

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 two alternative arguments. 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. The second argument suggests that borrower credibility depended above all on the political strength of merchants, irrespective of a state's constitutional regime. While merchant political power cannot be measured directly, given the strong indications that merchants were especially powerful in cities, we should expect city-states in Europe to have paid lower interest rates on their debt than did larger territorial entities where merchants would be a minority within the political elite. When we consider a broad sample of cases over a long time span there is strong support for the proposition that credibility was linked to merchant power, but less support for the argument that constitutional checks and balances were the primary determinant of credibility. The constitutional regime appears to have made a difference for credibility, but only within the group of city-states in the sample. My empirical results are robust to a number of controls for alternative determinants, for sample selection bias, and for possible "self-selection" involving 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 during the late sixteenth century. In this paper I use data on a broad set of European states over a long time period (1274 to 1785) 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 an alternative hypothesis that access to credit depended above all on the political predominance of merchants, and merchants had greater influence in city-states than in larger territorial entities.

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, particularly for the cases of Great Britain and France. 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 unprecedentedly low interest rates, and Schultz and Weingast (2003) have extended this argument to a comparison with *ancien regime* France.¹ Their views have been challenged by O'Brien (2001) and Epstein (2000) who argue that the British government's "revolution" in public finance was actually a slow process dependent

¹For more on this subject see also Hoffman and Norberg (1994), North and Thomas (1973), Stasavage, (2003, 2002), Tracy (1985), Tracy (1994), Velde and Weir (1992), Sargent and Velde (1995), and Weingast (1997). Studies by Summerhill (2004) and Razaghian (2001) have considered issues of political institutions and credibility of sovereign borrowing in nineteenth century Brazil and the United States in the early nineteenth century. Tomz (2001) presents an interesting study of the determinant of borrowing costs for a panel of eighteenth and nineteenth century states.

upon administrative and technical reforms involving tax collection and financial instruments.² This implies that the observed relationship between constitutional change and borrowing credibility is spurious. 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. Likewise, Acemoglu, Johnson, and Robinson (2003) 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. Based on data for British land rents, there is no evidence of a post-1688 shift in returns, which we would expect to observe if property rights suddenly became more secure.³

An alternative to the constitutional hypothesis is to suggest that irrespective of a state’s constitutional regime, what really determines borrower credibility is whether those who lend to a government have significant political power within a state. In the medieval and early modern European context since merchants were a primary source of credit, then we should expect to observe easier access to credit at low interest rates for states in which merchants were politically dominant. When merchants held significant influence within a state’s political institutions then they could ensure that decisions were taken to prioritize servicing debt rather than opting for opportunistic defaults. In the numerous cases where a merchant oligarchy actually directly controlled government, then there would also be a clear effect on credibility of debt issues. While merchant power is difficult to observe directly, particularly for a broad set of cases, there is abundant historical evidence suggesting that merchants were more politically powerful in city-states than in larger territorial entities. The political role of merchants in city states has been emphasized by authors like Pirenne (1910, 1925), Brady

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

(1991), Epstein (1996), Pezzolo (2004, 2003), Lane (1973) and Tilly (1990), and it has been considered in relation to public debt by Tracy (1985, 1994) and Fryde and Fryde (1963). In cities merchants made up a significant portion of the elite population. In contrast, in larger entities merchants had to contend for political power with the landholding nobility and other groups.

In addition to examining the two above arguments, one might also want to consider the possibility of interaction effects between state type (city-state or territorial state) and constitutional regime. It might be that merchants were most likely to gain political power in city-states that had more democratic constitutions. It might also be the case that city-states in regions where princely overlords faced constitutional constraints were more likely to be able to establish credibility as borrowers. This paper also considers these further possibilities.

I proceed by elaborating the alternative theoretical arguments in Section 2. In the subsequent section I draw on several different data sources in order to test the different arguments about politics and government borrowing. Section 3 begins by briefly presenting historical evidence on five specific cases involving constitutions, merchant power, and public borrowing - Italian city states (Venice, Genoa, and Florence), Germany city states, France, the Netherlands, and Great Britain. This historical evidence is useful for considering the plausibility of the different hypotheses, before turning to a broader set of quantitative evidence beginning in Sections 4, 5, and 6. In Section 2 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. I also suggest that 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 effect on borrowing credibility.

Section 4 turns to the quantitative assessment of the constitutional and merchant power 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.

While using nominal interest rates to gauge borrower credibility poses several potential problems (as discussed in detail 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 then use four separate measures of constitutional constraints on rulers. The strengths and weaknesses of each of these is discussed in Section 4. The first of these is Delong and Shleifer's (1993) binary classification of Western European regions into non-absolutist regimes and absolutist regimes, with the latter category characterized by "the subjection of the legal framework to the prince's will". The second measure is derived from Charles Tilly's (1990) distinction between regions where the development of state institutions was based primarily on the use of coercive force and those where state development depended primarily upon capital accumulation. The third measure is an indicator of constitutional protections for capital holders developed by Acemoglu, Johnson, and Robinson (2002) for early modern Europe. Finally, I also use Epstein's (2000) distinction between republics and monarchies.

Using the above data, in Section 5 I estimate the political determinants of borrowing costs, controlling for the private return on capital and the rate of urbanization in each region during the time considered. Based on a set of OLS regressions, I find consistent evidence that city-states paid lower interest rates on their debt. There is much less indication that constitutional checks and balances were associated with lower borrowing costs. None of the three constitutional measures based on Delong and Shleifer (1993), Tilly (1990), or Acemoglu, Johnson, and Robinson (2002) are associated with lower interest rates. There is a clear indication that states with republican institutions borrowed paid less to service their debt, but the vast majority of sample observations for republics were also city-states (the reverse was less true). In addition, I also find some evidence of an interaction effect between merchant power and political institutions. 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 nominal interest rate on its debt. There are two important reasons for this extension. First, 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 issues would lead to underestimating the magnitude of these political effects. Second, investigating whether a state borrowed is an interesting issue in its own right and one that allows a further test of my main hypotheses. If highly risky borrowers are rationed out of the debt market, then 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 suggest that city-states were less likely than other entities to be “rationed”, while there is also some evidence that constitutional constraints were associated with a lower likelihood of credit rationing. Based on these regressions, I also continue to observe that city-states paid lower interest rates on their debt than did larger states, and the magnitude of this effect is now substantially larger than in the OLS estimates. Finally, there continues to be little evidence of a general effect of constitutional checks and balances on interest rates. The constitutional regime does appear to have mattered for borrowing, but only within the group of city-states.

