

Cities, Constitutions, and Sovereign
Borrowing in Europe, 1274-1785

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Abstract

This paper investigates the politics of sovereign borrowing in Europe over the very long run. I consider three alternative hypotheses regarding the sources of borrower credibility. According to the first, European states with constitutional checks on executive authority found it easier to obtain credit at low interest rates than did states that lacked such constraints. My second hypothesis focuses on state type (city-state versus territorial state) and the way in which this may have influenced the balance of political power between owners of land and owners of capital in a society. It suggests that after controlling for other factors, we should observe that city-states in Europe found it easier to borrow than did larger territorial states, and that they paid lower interest rates on their debt. Finally, my third hypothesis suggests that borrower credibility depended on the simultaneous presence of both constitutional checks and balances and a city-state. When we consider a broad sample of cases over a long time span there is strong support for the proposition involving city-states and merchant power, but less support for the argument that constitutional checks influenced credibility regardless of state type (city-state or territorial state). There is, however, some empirical evidence of an interaction effect whereby constitutional constraints on rulers made city-states particularly credible as borrowers. My results are robust to a number of controls for alternative determinants, for sample selection bias, and for the endogeneity of city-state development.

1 Introduction

One of the most frequently cited recent arguments about European state development involves the effect of constitutional checks and balances on economic performance. In areas where political institutions placed constraints on rulers, it is argued that property rights were more secure, which helped stimulate investment, innovation, and growth. Authors have suggested that checks and balances also facilitated long-term borrowing by governments, since creditors found it less likely that constrained rulers would default on loans. Easier access to loans at low interest rates gave some states an advantage in international competition. The contrasting cases of Great Britain and France provide a popular illustration of the argument linking borrower credibility to constitutional regime, as does the example of the Netherlands. In this paper I use data on a broad sample of European states over a long time period in order to investigate the political conditions that gave governments easier access to credit at low interest rates. I test the proposition that institutional constraints facilitated access to credit. I also consider two alternative hypotheses regarding access to credit: a city-state/merchant power hypothesis, and a third hypothesis involving an interaction effect between city-states and constitutional constraints.

The idea of a link between constitutional checks and balances, access to credit, and economic performance in early modern Europe has generated a lively debate in recent years. North and Weingast (1989) argued that the constitutional changes of the Glorious Revolution allowed the British crown after 1688 to gain increased access to credit at low interest rates, and Schultz and Weingast (2003) have extended this argument to a comparison with *ancien regime* France.¹ These views have been challenged by O'Brien (2001) and by Epstein (2000) who argue that the British government's "revolution" in public finance was actually a slow process dependent upon administrative and technical reforms involving tax collection and financial instruments.² They take this as implying that the observed relationship between

¹For more on this subject see also Hoffman and Norberg (1994), North and Thomas (1973), Stasavage (2006, 2003), Tracy (1985), Tracy (1994), Velde and Weir (1992), and Weingast (1997). Summerhill (2004) has considered these issues for non-European states.

²Several papers have also challenged arguments about democratic institutions and commitment using more

constitutional change and borrowing credibility is spurious. Likewise, Sussman and Yafeh (2003) have emphasized that it took several decades after 1688 before interest rates on British government debt dropped significantly. When it comes to protection of property rights more generally, authors have also debated the importance of constitutional provisions. DeLong and Shleifer (1993) present evidence that between 1000 and 1800, European cities in areas with “absolutist” institutions grew more slowly than those in areas with non-absolutist regimes. Acemoglu, Johnson, and Robinson (2005) show that “executive constraints” were a significant factor influencing early modern European growth. However, using English evidence Clark (1996) has cast doubt on the argument linking constitutional institutions to property rights protection.³

My second testable proposition, the city-state/merchant power hypothesis, provides a different prediction from the constitutional hypothesis about the foundations of credible commitment. The core idea behind this hypothesis is that in city-states, merchants would be much more likely to be dominant within the political elite when compared with larger territorial states where they would be outnumbered by landowners within the elite. As a result, while “the political power of merchants” is not something that can be directly measured, particularly in a cross-country setting, the existence of an independent city-state may serve as a good proxy for merchant power. Given that merchants were also the primary source of credit for sovereign borrowing at this time, we should expect to observe that city-states found it easier to gain access to credit at low interest rates when compared with larger territorial states. When merchants controlled a city-state’s decision making institutions directly, they could ensure that decisions were taken to prioritize servicing debt rather than opting for opportunistic defaults. In other cases merchants could impose significant political and economic costs if the ruler of a city-state defaulted. There is abundant historical evidence suggesting that merchants were more politically powerful in city-states than in larger territorial entities.

recent evidence. Tomz (2002) presents evidence on the shifting effect of public opinion regarding default in Argentina in the late 1990s. Saeigh (2005) presents cross-country evidence suggesting that democratic states do not have an advantage with regard to borrowing.

³ Likewise, Hoffman, Postel-Vinay, and Rosenthal (2000) show that private financial markets in France developed rapidly during the late eighteenth century despite the weakness of representative institutions.

The political role of merchants in city states has been emphasized by a number of historians.⁴ It has been considered in direct relation to public debt by Tracy (1985, 1994), Pezzolo (2004), Fryde and Fryde (1963) and Fryde (1964). In emphasizing the political power of merchants within city-states, however, we should take note of the qualification emphasized by Greif (2006); the simple fact of political dominance by merchants creates no assurance that different merchant factions in a city will refrain from violent conflict between themselves, with potentially destabilizing effects on credit.

In addition to the constitutional and city-state/merchant power hypotheses, I also consider a third hypothesis that involves an interaction effect between state type (city-state or territorial state) and constitutional regime. According to this third hypothesis, borrower credibility depended on the simultaneous presence of a city-state and a constitutional regime placing constraints on rulers. There are two reasons we might expect this interaction effect to exist. The first involves the relationship between a city-state and princely overlords in its region. If princes were subject to a constitutional regime limiting their prerogatives, then we should expect it to be even more likely that merchants within a city-state would exert real influence, and that it would be more difficult for princes to extract finances from a city without the consent of its political elite. The second reason involves the constitutional regime of a city-state itself. If the presence of representative institutions made it more likely that state policies responded to majority opinion within the political elite, then it seems natural to predict that merchants would be more likely to be predominant in city-states with representative institutions as opposed to city-states without such institutions. We would not expect the establishment of representative institutions to have a similar impact in larger states where merchants made up a small fraction of the political elite.

I proceed by elaborating the alternative theoretical arguments in Section 2. This involves first drawing on a model of interaction between a borrower and a lender developed by Ghosh, Mookherjee, and Ray (2000) which can be used to suggest why any political factors that

⁴This would include Pirenne (1910, 1925), Brady (1991), Lopez (1976), Pezzolo (2004, 2003), and Lane (1973).

influence the interest rate at which a state borrows should also influence the extent to which the supply of credit to this state is rationed. I then suggest how constitutional checks and balances, the existence of an independent city-state, or an interaction between these two factors should influence both interest rates and access to credit.

In the subsequent sections I draw on several different data sources to test my three hypotheses. Section 3 begins by very briefly presenting historical evidence on five specific cases involving constitutions, merchant power, and public borrowing - Italian city states (Venice, Genoa, and Florence), German city states, France, the Netherlands, and Great Britain. This historical evidence is useful for considering the plausibility of the different hypotheses, before turning to quantitative evidence. I show that there is ample historical support for the idea that merchants had significant political influence in city-states and that this helped allow city-states to borrow at low interest rates. The evidence on the effect of constitutional checks and balances is more mixed. While increased constitutional constraints on rulers in Great Britain and the Netherlands appear to have been associated with improved access to credit, in Florence, Genoa, and France we observe instances where significant constitutional change had no apparent effect on borrowing credibility.

Section 4 turns to the quantitative assessment of my three hypotheses. Stephan Epstein (2000) has compiled an interesting database of nominal interest rates on long-term sovereign borrowing for 27 European states over the period 1274 to 1785, based on information contained in a range of secondary sources. While using nominal interest rates from a heterogeneous set of sources to gauge borrower credibility poses several potential problems (as discussed in Section 4), these data provide an important opportunity to test hypotheses about the political determinants of borrowing costs. Epstein presents summary statistics which suggest that while republics may initially have been able to borrow at lower rates of interest than monarchies, over time interest rates in these two groups of states converged. In order to test the alternative hypotheses about merchant power and institutional constraints, I divide the 27 states in the data set into city-states and other entities. I also use four separate measures of constitutional constraints on rulers.

Using the above data, in Section 5 I estimate the political determinants of borrowing costs, controlling for the return on capital in private markets and the rate of urbanization. I also attempt to control for the extent of barriers to entry in the sovereign lending market, which should have an effect on interest rates for sovereign loans. Based on OLS regressions, I find evidence that city-states paid lower interest rates on their debt. This is consistent with the idea that merchants were more likely to hold political power in city-states when compared with larger states. There is less indication that constitutional checks and balances were associated with lower borrowing costs. I do, however, find some evidence of an interaction effect between state type and constitutional regime. The city-states that paid the lowest interest rates on their debt were those that had republican institutions and those that were in regions where princely overlords were constitutionally constrained.

In Section 6 I extend the investigation by estimating a sample selection model where I consider what determined whether a state was active in the debt market, and given that a state borrowed, what determined the interest rate on its debt. If the political conditions that produce lower interest rates also make it less likely that a state is rationed out of the debt market, then ignoring sample selection could lead one to underestimate the magnitude of these political effects. Investigating whether a state borrowed is also an interesting issue in its own right that allows a further test of the observable implications of my main hypotheses. We should expect any political factor that lowers default risk to be associated with a greater likelihood of borrowing. The empirical results of Section 6 can be summarized as follows. When we consider the city-state and constitutional hypotheses, but no interaction effect, we observe that city-states were more likely to be able to borrow, and at lower interest rates than other states. There is less evidence of an overall effect of constitutional regime. However, when we allow for interaction effects between state type and constitutional regime we observe some evidence that ability to borrow, and at low interest rates, depended on the simultaneous presence of a city-state and constitutional constraints on rulers.

