, a 1 percent increase in the predicted (based on legal origin) ratio of total finance over GDP entails a 1 to 2 percent decrease in social security contributions over GDP.

3.4 Conclusions

In this work I investigated the relationship between financial development and social security. Building on literature on law and finance, I have taken legal origin as a proxy of exogenous financial frictions and shown that differences in legal origin explain a sizable part of the variation in the level of financial investment over GDP in my sample. Most importantly, I find that countries with lower levels of financial development due to their legal origin have higher levels of social security taxes.

The issue might be relevant for the political economy of social security. Most of the reforms that have been proposed during the last few years hinge upon the possibility of financing part of the future social security benefits through stock market investment. The results presented in this paper suggest, however, that the benefits of undertaking these reforms depend crucially on the quality of financial markets and legal arrangements. From a normative point of view, any policy proposal aimed at switching from PAYG to mixed or fully funded social security systems should carefully consider institutional quality as a key determinant of the payoffs of the reform; the political sustainability of the reforms could also be undermined by weak legal arrangements in terms of the quality of corporate governance, legal enforcement, transparency of financial markets. Both aspects are going to be particularly relevant in the aftermath of the recent turmoil in global financial markets.

Appendix: variables' definitions and sources

SOCSEC: log of social security taxes relative to GDP. Social security taxes include employer and employee social security contributions and those of self-employed and unemployed people. Source: International Monetary Fund, Government Finance Statistics Yearbook.

FINANCE: log of total finance relative to GDP. Total finance is the sum of domestic credit and stock market capitalization. Domestic credit refers to financial resources provided through loans, purchases of non-equity securities, trade credits, and other accounts receivable that establish a claim for repayment. Stock market capitalization refers to a country's main stock exchange. Source: World Development Indicators.

GDP: log of GDP per capita at purchasing power parity international dollars. GDP is the sum of gross value added by all resident producers in the economy plus any product taxes and minus any subsidies not included in the value of the products. It is calculated without making deductions for depreciation of fabricated assets or for depletion and degradation of natural resources. Source: International Comparison Programme.

TOTREV: total government revenues relative to GDP. Total government revenues include all revenues from taxes and non repayable receipts (other than grants) from the sale of land, intangible assets, government stocks, or fixed capital assets, or from capital transfers from nongovernmental sources. It also includes fines, fees, recoveries, inheritance taxes, and non recurrent levies on capital. Data are shown for central government only. Source: International Monetary Fund, Government Finance Statistics Yearbook.

ELDERLY: percentage of the total population that is 65 or older. Source: World Bank staff estimates from various sources including the United Nations Statistics Division's Population and Vital Statistics Report, country statistical offices, and Demographic and Health Surveys from national sources and Macro International; reported in the World Development Indicators.

POLITY: institutionalized democracy index, derived from codings of the competitiveness of political participation, the regulation of participation, the openness and competitiveness of executive recruitment, and constraints on the chief executive. Source: Polity IV Project

OPENNESS: sum of exports and imports of goods and services relative to GDP at purchasing power parities. Exports and imports represent the value of all goods and other market services provided to the rest of the world. They include the value of merchandise, freight, insurance, transport, travel, royalties, license fees, and other services, such as communication, construction, financial, information, business, personal, and government services. They exclude labor and property income as well as transfer payments. Source: World Bank Development Indicators.

LEGAL ORIGIN: binary variables for the origin of the legal system. Source: La Porta et al. (1998).

SOCSECEXP: log of social security expenditure relative to GDP. Consolidated central government expenditures on social services and welfare as ratio of GDP. Source: IMF - GFS Yearbook 2000 and IMF - IFS CD-Rom.

MAJ: dummy variable for electoral systems. Equals 1 if all the lower house is elected under plurality rule, 0 otherwise. Only legislative elections (lower house) are considered. Source: Persson and Tabellini (2003).

PRES: dummy variable for forms of government, equal to 1 in presidential regimes, 0 otherwise. Only regimes where the confidence of the assembly is not necessary for the executive (even if an elected president is not chief executive, or if there is no elected president) are included among presidential regimes. Source: Persson and Tabellini (2003)

Chapter 4

Delayed Privatization

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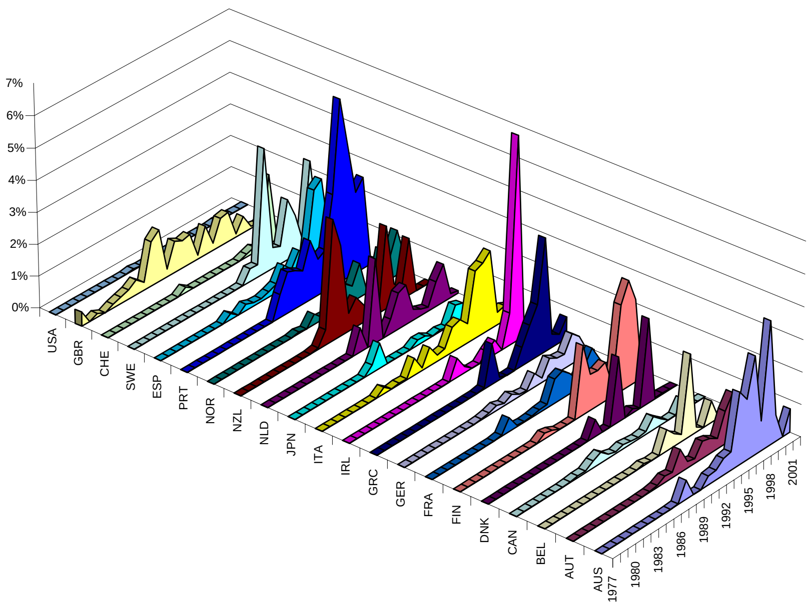
In the last two decades, a big privatization wave has redrawn the borders of the economic activity of the state in developed economies. Thanks to privatizations worth approximately $ 1 trillion, OECD countries have shrunk their state-owned enterprises (SOE) sector on average from more than 12 to 6 percent of GDP. In most cases, privatization represented a qualifying element of a package of measures including liberalization, deregulation, and corporate governance reforms. Thus, the implementation of privatization policy is certainly one of the most important experiments of structural reform ever attempted in market economies.

As Figure 4.1 shows, the process followed a similar cyclical pattern across countries. In all OECD economies (with the notable exception of the United Kingdom and the United States) privatization started in the late 80s or early 90s, peaked in the late 90s and dramatically declined after the turn of the century. Yet the timing of sales varied greatly across countries. Some governments have promptly entered the advanced stage of the process, and raised a significant fraction of their revenues earlier, while others have lagged behind.

Why are privatizations delayed? Why did it take just a few years for the United Kingdom to launch the largest scale privatization program in history, while the process started in Switzerland only in the late 90s?

We claim that political fragmentation, which is related to the number of agents with veto power in a given political system, hampers the implementation of policies with significant distributional consequences, such as privatization. A lower political fragmentation favors executive stability and allows incumbent governments to privatize a sizable fraction of their SOE sector sooner, as the constituency of the "losers" from the policy change is less likely to enjoy bargaining power. On the contrary, highly fragmented political systems tend to disperse decision-making power among different actors, so that executives are weaker and characterized by higher turnover. In this context, the different political actors will hardly reach an agreement about how to distribute the burden of the policy

Figure 4.1: The privatization wave



Note: This figure shows the trend in privatization revenues over GDP during the 1977-2002 period.

change, and privatization will be delayed by a "war of attrition" as in Alesina and Drazen (1991) and Spolaore (2004).

In this paper, we test this prediction by estimating a duration statistical model on data for 21 OECD economies during the 1977-2002 period. The results are broadly consistent with the empirical implications of the war of attrition theoretical model. Political systems with a smaller number of parties and operating under majoritarian electoral rules privatize sooner, while large-scale privatization is delayed in more fragmented democracies.

A tale of two countries, the United Kingdom and Switzerland, illustrates the role of political fragmentation in the timing of privatization. After winning the 1979 election, Mrs. Thatcher kicked off her program immediately with a first batch of sales in (reasonably) competitive industries. The process then gained momentum after the 1983 re-election and continued apace in the late 80s in the newly liberalized electricity market and in the water industry. Throughout, privatization was fiercely opposed by the trade unions and by the Labour party. Nevertheless, the majority enjoyed by the Conservatives in Parliament combined with the power granted to the cabinet by the British political system allowed the government to push back the opposition and to accomplish the announced program (Vickers and Yarrow, 1988).

Conversely, Switzerland was the last developed country to privatize, since it took decades for the four parties forming the Federal Council to find a consensus on reform.[1] After a long negotiation, the 1998 Telecommunications Act was eventually enacted yielding a timid liberalization of the sector, and the flotation of a minority stake of Swisscom. By the end of 2005, the Swiss state still held a 66.1% stake in the company. At the beginning of 2006, the executive set forth a plan to further the privatization of the company but the policy was immediately blocked by the opposition of the centre-left Social Democrats party, one of the permanent members of the Council. Besides, any further attempt would likely have to pass a popular referendum, definitely a distinguishing feature of the Swiss political system.

This study relates to empirical studies of privatization, surveyed by Megginson and Netter (2001). In particular, recent work has explored specifically the role of politics. Clarke and Cull (2002) examine the political and economic incentives for provincial governments in Argentina to privatize banks. They find that the likelihood of privatization is higher for poorly performing banks, while the overstaffed and larger banks tend to remain under state ownership. Boehmer et al. (2005) extend the analysis to a larger group of countries, finding that in non-OECD countries bank privatization is more likely the lower is the quality of the nation's banking sector, the more the government leans to the right and the greater the government's accountability to the people. Financial distress is instead the main determinant of bank privatizations in OECD countries. The authors also study the timing of bank privatization and conclude that countries with banks that have less equity-capital and extend more loans to the government, and with higher government accountability privatize state-owned banks faster. Political factors instead do not seem to affect the timing of bank privatization. In a case study on India, Dinc and Gupta (2007) analyze the decision to privatize at the central government level and find that the likelihood of privatization is higher in states where the party of the incumbent central government faces less local political competition.

While previous literature focused on ideological orientation as a political determinant, our paper is the first to study empirically the role of political fragmentation on the timing of privatization in developed economies.

Our work is also related to the empirical literature on the political economy of reform in the context of fiscal stabilization. In particular, in the last few years, several papers have tested the war of attrition model using fiscal data. For example, Padovano and Venturi (2001) provide a detailed case study of the effect of political fragmentation on public finance in Italy during the post-war period; Huber et al. (2003) and Woo (2003) extend the analysis to OECD and to almost 60 countries, respectively. All these papers find evidence of a positive relationship between political fragmentation and budget deficits or public debt.

The empirical strategy common to all these studies has been to fit OLS regressions

[1] The Federal Council is the executive body of the Swiss political system. It is formed by seven members that represent all and only the four major parties, which span the whole ideological spectrum. For detailed information, see chapter 2 of Lijphart (1999).

of some measure of fiscal imbalance on political fragmentation, along with other political and economic explanatory variables. An estimated positive coefficient on measures of political fragmentation, like for instance the number of parties, is interpreted as evidence in favor of the war of attrition model. However, such a methodology does not allow one to disentangle the empirical implication of the war of attrition model from those of alternative, more general models encompassing a "public good" type of market failure: the higher the number of veto players involved in the decision making, the larger should be the total draw from the common pool of government's budget. Put differently, in the war of attrition model higher political fragmentation results in deeper fiscal imbalances only indirectly, as a consequence of longer delays to reform, while the specific prediction of the model concerns the length of the delay itself.[2]

We improve in this respect by identifying a formal link between the war of attrition theoretical model and the duration econometric framework pioneered by Cox (1972). Such a link arises naturally from the central role played by time both in the theoretical and the statistical model.

We also contribute to this strand of literature by providing a new set of continuous and time-varying political indexes computed from electoral data. Our dataset survives an extensive cross-checking with independent data, proving itself a reliable tool for empirical work in political economy.

The remainder of the paper is structured as follows. Section 4.1 reviews the theoretical war of attrition model. Section 4.2 derives the estimating equation and shows that duration analysis provides a suitable statistical framework to perform this test. Section 4.3 introduces measures of the delay to privatize and of political fragmentation, it describes the data and compares them to existing datasets. Section 4.4 presents the empirical results. Section 4.5 concludes.

4.1 Theoretical framework

The political economy of policy adjustment (particularly, fiscal stabilization) has been studied by Alesina and Drazen (1991). In their model, the benefit of stabilization accrues to all citizens and stems from abandoning a highly distortionary method of financing public expenditure. However, the costs of stabilization (i.e. higher taxation) are apportioned differently between interest groups, with one group bearing a disproportionate fraction of the tax burden. Under these assumptions, the process leading to stabilization becomes a war of attrition between groups, characterized by political stalemate until one group concedes. Concession occurs at equilibrium when the group-specific costs and benefits of

[2]For instance, Velasco (2000) presents a dynamic model in which higher political fragmentation leads to higher public deficits without resorting to any war of attrition between political agents. A simpler, static example is presented in chapter 7 of Persson and Tabellini (2000). The more general relationship between political fragmentation and fiscal distress has been as well extensively tested since the seminal work by Roubini and Sachs (1989); more recent contributions are Alesina et al. (1998) and Perotti and Kontopoulos (2002).

waiting balance each other. Importantly, Alesina and Drazen note that large coalition cabinets made of diverse parties may hardly reach an agreement on how to allocate the tax increase among the different constituencies. Therefore delayed stabilization should be associated with higher political fragmentation.