Section 7 reports the results of a final empirical test that considers whether my conclusions may be biased by “self-selection” the possibility that certain conditions which led to the development of borrowing credibility also led to a region becoming an independent city-state. One possibility could involve initial constitutional constraints. In regions where princely overlords were initially constitutionally constrained then this could have led both to the emergence of city-states and to economic developments that would subsequently facilitate government borrowing. Other possible selection factors here may involve different degrees of initial urbanization and geographic factors. After considering a number of possibilities to

control for the possibility of self-selection, I continue to reach the conclusion that city-states paid consistently lower interest rates on their debt issues than did larger territorial entities.

2 Hypotheses about government borrowing

In this section I develop my two core hypotheses involving the effect of constitutional checks and balances and of merchant power on borrower credibility. I also consider the possibility of interaction effects between constitutional regime and state type (city-state or larger territorial entity). Before doing this, however, I first discuss why we might expect borrower credibility to be associated with both a lower interest rate and a higher probability of being able to borrow.

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).⁴ We can illustrate the possibility that political conditions might influence both interest rates and the quantity of credit using the following simple schema based on Ghosh, Mookherjee and Ray (2000). It is commonly argued that in the case of sovereign debt, repayment depends on reputational constraints, since third-party enforcement is weak. As a result, in a context of repeated interaction between a lender and a sovereign borrower, any loan contract will need to satisfy the following type of incentive constraint for the borrower (shown in expression 1).⁵ Take a loan that has a value of L an interest rate of i , and for which the borrower expects to produce $F(L)$ units of output (where $F(\cdot)$ is a standard production function reflecting decreasing marginal returns). Assume that the borrower has a

⁴Weingast (1997) has emphasized that given the nature of the sovereign debt problem, political institutions may influence both interest rates on debt and whether credit is rationed. See also Robinson (1998) on this point.

⁵By a loan contract here I am referring to an equilibrium where a lender follows a trigger strategy of refusing further finance if a borrower defaults.

discount factor of δ and, in addition, the borrower has an outside option in the case of default which gives a payoff of v in every period. This outside option represents the combined costs and benefits, both political and economic, that a government would encounter if it defaulted. The outside option could be influenced by a number of different political and economic features that I will consider at greater length below. One possibility is that v reflects constitutional features, so for example strong representative institutions may allow parliaments to punish sovereigns that default by withholding future tax receipts. In this case constitutional checks and balances would imply a lower level of v . The second possibility is that v reflects the balance of power between different social groups in a country. Irrespective of the constitutional regime, if merchants are politically powerful, then in case of default they may take actions like helping to remove a ruler from power, implying merchant power is associated with a lower level of v . On the other hand, if those who pay the taxes to service debt are politically powerful, then we might expect default to be associated with a higher value of v than would otherwise be the case.

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

The borrower's constraint in expression (1) 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. This idea is central to the prediction of credit rationing in the reputational model of sovereign debt developed by Eaton and Gersovitz (1981). In addition to the borrower's incentive constraint, Ghosh Mookherjee and Ray (2000) also assume that equilibrium in this debt market needs to satisfy the following constraint for lenders where z represents a fixed level of profits that reflects the bargaining power of an individual lender, and r reflects the return on an alternative loan to a "safe" borrower.

$$z = L(i - r) \quad (2)$$

Using the above assumptions, it can be shown that under certain conditions equilibrium credit will be rationed. The logic behind this result is that for each level of the interest rate, beyond a certain loan size it will be impossible to satisfy both the borrower's repayment constraint and the lender's profit constraint. The result is that in equilibrium the lender will offer a smaller loan than that desired by the borrower. Credit rationing will occur as long as the outside option v is above a certain threshold value. Above this threshold value an increase in v will lead to both an increase in the equilibrium interest rate and a decrease in the equilibrium size of the loan. As a result, to the extent we believe that political conditions influence the value of v , then we should expect these conditions to influence both the interest rate and the quantity lent. While I have presented v here as a known quantity, in order to make the model more realistic, one could just as easily assume that v is an expected outcome that is subject to uncertainty. So, a high level of v would imply an expected low cost for the sovereign from defaulting, and thus a high level of default risk.⁶ For early modern European states the data collected by Epstein (2000) provide information on loan rates. While this dataset does not provide information on loan quantities, we can make use of the fact that different states first appear in the data set at different dates. When we have a record of borrowing at a specific interest rate by Venice during the thirteenth century, but no such record for France or Castile this may imply that the French and Castilian monarchies were rationed out of the market for long-term debt at this time.⁷

In sum, we should expect that if certain political conditions lower the value of v and thus make it less likely that a sovereign borrower will default, then the effect of these conditions may be observed both with respect to the interest rate and the quantity lent. The next step is to consider what precise "political conditions" are likely to have these effects

⁶The model would stay the same provided we did not assume any asymmetry of information about v between borrower and lender. A more realistic assumption that borrowers have private information about v would generate more complex dynamics, but it would not be likely to change the underlying prediction regarding the effect of the level of v on the interest rate and quantity lent.

⁷There is one complication I overlook here in that standard theories of credit rationing explain why lenders provide less credit than desired by borrowers, but one may need to make further assumptions to generate a result where a potential first-time borrower is completely excluded from the market. See the discussion in Parikshit, Mookherjee, and Ray (2000).

2.2 Constitutional checks and balances

One possibility is that incentives involving default depend upon a country's constitutional regime. In cases where rulers share power with a representative assembly that has an important decisionmaking role, then default will 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. This argument is made by Robinson (1998). A second possibility, raised by Weingast (1997) is that a representative assembly can help coordinate the actions of lenders with respect to the crown. In cases where there are multiple lenders and a sovereign defaults, then the penalty from default v will be higher if lenders are able to enforce a credit boycott.⁸ 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 to default on a loan. 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 above hypothesis about constitutional checks and balances. As noted by Graves (2001), while the experience of English parliamentarianism is well known, most states in medieval Europe actually had active representative assemblies. A number of authors have argued that one of the key functions of 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

⁸This argument effect is also discussed by North and Weingast (1989).

others, and Tracy (1994) refers to a norm of “no taxation without negotiation” rather than a binding legal constraint on rulers.⁹ Epstein (2000) argues that instead of assuming medieval rulers had free rein to transgress the property rights of their subjects, they were actually often hemmed in by a series of institutional constraints. 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 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 Merchant power

Instead of emphasizing the role of constitutional constraints, an alternative argument is that irrespective of the constitutional regime, what really determines incentives involving default is whether those who lend to a state hold significant political power. If lenders enjoy political authority, then they will be able to use this to either block a decision to default or to impose a heavy cost on any ruler that might make such a decision. A ruler who has the option of defaulting may find it more likely that they are replaced in such cases, implying a lower value of v in expression (1). In the medieval and early modern European context, merchants provided an important, if not exclusive, source of capital for sovereign loans. As a result, we might expect that in those cases where merchants held substantial political power, then they would have been more likely to make loans, and to charge lower interest rates than would have otherwise been the case.