Section 7 reports the results of a final empirical test using instrumental variables to deal with the potential endogeneity of city-state development. The simple observation of a cor-

relation between city-states and low interest rates does not necessarily imply a causal effect running from the former to the latter. For example, it may be that certain historical conditions, like the medieval revival of trade that occurred beginning in the tenth century, led simultaneously both to the emergence of city-states and to a greater accumulation of capital (implying lower interest rates) in certain areas. This effect should be controlled for in my OLS regressions by the inclusion of an estimate of the return on private capital and by including urbanization as a proxy for the level of economic development, but these variables are inevitably imperfect proxies. In Section 7 I suggest that we can use a specific “historical accident”, involving a ninth century division of the Carolingian empire, to construct an instrumental variable that predicts future city-state development and which should logically have had no direct effect on the interest rates at which sovereign states borrowed several centuries later. As a part of the Treaty of Verdun in 843, the Carolingian empire that covered much of Western Europe was divided into three separate territories roughly following longitudinal strips: West Francia, East Francia, and a middle territory of Lotharingia. Subsequently, central control in Lotharingia collapsed to a much greater degree than in the other two. This collapse of central authority undoubtedly favored the subsequent development of numerous city-states in this north-south band stretching from the Netherlands to Northern Italy. It seems clear that the process whereby certain cities and regions were "selected" into this middle territory of Lotharingia depended heavily on idiosyncratic, accidental factors operating at the time of the division, and the same can be said for the factors explaining the subsequent collapse of central control in Lotharingia. This makes a dummy indicator for states subsequently located in the former territory of Lotharingia an ideal instrument for city-state development. In Section 7 using this and two other instruments for city-state development I show that my conclusions regarding city-states, merchant power, and sovereign borrowing remain robust.

2 Hypotheses about government borrowing

I begin by developing my three core hypotheses about government borrowing involving constitutional checks and balances, city-states as a proxy for merchant power, and the possibility of an interaction effect between these two factors. Before doing this, however, I first present a theoretical framework that demonstrates why we might expect these political factors to influence both access to credit and interest rates.

2.1 The politics of credit rationing

The empirical tests in this paper consider the possibility that certain political conditions are associated with lower interest rates on loans, in addition to asking whether political conditions determine whether the quantity of credit available to some borrowers is limited (credit is rationed).⁵ We can illustrate the possibility that political conditions might influence both interest rates and the quantity of credit using the following model of repeated interaction between a borrower (in this paper referred to as the sovereign) and a single lender developed by Ghosh, Mookherjee and Ray (2000). This model is also very useful for identifying factors to include as control variables in the statistical tests to follow.

It is commonly argued that in the case of sovereign debt, repayment depends on reputational constraints that limit incentives for governments to voluntarily default. In this game in each period a lender can extend a loan contract to the sovereign specifying the size of the loan L and an interest rate i . The opportunity cost for the lender is determined by r which is the return on capital in the private capital market. The sovereign produces output according to the production function $F(L)$ and he has a discount factor for future consumption of δ . For European states during the period considered here the most important "productive" activity involved military conflict. If the sovereign repays the loan he pays $L(1 + i)$, but he also has the option of defaulting in which case he has utility from an outside option equal to v in each subsequent period. As discussed below, the parameter v might be influenced by a

⁵Weingast (1997), Robinson (1998), and Schultz and Weingast (2003) have emphasized that political institutions may influence whether a sovereign is rationed.

number of different political and economic factors.⁶ In this game the maximum level of profit a lender can earn is determined by a second exogenous parameter z which in practice will depend on the extent to which other potential lenders face barriers to entry in the sovereign lending market. The following equation presents the isoprofit condition for the lender.

$$z = L(i - r) \tag{1}$$

In the absence of any other constraints, the sovereign would prefer to choose a loan quantity L that maximizes the difference between production and the cost of repayment.

$$\max_L (F(L) - (1 + i)L) \tag{2}$$

An equilibrium solution to the above game will not necessarily be determined exclusively by the two above conditions because of the possibility of voluntary default. I follow Ghosh, Mookherjee, and Ray (2000) by identifying whether there can exist a stationary subgame perfect equilibrium of this repeated game where the lender follows a trigger strategy of offering the same loan contract (L, i) in every period unless the sovereign defaults in which case the lender refuses to grant further loans on any terms.⁷ The lender will only agree to an equilibrium loan that he expects the sovereign to repay. This means that to find an equilibrium loan contract, we also need to consider whether a loan (L, i) satisfies the sovereign’s “no-default” constraint (referred to more frequently as the incentive compatibility

⁶The specification of this exogenous parameter for the utility a sovereign receives after defaulting is similar to the strategy adopted in Robinson (1998), Weingast (1997), and Schultz and Weingast (2003).

⁷The focus on stationary equilibria simplifies the analysis but rules out plausible dynamics such as the possibility that a lender might initially “test” a borrower with a small loan at a relatively high interest rate in order to form firmer beliefs about the future probability of default. Dynamics such as this have recently been considered in the political economy of sovereign debt markets by Tomz (2006). It should also be acknowledged that the complete information model presented here should probably be seen as a tractable shorthand for a more realistic model where the borrower can voluntarily default, but where there is uncertainty about an intrinsic ability of the borrower to repay, creating a potential problem of adverse selection. If we augmented the current model by specifying that with some probability the borrower is of a type that will always default irrespective of the circumstances, then this would create incentives for borrowers to repay in order to convey a signal about their type. Tomz (2006) has emphasized the greater plausibility of incomplete information models of reputation for sovereign debt markets.

constraint).

$$F(L) + \frac{\delta}{1-\delta}v \leq \frac{1}{1-\delta}[F(L) - (1+i)L] \quad (3)$$

The left side of the above inequality gives the present value for the sovereign of using the loan for production, choosing not to repay, and then receiving no further loans. The right hand side represents the present value from producing, repaying the loan and then repeating the same process in each period. The no-default constraint shows that a larger loan allows the borrower to produce more units of output, but it may also increase the likelihood of default, because high repayments make default a relatively more attractive option.⁸

Under the above conditions If the loan contract given by intersection of the isoprofit condition (1) and the sovereign's maximization problem (2) does not satisfy the no-default constraint, then credit will be rationed. When there is rationing, the equilibrium loan contract will be determined by a pair (L^*, i^*) that gives the sovereign the minimal incentive necessary to repay and which also satisfies the lender's isoprofit condition.⁹ The sovereign will be able to borrow less than he would prefer. We can simplify and rearrange the no-default constraint and the isoprofit condition as follows to make it easier to derive comparative statics for the equilibrium loan quantity and the interest rate under rationing.

$$L^* = \delta \frac{F(L^*) - v}{1 + i^*} \quad (4)$$

$$i^* = \frac{z}{L^*} + r \quad (5)$$

What conclusions can we derive from the above model for empirical analysis? The most important is that when the value of v , the sovereign's outside option from default, is high we are more likely to observe credit rationing, and when there is rationing, an increase in v will trigger a reduction in equilibrium loan size. Given the isoprofit condition, this also implies that an increase in v will result in a higher equilibrium interest rate. As a result,

⁸This idea is central to the prediction of credit rationing in the reputational model of sovereign debt first developed by Eaton and Gersovitz (1981).

⁹As I am reproducing an existing theoretical model to motivate my empirical specification, I do not present a full derivation of this result. See Ghosh, Mookherjee and Ray (2000).

for any political factors, such as constitutions or merchant power, that are hypothesized to determine v we should investigate whether they are correlated both with interest rates and with access to credit. While strictly speaking this model gives predictions of the amount that sovereigns are able to borrow and not *whether* they are able to borrow, in practice the data I consider provide information about interest rates, and whether we observe a loan or not, but they do not record the size of the loan. It nonetheless seems very likely that the binary indicator for whether we observe that a sovereign borrowed in a given year will be a very good proxy for the ability of the sovereign to borrow in significant quantities.

In addition to providing comparative statics about the value of v , the above discussion also identifies a number of other factors that should be included as controls in any empirical analysis of borrowing by European states.

First, high barriers to entry in the sovereign lending market (a high value of z) will be associated with a higher interest rate for a specified loan size (given the isoprofit condition). High barriers to entry will also be associated with a greater likelihood of rationing, and if there is rationing an increase in costs of entry will result in a smaller equilibrium loan size. In lending markets there can be important informational costs of entry. Existing lenders are likely to have better information about a sovereign borrower than are new entrants to the lending market. Below I suggest that barriers to entry can be proxied in part by whether a state's neighbors have already entered the debt market.

Second, the above discussion also suggests including an estimate of the opportunity cost of capital r in any empirical estimation of the determinants of the interest rate for sovereign loans. This will be critical for testing my city-state merchant power hypothesis, as I need to be able to distinguish between the strictly economic effect whereby one might expect a city-state to be a state where capital is abundant and thus where r is low, as opposed to the political effect I am proposing whereby merchant political power is greater in city-states and therefore v is lower.

Finally, while dealing with this issue in full lies beyond the scope of this article, we should also consider to what extent changes in production possibilities for different European states

over time, a change in $F()$, may have influenced access to credit and prevailing interest rates. One possibility I discuss below is that the “military revolution” of the sixteenth and seventeenth centuries may have privileged larger territorial states over city-states by increasing returns to scale in military operations.¹⁰

2.2 Constitutional checks and balances

One possibility is that the parameter v in expression (1) depends on a country’s constitutional regime. In cases where rulers share power with a representative assembly that has an important decisionmaking role, then default may be less likely to occur than in cases where there are weak constitutional constraints on a ruler’s authority. There are several different mechanisms through which this effect might operate. Say there is an independent sovereign who alone decides whether to default on debt, but there is a representative assembly that has some control over taxation. If the sovereign defaults then the representative assembly can “punish” the sovereign by withholding future taxes.¹¹ Weingast (1997) suggests that a representative assembly can help coordinate the actions of lenders with respect to the crown. This will be easier to achieve if a representative assembly has prerogatives over public finance, and it can act as a *de facto* agent of lenders. A third possible interpretation is that constitutional checks and balances do not increase the “cost” of default; they actually remove the possibility that a sovereign can independently decide not to repay. This would be the case in those instances where a representative assembly has sufficient prerogative to independently decide whether and how to service a loan.