The empirical implications of this model appear a bit far fetched to allow for a proper empirical test. Spolaore (2004) makes an important step in this direction, by developing a model that allows comparing patterns of adjustment policies in different systems of government. The primary focus is on the way control over decision making is allocated across political agents with different preferences. Two benchmark systems are considered: the "cabinet" system, giving full control over policies to one decision maker, and the pure "consensus" system, in which each political agent retains veto power over adjustment policies. The two systems differ therefore in terms of *political fragmentation*, which is defined simply as the number of political agents with veto power.

The cabinet system is shown to provide prompt adjustment, even if it may adjust too often as the policy-maker fails to internalize the adjustment costs of other political agents. On the contrary, the consensus system may fail to adjust even when adjustment is optimal. Interestingly, in the presence of large adjustments, like privatization, the model shows that the only equilibrium in the consensus system is a war of attrition *a la* Alesina and Drazen, and that the expected delay to reform depends on political fragmentation.

In particular, let T be the delay of reform, with $f(T)$ and $F(T)$ being, respectively, its density and cumulative distribution. The *concession hazard rate* $\lambda(T) = \frac{f(T)}{1-F(T)}$ is the probability that adjustment occurs after T periods given that the economy did not adjust before. Then, the prediction of the model is that

$$\lambda(T) = \left(\frac{n}{n-1}\right)\theta, \tag{4.1}$$

where n is the number of agents with veto power and θ is an exogenous parameter that depends on the size of adjustment at stake (or, in another way, on the initial conditions of the economy). Thus, the implied concession hazard rate is decreasing in political fragmentation.

Privatization is a major adjustment policy, defined as any efficient policy change with significant distributional consequences. First, privatization curbs political interference, improves managers' incentives, and tends on average to increase the efficiency of firms (Megginson and Netter, 2001). Second, privatization has important distributional effects as it typically involves a transfer of wealth from insiders of state-owned enterprises (such as employees) to outsiders, especially shareholders. Indeed, state sell-offs have been often associated with restructuring and layoffs, with efficiency gains accruing to shareholders of newly privatized firms (Megginson et al., 1994; Haltiwanger and Singh, 1999). If one country's political system is highly fragmented, the interest group of "losers" from privatization has voice in the political arena and engages in a war of attrition which delays the efficient policy change.

In this context, it is thus straightforward to interpret T as the time elapsed until privatization occurs. The next sections will describe how we take equation (4.1) to the data.

4.2 Empirical strategy

Equation (4.1) relates the concession hazard rate at T, i.e. the probability of observing the adjustment after T periods, to some explanatory variables. Duration analysis provides the exact translation of this relationship into a statistical model. The dependent variable of duration models is the conditional hazard rate

$$\lambda(T \mid x) = \frac{f(T \mid x)}{1 - F(T \mid x)}, \tag{4.2}$$

where T , $f(.)$ and $F(.)$ are defined as in (4.1) and x is a vector of covariates including proxies for n and θ, along with other political and economic controls.

Following the literature on survival analysis (Cox, 1972; Kiefer, 1988; Van den Berg, Van den Berg), we first assume a proportional hazard rate, which implies separability of $\lambda(.)$ in T and x:

$$\lambda(T \mid x) = \Gamma(x)\Lambda(T), \tag{4.3}$$

The proportionality assumption (4.3) allows the difference in hazard rates between countries i and l observed in period r to depend on the difference $[x_i(r) - x_l(r)]$ but *not* on the particular period r (at least not directly) and is key to the interpretation of many results. The additional term $\Lambda(T)$ is introduced to allow for flexible time dependence of the hazard rate and encompasses time independence (that is, a constant $\Lambda(T)$ like in 4.1) as a particular case.

We will fit two different versions of the proportional hazard rate model. First, we follow the original Cox (1972) semi-parametric approach, which leaves the baseline hazard $\Lambda(.)$ unspecified. In spite of its simplicity, the Cox model is already sufficient to identify the effect of changes in x on the hazard rate (this is a direct consequence of proportionality). We will then check the robustness of results by estimating a fully parametric model which specifies a functional form for the baseline hazard. In particular, we refer to the conventional Weibull (1951) specification

$$\Lambda(T) = \alpha T^{\alpha - 1},$$

where α is an ancillary nonnegative parameter which allows for duration dependence.[3]

Both models assume a non-negative exponential form for $\Gamma(.)$:

$$\Gamma(x) = \exp(x\prime\beta), \tag{4.4}$$

[3]In particular, for $\alpha > 1$ the process shows positive duration dependence, i.e. the probability of failure increases through time; the opposite holds true as $\alpha < 1$;finally, for $\alpha = 1$ the hazard rate is independent of time (in this last case the Weibull model collapses to the simpler exponential form).

where β is the vector of coefficients of interest, which is estimated by maximum likelihood (partial for Cox, full for Weibull). The direction of the effect of the k-th regressor on the hazard rate relates directly to the sign of the k-th element of β: an increase in x^k increases (decreases) the hazard rate as long as $\beta > 0$ ($\beta < 0$). In particular, we will be mainly interested in the coefficient of some proxy for the theoretical number of veto players n in (4.1).

The proportionality assumption imposed by the Cox and the Weibull models is convenient for several reasons. First, it allows to model very simply the effect of the explanatory variables on the hazard rate, which is often the relationship of primary interest. Second, in our particular case, proportionality characterizes as well the hazard rate in equation (4.1), which we want to test. Nevertheless, it remains a restrictive assumption and we may want to check how the results change as we relax it.

Consider the parametric Weibull model and notice that it can be restated as

$$\alpha \ln T = -x\prime\beta + \nu, \tag{4.5}$$

where ν has a type I extreme value distribution, which is implied directly by the proportionality assumption. We will relax proportionality by letting $\ln T$ follow a normal distribution, conditional on the vector of covariates x. In this case, maximizing the likelihood for the log normal distribution of T will provide efficient estimates of the vector of parameters $\phi = -\frac{\beta}{\alpha}$

4.3 Data

This section presents our dataset. Our sample includes 21 sound democracies with established political institutions enabling an orderly succession of powers: most of Western Continental European countries (Austria, Belgium, Denmark, Finland, France, Germany, Greece, Italy, Netherlands, Norway, Portugal, Spain, Sweden, Switzerland), Anglo-Saxon countries (Australia, Canada, Ireland, New Zealand, United Kingdom, United States) and Japan.

Given our focus on the timing of privatization policy and related reforms, the sample period is certainly a key dimension of the dataset. We set 1977 as the initial year, reporting what is conventionally considered the first privatization in recent times, the IPO of British Petroleum.[4] The final year of the sample period is 2002, when the privatization wave ends in most countries. Indeed, privatization activity in OECD countries peaked in 1999 and abruptly slowed down after the turn of the century, with revenues back to the levels reported in the early 80s, at times when only the United Kingdom was seriously engaged in privatization (Bortolotti and Siniscalco, 2004). Our sample period thus captures in its

[4] Some important historical antecedents were the sales of Volkswagen and VEBA implemented in the Federal Republic of Germany by Adenauer in the early 60s. However, these companies quickly returned in public hands and were bailed-out under the pressure from disappointed investors.

entirety the big privatization cycle of the last two decades and is thus suitable for the empirical analysis of the timing of reform.

Next, we introduce our privatization, political and economic variables. The most important ones are the empirical counterparts for T and n; they are also those involving the most critical measurement issues.

4.3.1 Delay of privatization

A reasonable starting point to measure the delay period is t_0=1977, when privatization definitely entered the world economic and political agenda. About the end year t_i, which is needed to set the length of the delay period in each i-th country, we may want to choose a date that takes into account genuinely the advancement of the privatization process in that country. Thus, we have first collected revenues data for all privatizations (public offers and private sales) reported in Securities Data Corporation, certainly one of the most comprehensive sources of information at the transaction level. We have aggregated them to construct *REVGDP*, equal to total privatization revenues as a fraction of GDP in each country-year. Then, the end year of the delay period is defined as

$$t_i = \min\left\{s : REVGDP_{is} \geq median\left(REVGDP_{ir}\right),\ r = 1977, ..., 2002\right\}, \tag{4.6}$$

that is, we consider the first year in which total privatization revenues raised in country i equaled or exceeded its median yearly revenues. Median revenues are adopted rather than the first transaction because initial privatizations are typically sporadic and small-scaled, so that they do not prefigure a real start of the reform. For analogous reasons we discarded using the year in which maximum privatization revenues were raised. Finally, median revenues were preferred to average revenues because of the invariance of the former to extreme (and possibly anomalous) values of the observed distribution. The delay of privatization in country i is thus defined as

$$T_i = t_i - t_0. \tag{4.7}$$

4.3.2 Political fragmentation

Conceptually, political fragmentation relates to the number of veto players n in expression (4.1). The larger is n, the higher the degree of political fragmentation. When it comes to making the notion operational one has to solve two issues. First, identifying the relevant political agents. In this respect, political parties are usually regarded as the basic cohesive entities representing specific interest groups. Second, how to weight them according to their actual bargaining power. Comparative political science has developed suitable measures that help address this issue. The Effective Number of Parties (*ENP*) introduced by Laakso and Taagepera (1979) parallels the Herfindahl index in evaluating political fragmentation according to the distribution of seats held by all parties:

$$ENP = \left[\sum_{j \in P} \left(\frac{s_j}{\sum_{k \in P} s_k} \right)^2 \right]^{-1}, \tag{4.8}$$

where s_j is the number of seats in the parliament held by the j-th party and P is some set of parties. Expression (4.8) says that if there are N parties, the *ENP* will take the value N if they all have the same number of seats, otherwise it will take lower values, in order to "discount" parties that are weaker in terms of parliamentary seats. As the number of parties increases, the single shares decrease on average and the *ENP* increases.

Since in any political system most of the veto power is held by the government, we first compute the index over parties forming the executive coalition; we call this variable *GENP*. At the same time, a highly fragmented parliament could also delay the implementation of reform policies which may require broader consensus than simple majority.[5] Thus, we will compute the index as well over all parties represented in the parliament; we will refer to this second measure as *PENP*.

Finally, a third measure of political fragmentation considers the barriers to entry imposed by different electoral systems. In particular, majoritarian systems tend to reduce the number of political parties (and thus veto players) gaining access to the parliament, as opposed to proportional systems (Torsten Persson and Tabellini, 2007). In empirical political economy studies, the electoral system is usually characterized in terms of binary variables. We refer instead to previous work by Gallagher (1991), who computed a continuous measure of the disproportionality of the electoral system:

$$DISPR = 100 \sqrt{\frac{1}{2} \sum_{j \in P} \left[\left(\frac{s_j}{\sum_{k \in P} s_k} \right) - \left(\frac{v_j}{\sum_{k \in P} v_k} \right) \right]^2},$$

where v_j is the number of electoral votes got by the j-th party and s_j and P are defined as in Gallagher (1991). The index equals 0 if there is perfect proportionality between seats and votes. It increases, on average, as the electoral rule moves toward the majoritarian system; it is maximum for presidential elections, when the only seat at stake goes to the winner, in which case the index equals the percentage of votes obtained by the defeated candidate.

All the three indexes, *GENP*, *PENP* and *DISPR* are continuous and defined for each country-year in the sample. As such, they account better than binary or discrete indexes for the extreme heterogeneity observed at the institutional level.

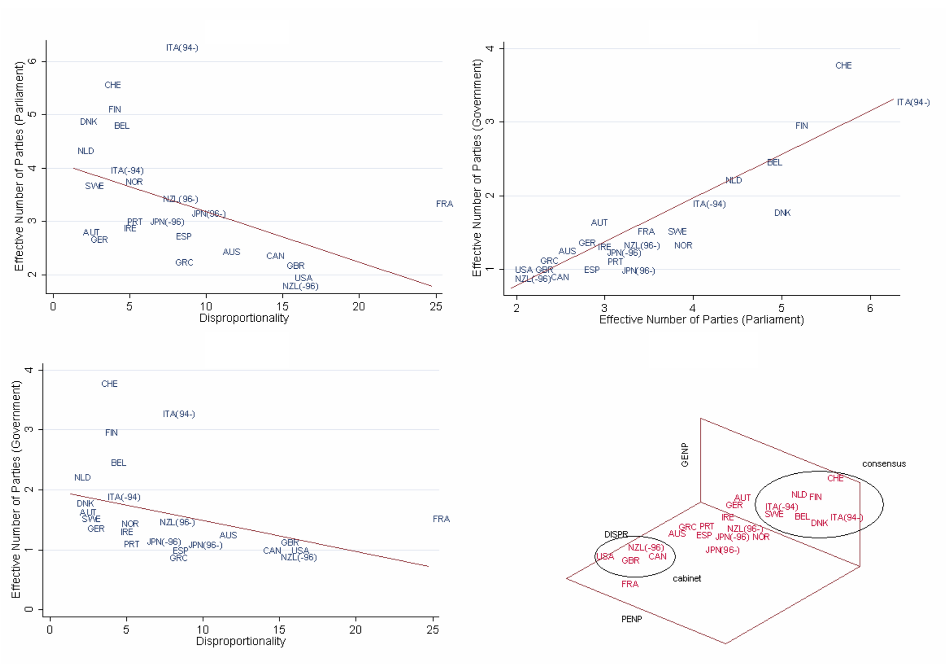
The main source for the electoral data needed to compute the political variables was

[5] Notice that, in several countries (for example France, Belgium, etc.) the implementation of privatization entailed constitutional reforms, which in turn required a qualified majority in the Parliament (for instance, 2/3 of the votes).