The political strength of merchants is not something that can be directly observed using existing data for such a broad group of countries over this long a time period. What I suggest instead is that we can test the argument indirectly by observing factors that are likely to

⁹See Hoffman and Rosenthal (1997) on this issue, as well as Major (1960) who provides a classic account of representative institutions in Renaissance France.

be associated with merchant political power. One logical possibility involves the difference between city-states and larger territorial entities. The literature on city development in Europe has emphasized that city states were often politically dominated by merchant groups. Merchants either had substantial direct influence in state politics, or in many cases they actually directly ruled a city. This applies whether one 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) or to city states in Germany (Fryde and Fryde, 1963). 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. I 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. Moreover, it seems clear that elites in city states had a habit of establishing an unequal burden of taxation, with revenues drawn disproportionately from subject rural areas and the urban poor (Tracy, 1994; Epstein 1996). In strong contrast to the description of merchant dominance in city-states, there are fewer suggestions that merchants were as politically dominant in larger territorial states like France or Castile during the early modern period. 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, conclusion) 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, as I will discuss in Section 3 below.

One point to emphasize with the merchant power hypothesis is that any empirical test of it will need to distinguish between the political power of merchants and the direct economic

effect on interest rates due to the presence of merchant capital. As the stock of capital in an economy grows, then we would expect the rate of return on capital to decrease (in the model above equivalent to a drop in r), and this would also imply a drop in the interest rate on sovereign loans. In the empirical tests in Sections 5, 6, and 7, I control for this effect both by using an estimate of the private return on capital in Europe during different time periods, as well as by including rates of urbanization for different countries for each time period.

A final further point involving the merchant power argument is that this is a prediction about its effect on sovereign borrowing, not about economic performance more generally, or the viability of the city-state model in a changing international context. As a result, this argument 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; Tarrow, 2004; Lachmann, 2003).

2.4 Interaction effects

One logical extension to the above two arguments is to consider the possibility of interaction effects. First, it may be that merchant power determined the costs for sovereigns of defaulting, and merchants were more likely to hold power in city-states, but in addition, merchants were more likely to hold political power in city-states where representative assemblies had significant prerogatives. One reason for this is that representative assemblies could allow merchant groups to better coordinate their actions with respect to a sovereign as suggested by Weingast (1997). We might also expect a second interaction effect involving the relationship between a city state and the larger political region within which it is located. 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, given the nature of the material, it makes sense to review historical evidence on several individual country cases. This historical evidence provides initial evidence on the plausibility of the constitutional and merchant power hypotheses. Below, I briefly review the politics of public debt in five different cases which are presented in chronological order according to the date at which states established a long-term debt: (1) the Italian city-states of Florence, Genoa, and Venice, (2) the German city-states (3) France, (4) the Netherlands, and (5) Great Britain. The evidence from these five historical cases 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. These cases also raise interesting questions about the role of constitutional arrangements in establishing credibility for government borrowing. In particular there are several examples where constitutional arrangements shifted significantly but this had no observable effect on borrower credibility.

3.1 Florence, Genoa and Venice

These are the three prototypical examples of independent city-states that quickly established a reputation for sophistication in long-term government borrowing and for consistent 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.¹⁰ It has long been observed that merchants had an important position in the establishment and subsequent governance of these three city republics.¹¹ 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. Some loans in Florence and

¹⁰See Fryde and Fryde (1963) on early developments in Flemish cities.

¹¹Lane (1973), Epstein (1996), Brady (1991), Waley (1989).

Venice 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. Over time, debt ownership spread to involve a substantial percentage of the citizenry. Pezzolo (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. 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.

One interesting feature of public borrowing in Italian city states is that the 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. Between 1494 and 1530 there were two attempts to revive Florentine republican institutions before the Medici established lasting control and a hereditary form of government in 1530. Despite this major constitutional shift, there is little indication that the Florentine government under the Medici found it any more difficult to borrow than did Florence during the republican period.¹² We observe a similar 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 revenue demands that pushed the Genoese republic into financial difficulty. In fact, there is little evidence that this was the case. Brady (1991) argues that the merchants who controlled government in Genoa actually benefited from outside control by the Hapsburgs, because it consolidated their position against domestic opponents. If we consider interest rates on Genoese debt, the data cited by Epstein (2000) suggest that the nominal interest rate remained near 4% both before and after the assertion of Hapsburg control. This is also supported by data on yields for debt issues made on behalf of the Genoese republic by the

¹²The data collected in Pezzolo (2001) show that during the pre-1434 Republican period, Florence paid between 3.33 percent and 5 percent on its debt. Between 1434 and 1494 under Medici control it paid between 2.25% and 3.75%.

Casa di San Giorgio during the sixteenth and seventeenth centuries (Homer and Sylla, 1996, p.119) In sum, we might conclude from the Florentine, Venetian, and Genoese cases that political dominance of merchants and widespread ownership of public debt played a role in establishing credibility, but the effect of constitutional arrangements is more open to question.

3.2 German city states

It is important to emphasize that the city-states of Northern Italy were by no means the only medieval and early modern examples of cities that issued their own long-term debt. In addition to a number of cities in Flanders, there are also historical records from a number of German city-states that were self-governing in financial matters. There is also historical evidence for the predominant role played by merchants in these cities (Fryde and Fryde, 1963). 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 absence of constitutional constraints on princely overlords in Germany should therefore logically have translated into a higher level of interest rates on debt issues. It is not clear that this was the case. Mainz 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). The quantitative tests in the following sections further support the argument that the Italian city states were not exceptional in paying low interest rates.