When considering medieval and early modern European experience, there is ample variation in the strength of representative institutions that can be used to examine the constitutional hypothesis. As noted by authors like Graves (2001), while the experience of English parliamentarianism is well known, most states in medieval Europe actually had some type of representative assembly. A number of authors have argued that one of the key functions of

¹⁰See Parker (1996) and Downing (1992) on the military revolution and Kohn (2005) on the link between European military conflict and sovereign borrowing.

¹¹This argument is made by Robinson (1998).

these representative assemblies was to provide consent for taxation and for other fiscal decisions. This principle of consent was more explicit in some cases than in others.¹² Historians have also emphasized, however, that in many European regions representative institutions grew dramatically weaker during the sixteenth and seventeenth centuries. With the rise of more powerful central state bureaucracies in countries like France and Spain and the extension of Hapsburg control over a number of Northern Italian cities, state institutions in a number of European regions took on a more coercive character that one might expect to have been associated with greater risk of default by sovereigns.

2.3 City-states and merchant power

My second hypothesis focuses on state type and in particular the way in which this may have influenced the balance of power between large landowners and owners of financial capital within a society, with direct implications for the parameter v in the formal framework above. If we accept that in medieval and early modern Europe, merchants were the primary sources of credit for sovereign borrowing, and merchants were more likely to form a sizeable fraction of the political elite in city-states, as opposed to larger territorial states, then we can predict that city-states should, on average, have found it easier to gain access to credit and at lower interest rates when compared with territorial states. In terms of the formal framework presented above, v would have been lower in merchant controlled city-states for several reasons. If they directly controlled a city-state's policymaking institutions, then merchants could use their political influence to ensure that sufficient taxes were levied to service existing debt. In cases where merchants did not rule directly but nonetheless exerted substantial influence, rulers who defaulted may have found it more likely that they would be replaced, again implying a lower value of v .

The literature on state development in Europe has emphasized that city states were often politically dominated by merchant groups. Merchants either had substantial influence in state politics, or in many cases they actually directly ruled a city. This applies whether one

¹²See Tracy (1994), Hoffman and Rosenthal (1997) on this issue, as well as Major (1960).

refers to city-states in what is today Belgium and the Netherlands (Pirenne, 1910), to Italian city-states like Florence, Venice, or Genoa (Waley 1989; Lane 1973; Epstein 1996; Pezzolo 2004, 2003; Lopez, 1976) or to city-states in Germany (Fryde and Fryde, 1963; Fryde, 1964). Since in many cases local merchants were also the primary purchasers of sovereign debt, we might suggest that a number of European city states were run under a *de facto* principle whereby lenders (the merchants) temporarily ceded control over their capital to the state, but in return the borrowers (the general public) ceded control over the state apparatus to these same lenders.¹³ There are far fewer suggestions that merchants were as politically dominant in larger territorial states like France or Castile. In larger states we would expect merchants to have been a small minority of the political elite. This is not to suggest that merchant influence was always absent in larger states. Hoffman and Norberg (1994) suggest that the political influence of merchants was what distinguished the public finance histories of Great Britain post-1688 and the Dutch Republic from those of France and Castile. But what is interesting with England and the Dutch Republic during the early modern period is that they appear to have been the exception among larger territorial states with regard to merchant political power. I return to this issue in Section 3 below.¹⁴

2.4 Interaction effects

One logical alternative to the above two hypotheses is to suggest that borrowing credibility depended upon the simultaneous presence of a city-state form of organization and constitutional checks and balances. There are two reasons why we might expect this interaction effect to have existed. First, it may be that merchants were more likely to hold political power in city-states, but they were particularly likely to wield significant power in city-states

¹³I emphasize the “*de facto*” aspect here, because the entire adult population in city states was, of course, not given the opportunity to consent to such arrangements.

¹⁴A final further point involving the merchant power argument is that this is a prediction about the effect of city-states on borrowing, not about economic performance more generally, or the viability of the city-state model in a changing international context. As a result, my merchant power hypothesis is perfectly consistent with the observation that city-states ruled by merchants tended to establish market regulations with high barriers to entry (Epstein, 2000) and that the small size of city-states and/or the interests of their elite groups would ultimately inhibit them in competition with larger states (Tilly, 1990; Lachmann, 2003).

that were governed by representative assemblies, as opposed to city-states with a monarchical form of organization. This would be true to the extent that representative institutions made it more likely that a state's policies responded to majority opinion of its political elite, and that merchants made up the majority of members of the elite in cities. A second reason involves the constitutional regime in the broader region within which a city-state is situated. If the constitutional regime places limitations on the extent to which a princely overlord can extract finances from a city without the consent of the city's political elite, then this would also reduce the risk that a city would be unable to service its debts because of such external impositions. As a result, cities in such regions should be more credible borrowers than cities in regions where princely overlords are not constitutionally constrained.

3 Evidence from individual country cases

Before proceeding with the quantitative tests that are the principal focus of this paper, I briefly review evidence from five historical cases: (1) the Italian city-states of Florence, Genoa, and Venice, (2) German city-states (3) France, (4) the Netherlands, and (5) Great Britain. The evidence from these examples supports the idea that merchants often held power in city-states and that this was associated with greater access to credit at low interest rates. Several cases suggest the significance of constitutional arrangements in contributing to borrower credibility, but there are also instances where constitutional arrangements shifted significantly without an apparent effect on borrower credibility.

3.1 Florence, Genoa and Venice

These are the three prototypical examples of independent city-states that established a reputation for sophistication in long-term government borrowing and for servicing of public debt. Along with several Flemish cities, Florence, Genoa, and Venice appear to have been the first European states to develop a system for issuing long-term annuities backed by specific future revenues. It has long been observed that merchants had an important position in the es-

establishment and subsequent governance of these three city republics.¹⁵ The particularities of how debt was managed differed in each case; in Genoa it was delegated to the *Casa di San Giorgio*, while in Florence and Venice it was directly controlled by representative assemblies. In all three cases, though, the emphasis was on long-term debts purchased primarily by citizens and serviced with stable revenue flows. These loans also often involved an element of compulsion for significant wealth holders, though this did not imply that wealthy citizens expected to earn no profit from these arrangements. In addition, there was a secondary market through which individuals could sell these assets.¹⁶ Over time, debt ownership spread to involve a substantial percentage of the citizenry.¹⁷ Widespread elite ownership of debt provides one potential explanation for the fact that each of the three cities also established a reputation for servicing these debts.

All three city states referred to here were characterized, particularly before 1500, by constitutional regimes that placed restraints on rulers. This was true both with regard to internal institutions for Republican governance, as well as with regard to princely overlords. This would seem to support the constitutional hypothesis. However, it should also be noted that shifts in constitutional regime that began in the second half of the fifteenth century do not appear to have had a significant impact on ability to borrow. In Florence, even though the formal institutions of the republic were preserved, after 1434 the city came under the domination of the Medici family. Despite this major constitutional shift, there is little indication that the Florentine government under the Medici found it more difficult to borrow than did Florence during the republican period.¹⁸ We observe a related pattern of events in Genoa. While Genoa officially remained a republic, during the sixteenth century it fell under the domination of the Hapsburg monarchy. Outside control might logically have increased the risk associated with Genoese public debt, in particular if the Hapsburgs could make

¹⁵Lane (1973), Epstein (1996), Brady (1991), Waley (1989), and Lopez (1976).

¹⁶See Pezzolo (2005) for a concise overview of debt financing in Florence, Genoa, and Venice.

¹⁷Pezzolo (2003) cites evidence suggesting that in 1427 a full 22 percent of Florentine households held debt, while by 1500 this was true of 14% of Genoese households.

¹⁸This conclusion is based on the evidence for nominal interest rates as well as market yields on Florentine debt presented in Pezzolo (2005).

revenue demands that pushed the Genoese republic into financial difficulty. In fact, there is little evidence that this was the case.¹⁹

3.2 German city states

The city-states of Northern Italy were by no means the only medieval and early modern examples of self-governing cities that issued their own long-term debt. In addition to cities in Flanders, there are also historical records from a number of German city-states that were self-governing in financial matters.²⁰ There is also historical evidence for the predominant role played by merchants in these cities (Fryde and Fryde, 1963; Fryde, 1964). The constitutional position of the German city-states was somewhat different from Italian cases, as the German cities remained formally subject to princely overlords. It was the *de facto* fragmentation of power in Germany under the Holy Roman Empire that gave cities like Mainz, Cologne, and Nuremberg their authority. Within Germany both imperial cities (*Reichstädte*) and free cities (*Freistädte*) gradually achieved almost complete autonomy in financial matters according to Fryde and Fryde (1963). These same two authors observe that while both German and Italian city-states frequently issued their own debt, the lower degree of political independence of German city-states from feudal overlords made it more likely that such states could be forced into financial distress by princely impositions. This weakness of constitutional constraints on princely overlords when compared with Northern Italy should therefore logically have translated into a higher level of interest rates on debt issues. It is not clear that this was the case, but the quantitative tests in Section 5-7 will consider this question in greater detail.²¹

¹⁹Brady (1991) makes an argument that the merchants who controlled government in Genoa actually benefited from outside control by the Hapsburgs, because it consolidated their position against domestic opponents.

²⁰One could add to this the case of several Swiss cities that issued their own debt. See Gilomen (2003).