Lijphart (1999). We have used his series for *DISPR* and *PENP*, updating to our end year. The other index, *GENP*, has been developed independently; as such, the relative series is compiled ex novo from various sources listed in Appendix A.[6]

To cross-check the reliability of our dataset, labeled FEEM Political Database (FPD), we have compared it with the World Bank Database on Political Institutions (DPI) by Beck et al. (2001), one of the most widely used sources in empirical political economy studies. Then we have compared FPD and DPI pair-wise with a third data base compiled by an independent source (Tsebelis, 2004) in the country years when the three overlap. Results of the cross-check are shown in Appendix B. Indeed, FPD and Tsebelis appear similar in several respects. The average difference between the number of seats is very low for each of the three main parties and for the government's coalition as a whole. Moreover, the percentage of "perfectly matched" cases is above 80% for each of the parties, and quite high for the government's coalition. On the contrary, the World Bank DPI does not seem to be related to any of the other two databases. First, the number of observations is much lower, which means that many electoral results are missing. Second, the pair-wise comparison yields a very high average difference in terms of reported seats (about 30 seats each election for the first party and for the government as a whole). Finally, the percentage of matched data is dramatically low, always under the 5% for the first party and for the government as a whole.

Figure 4.2: Measures of political fragmentation



[6]The dataset is available at http://www.feem.it/fpd

Table 4.1: Political data and timing of privatization

COUNTRIES	*GENP*	*PENP*	*DISPR*	*POLFRAG*	*T*
Australia	1.249	2.427	10.803	-0.759	18
Austria	1.636	2.8	1.679	0.169	14
Belgium	2.456	4.793	3.721	1.104	16
Canada	1	2.35	13.642	-1.101	10
Denmark	1.776	4.87	1.495	0.935	17
Finland	2.959	5.111	3.354	1.486	16
France	1.519	3.33	24.749	-1.259	10
Germany	1.357	2.661	2.204	-0.053	17
Greece	1.028	2.231	7.699	-0.728	15
Ireland	1.309	2.882	4.37	-0.149	22
Italy (-94)	1.898	3.955	3.505	0.56	17
Italy (94-)	3.278	6.267	7.111	1.777	
Japan (-96)	1.146	2.99	6.087	-0.31	11
Japan (96-)	1.084	3.147	8.779	-0.47	
Netherlands	2.221	4.321	1.316	0.99	14
New Zealand (-96)	1	1.965	14.858	-1.309	12
New Zealand (96-)	1.333	3.744	4.44	0.143	
Norway	1.467	3.432	6.889	-0.056	17
Portugal	1.103	2.993	4.589	-0.231	19
Spain	1	2.723	7.75	-0.583	16
Sweden	1.524	3.666	1.841	0.387	17
Switzerland	3.779	5.562	3.081	2.068	21
United Kingdom	1	2.173	14.968	-1.248	9
United States	1	1.94	15.538	-1.363	11
Average	1.63	3.431	7.27	0	15.19
Std. Dev.	0.776	1.187	5.848	1	3.614

Note: This table presents cross-country averages of the political fragmentation indexes over the period 1977-2002. The column *POLFRAG* reports the standardized average of the three measures (*DISPR* enters with negative sign in order to be increasing in the degree of political fragmentation, like *GENP* and *PENP*). In countries where an institutional reform occurred, averages are computed over the two sample periods defined by the first election under the new regime. Finally, the last column reports T, defined as the number of years between the first privatization ever (in 1977) and the year in which median (per country) privatization revenues are observed.

Table 4.1 reports cross-country averages of *GENP*, *PENP* and *DISPR*, and Figure 4.2 plots them on two- and three-dimensional scatters, along with the fitted OLS linear regressions.[7] The slope of the regressions is consistent with the expected pair-wise relationship between the three variables. Torsten Persson and Tabellini (2007) have recently shown that electoral rules determine the number of parties gaining access to the parliament (higher in proportional systems, lower in majoritarian ones), which in turn shapes the fragmentation of the executive. The preliminary inspection of the data presented here is in line with their results. Most importantly, the three indexes together univocally characterize the countries in the sample according to their political fragmentation. In particular, sticking to the terminology of Spolaore (2004), the cluster of Anglo-Saxon

[7]Three countries implemented institutional reforms in our sample period: Italy modified its electoral system in 1992, New Zealand and Japan in 1993. The two averages presented for these countries are computed over the two sub-periods before and after the first election taking place under the new regime.

countries provides a reasonable empirical counterpart to the cabinet theoretical model. At the opposite, the countries on the top-right (Switzerland, Italy, Netherlands, Belgium and part of Scandinavia) resemble well the features of the consensus system.

We next turn to the description of the control variables that enter the vector x in the estimating equation (4.2).

4.3.3 Control variables

While we investigate the role of political fragmentation on the timing of privatization, we may want to control for other possible determinants of privatization. Two of them deserve special attention.

First, initial conditions matter. In particular, privatization could be simply affected by the initial size of the SOE sector and/or the fiscal imbalance. Notice further that initial conditions determine the size of adjustment θ in equation (4.1).[8] We control for one country's initial size of the *SOE* sector by the average of the SOE value added as a percentage of GDP in the three years preceding the first privatization reported in SDC. Similarly, we measure fiscal pressure by one country's average budget *DEFICIT* in the same pre-privatization period.[9]

Second, the strong distributive effects of privatization suggest that the ideological orientation of the executive matters in explaining the timing of the reform. Measuring partisanship of the government faces methodological issues analogous to those described above for political fragmentation[10]. We refer to the study by Huber and Inglehart (1995) who, by the means of expert interviews (over 800 for 42 countries, including the 21 in our sample), have produced a comprehensive classification of political parties according to a score ranging between 1 (extreme left) to 10 (extreme right). We computed a weighted average of the scores obtained by all parties forming the executive in each country-year, with weights equal to the number of parliamentary seats held by each party over the number of seats held by the executive as a whole:

$$PARTISAN = \sum_{j \in G} \left(\frac{s_j}{\sum\limits_{k \in G} s_k} \right) HI_j,$$

where HI_j is the score attached by Huber and Inglehart (1995) to the j-th party, G is the set of parties forming the government and s_j is defined as in (4.8).

The *GDP* per capita is included as well in all the specifications since, even within our sample of OECD countries, it is still present some heterogeneity in terms of economic

[8] In general, initial conditions are key in almost any political economy model of reform; see, for instance, the discussion in chapter 13 of Drazen (2002).

[9] Stock variables such as the value of State-owned assets or central government debt would certainly provide better proxies for initial conditions. Unfortunately, complete time series on debt and comparable data on State assets in OECD countries are still missing.

[10] Empirical studies of partisan political economy usually rely upon dummy or discrete variables, with very limited methodological refinement since the seminal work by Hibbs (1977).

development, which could possibly play a role in the start of the reform. Finally, we will check the robustness of the results to the inclusion of further variables that previous work has found to be relevant for privatization: the stage of financial market development, which plays an important role since deep and liquid stock markets, as measured by *MKTCAP* and *TURNOVER* respectively, facilitate the flotation of large companies; and the set of legal origins (*COMMON*, *GERMAN*, *FRENCH* and *SCANDINAVIAN LAW*) by La Porta et al. (1998).[11]

4.3.4 Descriptive analysis

Table 4.1 presents summary statistics which are useful for a first account of the role of political institutions in the timing of privatization. The column *POLFRAG* reports the cross country average of the three measures of political fragmentation, standardized for the whole sample.[12] It takes the lowest values in the Anglo-Saxon countries and France. Interestingly, almost all of the countries in this group (with the exception of Australia) were among the few ones raising median revenues within the 80s (the only other one, out of the group, is Japan). On the contrary, privatization has been long delayed in highly fragmented political systems such as Switzerland, Belgium and Finland.

Table 4.2: Univariate tests

	T (Delay of Privatization)			
	Bottom 25%	Top 25%	Difference	t-statistic
SOE	11.248	14.083	-2.835***	-3.7
DEFICIT	3.885	7.256	-3.37***	-8.6
GDP	23192	19059	4133***	5.54
PARTISAN	6.403	5.67	0.733***	4.65
GENP	1.13	1.963	-0.834***	-7.85
PENP	2.565	3.724	-1.159***	-8.17
DISPR	15.142	5.519	9.623***	13.92
TURNOVER	0.531	0.607	0.077***	-1.07
CAPMKT	0.694	0.464	0.23***	4.89
COMMON LAW	0.6	0.4	0.2***	3.04
FRENCH LAW	0.2	0.4	-0.2***	3.04
GERMAN LAW	0.2	0.2	0	0

Note: This table reports: the average of each explanatory variable over all observations for the countries at the bottom and top quartile of the distribution of T (Delay of Privatization); the difference between the two averages; finally, the t-statistic of the null hypothesis of the difference being significantly different from 0. *** denote statistical significance at the 99% confidence level.

Table 4.2 provides more systematic evidence. The first two columns report the average values of the explanatory variables for early and late reformers, defined as the first and last five countries, respectively, to raise revenues above the median. The third and fourth

[11] For econometric evidence about the role of financial markets and legal origins, see Bortolotti et al. (2004) and Bortolotti and Siniscalco (2004).

[12] The index *DISPR* enters with a negative sign, in order to be consistent with *GENP* and *PENP*, which are increasing political fragmentation.

columns report the difference between the two and its t-statistic, respectively. The results reported for the control variables resemble those obtained in previous empirical studies of privatization, indicating a role for macroeconomic variables, legal origins, ideology and (to lesser extent) financial markets indicators in explaining also the timing of privatization. Political fragmentation appears to be the novelty: early privatizing are less politically fragmented democracies. The difference is highly statistically significant for all the three measures. This preliminary evidence suggests the potential explanatory power of political fragmentation, which we test extensively in the next section by estimating the econometric model (4.2)-(4.5).

4.4 Results

Tables 4.3, 4.4 and 4.5 show the estimation results for the Cox, Weibull and Lognormal models, respectively. The specifications we present are the same for all the three models. A benchmark equation in column [1] of all tables includes *SOE*, *DEFICIT*, *GDP* and *PARTISAN*. Columns [2]-[4] add the political fragmentation variables *GENP*, *PENP* and *DISPR*. They are never included together in the same regression since they all proxy for the same theoretical variable, namely political fragmentation, which would make it hard to disentangle their distinct effects. Finally, columns [5]-[7] and [8]-[10] check the robustness of the results to the inclusion of the financial markets variables and legal origins, respectively. We start by discussing the information conveyed by the proportional hazard models (Cox and Weibull), since it is most easily interpretable, especially in terms of marginal effects of changes in the explanatory variables. Then, we will check the robustness of the results as we relax the proportionality assumption.

The first conclusion we can draw by looking at tables 4.3 and 4.4 is that well-established economic determinants of the extent of privatization (for instance in terms of total revenues) fail instead to account for the timing of the reform. The univariate correlations found in the descriptive statistics do not survive in the multivariate analysis, which yields unstable and statistically not significant point estimates of the coefficients of all the economic controls. On the other hand, the *PARTISAN* index is strongly significant in any specification and apparently controls for an important effect that the ideology of the executive exerts on the start of the reform.

Turning to the measures of political fragmentation, they show considerable explanatory power. First, they are always statistically significant at conventional levels in the benchmark specifications and the significance is robust to the inclusion of the financial markets variables. When we additionally control for the legal origins, the t-ratios lower considerably and in one case *DISPR* falls slightly short of the 10% significance level (its t-ratio in the Weibull model is 1.50). Notice that the inclusion of legal origins represents an important (and severe) robustness check for our variables of interest, since legal origins and political fragmentation go hand by hand for most of the countries in our sample. Yet, point estimates for the coefficients of legal origins are extremely unstable across specifica-

Table 4.3: Cox model

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
SOE	-0.065	-0.074	-0.078	-0.082	-0.075	-0.089	-0.104*	0.093	0.048	-0.090
	(-0.96)	(-1.05)	(-1.13)	(-1.3)	(-1.16)	(-1.36)	(-1.69)	(1.41)	(0.43)	(-0.71)
DEFICIT	0.027	0.019	0.067	0.124**	-0.012	0.002	0.173**	-0.055	0.008	0.184
	(0.42)	(0.26)	(0.8)	(2.02)	(-0.07)	(0.01)	(2.23)	(-0.45)	(0.05)	(0.80)
GDP	1e-05	4.1e-05	7.8e-05*	4.7e-05*	1.4e-05	8.2e-05*	2.2e-05	0.0004***	0.0004***	2.1e-05
	(0.29)	(1.11)	(1.83)	(1.55)	(0.38)	(1.79)	(0.62)	(3.48)	(2.76)	(0.17)
PARTISAN	0.330**	0.442***	0.350***	0.575***	0.344***	0.283**	0.672***	0.917**	0.653*	0.661***
	(2.48)	(4.45)	(2.87)	(5.22)	(2.69)	(2.18)	(2.69)	(2.38)	(1.91)	(4.72)
GENP		-0.910**			-0.856**			-1.930***		
		(-2.2)			(-2.09)			(-3.8)		
PENP			-0.958**			-1.125**			-1.742*	
			(-2.15)			(-2.4)			(-1.85)	
DISPR				0.275***			0.430***			0.433*
				(4.42)			(2.73)			(1.76)
TURNOVER					-0.098	-0.900	1.148**	-1.112	-0.371	-0.779
					(-0.2)	(-1.45)	(2.29)	(-1.12)	(-0.32)	(-0.30)
MKTCAP					2.081	1.625	3.559*	-3.263*	-2.108	1.543
					(1.16)	(0.93)	(1.7)	(-1.65)	(-1.14)	(0.85)
COMMON LAW								1.706	0.523	3.732
								(1.53)	(0.27)	(1.44)
FRENCH LAW								-1.386	-2.865*	2.225
								(-1.51)	(-1.68)	(0.55)
SCAND. LAW								-5.609***	-4.678**	0.239
								(-4.46)	(-2.03)	(0.07)
Obs.	209	209	209	209	192	192	192	192	192	192
Log likelihood	-30.59	-29.1	-28.41	-25.27	-25.15	-24.31	-20.34	-19.59	-20.56	-18.21
Wald Test	6.86	36.59***	22.42***	42.65***	24.25***	24.26***	19.56***	67.22***	63.12***	208.55***

Note: This table reports estimated coefficients and associated t-statistics (in parentheses) for the Weibull model. The dependent variable is the hazard rate of observing median (per country) privatization revenues. The maximum value of the log-likelihood function is reported below. The Wald Test refers to the null hypothesis of all the coefficients being jointly equal to 0. *, ** and *** denote statistical significance at 90, 95 and 99% confidence level, respectively.

tions. Moreover, the null that they equal 0 can not be rejected in most of the equations. On the contrary, estimates of the effect of political fragmentation, while made somewhat noisier by the inclusion of legal origins, remain overall statistically significant.