3.3 France

The French monarchy has frequently been used as a prototypical case of a government that lacked credibility as a borrower. While this conclusion can be overdone, there is nonetheless much evidence to support it. First of all, despite the models provided by Italian city-states and by a number of cities within France which 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.¹³ One obvious reason for this persistence of high interest rates is that the French monarchy defaulted on a number of occasions. It is difficult to argue that the French monarchy's delay in establishing a long-term debt can be explained by the absence of demand for credit, because the French monarchy was constantly engaged in wars before this date. Given this fact, we might also want to ask whether the French monarchy's late development of a long-term debt was linked to the fact that it was initially rationed out of the credit market by lenders.

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. Sargent and Velde (1995) have argued that after the death of Louis XIV in 1715, if the Regent who ruled France had decided to reconvene the Estates-General, as a number of his advisors had suggested, then this would have allowed the French monarchy to establish financial credibility in the same way that the British crown is thought to have done so after 1688. This is an interesting counterfactual conjecture.¹⁴ What is less often recognized about the history of French repre-

¹³See Collins (1988), Hoffman and Norberg (1994), and Stasavage (2003 ch.3).

¹⁴Although historical evidence also suggests that those who advocated convening the Estates-General hoped the Regent would do so precisely because they believed that the Estates would prefer default over raising new taxes. See Stasavage (2003).

sentative institutions 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). One important reason for this series of events was that the financial needs of the Hundred Years' War placed French monarchs in a difficult position. 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, then 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. As noted above, 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 (along with Northern Italy), 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. 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, implying less rationing.¹⁵ 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

¹⁵See t'Hart (1993,1999) and Tracy (1985).

own military operations. By the middle of the sixteenth century, the independent Dutch republic was able to borrow at rates as low as 4% (t'Hart, 1999). One interesting question here is why the establishment of political independence from the Hapsburgs did not result in a more immediate drop in interest rates on debt issued by the Estates.

The Dutch case may be used to provide support for both the constitutional and merchant power 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 highly constrained institutionally. 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 believed to have resulted in greatly improved access to public credit. It also represents the first instance where a large territorial state was able to borrow in a sustained manner at the low interest rates that previously had prevailed only for city-states and for the Dutch republic. While the British crown contracted loans backed by customs revenues as 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.¹⁶ North and Weingast (1989)

¹⁶There are several episodes during the reign of Charles II (1660-1685) that support this argument.

have argued that the institutional changes of the Glorious Revolution established a balance between Parliament and the monarchy - monarchical ambitions were checked by Parliament, but parliamentary prerogatives were also checked by the maintenance of the monarchy. According to this argument, establishment of Parliamentary veto power over public finance allowed the British Crown after 1688 to borrow unprecedented sums at significantly lower interest rates than had prevailed before the Revolution. Historical evidence provides some support for this argument, but it also raises significant questions. It is true that the British Crown was on occasions between 1688 and 1715 able to borrow at interest rates as low as 6%, compared with rates of 6-8% before 1688, and in addition it was able to borrow substantially larger quantities than before the Revolution. However, 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).¹⁷ In work elsewhere (Stasavage 2005, 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. I have suggested that 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 focusing on how merchants were able to gain significant political power in Great Britain through party politics.

3.6 Summary

This section has used historical evidence to examine the plausibility of the two alternative arguments involving constitutional checks and balances and merchant political power. In the

¹⁷See the evidence presented in Sussman and Yafeh (2004) and in Stasavage (2005, 2003).

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 also evidence from Great Britain and the Netherlands that the reinforcement of constitutional checks and balances may facilitate access to credit. However, the cases here also raise important questions about constitutional considerations. In several instances where a state underwent a shift in its constitutional regime, as occurred in Genoa, in Florence, and in France, we do not observe a significant change in borrowing behavior. The next section describes the evidence that will be used to conduct quantitative tests of the merchant power and constitutional 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 that has been collected by Epstein (2000). 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 for measuring government costs of borrowing would use information from secondary markets to calculate yields on long-term government debt at different points in time. Not surprisingly, with a few exceptions such as England and France in the latter part of the eighteenth century or Venice during the fourteenth century, this type of data is simply not available for the vast majority of states 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) for his study *Freedom and Growth*. The long-term debt 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, ranging from small Italian city states to large monarchies like France or Castile. It should be acknowledged that there are potential weaknesses with using these interest rates, which are not direct indicators of market expectations, to measure credibility. For one, it is well known that in many European cases before the eighteenth century loans to sovereigns involved an element of compulsion. So, for example, some of the interest rates reported for French borrowing during the late seventeenth and early eighteenth century are imperfect measures of credibility, because they do not control for the fact that the French crown at this time made a habit of pressuring government officeholders into purchasing debt.¹⁸ However, it was hardly the case that only large monarchies or governments lacking credibility resorted to compulsion in this manner. As already noted, the Venetian and Florentine republics made regular use of forced loans. As a result, even if the existence of compulsion introduces measurement error, it does not necessarily imply a bias that would lead us to consistently underestimate borrowing costs for governments lacking credibility.

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 11 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. So, for example, there is probably no important causal factor driving the fact that we observe an interest rate for Genoa in 1600 and 1700, but not in 1650. Finally, while the interest rate data used were not collected by Epstein (2000) with the specific intention of identifying the

¹⁸See the extensive study by Doyle (1996) on venal officeholding for a discussion of this point.

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 Liege for example began borrowing long before 1650. Nonetheless, the overall order of appearance for states here is consistent with existing historical accounts of the development of sovereign borrowing in Europe.

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 between environments where political institutions placed constraints on executives versus 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. Alternative, more subjective coding schemes produced similar results for the empirical tests in the following sections.

I also use four different measures to proxy for the presence of constitutional checks and balances. The first of these *free* is a binary indicator produced by Delong and Shleifer (1993). According to these authors, a value of 0 for this variable corresponds to an “absolutist” state where the legal framework is subject to the prince’s will, and therefore property is insecure. A value of 1 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, an empirical result they suggest is explicable by the greater degree of property rights protection in free regions. One aspect of Delong and Shleifer’s variable worth 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 that have already been mentioned above. First, the extension of Hapsburg control in regions like Northern Italy resulted in a *de facto* change in the 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 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 (1) those where capital accumulation predominated (-1), and those where the two types of social organization were more equally matched (0). 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 (from 1 to 7) “depends on the formal rights given to urban merchants, particularly their

protection in the event of a dispute with the nobility or monarch”.¹⁹ The chief difference in practice between the “protection for capital” variable and the variables *free* and *coercion* is that the former measures 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 (1). One consequence of this coding choice is that the *protect* variable does not capture the fifteenth and sixteenth century decline in the strength of representative institutions to which many historians have referred (as described above).