²¹Mainz and Nuremberg were able to borrow at nominal rates of 4% and 4.5% during the early fifteenth century. This is only slightly higher than the average nominal interest rate of 3.6% on Florentine public debt during this period (based on the data in Epstein, 2000).

3.3 France

The French monarchy has been used as a prototypical case of a government that lacked credibility as a borrower, and there is much evidence to support this conclusion. Despite the models provided by Italian city-states and by a number of cities within France that issued their own annuities, the French monarchy did not take the first steps to establish a long-term debt until the fifteenth century. Once the French monarchy established a more regular system for issuing long-term debt, it was able to borrow at lower interest rates than during the preceding period, but these rates remained high relative to those at which the Italian and Germany city-states borrowed. The French monarchy's lack of credibility as a borrower has often been linked to the fact that French monarchs were not subject to significant constitutional constraints. Representative assemblies at the national level in France were weak, and it is well known that the Estates-General was not convened at all between 1614 and 1789. What is less often recognized is that during an earlier period, in the fourteenth and fifteenth centuries, the Estates-General had a significant amount of influence over royal policy, and monarchs frequently felt the need to call the Estates in order to justify tax increases (Major, 1960; Wolfe, 1972). The existence of an earlier period of strong representative institutions in France presents a potential challenge to those who argue that the French monarchy's lack of credibility as a borrower stemmed above all from its constitutional position. Given that the Estates-General had more political influence in the fourteenth century than in the sixteenth century, if the constitutional hypothesis is accurate, why did France not establish a long-term debt during this earlier period? In fact, financial innovation took place in France just as the monarchy was escaping from constraints posed by representative institutions.

3.4 The Dutch Republic

Even before the establishment of the Dutch republic in the late sixteenth century, the Netherlands had a lengthy experience with long-term public borrowing. A number of cities in the Netherlands had issued their own debt during the medieval period, and as one of the most

heavily urbanized regions in Europe, there was an abundance of capital that could be made available for such borrowing. During the sixteenth century the Netherlands was initially controlled by the Hapsburg monarchy, which used the Netherlands as an important source of finance for military campaigns. Rather than raise loans directly, the Hapsburgs chose to raise funds indirectly by having Dutch cities issue their own long-term debt, guaranteed by future revenues raised by these same cities. As a result, the Hapsburgs were initially able to borrow at an 6.25% rate of interest, and at this time there was also a dramatic increase in the availability of credit.²² In 1572, the northern areas of the Netherlands revolted against Hapsburg control, and as a result the Estates of Holland now maintained its procedures for borrowing while using the proceeds to finance its own military operations. By the middle of the sixteenth century, the independent Dutch republic was able to borrow at nominal interest rates as low as 4% (t'Hart, 1999). The Dutch case may be used to provide support for both the constitutional and city-state hypotheses. First, the Estates of Holland was a republic with highly decentralized institutions where each city that sent representatives to the Estates in effect had veto power over policy (Tracy, 1985; Israel, 1995). As a result, those who ruled Holland can be described as being institutionally constrained. At the same time, it is also clear that the Estates were dominated by urban groups, and that many of the members of the Estates were themselves significant holders of public debt. In a sense, the Dutch Republic can be described as a league of city-states dominated by merchants. James Tracy (1985 p.216) observes that "equitable or not, control of fiscal policy by men who themselves had heavy investments in state debt was the real genius of the Netherlands system of public borrowing both in its Hapsburg beginnings and in its seventeenth century grandeur."

3.5 Great Britain

Great Britain after the Glorious Revolution of 1688 remains the paradigmatic case of a constitutional change that is commonly said to have resulted in greatly improved access to public credit. While the British crown contracted loans backed by customs revenues as

²²See t'Hart (1999) and Tracy (1985).

early as the thirteenth century, unlike continental monarchies in France or Spain, it did not actually establish a regular system of long-term borrowing until after the Glorious Revolution of 1688. A likely reason for this lag involved Britain's relative disengagement from major continental wars during the seventeenth century, but it also seems plausible that financial operators may have rationed credit to the Crown. North and Weingast (1989) have argued that the institutional changes of the Glorious Revolution allowed the British Crown after 1688 to borrow unprecedented sums at significantly lower interest rates than had prevailed before the Revolution. Historical evidence provides support for this argument, but it also raises significant questions. There was significant volatility in interest rates on British public debt during the period 1688-1715, and it was not until after 1715 that interest rates on British government debt began to converge with those prevailing for states that were recognized at the time to be low risk borrowers (notably the United Provinces). In other work (Stasavage, 2006, 2003) I have argued that trends in British interest rates can be explained by shifts in the partisan control of Parliament between the Whig party, which was closely associated with the "monied interest" that purchased government debt, and the Tory party, a number of whose members called on several occasions for a default on public debt. According to this account, the main reason British interest rates after 1715 converged with those prevailing in the United Provinces is that this was the period where the Whig party established a lasting supremacy in British politics. Rather than focusing exclusively on the constitutional changes of 1688, then, the historical evidence suggests we should also ask how merchants were able to gain significant political power in Great Britain through party politics. What arguably made Great Britain exceptional was that it was the first large territorial state where merchants gained the same political influence that they had previously achieved only in city-states.

3.6 Summary

This section has briefly examined the plausibility of the alternative arguments involving city-states, merchant power, and constitutional checks and balances. In the five cases considered I have noted that long-term sovereign borrowing developed particularly early in city-states, and

not just in Northern Italy, and a number of city-states were able to borrow fairly continuously at low rates of interest. There is abundant historical evidence that merchants played a prominent role in these political communities. In fact, in a number of cases cities were ruled by individuals who themselves invested in public debt. There is evidence from several cases that constitutional checks and balances may have been significant for borrower credibility. The cases here also raise important questions about constitutional considerations. In several instances where a state underwent a shift in its constitutional regime we do not observe a significant change in access to credit. The next section describes the evidence that will be used to conduct quantitative tests of my different hypotheses.

4 Data on sovereign borrowing and political institutions

With any study investigating the politics of government borrowing over a 500 year time span there are inevitable questions regarding how to accurately measure both costs of borrowing and political institutions. This section deals with these issues in detail. I first discuss the data on interest rates for sovereign loans. I then describe how I divide states between city-states and larger territorial entities, while also presenting the four variables used to measure constitutional checks and balances.

4.1 Interest rates for sovereign loans

The ideal data source for government costs of borrowing would involve information from secondary markets to calculate yields on long-term government debt at different points in time. With a few exceptions, this type of data is not available for the vast majority of states for the period considered in this study. What we do have is a fairly extensive data set of nominal interest rates on long-term government debt collected by Stephan Epstein (2000). The data, which are collected from a wide range of secondary sources, cover the period 1274 to 1785, and they include information on 27 different political entities. The nature of debt contracts obviously varied greatly over the 500 year period considered here, as

well as across countries. As a result, the dataset does not control for important features such as loan maturities (apart from that they are classified as long-term). Nor does it control for the fact that governments in difficult times were often forced to sell annuities below par value. Finally, the data do not control for the fact that in many European cases before the eighteenth century loans to sovereigns involved an element of compulsion. It should be noted, however, that it was not the case that only large monarchies or governments lacking credibility resorted to compulsion in this manner. The Venetian and Florentine republics made regular use of forced loans. In the end, while the above limitations should be taken into account when interpreting the empirical results in this paper, it should also be recognized that Epstein's study constitutes the most comprehensive effort to date to compile interest rate data for a broad set of medieval and early modern European states.²³

In order to facilitate estimation of the determinants of government borrowing costs, I have used the interest rates reported in Epstein (2000) to construct a small time-series cross-section data set composed of 27 states over eleven fifty-year time periods running from 1250 to 1750. The names of the different states are listed in Table 1. The dataset contains 94 observations, meaning there is a large number of "missing" observations. In some cases missing observations are explained by the fact that states did not exist as independent entities during the period in question. In other instances the absence of data is no doubt explained by random factors. Finally, while the interest rate data used were not collected by Epstein (2000) with the specific intention of identifying the earliest date at which a state borrowed, there also appears to be a clear pattern of selection whereby certain states did not enter the debt market until significantly later than others. A brief glance at Table 1 suggests that city-states, on average, began long-term borrowing earlier than did larger territorial entities. This is consistent with the historical evidence presented in the previous section. There are some clear cases where the first appearance of a state in this dataset does not correspond to the date at which it began long term borrowing - the city of Liège, for example, began borrowing long before 1650. Nonetheless, the overall order of appearance for states here is

²³The important dataset collected by Tomz (2006) covers states from the eighteenth century onwards.

consistent with existing historical accounts of the development of sovereign borrowing.

4.2 Political Institutions

The hypotheses laid out in Section 2 raise two measurement issues regarding political institutions. First, we need to distinguish between city-states and larger entities. Second we need to distinguish environments where political institutions placed constraints on executives from environments where rulers were relatively unconstrained.

The first of these issues is the more straightforward. Few would contest the fact that Florence during the fifteenth century was a city-state while France was not. Cases like the Kingdom of Naples may be more debatable, but there are few such examples in the database. In order to produce an objective indicator of whether an entity is a city-state I have used the following simple coding rule - the dummy variable *city* is equal to 1 if the name of the country is the same as the name of its capital.²⁴

I use four different measures to proxy for the presence of constitutional checks and balances. Each of these four variables is defined so that higher values correspond to *lower* levels of constitutional restraints on rulers. I adopt this coding scheme in order to make interpretation of the interaction effects in my regressions more transparent, following a suggestion by Braumoeller (2004).