Second, the absolute values of the estimated coefficients of *GENP*, *PENP* and *DISPR* are reasonably stable among the semi-parametric (Cox) and the parametric (Weibull) model (once again, the only exceptions come with the inclusion of the legal origins, which seems to reinforce considerably, in absolute value, the effect of *GENP* and *PENP* in the Cox model). This is reassuring about the specification of the functional form for the parametric model. We re-estimated the model for different specifications of the dynamics as well, by introducing lags and leads of potentially endogenous variables (namely *MKTCAP* and *TURNOVER*). Such changes do not affect results at all; this leads us to exclude both the existence of significant simultaneity bias and possible misspecification of the dynamics.

Third, the estimated effect of our measures of political fragmentation is economically, other than statistically, significant. In particular, let us consider the effect of adding one (effective) party either to the government or to the parliament. We focus on such a unit increase in GENP and PENP because it is close to the sample standard deviation of both variables (0.848 and 1.247 respectively) and, further, because it relates to some very concrete feature of the political equilibrium (i.e. how many "important" parties enter the

Table 4.4: Weibull model

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
CONSTANT	-18.570***	-18.802***	-18.662***	-29.793***	-19.272***	-18.498***	-30.800***	-30.684***	-29.355***	-57.558**
	(-3.95)	(-3.68)	(-3.34)	(-3.90)	(-3.31)	(-3.05)	(-3.55)	(-6.53)	(-5.85)	(-2.20)
SOE	-0.054	-0.045	-0.055	-0.078	-0.069	-0.086*	-0.107*	-0.007	-0.077	-0.338
	(-1.28)	(-1.19)	(-1.36)	(-1.61)	(-1.43)	(-1.67)	(-1.93)	(-0.05)	(-0.43)	(-1.40)
DEFICIT	0.027	0.028	0.069	0.141*	-0.008	0.008	0.140	0.083	0.138	0.565
	(0.38)	(0.38)	(0.87)	(1.86)	(-0.05)	(0.04)	(0.77)	(0.43)	(0.59)	(1.24)
GDP	7.5e-06	3.7e-05	7.7e-05*	4.6e-05	-8e-07	6.1e-05	9.2e-07	0.0002	0.0002	-0.0001
	(-0.21)	(0.98)	(1.7)	(1.41)	(-0.03)	(1.11)	(0.02)	(1.08)	(1.37)	(-0.73)
PARTISAN	0.312**	0.399***	0.330***	0.561***	0.282**	0.206*	0.468***	0.723***	0.622**	1.042**
	(2.07)	(3.17)	(2.58)	(3.52)	(2.37)	(1.66)	(2.54)	(2.42)	(2.38)	(2.14)
GENP		-0.688*			-0.720**			-0.802*		
		(-1.85)			(-2.16)			(-1.73)		
PENP			-0.846**			-1.043**			-1.056*	
			(-2.19)			(-2.05)			(-1.73)	
DISPR				0.283***			0.303***			0.654
				(4.05)			(4.04)			(1.50)
TURNOVER					0.090	-0.646	1.023	1.306	1.484	2.306
					(0.16)	(-0.73)	(1.16)	(0.83)	(1.04)	(1.04)
MKTCAP					3.146**	3.163**	2.490*	1.047	1.596	1.502
					(2.34)	(2.19)	(1.83)	(0.49)	(0.66)	(0.71)
COMMON LAW								2.900	2.590	7.427*
								(1.34)	(1.20)	(1.67)
FRENCH LAW								-1.960	-2.656	2.222
								(-0.71)	(-1.08)	(0.57)
SCAND. LAW								-2.837	-2.231	3.436
								(-0.99)	(-0.75)	(0.85)
α	6.430	6.455	6.867	9.112	7.034	7.473	9.718	8.948	9.446	18.208
Obs.	209	209	209	209	192	192	192	192	192	192
Log likelihood	4.36	5.37	6.19	10.99	6.46	7.46	11.96	11.40	11.50	18.57
Wald Test	5.57	10.80**	15.64***	28.380***	33.04***	36.66***	63.300***	677.15***	183.61***	54.770***

Note: This table reports estimated coefficients and associated t-statistics (in parentheses) for the Weibull model. The dependent variable is the hazard rate of observing median (per country) privatization revenues. The maximum value of the log-likelihood function is reported below. The Wald Test refers to the null hypothesis of all the coefficients being jointly equal to 0. *, ** and *** denote statistical significance at 90, 95 and 99% confidence level, respectively.

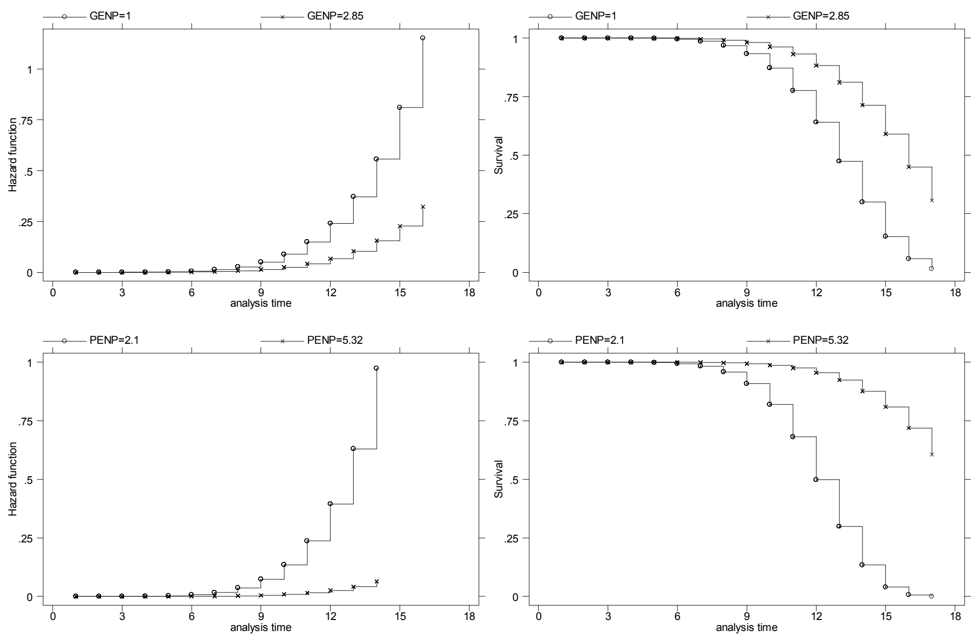
government or the parliament). These changes imply a reduction in the hazard rate of between 52 and 70 percent (according to Weibull and Cox estimates, respectively) if the additional party enters the executive, and of something more (62 to 72 percent) if it gains representation in the parliament.[13]

Finally, to get more a sense of what these numbers imply, in Figure 4.3 the hazard rate and the survival function $S(T \mid x) = 1 - F(T \mid x)$ estimated for the parametric model are plotted against the delay to reform. The top-left panel plots the hazard rates when *GENP* is equal to 1 and 2.85, which are, respectively, the averages for the clusters of cabinet and consensus countries identified in Figure 4.2; the top-right panel does the same for the survival functions; finally, the bottom panels repeat the exercise by considering average values of *PENP*.

The qualitative and quantitative differences between the two cases, cabinet and consensus, are striking. The hazard rate is always increasing because of $\widehat{\alpha} > 1$. However, in the cabinet system the increase in the slope is noticeable already after about 5 years,

[13]The percentage change in the hazard rate is computed as $\Delta(\beta^k) = 100\left(\exp \beta^k - 1\right)$. To see this, consider two vectors of covariates x and $x' = x + e^k$, where e^k is the unit vector having its k-th element equal to 1 and all the other ones equal to 0. Recalling the proportionality assumption (4.3) and equation (4.4), $\frac{\lambda'}{\lambda} = \exp\left[(x' - x)\prime\beta\right] = \exp \beta^k$, which implies $100\left(\frac{\lambda' - \lambda}{\lambda}\right) = 100\left(\exp \beta^k - 1\right)$, where $100\left(\frac{\hat{\lambda} - \lambda}{\lambda}\right)$ is nothing else than the percentage change in the hazard rate implied by increasing the k-th element of x by one unit (one party, in our case).

Figure 4.3: Hazard rates and survival functions



which is at the beginning of the 80s, and reform becomes almost sure in the early 90s, according to both *GENP* and *PENP* estimates of political fragmentation. On the contrary, in the consensus system privatization never gains momentum. Indeed, according to *GENP* estimates, by the end of the sample period the predicted hazard rate is still below 40%; as a consequence, 25% of consensus countries should have not reformed yet. Measuring political fragmentation by *PENP* entails even greater difference between the two cases.

The results of both Cox and Weibull estimates strongly support the empirical implications of the war of attrition model. The data show that indeed greater political fragmentation entails longer delays to implement large scale divestiture. We now check further the robustness of these results by relaxing the proportionality assumption (4.3). Table 5 shows the results for the Log-normal model. The estimates for the coefficients of the variables of interest remain always statistically significant at conventional levels. Moreover, their absolute value is always close to the ratio $-(\beta/\alpha)$ estimated by the Weibull model. Thus, while proportionality of the hazard rate is convenient both in terms of tractability and interpretation of the results, the qualitative and quantitative results we discussed above do not rely heavily on such assumption.

Table 4.5: Log-normal model

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
CONSTANT	3.001***	2.853***	2.660***	3.311***	2.740***	2.549***	3.164***	3.454*	3.234***	3.151***
	(11.83)	(12.80)	(10.31)	(13.15)	(9.70)	(9.51)	(10.67)	(11.17)	(9.19)	(20.99)
SOE	0.011	0.013*	0.012	0.011	0.017*	0.015*	0.013*	0.001	0.003	0.014**
	(0.90)	(1.67)	(1.52)	(1.62)	(1.80)	(1.85)	(1.75)	(0.12)	(0.29)	(2.29)
DEFICIT	0.007	0.006	-0.001	-0.101	0.003	-0.002	-0.015	-0.010	-0.014	-0.023***
	(0.54)	(0.39)	(-0.10)	(-1.13)	(0.17)	(-0.08)	(-1.55)	(-1.31)	(-1.58)	(-3.32)
GDP	-0.00001	-7.72e-06	-0.00001	-5.37e-06	-2.01e-06	0.00001	8.97e-07	-2e-05	-3e-05*	2.43e-06
	(-0.42)	(-0.72)	(-1.30)	(-0.85)	(-0.23)	(-1.24)	(0.21)	(-1.57)	(-1.76)	(0.28)
PARTISAN	-0.088**	-0.109***	-0.091***	-0.083***	-0.091**	-0.085**	0.051**	-0.102***	-0.091***	-0.062***
	(-2.54)	(-2.89)	(-3.24)	(-3.16)	(-1.96)	(-2.32)	(-2.03)	(-2.98)	(-2.94)	(-3.58)
GENP		0.216**			0.182*			0.105**		
		(1.96)			(1.80)			(2.42)		
PENP			0.183**			0.183***			0.127*	
			(2.32)			(2.66)			(1.88)	
DISPR				-0.037***			-0.035***			-0.027***
				(-3.39)			(-4.12)			(-3.89)
TURNOVER					0.102	0.196	-0.081	-0.099	-0.116	-0.115
					(0.72)	(1.50)	(-1.03)	(-0.70)	(-0.92)	(-1.31)
MKTCAP					-0.467	-0.403	-0.469**	0.027	0.027	-0.180
					(-1.53)	(-1.57)	(-2.09)	(0.17)	(0.16)	(-1.51)
COMMON LAW								-0.397***	-0.283**	-0.356***
								(-3.21)	(-1.96)	(-4.51)
FRENCH LAW								0.253*	0.362**	-0.012
								(1.84)	(2.21)	(-0.09)
SCAND. LAW								0.397**	0.376*	-0.020
								(2.00)	(1.74)	(-0.12)
Obs.	209	209	209	209	192	192	192	192	192	192
Log likelihood	2.41	4.59	5.76	9.93	5.25	6.83	11.52	12.52	12.38	16.64
Wald Test	7.55	9.40*	14.41**	16.92***	7.99	17.33**	26.02***	508.86***	251.19***	618.44***

Note: This table reports estimated coefficients and associated t-statistics (in parentheses) for the Lognormal model. The dependent variable is the log of the delay between the first privatization ever (in 1977) and the year in which median (per country) privatization revenues are observed. The maximum value of the log-likelihood function is reported below. The Wald Test refers to the null hypothesis of all the coefficients being jointly equal to 0. *, ** and *** denote statistical significance at 90, 95 and 99% confidence level, respectively.

4.5 Conclusions

Political economy has recently provided several models to understand the determinants of economic reform. Yet empirical analysis on this topic faces severe measurement problems in finding suitable variables to gauge the economic relevance of reform processes and to link political-economic equilibria to factual institutional settings.