The final constitutional variable I use is a dummy variable, *republic*, which equals 1 for states that are republics and 0 for states that are monarchies. 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. This applies to the cases of Florence under the domination of the Medici and Genoa during the period where it was controlled by the Hapsburg empire. 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.²⁰ The only examples of larger territorial states that were republics are Switzerland and the Netherlands. As a consequence, any empirical finding regarding the *republic* dummy variable applies especially to city-states, but it may be more difficult to draw inferences about the effect of republican institutions within the group of larger territorial entities.

¹⁹They also produced a measure of “executive constraints” for European countries pre-1800, based on the definition of this variable from the Polity dataset. In my sample their executive constraints measure was virtually identical to their protection for capital measure.

²⁰The proportion of city-state observations that were also republics is not as large (39 out of 57).

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 (3). For the moment I leave aside issues of sample selection and of "self-selection" which will be considered in the following two sections. In equation (3) i_{it} represents the nominal interest rate on public debt in country i at time t . One obvious economic determinant of this rate is the rate of return on private capital r . While we do not have country-by-country data available for the private return on capital, Epstein (2004) has produced an estimate of the average private return on capital broken down into fifty-year periods, and I use this measure in the regressions below.²¹ In addition, I control for the level of urbanization, which serves two purposes.²² First, more urbanized regions were those in which capital was more abundant (urbanization is often used as a proxy for income per capita for Europe during this period), and therefore in highly urbanized areas we should expect the return on capital to have been lower than elsewhere. Since the r_t measure does not vary across states, the urbanization measure $urban$ should thus help control for regional variations in the return to capital. In addition, the measure of urbanization should help control for a number of other possible determinants of interest rates on government debt related to levels of development. In concrete terms this measure will distinguish areas in the low countries and in Italy which were heavily urbanized at an early stage from areas like Germany and France where rates of urbanization were much lower throughout the period considered here.²³

$$\ln i_{it} = \beta_0 + \beta_1 city_i + \beta_2 const_{it} + \beta_3 city_i const_{it} + \beta_4 \ln r_t + \beta_5 \ln urban_{it} + u_{it} \quad (3)$$

²¹I 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).

²²The urbanization data was constructed by Bairoch, Batou, and Chevre (1988) based on an extensive survey of European city populations.

²³I also experimented with adding a dummy variable for new borrowers, based on the previous finding from Tomz (2001) who uses 18th and 19th century time series evidence to show that new borrowers paid an interest rate premium when compared with established sovereign borrowers. The dummy variable for new borrowers was not statistically significant in my sample and was excluded from the final reported regressions. It is likely that the low frequency of the interest rate data I use makes it difficult to capture this type of effect.

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 - *free* which is De Long and Shleifer’s measure of absolutist vs. free regions, *coercion* which is designed to capture Charles Tilly’s distinction between coercion and capital, *protect* which is Acemoglu, Johnson, and Robinson’s measure of constitutional protections for merchants, and finally *republic* which is the dummy variable distinguishing between monarchies and republics (based on the classification in Epstein (2000)). While the specific definitions of these four constitutional variables differ, given that they are each designed to capture a similar phenomenon, it makes sense to consider them separately in a series of regressions rather than including them simultaneously.

Table 2 reports the OLS estimation results. For each constitutional variable I first consider a specification that does not include the *city * const* interaction term, followed by a specification with the interaction effect. In the specifications without the interaction term the dummy variable *city* is negative and statistically significant in the first three cases. The substantive magnitude of this effects is also significant, although not large. Taking the first specification and setting the level of urbanization and of r at the sample mean, a city state in an “unfree” region would be expected to pay 4.6% interest on its debt, whereas a larger territorial state in an unfree region would pay 5.6% interest. It should be noted that this result with regard to city states is not driven primarily by the influence of Italian city-states like Florence, Venice, and Genoa. When one substitutes two separate dummy variables into equation (3), 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 actually the more negative of the two. The same conclusion holds for the selection model estimates in the following section.

None of the first three constitutional variables is statistically significant in the specification without interaction effects. The *protect* variable is borderline significant, but it actually has the “wrong” sign. In the fourth regression the coefficient on the *city* dummy is smaller in magnitude and no longer statistically significant while the coefficient on the *republic* variable is negative and highly significant. As discussed above, it seems unlikely that this result can

be used to make a general prediction about the effect of republican institutions, because the vast majority of republics in the sample were also city-states, and there are also questions about the extent to which states like Genoa and Florence after 1500 were actually republics in practice. In these regressions we also observe that the coefficient on urbanization is consistently negative and statistically significant, as we would expect, and the variable for private returns to capital has a positive and highly significant coefficient. The coefficient on r is positive and significant as we would expect, although even when taking the 95% interval into account we would conclude that a change in r has a greater than one-for-one effect on sovereign borrowing rates. One reason for this may be that in addition to capturing the effect of a falling return to capital over time, this variable is also capturing other trends. Unfortunately, since r varies only over time it is not possible to include it simultaneously in the regression with period-specific effects.²⁴

The next specifications in Table 2 include the interaction term *city * const*. In these regressions there is continued, although not unambiguous, evidence that city-states borrowed at lower rates of interest. There is also some evidence of an interaction effect whereby the combination of being a city-state in a region where a ruler was constitutionally constrained allowed for borrowing at particularly low rates of interest. Finally, there is no indication in any of the four specifications with the interaction term that the constitutional regime alone was associated with lower interest rates. In the specification using the constitutional variable *coercion* we observe that city-states are estimated to borrow at lower cost, and in addition city states in regions where rulers were institutionally constrained are estimated to have borrowed at even lower rates of interest. In the specification using the *protect* constitutional variable, only the dummy variable for city-states is significant, and it continues to have the expected negative sign. Finally, in the specification using the *republic* dummy variable only the interaction term is statistically significant, and it has a negative sign. This suggests that republican institutions only had an effect on interest rates within the group of city-states.

²⁴In all the regressions in this paper very similar results for the constitutional and city variables were obtained when including period dummies instead of the private return to capital variable.