The first of the four constitutional measures, *unfree* is a binary indicator produced by DeLong and Shleifer (1993). A value of 1 for this variable is intended to correspond to an “absolutist” state where the legal framework is subject to the prince’s will, and therefore property is insecure. A value of 0 corresponds to “non-absolutist” regimes where the prince (if a prince existed) was not above the law. In such regimes we might expect to find that “legal judgments could be enforced only with the consent of parlements.” DeLong and Shleifer (1993) have shown that cities in “free” regions experienced more rapid population growth than cities in absolutist regions. One aspect of DeLong and Shleifer’s variable worth

²⁴One should also note that while some city-states, like Venice, at times actually controlled large amounts of territory, in these cases citizenship rights generally remained restricted to inhabitants of the core city itself, so in terms of governance these states remained different from territorial entities.

emphasizing is that for the sample considered in this paper we observe a very significant decline beginning in 1500 in the number of regions classified as being free. There are several related reasons for this. First, the extension of Hapsburg control in regions like Northern Italy resulted in a *de facto* change in constitutional arrangements. Second, a number of historians have referred to a growth in the role of state bureaucracies in countries like France and Castile that allowed rulers to undermine or ignore existing representative assemblies (Major, 1960; Graves, 2001).

I also use a second indicator of constitutional constraints, a variable named *coercion* which is based on Charles Tilly’s distinction between “coercion” and “capital”. Tilly (1990) suggests that in some European cases at some times, state development has depended primarily on the accumulation of means of military coercion, while in other cases or at other times state development has depended primarily upon the accumulation of financial and physical capital. DeLong and Shleifer (1993) have used Tilly’s classification to divide European regions into those where development of coercive means had exceeded development of capital (2) those where capital accumulation predominated (0), and those where the two types of social organization were more equally matched (1). They then show that this coercion-capital indicator is also significantly correlated with European city growth. For those who are interested in explaining how political conditions affect economic outcomes there may be some question about the extent to which Tilly’s coercion-capital distinction is a measure of the dependent or the independent variable.

The third constitutional indicator I use is a measure produced by Acemoglu, Johnson, and Robinson (2002) of “protection for capital”. They suggest that the coding of this variable “depends on the formal rights given to urban merchants, particularly their protection in the event of a dispute with the nobility or monarch”.²⁵ The chief difference in practice between the *protect* variable and the variables *unfree* and *coercion* is that the latter measures

²⁵Though the original coding scheme for this variable was based on a seven point scale, for the variable named *protect* in this paper I have used a three point scale with 0 representing the highest level of constitutional protection for capital and 2 the lowest. I adopted this coding scheme because in the sample considered here almost all observations fell into three categories of the original Acemoglu, Johnson, and Robinson variable.

classify several late medieval and Renaissance states as having institutionally constrained rulers while the Acemoglu, Johnson, and Robinson (2002) measure does not. This is the case of France in the fifteenth century, for example, which is coded by DeLong and Shleifer (1993) as being “free” but by Acemoglu, Johnson, and Robinson (2002) as having the lowest value for protection of capital.

The final constitutional variable I use is a dummy variable, *monarchy*, which equals 1 for states that are monarchies and 0 for states that are republics. The coding for this variable is based on that provided by Epstein (2000). There are several things worth noting about this indicator. First, unlike the other three constitutional variables, it does not vary over time. One effect of this is that several states that were initially republics but later evolved into autocracies continue to be coded as republics. A second thing to note is that the vast majority (39 out of 49) of observations in the data set for republics are also city-states.²⁶

5 Estimating Determinants of the Cost of Borrowing

This section presents OLS estimates of the determinants of interest rates on sovereign loans, based on equation (6) below. In equation (6) i_{it} represents the nominal interest rate on public debt for state i at time t . The OLS regression also includes the dummy variable *city*, for city states, as well as a constitutional variable *const*. As described above, I use four alternatives for measuring constitutional constraints. Table 2 provides a list of summary statistics for all variables.

$$\begin{aligned} \ln(i_{it}) = & \beta_0 + \beta_1 city_i + \beta_2 const_{it} + \beta_3 city_i * const_{it} \\ & + \beta_4 \ln(r_{it}) + \beta_5 \ln(urban_{it}) + \beta_6 neighbors_{it-1} + u_{it} \end{aligned} \quad (6)$$

Following the theoretical discussion in Section 2, I include an estimate of the rate of return on private capital r as a control variable. While we do not have country-by-country

²⁶The proportion of city-state observations that were at least nominally republics is not as large (39 out of 57).

data available, Epstein (2004) has produced an estimate of the average return on capital in private markets broken down into fifty-year periods, and which distinguishes between a number of European regions. I use this measure in the regressions reported in Table 3. This measure shows a clear, although not constant, decline in the return on capital in Europe beginning in the thirteenth century.²⁷ In addition, I control for the level of urbanization.²⁸ The urbanization measure *urban* may thus help control for country level variations in the return to capital that might not be picked up by Epstein’s estimate of r . Like the return on capital in private markets variable, as well as the first three constitutional variables (*unfree*, *coercion*, and *protect*) urbanization is measured by regions that correspond to present day nation-states.

The theoretical framework presented above suggests that interest rates for sovereign loans will also be higher the greater the barriers to entry into the sovereign loan market. While we do not have direct measures of these barriers to entry, the data I use here do provide a potential proxy. If a state’s nearest neighbors are established as borrowers, then it seems likely that it should be relatively easier for new lenders to enter the market. The variable *neighbors* represents the fraction of a state’s three nearest neighbors that have already entered the loan market.

A final issue for control variables concerns the fact that I am estimating determinants of nominal interest rates here, and as a result we should ideally control for the effect of expected inflation on these rates. This would not be an issue if there was no measurement error in the estimate of r , and this variable thus perfectly controlled for differences in expected inflation between countries and over time, but this seems unlikely to be the case. Otherwise, differences in expected levels of inflation might explain cross-country variation in interest rates for sovereign debt. These differences might also explain variation in interest rates over time, in particular since it is known that the “price revolution” of the sixteenth century

²⁷I would like to thank Stephan Epstein for kindly providing this data. This measure of the average return on private capital is highly correlated with the observed trend in long-term private interest rates reported by Homer and Sylla (1991).

²⁸The urbanization data was constructed by Bairoch, Batou, and Chevre (1988).

was a period of rapid increase in price levels when compared with either the preceding or subsequent centuries. In order to attempt to deal with this issue I estimated average inflation levels for each state in the dataset for each century from 1250 to 1750.²⁹ Using this estimate of country-by-country inflation (which in turn serves as a proxy for expected inflation), I found that the variable was never statistically significant, and as a consequence, I have not included it in the final reported regressions.³⁰

Table 3 reports the OLS estimation results. In order to take account of the fact that errors for multiple observations from the same state are not likely to be independently distributed, these and all subsequent estimates in the paper use standard errors adjusted for clustering by state. For each constitutional variable I first consider a restricted specification that does not include the *city*const* interaction term, followed by a specification that does include the interaction effect. In the specifications without the interaction term the dummy variable *city* is negative in all four cases but not generally statistically significant at conventional levels (for example $p=.126$ in the first regression).³¹ Neither of the four constitutional variables is statistically significant in the specification without interaction effects. In the Table 3 regressions we also observe that the coefficient on *neighbors_{t-1}* is consistently negative and statistically significant, as we would expect to the extent that this variable proxies for barriers to entry in the sovereign lending market and that barriers to entry produce higher equilibrium

²⁹To do so I used available data on wheat prices in four European markets: Brugges, Cologne, Tuscany, and France, in addition to a price index available for England. I based the estimated average inflation rate for a city-state or territorial state on the change in price levels of the nearest of these five markets or, in the case of those more distant states, on the average price change in the five markets. The series for Brugges was compiled by A.E. Verhulst from the original source Verlinden (1965). Wheat prices in Tuscany were compiled by Paolo Malanima from several historical sources and are available at the International Institute of Social History, Prices and Wages Archive <http://www.iisg.nl/hpw/>. Grain prices for Paris and Cologne were collected by Poynder (1999) and are also available at the IISG archive. The London price index for consumables was collected by W. M. Ormrod and it available online at the European State Finance Database.

³⁰Aside from measurement error, which is certainly present, there is at least one obvious reason for this result. While data for all five markets show high inflation in the sixteenth century, this was a century where the average interest rate on sovereign loans actually declined significantly. Neal (1990, pp.3-4) suggests that this drop in interest rates during the midst of a general rise in prices can best be explained by financial innovations that led to increased efficiency in financial markets.

³¹This result with regard to city states is not driven exclusively by the influence of Italian city-states like Florence, Venice, and Genoa. When one substitutes two separate dummy variables into equation (6), one for Italian city states and one for non-Italian city-states, we observe that the coefficient on the variable for non-Italian city states is the more negative of the two.

interest rates. The coefficient on r is positive, significant, and very close to 1.0 which is consistent with the theoretical framework presented above; a change in the opportunity cost of capital should have a one-for-one effect on interest rates for sovereign loans.

The next specifications in Table 3 include the interaction term $city*const$. We observe that this variable has a positive and sizeable coefficient in three of the four regressions, though the coefficients are not statistically significant. It is worth remembering that all four constitutional variables are defined such that a value of 0 corresponds to the highest level of constitutional constraints. This value is empirically relevant, because between 37% and 47% of observations take a value of 0 for each of the four constitutional variables. In these interaction specifications the coefficient on the variable $city$ now captures the difference between city-states and territorial states when constitutional constraints are at their maximum, implying that $city*const=0$. As can be seen, in all four regressions the coefficient on $city$ is now more negative, and in three of the four cases it is statistically significant. In substantive terms the estimated difference between interest rates for city-states and territorial states is roughly two percentage points when constitutional constraints are at their maximum. This result is consistent with the third hypothesis suggesting that the simultaneous presence of a city-state form of organization and of constitutional checks and balances will have a large impact on borrowing credibility.

In sum, the OLS estimates in Table 3 provide some support for the idea that city-states were able to borrow at lower rates of interest than other states, and this is consistent with the idea that merchants were more likely to be politically dominant in city states when compared with larger territorial states. The Table 3 results provide relatively little support for an unconditional version of the constitutional hypothesis, the idea that across different types of states we should observe a statistically significant difference in borrowing costs between rulers subject to constitutional constraints and those not subject to such constraints. Finally, the Table 3 results also provide initial support in favor of a third hypothesis that the simultaneous presence of a city-state form and of constitutional checks and balances had a particularly large effect on borrowing costs.