The big privatization wave that started in the United Kingdom in the late 70s, swept the world in the last two decades, and declined abruptly right after the turn of the century provides an ideal experiment to analyze empirically the timing of large-scale reform. Importantly, research is not limited by data availability given that reliable sources provide comprehensive information on privatization processes both across countries and overtime.

War of attrition models suggest that political fragmentation is a fundamental factor in explaining the timing of reform. In particular, these models posit a positive relationship between the delay of reform and the number of agents with veto power in a given political system.

In this paper, we first identify a formal link between the theoretical war of attrition model and the statistical duration model, then we study the delay of privatization in a large sample of developed countries over the 1977-2002 period. Our results confirm the empirical validity of war of attrition model: large scale divestitures is delayed longer the larger the number of parties and the greater the proportionality of the electoral rule.

The estimated coefficients of these variables are significant and robust across different specifications. Moreover, the hazard rates predicted by the model, conditional on our proxies for political fragmentation, generate expected delays of privatization that are consistent with those observed in reality in more versus less fragmented democracies.

Appendix A: variables' definitions and sources

DEFICIT: Central government deficit as percentage of GDP. Source: International Financial Statistics, IMF.

DISPR: Disproportionality index computed over the difference between the shares of votes and seats held by each party. Mathematical formulation of the index is presented in section 4.3. Source: Original dataset from Lijphart (1999) updated using the review Electoral Studies, various issues; Banks et al. (2002); Elections Around the World.

GDP: Ratio of Gross Domestic Product in constant 1996 US Dollars to population. Total population counts all residents regardless of legal status or citizenship.. Source: World Development Indicators.

GENP (*PENP*): Concentration index computed over government parties' seats in the legislative (executive) chamber. Mathematical formulation of the index is presented in section 4.3. Source: Electoral Studies, various issues; Banks et al. (2002); Elections Around the World.

MKTCAP: Stock market capitalization to Gross Domestic Product. Stock market capitalization in year t is calculated as the average between the end-of-year market capitalization deflated by the end-of-year Consumer Price Index in year t and $t-1$. Stock market capitalization refers to a country's main stock exchange. Source: Beck et al. (1999).

PARTISAN: Government's ideology. It is computed as the weighted average of the ideology attached by Huber and Inglehart (1995) to parties forming the government coalition. Mathematical formulation of the index is presented in section 4.3. Source: same as *GENP*.

REVGDP: Total revenues from privatization, both Public Offers and Private Sales, as a fraction of GDP. Source: Securities Data Corporation, Privatization Barometer.

SOE: Average of the SOE value added as a percentage of GDP in the three years preceding the first privatization reported in SDC. Source: World Development Indicators.

T: Delay to reform. Defined for each country as $T = t - 1977$, where

$$t = min\left\{t : REVGDP \geq median(REVGDPs),\ s = 1977,,2002\right\}.$$

Source: same as *REVGDP*.

TURNOVER: Stock market total value traded to total market capitalization. Total market value in year t is deflated by the Consumer Price Index in year t. Market capitalization in year t is calculated as the average between the end-of-year market capitalization deflated by the end-of-year Consumer Price Index in year t and t-1. Trading value and market capitalization refer to a country's main stock exchange. Source: Beck et al. (1999).

LEGAL ORIGIN: binary variables for the origin of the legal system. Source: La Porta et al. (1998).

Appendix B: Comparison between alternative political datasets

			Tsebelis									**FPD**								
			obs	seats diff				% matched seats				obs	seats diff				% matched seats			
				P1	P2	P3	GOV	P1	P2	P3	GOV		P1	P2	P3	GOV	P1	P2	P3	GOV
FPD		obs	126																	
	seats diff	P1		3.73																
		P2			2.76															
		P3				1.81														
		GOV					6.36													
	% matched seats	P1						80.16												
		P2							81.75											
		P3								84.13										
		GOV									66.67									
World Bank DPI		obs	103									109								
	seats diff	P1		28.58									29.64							
		P2			13.85									13.3						
		P3				4.21									5.01					
		GOV					30.79									30.46				
	% matched seats	P1						4.85									4.59			
		P2							43.69									44.95		
		P3								66.99									66.06	
		GOV									2.91									4.59

This table presents a comparisons between the electoral data reported in the FEEM Political Dataset (http://www.feem.it/fpd), the World Bank DPI by Beck et al. (2001) and Tsebelis (2004). Column "obs." reports the number of common observations (i.e., elections) between two datasets; "seats diff." is the average difference between the number of seats reported for, respectively, the first, second and third party forming the executive, and for the government as a whole; finally, "% matched seats" is the percentage of cases in which the number of seats coincides exactly for two datasets.

Chapter 5

Do immigrants cause crime?

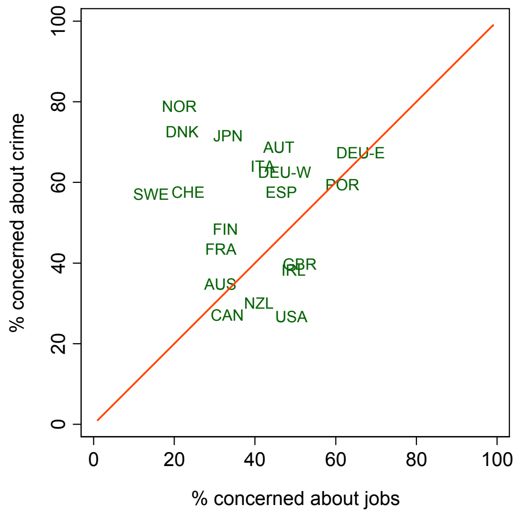
Immigration is a contentious issue in all destination countries for at least two reasons. First, worker flows from countries characterized by a different composition of the labor force may have significant redistributive consequences for the native population. Second, there are widespread concerns that immigrants increase crime rates. While the economic literature has devoted much attention to the first issue, the second one has remained largely unexplored.

However, citizens and policymakers in recipient countries seem more concerned about the impact of immigrants on crime. Figure 5.1 shows the results of the National Identity Survey carried out in 1995 and 2003 by the International Social Survey Programme. It clearly emerges that, within OECD countries, the majority of the population is worried that immigrants increase crime rates. In most cases this fraction is greater than that of people afraid of being displaced from the labor market.

The economic theory of crime (Becker, 1968; Ehrlich, 1973) also provides several underpinnings for the existence of a relationship between immigration and crime. Immigrants and natives may have different propensities to commit crime because they face different legitimate earning opportunities, different probabilities of being convicted and different costs of conviction. Yet, the direction of such effects is unclear. For example, immigrants may experience worse labor market conditions (LaLonde and Topel, 1991; Borjas, 1998) but higher costs of conviction (Butcher and Piehl, 2005). Hence, identifying this relationship is ultimately an empirical issue.

In this paper we investigate the empirical relationship between immigration and crime across Italian provinces during the period 1990-2003. As we discuss in the next section, this sample displays some interesting features for the purpose of our analysis. First, the dramatic increase in Italy's immigrant population was mainly driven by political turmoil in neighboring countries, which provides a source of exogenous variation to address causality from immigration to crime. Second, Italian authorities have implemented several massive regularizations of previously unofficial immigrants, which allow us to assess the extent of measurement errors induced by immigrants who reside in Italy without holding a valid residence permit.

Figure 5.1: Opinions about immigrants (crime vs. labor market concerns)



Note: This graph presents the results of the "National Identity" survey conducted in 1995 and 2003 by the International Social Survey Programme. It plots, for each country, the percentage of people who declared to "*Strongly Agree*" or "*Agree*" that "*Immigrants increase crime rates*" (on the vertical axis) against percentage of people who declared to "*Strongly Agree*" or "*Agree*" that "*Immigrants take jobs away from natives*" (on the horizontal axis), together with the 45-degree line.

In Section 5.2 we present the results of OLS regressions. The identification of the effect of immigration on crime relies on within-province changes in both variables, controlling for other determinants of criminal activity and for year-specific unobserved shocks. This two-way fixed effects specification also removes errors in the measurement of immigration and crime (due to unofficial immigrants and non-reported crimes) that are constant within provinces or years. According to these estimates, a 1% increase in the immigrant population is associated with a 0.1% increase in the total number of criminal offenses. Once we distinguish between categories of crime, the effect seems particularly strong for property crimes.

In Section 5.3 we ask whether this evidence can be attributed to a causal effect of immigration on crime. In particular, the location choice of immigrants could respond to unobserved factors that are themselves correlated with crime; as a result, OLS estimates may be biased. To solve this problem, we exploit differences in the intensity of migration across origin countries as a source of (exogenous) variation in the distribution of immigrants in Italy. In particular, we use changes in the immigrant population in the rest of Europe as an instrument for changes in immigration across Italian provinces. Our identification strategy relies on the fact that the supply-push component of migration by nationality is common to flows toward all destination countries. At the same time, flows toward the rest of Europe are exogenous to demand-pull factors in Italian provinces. Variation across provinces of supply-driven shifts in the immigrant population results from differences in the beginning-of-period distribution of immigrants by origin country. First stage estimates confirm that our instrument provides a strong statistically significant prediction of immigration in Italy.

After taking into account the endogeneity of immigration, we find that the effect on total crime or property crime is not significantly different from zero. When we examine different types of property crime, we only find an effect on robberies. These are a very small fraction of crimes, which is why we do not find an effect on total crime rates. As discussed in Section 5.4, these results appear robust with respect to measurement error, the spatial correlation of provincial crime data and heterogeneity across different nationalities of immigrants.

This paper is related to the empirical literature on the effects of immigration in the host countries. This lively research area has emphasized the labor market competition between immigrants and natives (surveys include Borjas, 1994; Friedberg and Hunt, 1995; Bauer and Zimmermann, 2002; Card, 2005) and the effects of immigration on fiscal balances (Storesletten, 2000; Lee and Miller, 2000; Chojnicki et al., 2005). We contribute to this literature by estimating the effect of immigration on crime.

A few previous papers have investigated the existence of this relationship in the United States. At the micro level, Butcher and Piehl (1998b, 2005) find that current immigrants have lower incarceration rates than natives, while the pattern is reversed for the early 1900s (Moehling and Piehl, 2007). At the aggregate level, Butcher and Piehl (1998a) look at a sample of U.S. metropolitan areas over the 1980s and conclude that new immigrants' inflows had no significant impact on crime rates. Finally, Borjas et al. (2006) argue that recent immigrants have contributed to the criminal activity of native black males by displacing them from the labor market. We complement these findings by providing the first available evidence on a European country in which, as suggested by Figure 5.1, crime concerns are more widespread and, therefore, they are likely to play a greater role for the setting of immigration restrictions.

5.1 Immigration and crime in Italy: data and measurement

We assembled data on immigration and crime for all 95 Italian provinces during the period 1990-2003. Italian provinces correspond to level 3 in the Eurostat Nomenclature of Territorial Units for Statistics (NUTS) classification; they are comparable in size to U.S. counties. In 1995, eight new provinces were created by secession. In order to keep our series consistent, we attribute their post-1995 data to the corresponding pre-1995 province.

5.1.1 Crime rates

Our measure of criminal activity is the number of crimes reported by the police to the judiciary authority over the total province population, published yearly by Italy's National Institute of Statistics (ISTAT). Reported crimes are disaggregated by type of criminal offense: violent crimes, property crimes (robbery, common theft, car theft) and drug-related crimes. Availability of these data determined our sample period. In 2004, in fact, a new national crime recording standard was adopted, implying a lack of comparability of data before and after that year (ISTAT, 2004, p.27).

Reported crimes underestimate the true (unobserved) number of committed crimes, which may bias econometric estimates of the effect of those determinants of criminal activity that are correlated with the extent of under reporting. This problem is well known in the crime literature and it is usually dealt with by taking logarithms of crime rates and exploiting the panel structure of data to include fixed effects for geographical areas and time periods; see, for instance, Ehrlich (1996), Levitt (1996), Gould et al. (2002) and Oster and Agell (2007). This approach sweeps out measurement errors that are constant within geographical areas (over time) or within periods (across areas). This is most likely the case for many sources of under reporting (e.g. law enforcement, culture, etc.), which in turn implies that

$$crime^*_{it} = \theta_i + \theta_t + crime_{it}, \tag{5.1}$$

where $crime^*_{it}$ and $crime_{it}$ are the logarithms of actual and reported crimes over the population in province i and year t, respectively, and θ_i and θ_t are province- and year-fixed effects. Therefore, we will use $crime_{it}$ as a proxy for the true (unobserved) crime rate. Accordingly, *total*, *violent*, *property* and *drug* will denote the logarithms of reported crimes over the total population for each category of criminal offense.

5.1.2 Immigration

The first law regulating the inflows of foreigners was approved in 1990 and subsequently amended in 1998 and 2002. Throughout this period, Italian migration policy has remained grounded on the residence permit, which allows the holder to stay legally in the country for a given period of time. We have drawn directly on police administrative records for recovering the number of valid residence permits over the population, which will serve as our measure of immigration.