In sum, the OLS estimates in Table 2 provide support for the merchant power hypothesis to the extent that city-states were more politically dominated by merchants than were larger states. They provide less support for the argument that constitutional constraints determine borrower credibility irrespective of the size or social composition of a state. This is consistent with the historical evidence presented in Section 3. The next two sections demonstrate that these results continue to hold when considering issues of sample selection involving the decision to lend, as well as self-selection whereby unobserved factors might lead both to city-state development and lower 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.²⁵ The OLS estimates also ignore the opportunity to provide a more complete test of my political hypotheses by examining both how much states paid on their loans and what determined whether states borrowed at all. As argued above, existing historical accounts suggest that some states entered the market for long-term sovereign debt much earlier than others. One possibility emphasized by Epstein (2000) is that this depended on lags in adopting the financial mechanisms for long-term credit. Another possibility, and the one that I will focus on here, is that states did not actually enter the debt market until they were judged to be sufficiently creditworthy. In this section I estimate a selection model that attempts to take direct account of this fact. Since borrower credibility may well produce both lower interest rates and a higher volume of borrowing, the selection model also allows a more complete opportunity to test the alternative political hypotheses involving constitutional constraints and merchant power.

Estimation of a selection model following the method developed by Heckman (1979)

²⁵Eichengreen and Mody (2004) have recently estimated a model of borrowing behavior that controls for sample selection bias of this type.

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 conditions producing lower interest rates on government debt should also lead to a lower likelihood that a state is rationed out of the long-term debt market. Any factor that lowers the equilibrium interest rate by lowering v will also raise the quantity lent. 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 be identified in econometric terms, the failure to include any additional variables in the selection equation would also likely make it very difficult to estimate any effects precisely.

Fortunately, there are several additional factors that might influence the probability that a government is rationed without necessarily altering the equilibrium interest rate. In presenting a model of credit rationing, Williamson (1984) emphasizes the importance of monitoring costs for the lender. When it is more costly for a lender to monitor a borrower's *ex post* financial situation, this can produce an incentive to ration credit. In the historical context considered here, plausible determinants of monitoring costs could involve distance and the development of financial institutions. To the extent that states relied on foreign borrowers, greater distance from these borrowers should raise monitoring costs. Likewise, adoption of the most recent (for the period) financial techniques could lower monitoring costs. For the selection equation below, I have hypothesized that both the distance and institutional development factors may be proxied by two variables. The variable n is a dummy that takes a value of 1 if a state's nearest neighbor has already entered the debt market, which implies that foreign borrowers may have less distance to travel to monitor their operations and likewise, a state may find it easier to imitate a neighboring state's financial institutions (as was the case for example with Great Britain imitating Dutch financial institutions after 1688). The variable $\ln pop$ represents the log of a state's capital city's population in thousands, drawn

from the Bairoch, Batou, and Chevre (1988) database. This variable is redundant in practice for explaining interest rates when urbanization is included in the interest rate equation, but capital city size may also be closely related to development of the institutions that lower costs of monitoring sovereign financial behavior.

Finally, 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. 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 a random process.²⁶

The full specification for the selection model is presented below. The interest rate equation remains identical to that in the OLS estimates from the previous section (excepting of course the inclusion of the additional selection parameter. $\widehat{\lambda}_{it2} \equiv \lambda(x_i \widehat{\delta}_2)$ referred to as the inverse Mills ratio, where x_i represents the vector of variables in the selection equation and $\widehat{\delta}_2 = \Pr(\ln i_{it} \gamma \text{ observed})$). The selection equation includes all variables from the interest rate equation, in addition to the neighbor and log capital city population variables.

$$\ln i_{it} = \beta_0 + \beta_1 city_i + \beta_2 const_{it} + \beta_3 city_i const_{it} + \beta_4 \ln r_t + \beta_5 \ln urb_{it} + \beta_6 \widehat{\lambda}_{it2} + u_{it} \quad (4a)$$

$$\Pr(\ln i_{it} \gamma) = \Phi(\gamma_0 + \gamma_1 n_{it-1} + \gamma_2 \ln pop_{it} + \gamma_3 \ln urb_{it} + \gamma_4 \ln r_t + \gamma_5 city_i + \gamma_6 const_{it}) \quad (4b)$$

²⁶When I estimated the selection model assuming that sample selection both before and after time t^* was determined by the selection equation below, the overall results were nearly identical to those obtained from the OLS estimates in the previous section.

Table 3 provides maximum likelihood estimates of the selection model. As before, I consider a specification with each constitutional variable separately, first without and then including an interaction term. In all cases the estimate of the selection parameter $\widehat{\lambda}_{it2}$ is negative and in most cases statistically significant. This is what one would expect based on the theoretical discussion in Section 2, as it implies that any unobserved factor that increases the likelihood of a state being rationed will be associated with an estimated increase in the equilibrium interest rate. The results also imply that the OLS estimates for the coefficient on the city-state dummy variable are biased upward (are less negative).

The estimates of the interest rate equation suggest that city-states paid consistently lower rates of interest on their loans, and the magnitude of this effect is now substantially larger than in the case of the OLS estimates. Based on the first specification, setting other variables at their means a non-city state would be expected to borrow at 10.8% interest, and a city-state at 7.8%. This advantage for city states is also reasonably close to that described by Pezzolo (2004, 2003), to that observed by Velde and Weir (1992) who compared England and France during the late eighteenth century, and by that observed for Stasavage (2005) when comparing Great Britain post-1688 when comparing periods of Tory and Whig control. In this first regression we also observe that expected interest rates overall are higher than in the OLS estimates, which fits with the assumptions of the credit rationing model presented in Section 2.

The *city* dummy variable continues to be statistically significant in three of the four specifications without the interaction term (the exception here is when the *republic* dummy variable is used). Based on these estimates, there continues to be relatively little evidence that constitutional constraints are associated with lower borrowing costs irrespective of state size. We do, however, see fairly consistent evidence of an interaction effect between constitutional constraints and state size. Using three of the four constitutional variables city-states in regions where constitutions constrained rulers are estimated to have been able to borrow at lower rates of interest than city states in other areas. Taking the specification using *coercion* and including the interaction term as an example, if we set other variables at their sample

means, then a territorial state in a region where *coercion*= 0 is predicted to borrow at 11.1%, a city-state in the same type of region would be predicted to borrow at 7.8%, and a city-state in a region where *coercion* is weak (=1) would be estimated to borrow at 6.7% interest.

Since the political hypotheses presented in Section 2 involve predictions about both interest rates and credit rationing, the selection equation in the model actually provides further important empirical tests, rather than simply controlling for potential bias. We see a consistent pattern where city-states are significantly less likely to be rationed than are larger territorial states. In addition, we also see greater support for the constitutional checks and balances hypothesis than is apparent when considering only the determinants of interest rates. In several of the specifications for the selection equation the linear term for constitutional regime has a positive and statistically significant coefficient. The substantive magnitude of both of these effects is sizeable and comparable. Setting other variables at their means, a state which is not a city-state and which has an “unfree” constitutional regime is estimated to have a 24% probability of appearing in the sample. In contrast, city-states in “unfree” areas and non-city states in “free” areas have 44% and 40% probabilities of appearing in the sample. Finally there is also evidence of an interaction effect between state size and constitutional regime, as a city-state in a “free” region has an estimated 62% probability of appearing in the sample.