6 Sample selection involving the decision to lend

One of the potential weaknesses with the regression results presented in the previous section is that they do not take account of the possibility of sample selection bias - it may be that factors that determine the interest rate on government debt also determine whether we observe a recorded interest rate for a state in a given period.³² The OLS estimates also ignore the opportunity to provide a more complete test of the different hypotheses by examining both how much states paid on their loans and whether they had access to credit. Estimation of a selection model following the method developed by Heckman (1979) involves specifying both an equation for the determinants of the interest rate on government debt, as well as specifying a relationship that determines whether we observe an interest rate for a given country in a given time period. The specification for the interest rate equation remains the same as in the previous section. For the selection equation the theoretical framework presented in Section 2 suggests that the same political and economic conditions producing lower interest rates on government debt should also lead to less rationing. One option would be to estimate a selection equation that simply includes the same set of variables as in the interest rate equation. While an empirical model of this sort would actually be econometrically identified, the failure to include any additional variables in the selection equation might make it very difficult to estimate any effects precisely.

Given the theoretical framework, it is not an obvious task to identify a variable that could be included in the selection equation but plausibly excluded from the interest rate equation. Any variable that affects the equilibrium loan size L^* will also affect the equilibrium interest rate i^* . One possibility, however, is to consider if, irrespective of whether a certain state actually borrowed in a certain year, there might be certain factors that made it more likely that the loan would have been reported by chroniclers and that historians would have subsequently collected data from these initial reports. Larger states in terms of population and territorial states for which capital cities were larger metropolitan centers may have been

³²Eichengreen and Mody (2004) have recently estimated a model of borrowing behavior that controls for sample selection bias of this type.

more likely to have attracted the attention of historians. So, if the French monarchy borrowed funds it may have been more likely to attract the subsequent attention of historians than if a small Swiss or German city-state contracted a loan.³³ There is very good evidence that capital city size is positively correlated with observing a loan for a given country in a given year. In the selection estimates to follow the variable $\ln(\textit{size})$ represents the log of a state's capital city's population in thousands, drawn from the Bairoch, Batou, and Chevre (1988) database.

I also make one further assumption when estimating the selection equation. For the reasons already presented, the process determining whether a given observation is missing is likely to vary depending on whether a state has already issued long-term debt at a prior date. If a state has already entered the market, then an observation may be missing for essentially random reasons. So, for example, there is probably no significant political or economic explanation why we observe an interest observation for Genoa in 1600 and 1700, but not in 1650, with this particular dataset. In contrast, if a state began borrowing in 1600 and we have no interest rate data for several centuries before 1600, then it seems much more plausible that the pattern of missing data is explained by credit rationing. In what follows, I include in the selection model all observations where an interest rate is recorded, and all observations at dates prior to the first recorded interest rate for a given state, time t^* . This implies that the process driving sample selection for all $t < t^*$ is determined by the selection equation below and that sample selection for all $t > t^*$ where i_{it} is not observed is assumed to be ignorable without having to condition on further information.

The full specification for the selection model is presented in equations (4) and (5) below. The interest rate equation remains identical to that in the OLS estimates from the previous section (excepting the inclusion of the additional selection parameter $\widehat{\lambda}_{it} \equiv \lambda(x_{it}\widehat{\delta})$, where x_{it}

³³In addition, once we control for the state's level of economic development by including the urbanization rate and the estimated return on private capital, it is difficult to suggest why the simple fact of having a larger capital city should produce lower interest rates on sovereign debt. This is further supported by the fact that if we estimate the OLS equations from Table 3 while including capital city size as an additional variable, its coefficient is generally not statistically significant, and it is positive. This would run contrary to the prediction that capital city size might be a further proxy for the level of economic development and should therefore be negatively correlated with interest rates.

represents the vector of variables in the selection equation and $\hat{\delta} = \Pr(\ln i_{it} \text{ observed})$. The selection equation includes the same set of variables, in addition to capital city size.

$$\ln(i_{it}) = \beta_0 + \beta_1 city_i + \beta_2 const_{it} + \beta_3 city_i * const_{it} + \beta_4 \ln(r_{it}) + \beta_5 \ln(urban_{it}) + \beta_6 neighbors_{it-1} + \beta_7 \hat{\lambda}_{it} + u_{it} \quad (7)$$

$$\Pr(\ln(i_{it}) \text{ observed}) = \Phi(\gamma_0 + \gamma_1 city_i + \gamma_2 const_{it} + \gamma_3 city_i * const_{it} + \gamma_4 \ln(r_{it}) + \gamma_5 \ln(urban_{it}) + \gamma_6 neighbors_{it-1} + \gamma_7 \ln(size)) \quad (8)$$

Table 4 provides maximum likelihood estimates of the selection model. As before, for each constitutional variable I consider first a restricted specification that does not include the interaction term $city_i * const_{it}$. I then report a full specification that does include the interaction term. Beginning with the restricted specifications that do not include the interaction term, the estimates of the interest rate equation suggest that city-states paid consistently lower rates of interest on their loans. The *city* dummy variable is statistically significant in three of the four specifications without the interaction term. The size of the coefficient on the *city* dummy variable is similar across the specifications. Based on these estimates, there also continues to be relatively little evidence for the unconditional version of the constitutional hypothesis which would lead us to expect positive and statistically significant coefficients on the four constitutional variables. The one partial exception here is the *monarchy* dummy.

In the Table 4 estimates the selection equations are also of direct interest since they have provide a further test of the different political hypotheses about government borrowing. In three out of four cases the coefficient on *city* is positive and statistically significant, implying that we are more likely to “observe” an interest rate for a city-state than for other types of states, meaning city-states are more likely to have borrowed. In contrast, apart from the *monarchy* dummy variable there is less evidence that constitutional constraints were

associated with a greater likelihood of access to credit.

When we turn to the Table 4 regressions that include the interaction term $city*const$ we see that the $city$ dummy variable has a negative sign in the interest rate equations and it is statistically significant at at least the 10% level in all but one case. As with the OLS estimates, we also observe that the coefficient on the $city$ dummy is more negative in the full specification where the $city$ dummy captures the estimated effect of this variable when constitutional constraints are at their maximum. This is consistent with my third hypothesis, though it should be noted that the interaction terms $city*const$ are again not statistically significant.

In all but one case in Table 4 the estimate of the selection parameter $\hat{\lambda}_{it}$ is negative. This is what one would expect based on the theoretical framework in Section 2. It implies that any unobserved factor that increases credit rationing will be associated with an increase in the equilibrium interest rate. Though the coefficient on $\hat{\lambda}_{it}$ has the expected sign, it is generally not statistically significant, implying that we cannot reject the hypothesis that the selection and interest rate equations are actually independent.

In order to give the reader a sense of the substantive magnitude of the differences between city-states and territorial states, with and without checks and balances, Table 5 presents a series of predicted values based on the Table 4 regressions that include the interaction term $city*const$. Each box shows the predicted probability of an interest rate being observed for different levels of the $city$ and constitutional variables, taking into account the interaction term and setting other variables at their sample means. It also shows the predicted interest rate, conditional on there being an interest rate observed, again setting all other variables at their sample means. As can be seen, when using the constitutional variables $unfree$, $coercion$, and $monarchy$ there is an effect whereby city-states in areas where rulers are constitutionally constrained are more likely to be able to gain access to credit, and at lower interest rates than are other types of states.

Overall, the results of the selection model provide strong support for the idea that there was a significant difference between city-states and other states in medieval and early modern

Europe in terms of ability to borrow at low interest rates. It is consistent with the idea that the distinctiveness of city-states stemmed from the fact that they were frequently politically dominated by merchants. When we expand the specification to allow an interaction effect between state type (city-state or territorial) and constitutional regime, we also see some evidence that the most credible borrowers appear to have been city-states when rulers were constitutionally constrained.

7 Controlling for the endogeneity of city-state development

The empirical tests have so far considered whether my conclusions regarding city-states are robust to controls and to sample selection bias, but they have not considered a third possibility. Rather than reflecting an underlying causal relationship, it may be that my results are driven by unobserved factors that led simultaneously both to the development of city-states in certain areas and to easier sovereign access to credit at low interest rates. So, for example, it may be that independent city-states emerged in areas where trade grew particularly rapidly in the early medieval period, and presence of abundant merchant capital from trade should have also produced lower interest rates.³⁴ This possibility should be controlled for to a significant extent in my OLS regressions by including estimates of the return on capital in private markets and the level of urbanization (as a proxy for economic development). Nonetheless, if measurement error in these two variables implies that they will fail to control completely for the possibility that certain capital-rich areas “self-selected” into becoming city-states, then my estimate of the effect of merchant political power on sovereign access to credit would be biased. Dealing with this potential bias econometrically would involve identifying one or more instruments for city-state development - variables that are plausibly correlated with the likelihood of a city-state being formed but which should have no plausible direct effect on interest rates for sovereign debt.

When attempting to find a suitable instrument for an endogenous regressor, one strat-

³⁴The literature on medieval city development beginning with Pirenne (1925) has emphasized the role of the growth of trade in their development. This phenomenon has also been emphasized by Spruyt (1994).

egy is to focus on historical accidents. Banerjee and Iyer (2005) have demonstrated how historical accidents involving the timing of British colonization in Indian regions led to the adoption of different systems of land tenure, and so timing of colonization can serve as a useful instrument. Historical accidents that are sources of exogenous variation can also involve country boundaries established during the colonial period, particular when these split ethnic groups that are otherwise similar.³⁵ In what follows I propose that we can use a particular historical accident as an instrument for city-state formation in medieval Europe.