This measure neglects the presence of unofficial immigrants, who reside in the country without holding a valid residence permit. Most importantly, correlation of unofficial immigration with the level of criminal activity would lead to a bias in the estimates of the effect of immigration on crime. However, the combination of logarithms and fixed effects may attenuate the influence of this source of measurement error too. Analogously to the case of crime, if official immigrants are proportional to total immigrants and the constant of proportionality is the product of province- and year-specific constants, it follows that

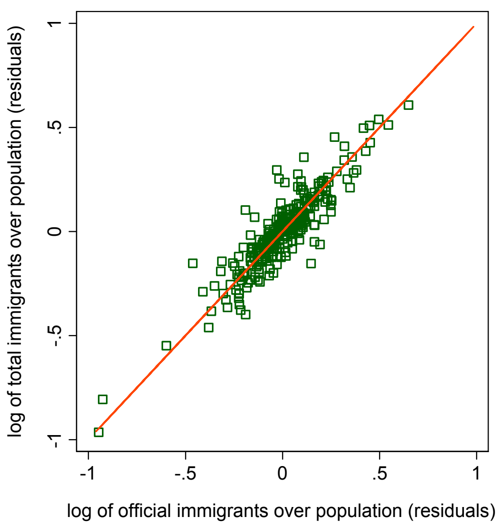
$$migr^*_{it} = \mu_i + \mu_t + migr_{it}, \tag{5.2}$$

where $migr^*_{it}$ and $migr_{it}$ are the logarithms of total and official immigrants over the population, respectively, and μi and μ_t are province- and year-fixed effects.

Regularizations of previously unofficial immigrants provide us with the opportunity to assess the accuracy of this approximation. In these occasions, unofficial immigrants already residing in Italy can apply for a valid residence permit. The last three regularizations took place in 1995, 1998 and 2002, and involved 246, 217 and 700 thousand individuals, respectively. The acceptance rate of applications was always close to 100%, so that foreigners had clear incentives to report their irregular status. Hence, under reporting may be less serious and less correlated with other variables than in survey data and in apprehension statistics.[1]

Therefore, we extracted from police administrative archives also the number of applications for regularization in each province. These data allowed us to reconstruct the log of total (official plus unofficial) immigrants over province population in the three years in which there was a regularization. The relationship between $migr^*_{it}$ and $migr_{it}$ in (5.2) (after regressing both variables on province- and year-fixed effects) is presented in Figure 5.2. The OLS estimated coefficient of $migr_{it}$ is 0.92, thus very close to 1; the R^2 coefficient is 99%. These two findings confirm that logarithms and fixed effects remove most of the measurement error induced by the use of official immigrants only. Since total immigrants would be unobserved out of regularization years, we will use the (log of) official immigrants instead.

Figure 5.2: Total and official immigrants



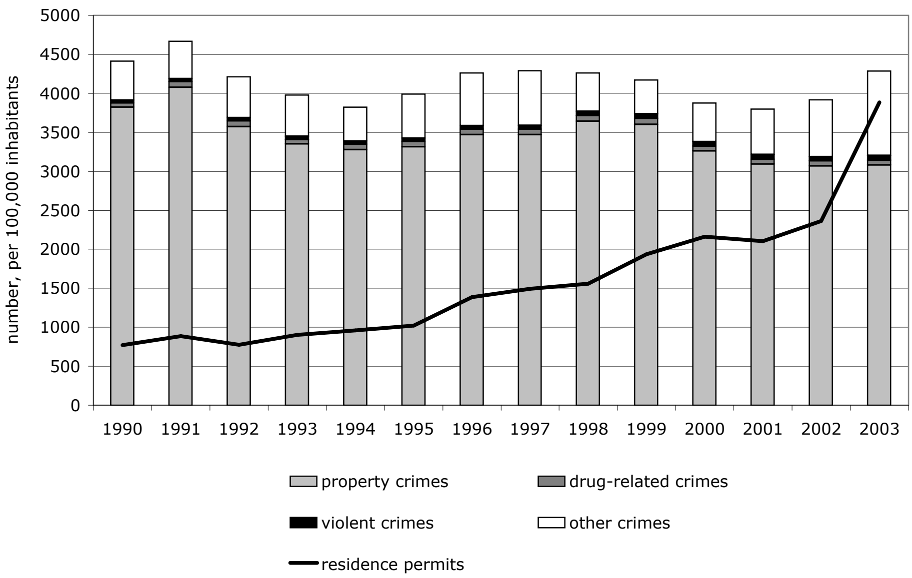
Note: This figure plots the residuals obtained after regressing the log of total immigrants (on the vertical axis) and the log of official immigrants (on the horizontal axis) on province- and year-fixed effects, together with the 45-degree line. The (estimated) number of total immigrants is given by the sum of residence permits and applications for regularization. The source of data on both residence permits and demands for regularization is the Italian Ministry of the Interior.

[1]In any case, all these alternative measures of unofficial immigration are strongly correlated with each other, as we discuss further in Section 5.4.

5.1.3 Trends in immigration and crime

Over the period 1990-2003, the number of residence permits rose by a factor of 5, from 436,000 (less than 1% of the total population) to over 2.2 million (4% of the population). This dramatic increase was mainly driven by the collapse of the Soviet Union and the Balkan Wars (Del Boca and Venturini, 2003). Indeed, immigration from Eastern Europe grew at a rate of 537%, as compared with 134% from Northern Africa and 170% from Asia. Accordingly, our estimating strategy will exploit the role of external factors in origin countries to identify the causal effect of immigration on crime.

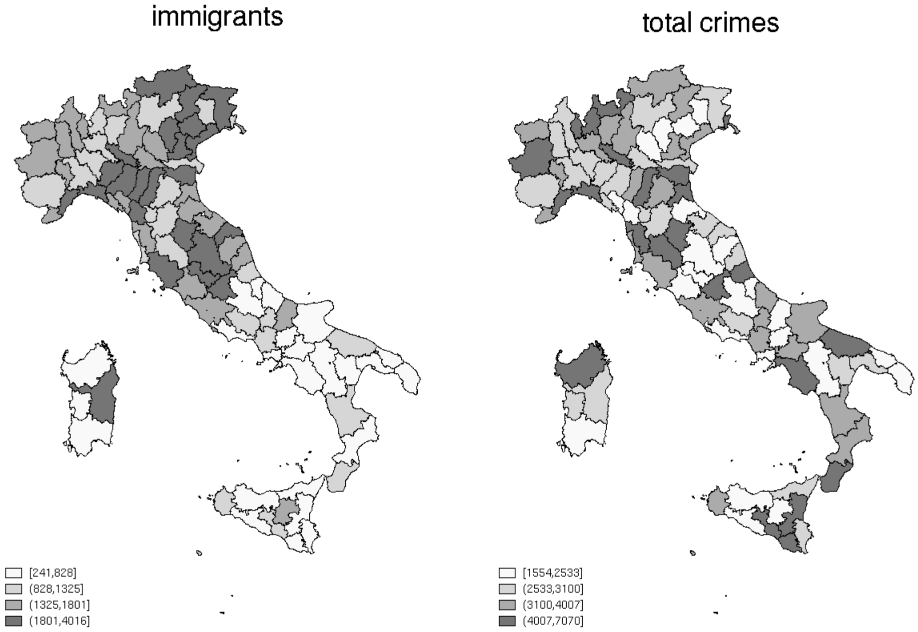
Figure 5.3: Immigration and crime over time



Note: This graph shows the evolution over time of reported crimes and residence permits in Italy. The histogram refers to the number of reported crimes per 100,000 inhabitants, distinguishing between different categories of criminal offenses. The line refers to the number of residence permits awarded to immigrants in Italy, per 100,000 inhabitants. The source of data on reported crimes and residence permits are ISTAT and the Italian Ministry of the Interior, respectively.

During the same period, the level of criminal activity did not display any significant trend. At a first glance, therefore, the two variables do not appear to be systematically correlated over time (Figure 5.3). On the other hand, immigration is positively correlated with crime across provinces (Figure 5.4). However, this finding could be due to the fact that both variables respond to other (omitted) factors. For instance, higher wealth in Northern Italy seems to encourage both immigration and property crimes, which represent 83% of all criminal offenses in our sample. In the next section we move beyond these simple correlations and into multivariate econometric analysis.

Figure 5.4: Immigration and crime across provinces



Note: These figures show the distribution, across Italian provinces, of the number of immigrants and reported crimes per 100,000 inhabitants. All variables are yearly averages during the period 1990-2003. Provinces are colored according to which quartile of the distribution they belong to; darker colors refer to higher values. The extremes of each quartile, along with the corresponding color, are reported at the bottom of each map. The sources of data for residence permits and reported crimes are ISTAT and the Italian Ministry of the Interior, respectively.

5.2 Panel analysis

Identifying the effect of migration on crime requires to control for other factors that may affect both variables. Taking into account the measurement issues discussed in the previous section, our main estimating equation is

$$crime_{it} = \beta migr_{it} + \gamma' X_{it} + \phi_i + \phi_t + \varepsilon_{it}, \tag{5.3}$$

where $crime_{it}$ is the log of the crime rate reported by the police in province i during year t; $migr_{it}$ is the log of immigrants over population; X_{it} is a set of control variables; ϕ_i and ϕ_t are province- and year-fixed effects; finally, ε_{it} is an error term. We are mainly interested in identifying the coefficient β.

The set of observables X_{it} comprises demographic and socioeconomic determinants of crime.[2] Demographic variables include the log of resident population in the province, *pop*. Since equation (5.3) includes province fixed effects, *pop* implicitly controls for population density, which is considered a key determinant of the level of criminal activity (Glaeser and Sacerdote, 1999). For the same reason, we also include the share of the population

[2]Freeman (1999), Eide et al. (2006) and Dills et al. (2008) review the empirical literature on the determinants of crime.

Table 5.1: Descriptive statistics

	obs.	*mean*	*std. dev.*	*min*	*max*
Residence permits per 100,000 inhabitants	1330	1353	1187	44	7873
Total crimes per 100,000 inhabitants	1330	3388	1350	1072	13404
Violent crimes per 100,000 inhabitants	1330	50	29	1	230
Property crimes per 100,000 inhabitants	1330	2615	1216	442	7879
robberies	1330	37	41	2	385
thefts	1330	1943	909	321	6049
car thefts	1330	287	281	29	1648
Drug-related crimes per 100,000 inhabitants	1330	64	45	5	918
migr	1330	-4.64	0.85	-7.73	-2.54
total	1330	-3.45	0.37	-4.54	-2.01
violent	1330	-7.75	0.55	-11.19	-6.07
property	1330	-3.75	0.45	-5.42	-2.54
robbery	1330	-8.23	0.75	-10.74	-5.56
theft	1330	-4.04	0.45	-5.74	-2.81
car theft	1330	-6.22	0.83	-8.14	-4.11
drug	1330	-7.52	0.57	-9.84	-4.69
pop	1330	13.01	0.70	11.41	15.18
urban	1235	14.62	20.15	0.00	88.11
*male*1539	1330	18.01	1.23	14.41	21.03
gdp	1235	9.55	0.26	8.94	10.11
unemp	1045	10.43	7.09	1.68	33.16
clear-up (total crimes)	1330	30.54	10.47	9.20	82.75
clear-up (violent crimes)	1330	82.03	12.93	23.32	100.00
clear-up (property crimes)	1330	6.92	3.18	1.60	30.83
clear-up (drug-related crimes)	1330	95.77	5.61	37.71	100.00
partisan	1330	10.26	1.75	5.90	16.30

Note: This table reports the descriptive statistics for all dependent and explanatory variables across the 95 Italian provinces during the period 1990-2003.

Table 5.2: Correlation matrix

	migr	*total*	*violent*	*property*	*drug*	*pop*	*urban*	*male*1539	*gdp*	*unemp*	*clear-up*
total	0.356										
violent	0.289	0.377									
property	0.287	0.879	0.176								
drug	0.205	0.383	0.147	0.244							
pop	0.125	0.465	-0.062	0.598	0.036						
urban	0.208	0.550	0.091	0.557	0.171	0.475					
*male*1539	-0.471	-0.074	-0.120	0.036	-0.209	0.375	-0.053				
gdp	0.710	0.328	0.266	0.298	0.199	0.072	0.216	-0.407			
unemp	-0.607	-0.084	-0.179	-0.070	-0.136	0.138	0.057	0.425	-0.858		
clear-up	-0.276	-0.511	0.069	-0.723	0.070	-0.459	-0.404	-0.009	-0.288	0.113	
partisan	0.164	0.024	0.134	0.055	-0.065	0.116	-0.112	0.196	0.068	-0.060	-0.092

Note: This table reports the correlation matrix between the dependent and explanatory variables across the 95 Italian provinces during the period 1990-2003.

living in cities with more than 100,000 inhabitants, *urban*. Finally, since young men are said to be more prone to engage in criminal activities than the rest of the population

(Freeman, 1991; Levitt, 1998; Grogger, 1998), we add the percentage of men aged 15-39, $male1539$.

Turning to the socioeconomic variables, we include the (log of) real GDP per capita, *gdp*, and the unemployment rate, *unemp*, which measure the legitimate and illegitimate earning opportunities (Ehrlich, 1973; Raphael and Winter-Ember, 2001; Gould et al., 2002). As a proxy for the expected costs of crime, we follow Ehrlich (1996) in using the *clear-up* rate, defined as the ratio of crimes cleared up by the police over the total number of reported crimes (by type of offense). The political orientation of the local government may also affect the amount of resources devoted to crime deterrence (while being at the same time correlated with immigration restrictions at the local level).[3] Therefore, we include the variable $partisan$, which takes on higher values the more the local government leans toward the right of the political spectrum. Finally, fixed effects control for other unobserved factors that do not vary within provinces or years, including constants θ's and μ's in equations (5.1) and (5.2), respectively.

Detailed definitions and sources are presented for all of the variables in the Appendix. Table 5.1 shows some descriptive statistics and Table 5.2 reports the correlation matrix among all dependent and explanatory variables. The univariate correlation between the immigration and crime rates over the total population is positive for all types of crime.