7 Selection involving endogeneity of institutions

The empirical tests have so far considered whether my conclusions regarding city-states and constitutions are robust to the inclusion of control variables, as well as 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 with regard to city-states are driven by unobserved factors (political, economic, or other) that lead simultaneously both to the development of city-states in certain areas and to easier sovereign access to credit at low interest rates. One way to deal with this potential "self-selection" bias would be through instrumental

variables estimation. This would require identifying some factor that was associated with the development of city-states but which is uncorrelated with the unobserved factors referred to above, and which therefore is redundant for explaining the interest rates at which states borrowed. It is not an obvious task to find suitable instruments that would satisfy this requirement. If it is not feasible to find a suitable instrument for city state development, then one alternative is to formulate a selection model using variables that are believed to be good proxies for the unobserved factors that influence both city state development and the interest rate. In other words, rather than finding variables that are uncorrelated with the unobserved factors, we need to identify variables that are highly correlated with the unobserved factors.²⁷ These variables can be used to estimate the probability of being a city-state, and then this estimated selection effect can be included as an additional parameter in the interest rate equation.

The key for estimating the selection model is to find one or more variables that influence city-state development, but which are redundant for explaining current interest rates. One possibility here is to consider the role of initial conditions in leading to the development of city states, as has been emphasized by historians like Pirenne (1925) and historical sociologists like Tilly (1990). First of all, the initial constitutional regime of a country may have influenced whether cities developed and become independent states. If the constitutional regime initially placed strong checks on a ruler, then this may have created opportunities for cities to assert their independence. In terms of other initial conditions, one might suggest that the initial degree of urbanization and the level of literacy in a country might also have had an impact on city state development. The initial degree of urbanization is particularly emphasized by Tilly (1990) who argues that different European regions embarked upon a path dependent process of development - those regions where the concentration of capital was high circa 1000AD were likely to develop city-states while other regions were not. Finally, I also

²⁷I should note that the substantive results of this section are not sensitive to the assumption that the initial institutions variables I use below are better suited to a selection model than to an instrumental variables estimation. I obtained quite similar results using the initial institutions variables in an IV estimation. See Wooldridge (2002) chapter 18 for discussion of the differences in assumptions between IV and selection models in this type of case.

include two other factors in the selection equation. A dummy variable is included for port cities, based on the idea that greater trade opportunities might create opportunities to assert political independence. Finally, in the selection equation I also include a variable to capture the differential degrees of Roman heritage in European regions. 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. This would have clear effects on city development and politics.

The full specification for the selection model is presented below - the interest rate equation again remains identical to the specification used in the previous sections, with the exception of the inclusion of the additional selection term $\widehat{\lambda}_{it2}$, which now depends on the estimated hazard rates for being a city-state and for being a larger territorial entity.²⁸

$$\ln i_{it} = \beta_0 + \beta_1 city_i + \beta_2 const_{it} + \beta_3 city_i const_{it} + \beta_4 \ln r_t + \beta_5 \ln urb_{it} + \beta_6 \widehat{\lambda}_{it2} + u_{it} \quad (5a)$$

$$\Pr(city_i) = \Phi(\gamma_0 + \gamma_1 const_{1200} + \gamma_2 \ln urb_{1200} + \gamma_3 lit_{1300} + \gamma_4 port_i + \gamma_5 rom_i) \quad (5b)$$

The results of the self-selection model are presented in Table 4. In each of the three specifications the coefficient on the city-state dummy variable is negative, and it is statistically significant.²⁹ The magnitude of the coefficients is similar to those for the city variable in

²⁸ I use the formulation for this term provided by Maddala (1983 pp.117-122). The estimation method depends upon the assumption that the error term in the interest rate equation and the unobserved component of the selection probability (after controlling for the other selection determinants including here) are jointly normally distributed.

²⁹ I did not include a regression using *republic* in this table, because this variable does not vary over time, and thus it cannot be used to investigate whether a state that was initially a republic was likely to become a city-state. As discussed in Section 3 it should also be remembered that the vast majority of republics in the sample were city states (though the reverse was not true).

the Table 3 estimates. This is strong evidence that the tendency for city-states to be able to borrow at lower rates of interest was not produced by the fact that certain initial conditions led both to the development of city-states and to the conditions for credible sovereign borrowing. In the interest rate equation in Table 4 we continue to observe that the constitutional variables *free*, *coercion*, and *protect* are not statistically significant. Finally, the estimate of the coefficient on $\hat{\lambda}_{it2}$ is positive in all three cases, though only significant in the third regression in the table. For the first two specifications this suggests that the OLS estimates presented in Table 2 are not biased by the failure to control for “self-selection” of city states.

The estimates of the selection equation in Table 4 are also worth consideration. As one might suspect, the initial level of urbanization is a very strong predictor of becoming a city-state, as is being a port and having a longer period of Roman control (based on the first two regressions). There are also indications from these regressions that initial political conditions may have had a significant impact on the development of city states. While the coefficient on the initial constitution variable is not statistically significant when using *free*, it is significant when using *coercion* and *protcap* as indicators. In addition, the substantive magnitude of this effect is large. Setting other variables at their means, in a region where coercion initially dominated, we would expect there to be a 40% chance of a city-state emerging. In a region where coercion was initially weak we would expect there to be a 70% chance of a city-state emerging. A similar result obtains for the *protect* variable. It is worth re-emphasizing that even when controlling for this selection effect, we still continue to observe that city-states were able to borrow at lower rates of interest.

While I did not report them here, I also considered the possibility that a region’s current constitutional regime depended upon initial conditions, using the same set of selection variables reported in Table 4 (dropping the initial constitution variable). After controlling for “self-selection” of the constitutional regime the results of the interest rate equation remained very similar to those reported so far - city states continue to be associated with lower borrowing costs while the constitutional regime did not.