In the year 843 as part of the Treaty of Verdun, the Carolingian empire that covered much of Western Europe was divided into three separate territories: (1) West Francia which gradually evolved into France (2) East Francia which gradually evolved into Germany and (3) Lotharingia which covered a central portion of territory running from the present day Netherlands to Northern Italy but which never evolved into a single state. While central authority receded throughout the Carolingian empire in the years after 843, quite arguably it collapsed most dramatically in Lotharingia. This territory derived its name from its ruler, Lothar, who nominally retained the imperial title.³⁶ The majority of city-states that eventually emerged in Europe several centuries later were situated in this central swath of territory. There are a number of reasons to suggest that the division of Europe that occurred at the Treaty of Verdun had the sort of "accidental" characteristics that make it useful for constructing an instrument for city-state development.

First, historians since Barraclough (1946) have emphasized that even if the settlement at Verdun in 843 had clear long lasting effects, as it helped lead to the development of France and Germany, the treaty had little to do with the prior emergence of homogeneous ethnic groups or national sentiments in any of the three territories; the division was instead largely dictated by dynastic considerations involving the desire to establish three relatively equally-sized territories, each of which had similar economic resources.³⁷ The division did

³⁵See Posner (2004) for an interesting recent example.

³⁶Technically this region referred to as *Franca Media* comprised both Lotharingia and the Kingdom of Italy, but I will follow the usage of referring to the entire region under the control of Lothar as Lotharingia.

³⁷See also de Planhol (1994) on this point.

not correspond to either pre-existing linguistic or ethnic boundaries. The division was also influenced by temporary military factors involving which of the three brothers who negotiated the Treaty of Verdun had military control of a territory and which local baron was allied with each of the three brothers that negotiated the division.³⁸

Second, it is difficult to argue that the subsequent collapse of the central territory of Lotharingia and the lesser degree of fragmentation that occurred in West Francia and East Francia was somehow predetermined. It appears instead that idiosyncratic dynastic events played a very important role. Airlie (1998) makes a convincing argument that one important element helping to lead to the collapse of Lotharingia was the bitter divorce dispute of King Lothar II who ruled from 855 to 869. Inheritance in the Frankish kingdoms did not yet follow a rule of primogeniture, which made the survival of kingdoms more susceptible to dynastic succession disputes than would have otherwise been the case.³⁹

In sum, while the settlement at Verdun in 843 has had generally recognized effects on the future path of European state development, the fact that some cities found themselves selected into a region where central control eventually collapsed to a greater degree than elsewhere seems to have occurred in large part as the result of historical accidents. Beyond the accidental features of the settlement at Verdun, it is also important to emphasize that this division occurred well before the emergence of city states in Europe and well before the resurgence in trade after the year 1000 that authors like Pirenne (1925) have emphasized as being crucial for city-state development. As a result, the boundaries of Lotharingia and its collapse cannot be explained by the prior existence in large and powerful cities in Lotharingia but not elsewhere. Nor does it seem plausible that the many accidental features that determined this outcome could have had a direct effect on the interest rates at which sovereign states borrowed several centuries later. These arguments imply that the division of the Treaty of Verdun should satisfy the requirements of an instrumental variable for subsequent

³⁸See Nelson (1997) and Fried (1997) for an in-depth account of Carolingian politics during this period, highlighting the chaotic nature of dynastic politics.

³⁹See Sharma (2005) for a consideration of the role of the institution of primogeniture in establishing political stability in early modern Europe.

city-state development. In the analysis that follows I use a dummy variable *Lotharingia* that takes a value of 1 for all states located in the former territory of Lotharingia, 0 for those states located in the former Carolingian territories of West Francia and East Francia, and 0 for states in territories that were not formerly part of the Carolingian empire

In addition to the *Lotharingia* variable, I also use one further variable as an instrument for city-state development. The variable *roman* represents the date at which a specific region was integrated into the Roman empire, with earlier integration implying a longer duration of Roman presence, and thus potentially greater influence of Roman heritage. There are a number of ways in which Roman heritage may have influenced the development of city-states. Pirenne (1925) argued that in Northern Italy and in Provence, members of the elite retained a Roman tradition of living in cities, rather than moving to rural areas as was the case in a number of other European regions.

Table 6 reports the results of the instrumental variables estimates. The regression results are quite consistent across the different constitutional variables. In all but one case we observe a large and statistically significant difference between the estimated interest rate for city-states and territorial states. In addition, the coefficient on the *city* variable is now more negative than in the OLS estimates, and it does not differ sizably between the regressions that include the *city*const* interaction term and those that do not. Taking the first regression using the constitutional variable *unfree* as an example, the estimated interest rate differential between a city-state and a territorial state is 3.2 percentage points. This evidence goes against the conclusions from the OLS estimates that city-states where rulers were constitutionally constrained were likely to be able to borrow at lower interest rates than were city-states in areas with unconstrained rulers. The instrumental variables estimates are similar to the previous results in suggesting that there was not a significant interest rate differential according to constitutional regime. As with the previous results one also continues to observe a coefficient on $\ln(r)$ that is consistent with a one-for-one effect of a change in the private return to capital (taking into account the confidence interval). The coefficient on the *neighbors* variable, designed to proxy for barriers to entry in the sovereign

lending market, also remains negative and statistically significant in all cases.

The final four rows in Table 6 report the results of several tests regarding the excluded instruments. The F-statistics correspond to a test that the coefficient on the excluded instruments is jointly zero in the first stage regression for the endogenous variable *city* and in the first-stage regression for the endogenous variable *city*const*. In all cases these lead to a clear rejection of the null. The next two lines report the results of a test of overidentifying restrictions which can help establish whether the instruments can be legitimately excluded from the interest rate equation. In all but one case the Hansen J-statistic and corresponding Chi² p-value suggest that the null hypothesis that the instruments are uncorrelated with the error term in the interest rate equation cannot be rejected. The result of the overidentification test for the remaining equation using the constitutional variable *protect* is more questionable. Together these tests provide strong evidence for the validity of the instruments.

In addition to the possibility of "self-selection" whereby certain country-level factors might lead simultaneously to the development of a city-state and to lower interest rates on sovereign debt, there is also a final endogeneity issue one might consider with my results. This involves the "military revolution" of the sixteenth and seventeenth centuries and the potential effect of this change on both state institutions and financial markets in Europe. The military revolution is generally recognized to have involved a very sizeable increase in the number infantry fielded by major European powers.⁴⁰ The exact causes of this revolution remain a matter of debate, with some scholars like Parker (1988) emphasizing exogenous changes in military technology and others suggesting that an increased intensity of inter-state competition in Europe drove these developments.⁴¹ In either case the advent of the military revolution may have influenced both the viability of city-states, and thus our likelihood of observing them, as well as constitutional restraints on executives. It is commonly argued that by the sixteenth century, city-states in Western Europe became obsolete relative to ter-

⁴⁰Downing (1992) presents figures showing that between the 1550s and the 1630s Spanish and French military strength doubled, with similar increases for other powers.

⁴¹See Hoffman (2005) for an interesting recent investigation of technological change and its effects on firearms prices in Europe before the Industrial Revolution.

ritorial states.⁴² This could have been linked to the military revolution to the extent that it altered the returns to scale in war-fighting. It has also been argued by Downing (1992) that the military revolution led to the demise of representative institutions in a number of countries, as rulers established “military-bureaucratic absolutist” institutions that were more efficient at quickly raising the increasingly large sums necessary for warfare. To the extent that the military revolution also triggered developments in terms of adoption of improved institutions of revenue collection and increasingly sophisticated institutions for government borrowing, this could point to another source of endogeneity bias in my statistical results. While fully examining the influence of the military revolution on my empirical results would require considerably more space, I did consider one initial possibility. By splitting the sample in 1550 and rerunning the Table 3-5 regressions while only considering the period 1250-1550 we should be able to estimate the effect of city-states, merchant power, and constitutional constraints on sovereign borrowing without any potential bias from the effects of the military revolution on these estimates.⁴³ When I did this I observed that in the OLS estimates the coefficient on the *city* dummy variable remained negative and statistically significant while being roughly twice the magnitude of the coefficients reported in Table 3. When I repeated the selection model estimates while using only pre-1550 data the results were similar while in the instrumental variables estimates the coefficient on *city* was similar in the case of the regressions using the constitutional variable *unfree*, but it was less statistically significant in the other regressions. Finally, in all of the estimates using the pre-1550 sample there was no evidence of a larger effect of constitutional constraints on borrower credibility.

8 Conclusion

In this paper I have considered three alternative hypotheses about the politics of sovereign borrowing in Europe over the very long run. First, access to credit at low interest rates may

⁴²This would follow Tilly (1990) in particular.

⁴³The chosen date of 1550 results in the sample being split roughly in half and also corresponds closely with the date first advanced by Roberts (1956) for the beginning of the military revolution.

have depended above all on the creation of constitutional limitations on rulers. This is the type of explanation emphasized by numerous recent papers on institutions and commitment. Alternatively, it may have been that the difference between constitutionally constrained and unconstrained rulers was less salient than was the difference between city-states and larger territorial states, because state type was critical for determining whether merchants were politically dominant. Finally, borrower credibility may have depended on the simultaneous presence of both a city-state and constitutional restraints on rulers. My empirical results provide relatively weak support for the unconditional form of the constitutional hypothesis and strong support for the second hypothesis involving city-states and merchant power. They also provide some support for the third hypothesis involving interaction effects. City-states were more likely than larger states to be able to borrow, and at low rates of interest, but city-states with republican institutions and city-states in regions where princely overlords faced institutional constraints on their authority appear to have been even more credible as borrowers than were city-states elsewhere. These findings have important implications for the study of the determinants of government credibility. They also suggest that it would be useful to extend the inquiry of this paper by further developing the key hypotheses, by collecting data that can test these hypotheses more accurately, and by making efforts to take account of the endogeneity of both city-state development and constitutional development.