OLS estimates on equation (5.3) are presented in Table 5.3 and suggest that the total crime rate is significantly correlated with the incidence of immigrants in the population. This relationship is robust to controlling for other determinants of crime. According to these findings, a 1% increase in the immigrant population is associated with a 0.1% increase of total crimes.

Distinguishing between types of crime, the effect is driven by property crimes, while violent and drug-related crimes are unaffected by immigration. In order to better uncover this relationship, in Table 5.4 we disaggregate property crimes further. It turns out that immigration increases the incidence of robberies and thefts. Since the latter represent about 60% of total crimes in our sample, the relationship between immigration and property crimes may be the main channel through which immigrants increase the overall crime rate.

However, there could be several reasons why the size of the immigrant population is systematically correlated with property crimes, some of which may not be adequately captured by control variables. Therefore, identifying causality requires a source of exogenous variation in the immigrant population, an issue that we tackle in the next section.

5.3 Causality

Even after controlling for other determinants of crime and for fixed effects, the distribution of the immigrant population across provinces could be correlated with the error term

[3]The distribution of residence permits across provinces is decided on a yearly basis by the government in accordance with provincial authorities.

Table 5.3: Panel regressions, baseline

	total	*violent*	*property*	*drug*	*total*	*violent*	*property*	*drug*
migr	0.103***	-.007	0.126***	-.190***	0.102***	0.003	0.084***	-.103
	(0.034)	(0.057)	(0.031)	(0.06)	(0.039)	(0.084)	(0.028)	(0.074)
pop					0.028	-.338	0.96	-2.550
					(0.641)	(1.660)	(0.718)	(1.552)
urban					0.003*	-.003	0.003	-.010***
					(0.002)	(0.003)	(0.003)	(0.002)
*male*1539					0.131***	0.236**	0.041	0.325***
					(0.045)	(0.11)	(0.053)	(0.108)
gdp					0.15	-.116	0.171	0.423
					(0.14)	(0.319)	(0.166)	(0.378)
unemp					-.004	0.011	-.007*	0.019*
					(0.004)	(0.01)	(0.003)	(0.01)
clear-up					-.004	-.008***	-.030***	0.0003
					(0.003)	(0.002)	(0.006)	(0.003)
partisan					0.007	0.045**	0.007	0.023
					(0.01)	(0.019)	(0.009)	(0.015)
Obs.	1,330	1,330	1,330	1,330	1,045	1,045	1,045	1,045
Provinces	95	95	95	95	95	95	95	95
Prov. FE	yes	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.153	0.266	0.162	0.171	0.220	0.321	0.302	0.189
F-stat.	21.54	9.69	17.70	35.76	14.81	7.37	11.68	17.26

Note: This table presents the results of OLS estimates on a panel of yearly observations for all 95 Italian provinces during the period 1991-2003. The dependent variable is the log of crimes reported by the police over the total population, for each category of criminal offense. The variable *migr* is the log of immigrants (i.e. residence permits) over province population. The sources of data for residence permits and reported crimes are ISTAT and the Italian Ministry of the Interior, respectively. All other variables are defined in the Appendix. Province and year fixed-effects are included in all specifications. Robust standard errors are presented in parenthesis. *, ** and *** denote rejection of the null hypothesis of the coefficient being equal to 0 at 90%, 95% and 99% confidence level, respectively.

for at least two reasons. First, our set of controls could neglect some time-varying, possibly unobserved demand-pull factors that are also correlated with crime. For instance, improvements in labor market conditions that are not adequately captured by changes in official unemployment and income could increase immigration and decrease crime, which would bias OLS estimates downward. On the other hand, economic decline could attract immigrants to some areas (e.g. because of lower housing prices) where crime is on the rise, which would bias OLS estimates upward. Finally, changes in crime rates across provinces could themselves have a direct effect on immigrants' location.

5.3.1 Methodology

In order to take these concerns into account, we adopt a Two-Stage-Least-Squares (2SLS) approach that uses the (exogenous) supply-push component of migration by nationality as an instrument for shifts in the immigrant population across Italian provinces. Supply-push factors are all events in origin countries that increase the propensity to emigrate;

Table 5.4: Panel regressions, property crimes breakdown

	robbery	*theft*	*car theft*	*robbery*	*theft*	*car theft*
migr	0.197***	0.14***	0.045	0.092*	0.093***	0.057
	(0.05)	(0.032)	(0.041)	(0.05)	(0.03)	(0.041)
pop				4.285***	1.155*	0.365
				(1.026)	(0.686)	(0.958)
gdp				-.155	0.113	0.611***
				(0.267)	(0.164)	(0.232)
unemp				-.022***	-.006*	-.003
				(0.007)	(0.003)	(0.005)
urban				0.0007	0.004	0.004**
				(0.004)	(0.002)	(0.002)
*male*1539				-.145*	0.052	0.1
				(0.084)	(0.053)	(0.072)
clear-up				-.005***	-.030***	-.005**
				(0.001)	(0.006)	(0.003)
partisan				0.006	0.007	-.003
				(0.013)	(0.009)	(0.011)
Obs.	1,330	1,330	1,330	1,045	1,045	1,045
Provinces	95	95	95	95	95	95
Prov. FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
R^2	0.156	0.146	0.296	0.241	0.28	0.323
F-stat.	14.91	15.12	23.31	14.17	9.77	14.72

Note: see table 5.3

examples include economic crises, political turmoil, wars and natural disasters (Card, 1990; Friedberg, 2001; Angrist and Kugler, 2003; Munshi, 2003; Saiz, 2007). Since these are both important in determining migration outflows and independent of regional differences within the host country, they have often been used as a source of exogenous variation in the distribution of the immigrant population.

In particular, several papers have constructed outcome-based measures of supply-push factors using total migration flows by nationality toward the destination country of interest; see, among others, Card (2001), Lewis (2005) and Ottaviano and Peri (2006). Variation of the instrument results from differences in the composition by nationality of the immigrant population across different areas within the destination country. The predictive power of the instrument exploits the fact that new immigrants of a given nationality tend to settle into the same areas as previous immigrants from the same country (see e.g. Munshi, 2003; Jaeger, 2006; McKenzie and Rapoport, 2007). For the same reason, however, total inflows by nationality could still be correlated with local demand-pull factors. As an extreme case, if all immigrants from a given country moved to the same Italian province, it would be impossible to disentangle push and pull factors based on total inflows by nationality.

To obviate this problem, our measure of supply-push factors will be based on bilateral migration flows toward European countries other than Italy. Specifically, we first take within-province differences of equation (5.3) and decompose $\Delta migr_{it} = migr_{it} - migr_{it-1}$ as

$$\Delta migr_{it} \approx \sum_{n} \omega^{n}_{it-1} \times \Delta \ln MIGR^{n}_{it} - \Delta pop_{it}, \tag{5.4}$$

where $\Delta \ln MIGR^{n}_{it}$ is the log change of the stock of immigrants of nationality n in province i between period $t-1$ and t; Δpop_{it} is the log change of province population; finally, ω^{n}_{it-1} is the share of immigrants of nationality n over total immigrants residing in province i in period $t-1$, i.e. $\omega^{n}_{it-1} = MIGR^{n}_{it-1} / \sum_{n'} MIGR^{n'}_{it-1}$. The first term on the right hand side of equation 5.4 is the weighted sum of the log changes of immigrants of each nationality into destination province i. These depend on both supply-push factors in each origin country (which affect that nationality in all provinces) and demand-pull factors in each province (which affect all nationalities in that province). In order to exclude the latter, we substitute $\Delta \ln MIGR^{n}_{it}$ with the log change of immigrants of nationality n in the rest of Europe, $\Delta \ln MIGR^{n}_{t}$. Hence, we define the predicted log change of immigrants over population in each province as

$$\widehat{\Delta migr}_{it} = \sum_{n} \omega^{n}_{it-1} \times \Delta \ln MIGR^{n}_{t}. \tag{5.5}$$

Since demand-pull factors in other European countries can be reasonably thought of as exogenous to variation in crime across Italian provinces, the correlation between $\Delta migr_{it}$ and $\widehat{\Delta migr}_{it}$ must be due solely to supply-push factors in origin countries.

To construct our instrument we use the log changes of the immigrant population from 13 origin countries in 11 European countries based on decennial census data in the host countries.[4] Figure 5.5 shows that the patterns of immigration toward the rest of Europe resemble those observed in Italy, which points to the importance of supply-push factors.

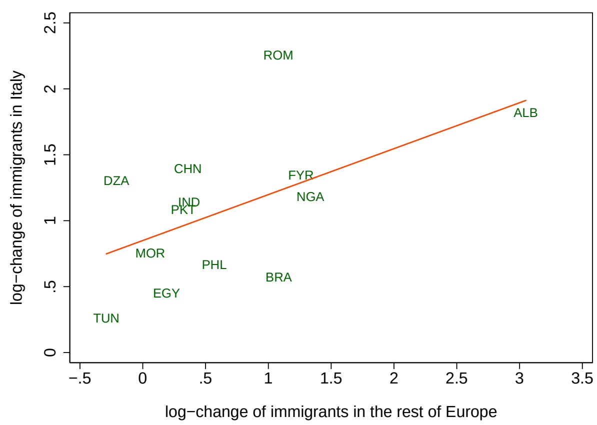
Indeed, the univariate regression confirms that our instrument fits well the actual changes in the immigrant population across provinces in the 1990s,

$$\Delta migr_{it} = \underset{(0.094)}{0.671} + \underset{(0.178)}{0.673}\widehat{\Delta migr}_{it},$$

where the numbers in parenthesis are the standard errors of the estimated coefficients. The F-statistic of the regression is equal to 14.24, which is above the lower bounds indicated by the literature on weak instruments (see Bound et al., 1995b; Stock and Yogo, 2002).

[4]Ideally, one would use total outflows from origin countries (possibly excluding inflows to Italy) as a measure of supply-push factors. Unfortunately, these data are not generally available. The destination countries for which we obtained census data are Austria, Belgium, Denmark, Finland, France, Greece, Netherlands, Norway, Spain, Sweden and Switzerland. Information on the stock of immigrants in those countries was available for the following nationalities: Albania, Algeria, Brazil, China (excluding Hong Kong), Egypt, India, Morocco, Nigeria, Pakistan, Philippines, Romania, Tunisia and the Former Yugoslavia. Overall, immigrants from these countries accounted for 48% and 56% of Italian residence permits in 1991 and 2001, respectively.

Figure 5.5: Immigration to Italy and the rest of Europe



Note: This figure plots the log change of the immigrant population in Italy during the 1991-2001 period (on the vertical axis) against the log change of immigrant population in other European countries during the same period (on the horizontal axis), by country of origin. Immigrant population in Italy is measured by the number of residence permits, as reported by the Italian Ministry of the Interior. Immigrant population in other European countries is measured using the 1991 and 2001 rounds of national census.

5.3.2 Results

Once equipped with this instrument for the immigrant population, we proceed to examine its effect on crime rates in the second stage. The results are reported in Tables 5.5 and 5.6. For the sake of comparability between OLS and 2SLS, in each table we also present OLS estimates on the cross section of log changes between 1991 and 2001.

While the OLS estimates on 10-year changes are broadly consistent with panel estimates using all years, the 2SLS estimates present significant differences. First, the effect of immigration on the total number of criminal offenses is smaller and not statistically significant anymore, and the same is true for property crimes. Once we distinguish between different typologies of property crime (Table 5.6), immigration has no significant effect on theft. On the other hand, the effect on robberies is still significant. Indeed, its magnitude is much greater than the OLS estimate. This finding may point to the existence of demand-pull factors (not captured by control variables) that have opposite effects on immigration and on the incidence of robberies.[5]

Overall, these results suggest that the causal effect of immigration on either violent, property or drug-related crimes is not significantly different from zero. Robberies are the only type of criminal activity that we found to be positively and significantly affected by immigration. According to our estimates, the incidence of robberies varies approximately one-to-one (in percentage terms) with the ratio of immigrants over population. Yet, within

[5] An alternative explanation could be that OLS estimates suffer from attenuation bias due to measurement errors in the immigrant population. However, if this were the reason, we should observe an analogous bias for all types of crime, which does not seem to be the case.

Table 5.5: Ten-year difference regressions

Panel A: Second-stage

	Ten-year differences: OLS				Ten-year differences: IV			
	$\Delta total$	$\Delta violent$	$\Delta property$	$\Delta drug$	$\Delta total$	$\Delta violent$	$\Delta property$	$\Delta drug$
Δ *migr*	.1558***	-.0389	.1362***	-.2680***	.1051	-.0964	.0456	-.1852
	(.0492)	(.1120)	(.0452)	(.0934)	(.1874)	(.4474)	(.1498)	(.2990)
Obs.	95	95	95	95	95	95	95	95
R^2	0.241	0.062	0.276	0.219				
F-stat.	5.40	1.56	6.95	4.95				

Panel B: First-stage

$\widehat{\Delta migr}_{it}$					0.5223***	0.4585***	0.5346***	0.5446***
					(0.1441)	(0.1672)	(0.1604)	(0.1620)
F-stat. (excl. instr.)					13.14	7.52	11.11	11.30

Note: The top panel of this table presents the results of OLS and IV (second-stage) estimates on the cross section of ten-year differences between 1991 and 2001 across all 95 Italian provinces. The dependent variable is the log change of the number of crimes reported by the police over the total population, for each category of criminal offense. The variable $\Delta migr$ is the log change of immigrants (i.e. residence permits) over province population. The bottom panel reports first-stage estimates of IV regressions. The first-stage instrument, $\Delta \ln MIGR^{IV}$, is the weighted sum of the log changes of the immigrant population by nationality in other European countries. The weights are the shares of permits held by each nationality over the total permits in that province in 1990 (see equation 5.5 in the main text). The ten-year differences of all control variables in Tables 5.3 and 5.4 are always included, both in the first and second stage. The sources of data for residence permits and reported crimes are ISTAT and the Italian Ministry of the Interior, respectively. Immigrant population in other European countries is measured using the 1991 and 2001 rounds of national census. The F-statistic for excluded instruments refers to the null hypothesis that the coefficient of the excluded instrument is equal to zero in the first stage. Robust standard errors are presented in parenthesis. *, ** and *** denote rejection of the null hypothesis of the coefficient being equal to 0 at 90%, 95% and 99% confidence level, respectively.