8 Conclusion

In this paper I have considered two alternative hypotheses about the politics of sovereign borrowing in Europe over the very long run. First, access to credit at low interest rates may have depended above all on the creation of constitutional limitations on rulers. This is the type of explanation emphasized by the numerous recent papers on institutions and commitment. Alternatively, it may have been the case that borrower credibility depended above all on the extent to which merchants had political power, and merchants were more likely to hold power in city-states than in larger territorial entities. My empirical results provide strong support for the second hypothesis and relatively little evidence for the first. We observe that city-states consistently borrowed at lower rates of interest than did larger states, and they were also more likely to begin borrowing in earlier periods, implying that they had less likelihood of being rationed out of the debt market. In contrast, constitutional checks and balances appear to only have had an effect within the group of city states in the sample. City-states with republican institutions and city-states in regions where princely overlords faced institutional constraints on their authority were able to borrow at lower rates of interest than city-states elsewhere. But there is much less indication that the constitutional regime made a difference in larger territorial entities. My empirical results have clear implications regarding the possibility of institutional engineering, that states can adopt new political institutions in order to credibly commit to repaying their debt. My findings suggest that establishing or reinforcing constitutional checks and balances will only have an impact on credibility in a subgroup of states, those where lenders to government are more likely to have political power to begin with. Reforming constitutions elsewhere would be expected to have little effect.

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Table 1: States by classification and period of entry into debt market

City states	Larger 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: OLS Estimates of Borrowing Costs

	Constitutional Measure							
	Free		Coercion		Protect		Republic	
city	-.198 (.101) .053	-.054 (.159) .736	-.216 (.101) .035	-.277 (.094) .004	-.194 (.105) .067	-.278 (.264) .296	-.110 (.097) .257	.076 (.155) .626
constitution	.065 (.085) .443	.254 (.156) .107	-.023 (.047) .626	.123 (.094) .195	.061 (.039) .125	.049 (.053) .356	-.298 (.089) .001	-.047 (.166) .777
city*const		-.371 (.249) .140		-.275 (.153) .076		.042 (.102) .682		-.421 (.224) .063
ln r	1.43 (0.19) .000	1.52 (0.22) .000	1.51 (0.20) .000	1.69 (0.25) .000	1.44 (0.19) .000	1.40 (0.23) .000	1.64 (0.21) .000	1.69 (0.22) .000
lnurban	-.120 (.056) .036	-.157 (.073) .035	-.135 (.058) .021	-.193 (.078) .016	-.162 (.066) .016	-.163 (.066) .017	-.136 (.060) .026	-.186 (.072) .011
constant	-.481 (.345) .166	-.621 (.388) .114	-.551 (.350) .119	-.680 (.383) .080	-.487 (.341) .157	-.394 (.437) .370	-.677 (.366) .068	-.697 (.368) .062
R^2	0.45	0.47	0.45	0.44	0.45	0.45	0.50	0.43
prob>F	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
N=	94	94	94	94	94	94	94	94

heteroskedastic consistent standard errors in parentheses, p-values below

Table 3: Sample Selection - Decisions to Lend and Borrowing Costs

	Constitution measure							
	Free	Coercion	Protect	Republic				
Interest rate equation								
city	-.324 (.094)	-.212 (.137)	-.323 (.092)	-.361 (.093)	-.300 (.093)	-.418 (.244)	-.112 (.101)	.094 (.142)
constitution	.003 (.091)	.136 (.133)	-.042 (.050)	.102 (.086)	.068 (.042)	.046 (.051)	-.300 (.093)	.015 (.173)
city*const		-.254 (.227)		-.259 (.139)		.067 (.098)		-.604 (.278)
ln r	2.01 (0.37)	2.03 (0.39)	2.04 (0.39)	2.16 (0.38)	1.95 (0.35)	1.80 (0.50)	1.65 (0.31)	2.07 (0.44)
lnurban	-.149 (.064)	-.173 (.075)	-.171 (.069)	-.219 (.080)	-.210 (.078)	-.207 (.087)	-.138 (.071)	-.240 (.087)
Selection equation								
city	.566 (.228)	.355 (.363)	.548 (.223)	.573 (.216)	.546 (.222)	1.36 (0.82)	.158 (.268)	-.433 (.305)
constitution	.456 (.217)	.196 (.349)	.241 (.138)	-.060 (.232)	-.104 (.111)	.024 (.189)	1.16 (0.31)	.219 (.561)
city*const		.465 (.507)		.481 (.302)		-.372 (.330)		1.40 (0.60)
lnurban	.156 (.154)	.195 (.185)	.190 (.154)	.252 (.175)	.230 (.176)	.230 (.194)	.128 (.176)	.317 (.206)
ln r	-3.05 (0.64)	-3.10 (0.66)	-3.05 (0.65)	-3.17 (0.68)	-2.64 (0.58)	-2.55 (0.62)	-2.64 (0.74)	-2.93 (0.69)
lnpop	.405 (.092)	.402 (.098)	.421 (.102)	.402 (.103)	.382 (.084)	.428 (.121)	.661 (.120)	.537 (.158)
neighbors	-.162 (.315)	-.212 (.328)	-.273 (.329)	-.292 (.340)	-.178 (.348)	-.089 (.449)	-.113 (.412)	-.113 (.376)
λ	-.363 (.154)	-.350 (.177)	-.347 (.166)	-.332 (.161)	-.346 (.151)	-.291 (.251)	-.009 (.167)	-.283 (.213)
s.e. λ								
prob>chi ²	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
N=	214	214	214	214	214	214	214	214

maximum likelihood estimates, heteroskedastic consistent standard errors in parentheses

Table 4: Selection Involving Institutional Endogeneity

	Constitution measure		
	Free	Coercion	Protect
Interest rate equation			
city	-.254 (.125)	-.354 (.133)	-.410 (.131)
constitution	.053 (.087)	.041 (.046)	.060 (.037)
$\ln r$	1.43 (0.19)	1.55 (.208)	1.37 (0.17)
$\ln \text{urban}$	-.107 (.056)	-.113 (.054)	-.109 (.061)
Selection equation for city			
Initial constitution	-.458 (.344)	.380 (.201)	5.42 (0.77)
Roman	-.0034 (.0020)	-.0030 (.0019)	.009 (.002)
initial literacy	-.285 (.119)	-.405 (.115)	-2.42 (0.36)
initial urbanization	1.10 (0.32)	1.34 (0.34)	1.78 (0.49)
port city	.526 (.282)	.845 (.307)	-.170 (.411)
λ	.044	.111	.272
s.e. λ	(.092)	(.103)	(.098)
prob>chi ²	<0.01	<0.01	<0.01
N=	94	94	94

maximum likelihood estimates, heteroskedastic
consistent standard errors in parentheses