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Table 1: States by Classification and Period of Entry into the Debt Market

City-states	Other states
Venice (1250-1750)	France (1400-1700)
Vicenza (1250-1300)	Netherlands (1400)
Florence (1300-1450)	Saxony (1450)
Genoa (1300-1750)	Castile (1500-1650)
Basel (1350-1400)	Low Countries (1500-1550)
Zurich (1350-1400)	United Provinces (1550-1750)
Cologne (1350-1450)	Austria (1550-1750)
Mainz (1400)	Piedmont (1650-1750)
Nuremberg (1400-1550)	Great Britain (1650-1750)
Naples (1500-1750)	Denmark (1700-1750)
Geneva (1500-1650)	Switzerland (1700-1750)
Bologna (1550-1650)	Tuscany (1700-1750)
Milan (1550-1750)	
Rome (1550-1750)	
Liege (1650-1700)	

Table 2: Variable Names and Summary Statistics

name	min.	max.	mean	std. dev.	source
$\ln(i)$	-0.80	3.33	1.62	0.60	Epstein (2000)
$\ln(r)$	1.25	2.56	1.90	0.30	Epstein (2004)
$\ln(urban)$	-0.22	3.66	2.50	0.83	Bairoch et al. (1998)
<i>city</i>	0	1	0.50	0.50	Epstein (2000)
<i>unfree</i>	0	1	0.53	0.50	DeLong and Shleifer (1993)
<i>coercion</i>	0	2	0.97	0.86	Tilly (1990)
<i>protect</i>	0	2	0.96	0.88	Acemoglu et al (2002)
<i>monarchy</i>	0	1	0.53	0.50	Epstein (2000)
$\ln(size)$	0	6.51	3.30	1.39	Bairoch et al. (1998)
<i>neighbors_{t-1}</i>	0	1	0.49	0.39	calculated from other vars
<i>lotharingia</i>	0.06	0.12	.084	.021	coded from Nelson (1995)
<i>roman</i>	-272	43	-121	126	coded by author

Number of obs=214 for all variables except $\ln(r)$ (n=94)

Table 3: OLS Estimates of Borrowing Costs

	Constitutional Measure							
	<i>unfree</i>		<i>coercion</i>		<i>protect</i>		<i>monarchy</i>	
<i>city</i>	-.188 (.119)	-.368 (.156)	-.192 (.113)	-.498 (.181)	-.206 (.112)	-.228 (.155)	-.126 (.122)	-.284 (.136)
<i>constitution</i>	-.019 (.128)	-.171 (.173)	.007 (.069)	-.129 (.105)	-.116 (.070)	-.125 (.112)	.149 (.122)	-.025 (.182)
<i>city*const</i>		.299 (.264)		.255 (.164)		.019 (.147)		.292 (.260)
$\ln(r)$	1.00 (0.22)	1.04 (0.23)	1.00 (0.22)	1.08 (0.23)	0.96 (0.21)	0.96 (0.22)	0.93 (0.23)	0.92 (0.22)
$\ln(urban)$	-.046 (.069)	-.073 (.083)	-.049 (.073)	-.093 (.083)	-.097 (.076)	-.097 (.069)	-.047 (.075)	-.080 (.082)
<i>neighbors_{t-1}</i>	-.405 (.226)	-.427 (.216)	-.428 (.203)	-.506 (.196)	-.366 (.209)	-.372 (.211)	-.520 (.161)	-.548 (.165)
<i>constant</i>	.375 (.529)	.483 (.529)	.380 (.528)	.587 (.525)	.685 (.479)	.690 (.487)	.463 (.502)	.719 (.496)
R^2	0.46	0.48	0.46	0.49	0.49	0.49	0.47	0.48

N=94, Heteroskedastic-consistent standard errors in parentheses, adjusted for clustering by state.

Table 4: Sample Selection - Access to Credit and Borrowing Costs

	Constitution measure							
	<i>unfree</i>		<i>coercion</i>		<i>protect</i>		<i>monarchy</i>	
Interest rate equation								
<i>city</i>	-.284 (.124)	-.383 (.186)	-.278 (.119)	-.549 (.211)	-.301 (.101)	-.303 (.210)	-.123 (.122)	-.305 (.185)
<i>constitution</i>	.038 (.125)	-.061 (.195)	.012 (.065)	-.121 (.083)	-.161 (.079)	-.160 (.111)	.148 (.117)	-.037 (.192)
<i>city*const</i>		.183 (.319)		.241 (.148)		.010 (.156)		.317 (.306)
$\ln(r)$	1.32 (0.39)	1.32 (0.50)	1.32 (0.39)	1.34 (0.41)	1.24 (0.29)	1.22 (0.30)	0.92 (0.26)	0.95 (0.30)
$\ln(urban)$	-.084 (.080)	-.010 (.087)	-.090 (.087)	-.126 (.091)	-.171 (.089)	-.166 (.091)	-.044 (.079)	-.091 (.112)
<i>neighbors_{t-1}</i>	-.509 (.240)	-.509 (.244)	-.506 (.217)	-.560 (.228)	-.464 (.206)	-.473 (.213)	-.519 (.156)	-.556 (.154)
<i>constant</i>	.208 (.607)	.290 (.700)	.242 (.600)	.448 (.603)	.763 (.520)	.777 (.515)	.468 (.494)	.730 (.479)
Selection equation								
<i>city</i>	.464 (.268)	.611 (.489)	.429 (.254)	.834 (.573)	.515 (.245)	.298 (.489)	.027 (.354)	.444 (.694)
<i>constitution</i>	-.343 (.331)	-.191 (.610)	-.134 (.203)	.106 (.388)	.292 (.171)	.176 (.276)	-1.19 (0.44)	-.737 (.670)
<i>city*const</i>		-.278 (.746)		-.386 (.444)		.240 (.361)		-.754 (.856)
$\ln(urban)$.120 (.209)	.134 (.300)	.152 (.219)	.190 (.269)	.336 (.219)	.320 (.247)	.092 (.250)	.159 (.302)
$\ln(r)$	-2.20 (0.57)	-2.23 (0.58)	-2.15 (0.57)	-2.20 (0.57)	-1.96 (0.57)	-1.91 (0.57)	-1.87 (0.55)	-1.78 (0.54)
<i>neighbors_{t-1}</i>	.103 (.522)	.082 (.530)	.033 (.512)	.081 (.577)	-.012 (.480)	.009 (.473)	.135 (.512)	.277 (.548)
$\ln(size)$.467 (.203)	.477 (.287)	.470 (.232)	.463 (.279)	.413 (.133)	.447 (.167)	.778 (.157)	.738 (.178)
<i>constant</i>	2.02 (1.25)	1.93 (1.40)	1.84 (1.26)	1.56 (1.42)	0.79 (1.44)	0.74 (1.49)	1.06 (1.30)	0.48 (1.45)
λ	-.336 (.258)	-.310 (.446)	-.336 (.278)	-.308 (.361)	-.352 (.167)	-.324 (.203)	.016 (.096)	-.040 (.208)

N=214 Heteroskedastic-consistent standard errors adjusted for clustering by state

Table 5: Interpreting the Effects of the Selection Model Estimates

	city-state	territorial		city-state	territorial
free	5.6 p=0.60	7.2 p=0.36	high protect (=0)	6.1 p=0.38	7.7 p=0.30
unfree	5.8 p=0.42	6.5 p=0.29	low protect (=2)	5.4 p=0.70	6.1 p=0.40
	city-state	territorial		city-state	territorial
low coercion (=0)	5.3 p=0.60	7.8 p=0.28	republic	5.5 p=0.69	7.4 p=0.52
high coercion (=2)	6.0 p=0.38	6.4 p=0.36	monarchy	7.0 p=0.16	7.0 p=0.25

Each cell records predicted interest rate and probability of rate being observed

Table 6: Instrumental Variables Estimates of Borrowing Costs

	Constitutional Measure							
	<i>unfree</i>		<i>coercion</i>		<i>protect</i>		<i>monarchy</i>	
<i>city</i>	-.625 (.219)	-.610 (.251)	-.620 (.225)	-.650 (.315)	-.617 (.233)	-.307 (.204)	-.548 (.220)	-.533 (.266)
<i>constitution</i>	.074 (.107)	.097 (.175)	.021 (.060)	.004 (.095)	-.133 (.068)	.090 (.141)	-.029 (.189)	-.007 (.236)
<i>city*const</i>		-.041 (.346)		.032 (.196)		-.462 (.287)		-.042 (.379)
$\ln(r)$	1.25 (0.35)	1.25 (0.35)	1.26 (0.35)	1.26 (0.34)	1.20 (0.32)	1.17 (0.41)	1.23 (0.35)	1.23 (0.38)
$\ln(urban)$.012 (.080)	.017 (.090)	.010 (.080)	.003 (.089)	-.045 (.087)	-.024 (.118)	.004 (.072)	.009 (.082)
<i>neighbors_{t-1}</i>	-.420 (.216)	-.417 (.226)	-.385 (.215)	-.395 (.242)	-.302 (.217)	-.134 (.354)	-.353 (.220)	-.346 (.235)
<i>constant</i>	-.005 (.715)	-.029 (.798)	-.011 (.712)	.022 (.773)	.351 (.638)	.056 (.903)	.041 (.654)	-.003 (.868)
R^2	0.35	0.34	0.36	0.36	0.39	0.17	0.39	0.38
$F_{instruments (city)}$	20.77	10.19	19.4	10.45	19.54	13.80	22.24	14.51
$F_{instruments (city*const)}$		7.27		10.00		5.17		16.26
Hansen J-statistic	0.03	0.11	0.08	0.13	2.37	0.87	0.56	0.72
Chi ² p-value	0.85	0.95	0.77	0.94	0.12	0.65	0.39	0.70

Heteroskedastic-consistent standard errors adjusted for clustering by state. The endogenous regressors are *city* and *city*constitution*. In the regressions where only *city* appears it is instrumented with *lotharingia* and *roman*. When both *city* and *city*const* appear they are instrumented with these two variables in addition to *lotharingia*const* and *roman*const*.