Table 5.6: Ten-year difference regressions, property crimes breakdown

Panel A: Second-stage

	Ten-year differences: OLS			Ten-year differences: IV		
	$\Delta robbery$	$\Delta theft$	$\Delta car\ theft$	$\Delta robbery$	$\Delta theft$	$\Delta car\ theft$
$\Delta migr$	.3508***	.1555***	-.0328	1.0234***	.1437	-.1167
	(.0890)	(.0453)	(.0807)	(.3602)	(.1509)	(.2412)
Obs.	95	95	95	95	95	95
R^2	0.193	0.308	0.124			
F-stat.	4.14	7.87	2.36			

Panel B: First-stage

$\widehat{\Delta migr}_{it}$				0.5528***	0.5311***	0.5549**
				(0.1626)	(0.1593)	(0.1599)
F-stat. (excl. instr.)				11.56	11.11	12.04

Note: see table 5.5

our sample robberies represent only 1.8% and 1.5% of property and total crimes, respectively, which explains why the incidence of neither property nor total crimes is significantly related to immigration.

5.4 Robustness

Our findings are subject to several caveats, the most significant of which concern the measurement of the immigrant population. A first issue relates to its composition by nationality. In order to avoid arbitrary classifications, our measure includes all residence permits. On the other hand, most crime concerns are directed toward immigrants from developing countries. While it is beyond the scope of this paper to investigate the relationship between nationality and propensity to commit crime, one may wonder whether adopting this broader definition introduces error in the measurement of those immigrants that could be more at risk of committing crime.[6] Therefore, we checked the robustness of our estimates to using only residence permits awarded to immigrants from developing countries (as defined by ISTAT), $migr_{it}^{dc}$. The results are presented in Tables 5.7 and 5.8, and are remarkably similar to those obtained using all residence permits.

Table 5.7: Robustness, immigrants from developing countries

Panel A: Second-stage

	Ten-year differences: OLS				Ten-year differences: IV			
	$\Delta total$	$\Delta violent$	$\Delta property$	$\Delta drug$	$\Delta total$	$\Delta violent$	$\Delta property$	$\Delta drug$
$\Delta \ln migr^{dc}$	.1477***	-.0478	.1321***	-.2462**	.1009	-.0934	.0438	-.1779
	(.0506)	(.1155)	(.0487)	(.0936)	(.1797)	(.4345)	(.1435)	(.5669)
Obs.	95	95	95	95	95	95	95	95
R^2	0.236	0.062	0.273	0.210				
F-stat.	5.16	1.57	6.73	4.66				

Panel B: First-stage

$\widehat{\Delta migr}_{it}$					0.5439***	0.4730***	.5570***	0. .5670***
					(0.1533)	(0.1735)	(0.1698)	(0.1714)
F-stat. (excl. instr.)					12.59	7.44	10.76	10.94

Note: see table 5.5. The variable $\Delta migr^{dc}$ is the log change of immigrants from developing countries over the province population.

Also, differences among nationalities could explain the discrepancy between OLS and 2SLS estimates. The latter are based on a subset of nationalities (those for which we found Census data for other European countries). Therefore, if the excluded nationalities had a higher propensity to commit crime than those included in the instrument, that could cause the observed drop in magnitude and significance from OLS to 2SLS (Imbens and Angrist, 1994). In order to check whether this is the case, we ran OLS regressions again including in the measure of immigration only those nationalities included also in the instrument. Results are reported in Tables 5.9 and are not significantly different from those in Tables

[6]This measurement issue is particularly relevant for Italy. In our sample, about 85% of all immigrants from outside developing countries came from the U.S. and Switzerland. These are very specific groups: the first includes mostly U.S. military servants, the second Swiss citizens who commute daily between Switzerland and Italy.

Table 5.8: Robustness, immigrants from developing countries (property crimes breakdown)

Panel A: Second-stage

	Ten-year differences: OLS			Ten-year differences: IV		
	$\Delta robbery$	$\Delta theft$	$\Delta car\ theft$	$\Delta robbery$	$\Delta theft$	$\Delta car\ theft$
$\Delta migr^{dc}$	.3372***	.1519***	-.0370	.9846***	.1379	-.1123
	(.0884)	(.0488)	(.0787)	(.3461)	(.1440)	(.2331)
Obs.	95	95	95	95	95	95
R^2	0.185	0.305	0.125			
F-stat.	4.03	7.69	2.36			

Panel B: First-stage

$\widehat{\Delta migr}_{it}$				0.5746***	0.5537***	0.5769**
				(0.1833)	(0.1802)	(0.1816)
F-stat. (excl. instr.)				9.83	9.44	10.09

Note: see table 5.5. The variable $\Delta migr^{dc}$ is the log change of immigrants from developing countries over the province population.

5.5 and 5.6. Hence, basing our instrument on a subset of nationalities does not drive the difference between 2SLS and OLS estimates.

Table 5.9: Robustness, composition by nationality

	$\Delta total$	$\Delta violent$	$\Delta property$	$\Delta drug$	$\Delta robbery$	$\Delta theft$	$\Delta car\ theft$
$\Delta migr^{nat}$	.1301***	-.0252	.1224***	-.2455***	.3213***	.1385***	-.0120
	(.0407)	(.1176)	(.0375)	(.0754)	(.0737)	(.0373)	(.0618)
Obs.	95	95	95	95	95	95	95
R^2	0.236	0.061	0.282	0.227	0.210	0.314	0.123
F-stat.	5.83	1.58	7.58	5.42	4.73	8.61	2.38

Note: see table 5.5. The variable $\Delta migr^{nat}$ is the log change of immigrants of those nationalities included in $\widehat{\Delta migr}$ (listed in Section 5.3) over the province population.

Another issue relates to the dimension of unofficial immigration in Italy. As discussed in Section 5.1, we used demands for regularization to infer the distribution of unofficial immigrants, arguing that this approach minimizes under reporting. In principle, however, one can not exclude that immigrants self-select into regularization, which would introduce measurement error into the equation (5.2). In particular, if immigrants who are more at risk of committing crime are also less likely to apply for a residence permit, we would be understating immigrants exactly where they contribute the most to crime, which in turn would bias the coefficient of *migr* downward.

For this reason, we also looked at apprehensions of unofficial immigrants (as recorded by Italian Ministry of the Interior, 2007), which do not depend on self-selection. Indeed, after controlling for province- and year-fixed effects (which are always included in our specifications) the log of apprehensions is positively and significantly related to the log of applications for regularization. In particular, the OLS estimated coefficient of the univariate regression is 0.35, the t-ratio is 3.87 and the R^2 is 85%. Therefore, apprehension- and regularization-based measures of unofficial immigration seem consistent with each other. At the same time, regularizations provide a more representative picture of the phenomenon.[7]

In addition, the 2SLS approach adopted in Section 5.3 should attenuate any bias due to under reporting of unofficial immigrants. In fact, if both official and unofficial immigrants of the same nationality cluster into the same areas, then our instrument provides a measure for the predicted log change of total immigrants that depends solely on the geographic distribution of these clusters and the supply-push factors in origin countries.

Finally, mobility across the borders of different provinces may give rise to spatial correlation in provincial crime data. In line with the literature on spatial econometrics and crime (Anselin, 1988; Gibbons, 2004; Zenou, 2003), we thus control for spatially lagged crime rates. These consist of weighted averages of crime rates in neighboring provinces. In particular, crime in province i is assumed to depend also on crime observed in any other province j, weighted by the inverse of the distance between their capital cities. The results, presented in Table 5.10, are consistent with those in our baseline specification. Hence, spatial correlation does not affect our results. This is probably due to the fact that provinces are rather large geographical areas, so that crime trips occur within rather than across provinces.

Table 5.10: Robustness, spatial correlation

	$\Delta total$	$\Delta violent$	$\Delta property$	$\Delta drug$	$\Delta robbery$	$\Delta theft$	$\Delta car\ theft$
$\Delta migr$	.0977	-.1349	.0453	-.1711	.9556***	.1416	-.2015
	(.1903)	(.4395)	(.1516)	(.3015)	(.2664)	(.1525)	(.2606)
Spatial lag	.3799	.6074*	.0588	.6601**	.7216***	.1644	.0581
	(.4792)	(.3281)	(.6110)	(.2914)	(.2559)	(.5716)	(.5570)

Note: This table presents the results of IV (second-stage) estimates on the cross section of ten-year differences between 1991 and 2001 across all 95 Italian provinces. The dependent variable is the log change of the number of crimes reported by the police over the total population, for each category of criminal offenses. The variable $\Delta migr$ is the log change of immigrants over the province population and is instrumented in the first stage by $\widehat{\Delta migr}_{it}$ (see equation 5.5 in the main text). The spatial lag is the weighted sum of the log of crimes over the population in all other provinces, with weighting matrix based on the inverse of road travelling distance between provinces. The ten-year differences of all control variables in Tables 5.3 and 5.4 are always included, both in the first and second stage. The sources of data for residence permits and reported crimes are ISTAT and the Italian Ministry of the Interior, respectively. Robust standard errors are presented in parenthesis. *, ** and *** denote rejection of the null hypothesis of the coefficient being equal to 0 at 90%, 95% and 99% confidence level, respectively.

5.5 Conclusions

In this paper, we investigated the causal impact of immigration on crime across Italian provinces during the 1990s. Our results do not support the widespread perception of

[7] In 1995 there were less than 64,000 apprehensions and 260,000 applications for regularization; this ratio was 61,000 over 250,000 in 1998 and 106,000 over 700,000 in 2002

a causal relationship between immigration and crime. Indeed, we find that neither the overall crime rate nor the number of most types of criminal offense are significantly related to the size of the immigrant population (once endogeneity is taken into account). These results leave many avenues for future research open, and we sketch only a few here.

First, our results raise the question of what determines the widespread perception of a link between immigration and crime. This issue is of the utmost importance given that such perceptions have far-reaching consequences for immigration policies (Bauer et al., 2000). Second, our analysis can be extended in search of more detailed mechanisms. For example, it would be interesting to explore natives' response to an increase in immigration. In particular, our results could be due to the fact that there is substitution between immigrants' and natives' crime. Finally, it would be interesting to estimate separately the effect of official and unofficial immigrants. A better understanding of such mechanisms appears crucial for detailed policy prescriptions.

Appendix: Variables: definitions and sources

migr: log of residence permits over the total province population, as of December 31 of each year. Source: Ministry of the Interior.

total: log of reported crimes in each province and year. This category includes murder, serious assault, rape, sex offence, theft, robbery, extortion, kidnapping, incrimination for criminal association, arson, terrorism, drug-related crime, forgery and counterfeiting, fraud, prostitution and other crimes. Source: *Statistiche Giudiziarie Penali* - Italian National Institute of Statistics.

violent: log of reported violent crimes over the total population in each province and year. Source: *Statistiche Giudiziarie Penali* - Italian National Institute of Statistics.

property: log of reported property crimes over the total population in each province and year. This category includes robberies, thefts and car thefts. Source: *Statistiche Giudiziarie Penali* - Italian National Institute of Statistics.

theft: log of reported thefts over the total population in each province and year. This category includes several types of crime such as: bag snatch and pickpocketing. Source: *Statistiche Giudiziarie Penali* - Italian National Institute of Statistics.

robbery: log of reported robberies over the total population in each province and year. Source: *Statistiche Giudiziarie Penali* - Italian National Institute of Statistics.

car theft: log of reported car thefts over the total population in each province and year. Source: *Statistiche Giudiziarie Penali* - Italian National Institute of Statistics.

drug: log of reported drug-related crimes over the total population in each province and year. This category includes trafficking, consumption and pushing. Source: *Statistiche Giudiziarie Penali* - Italian National Institute of Statistics.

pop: log of the resident population in each province and year. Source: *Popolazione e movimento anagrafico dei comuni* - Italian National Institute of Statistics.

urban: percentage of the population living in cities with more than 100,000 inhabitants in each province and year. Source: *Popolazione e movimento anagrafico dei comuni* - Italian National Institute of Statistics.

*male*1539: percentage of young males aged 15-39 in the population in each province and year. Source: *Popolazione e movimento anagrafico dei comuni* - Italian National Institute of Statistics.

gdp: log of real GDP per capita in each province and year. Source: *Conti Economici Territoriali* - Italian National Institute of Statistics.

unemp: percentage unemployment ratio in each province and year. Source: Labour Force Survey - Italian National Institute of Statistics.

clear-up: percentage ratio of the number of crimes solved by the police to the total number of reported crimes, for each province, year and crime category. Source: *Statistiche Giudiziarie Penali* - Italian National Institute of Statistics.

partisan: ideology of the provincial government. This variable is constructed as follows. First, a score between 0 (extreme left) and 20 (extreme right) is attached to each political party according to the expert surveys presented in Benoit and Laver (2006) (these data are available at http://www.tcd.ie/Political_Science/ppmd/). Then, the score of the local government is computed as the average score of all parties entering the executive cabinet weighted by the number of seats held by each party in the local council (the composition of Italian local councils is available at http://amministratori.interno.it/).

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